Do Banking Crises Improve Democracy?

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Abstract:
We study the impact of banking crises on the level of democracy. We use an event-study method on a sample of up to 129 countries over the period 1975-2010 accounting for 94 systemic banking crises. We find that banking crises contribute to improve democracy. The bulk of the improvement takes place between 3 and 10 year after the banking crisis. The impact of a banking crisis is greater in non-democratic countries and when the banking crisis is severe. We explain this finding by the fact that banking crises create windows of opportunity to contest autocratic regimes in line with Acemoglu and Robinson (2001).

JEL Codes: D72, H11

Keywords: banking crisis, democracy.

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1. Introduction

Banking crises have been widely investigated in the literature. A huge amount of studies has identified their causes (e.g., Kaminsky and Reinhart, 1999; Schularick and Taylor, 2012), but also their consequences. Empirical literature has shown that banking crises contribute to weaken bank lending (e.g., Ivashina and Scharfstein, 2010) which turn in employment decline (e.g., Chodorow-Reich, 2014) and generate output loss (e.g., Hoggarth, Reis and Saporta, 2002).

However the political consequences of banking crises have been much less analyzed than their economic consequences with only a handful of studies investigating this area. Gries and Meierrieks (2013) find that banking crises are associated with greater terrorist activity in developing economies, while Gutmann, Pfaff and Voigt (2017) show that banking crises deteriorate human rights, in particular in non-democracies. Andersson (2016) observes that major banking crises enhance the market orientation of economic institutions and the stability and accountability of political institutions.

A straight question that arises is if banking crises influence democratization. It is a major issue to appraise the political consequences of banking crises, given the key importance of the regime for a country, but also their long-term economic consequences since democracy has been shown to foster growth (Acemoglu et al., 2018). Several views can be proposed on the influence of banking crises on democratic change.

The first view suggests a positive influence of banking crises on democratic change. It is based on the argument from Acemoglu and Robinson (2001) that negative economic shocks reduce the cost of fighting autocratic regimes, generating a window of opportunity to contest power prompting the autocratic elite to make democratic concessions. In line with Acemoglu and Robinson's (2001) intuition, recent empirical work has observed that economic recessions encourage democratic change (Burke and Leigh, 2010; Brückner and Ciccone, 2011). Thus banking crises being major negative shocks for the economy can serve as a triggering event in opposing autocracy.

The second view predicts the negative influence of banking crises on democratic change. First, the resolution of banking crises may necessitate painful policies like the closure of banks and losses for depositors which are difficult to implement in democracies. It can then lead to a democratic breakdown. Second, financial crises can generate extremist political opinions which are antagonistic to democratization. In a study on 20 advanced democracies

from 1870 to 2014, Funke, Schularick and Trebesch (2016) observed that financial crises lead to greater votes for far-right parties. This political radicalization takes place after financial crises but not after normal recessions or severe macroeconomic shocks without financial contents, which stresses the specific political effects of financial crises. The interpretation is that financial downturns would be considered as less excusable than other crises because they would be perceived as the result of policy failures. As such, financial crises would be detrimental for democracy. Third, banking crises give autocrats incentives to enhance repression to intimidate citizens and thus can reduce democracy. Gutmann, Pfaff and Voigt (2013) explain their finding of a negative impact of banking crises on human rights by the fact that autocrats have two reasons to increase repression during banking crises. On the one hand, increased unemployment generates greater grievance in the population which can lead to a revolution. So autocrats must intimidate citizens through enhanced repression. On the other hand, banking crises reduce the ability of autocrats to buy loyalty by giving perks. Therefore, since autocrats use loyalty and repression to stay in power, lower availability of loyalty leads them to foster repression.

Surprisingly the influence of banking crises on democracy remains an unsolved question in the literature. To fill this loophole, the objective of this paper is to examine how banking crises can influence democratic change. To this end, we estimate the effect of banking crises on democracy for a sample of 116 countries from 1975 to 2010. We combine data on democracy index from the Polity IV database with data on banking crises from Laeven and Valencia (2012) to create our dataset. We perform an event study by regressing democracy index on a series of dummy variables coding the date of banking crises. We are then able to examine the timing of the effects of banking crises on democratic change. We furthermore analyze if the severity of banking crises exerts an impact on democratic change. We define the severity of banking crises according to their duration and to the peak of non-performing loans.

Our primary finding is that banking crises contribute to enhance democracy. The bulk of the improvement takes place between 3 and 10 years after the banking crisis. We observe no anticipation effect. These results are robust to a battery of robustness tests, including alternative variables and techniques. Furthermore, we find that a more severe banking crisis has a higher impact on democracy. We explain these findings by the fact that banking crises create windows of opportunity to contest autocratic regimes and consequently favor democratic improvement in line with the view of Acemoglu and Robinson (2001).

With this work, we contribute to three strands of literature. First, we extend the literature on the consequences of banking crises in the direction of democratization. We can then add to the handful of works considering the political effects of banking crises by helping understanding how these crises shape political regimes. Second, we contribute to the burgeoning literature on the impact of banking on political outcomes. Recent works have focused on how lending of state-owned banks can be used to influence elections (Dinc, 2005, Carvalho, 2014, Englmaier and Stowasser, 2017). We extend this literature in the direction of the effects of banking crises. Third, we augment the literature on the causes of democratization by examining the impact of banking crises. The debate on the causes of democratization is far from settled with some arguing that democratization is unpredictable by nature (Kuran 1989, 1991) while others support the influence of some factors (e.g., Acemoglu and Robinson, 2001).

The paper proceeds as follows. Section 2 presents the methodology. Section 3 reports the baseline results. Section 4 displays additional estimations. Section 5 concludes.

2. Methodology

2.1 Econometric Specification

To study the impact of banking crises on democracy, we use an event study method. The first step to perform such an analysis is to define the event around which we want to measure the evolution of the dependent variable. In our case, the event is defined as the start of a financial crisis.

We then study the evolution of the level of democracy around the event by comparing the level of democracy before and after the start of a crisis. To do so, we estimate the following equation:

$$Demo_{it} = \alpha Demo_{it-1} + \beta BC_{it} + \sum_{j=1}^{J} \theta_j X_{jit} + \delta_i + \mu_t + \varepsilon_{it} \quad (1)$$

Where $Demo_{it}$ is country *i's* level of democracy in year *t*. BC_{it} is our variable of interest. It is a dummy variable that is equal to one for country *i* in years $t + d$, ($d = 0,\ldots,10$) if country *i* experienced a systemic banking crisis that started in year *t*. BC_{it} is equal to zero otherwise. X_j is a set of control variables. δ_i and μ_t respectively represent country fixed effects and year

fixed effects. ε_{it} is the error term. The lagged dependent variable is included on the right-hand side of Equation (1) to control for persistence in democracy.

The choice of an event window ending 10 years after the start of a banking crisis trades-off the possibility to analyze the long-term consequences of banking crises and the length of the period of study: the longer the study-window the shorter the estimation period. Specifically, if we measure the impact of banking crises T years after the start, we lose T years of observations at the beginning of the sample period. On the other hand, a small value of T limits our time horizon.

Because we control for both country- and year-fixed effects in addition to the persistence in democracy, the coefficient β measures the annual change in the level of democracy that a country experiences after the start of a banking crisis. In other words, the method measures the effect of a treatment consisting in experiencing a financial crisis. The treated group is the group of countries that have endured a crisis while the control group consists of the countries that have not.

2.2 Data

Our dependent variable measuring democracy is the polity2 democracy score. This variable comes from the Polity IV database of Marshall, Gurr and Jaggers (2016). They code political regimes by combining different scores based on the competitiveness of political participation, the openness and competitiveness of executive recruitment, constraints on the chief executive, and the regulation of participation. The scores are then converted to be adapted to time-series analysis to form the polity2 variable.[1] The resulting polity2 scores range from -10 to +10. [2] Highest scores indicate most democratic regimes while low scores indicate most autocratic regimes.

To identify systemic banking crises, we use the dataset constructed by Laeven and Valencia (2008, 2012). This database is commonly used in works on banking crises (e.g., Caballero, 2014). It identifies a banking crisis in a country in a year when two criteria are met: the financial and real sectors experience a financial distress, and significant policy measures

[1] We test the robustness of our results when using another measure of democracy in the robustness check section.
[2] We use the polity2 variable with a slight modification. The database assigns a zero score for polity2 in periods where politics cannot exercise effective authority over at least half of their territory due to foreign intervention. These are the so-called interregnum periods. For our analysis, we treat these observations as missing. The reason is that since we are interested in the evolution of democracy, we want to avoid misinterpretation of improvement or deterioration relative to preceding years. For instance, suppose a country has a negative score of democracy in year *t-1*. Then, if that country experiences a foreign intervention in year *t*, we will interpret it as an improvement in democracy according to the initial database. The reverse holds for a country that has a positive score of democracy in year *t-1* and a foreign intervention in year *t*. We thus decide to treat observations related to interregnum periods as missing values. This treatment concerns 70 out of 4680 observations in our sample.

are put in place in order to limit the real consequences of the banking crisis.

The financial and real sectors experience a financial distress when the banking system encounters heavy losses (when the share of non-performing loans exceeds 20%) and/or bank liquidation takes place, in addition to considerable bank runs (when deposits drop by more than 5% from one month to another).

To consider that significant policy measures are put in place, at least three out of six following conditions must be fulfilled:

- An extensive liquidity support. This condition is satisfied if the liquidity support is implemented directly by the Treasury, or if the ratio of central bank claims on the financial sector to deposit and foreign liabilities exceeds 5%, or more than doubles compared to its pre-crisis level;
- Bank restructuring takes place as a result of the banking system difficulties so that the gross fiscal costs of banks' restructuring exceed 3% of GDP.
- Assets purchases by the Treasury or the central bank exceeds 5% of GDP.
- Bank nationalisation;
- Guarantees on banks' liabilities (full protection or extended to non-deposit liabilities);
- Deposit freezes and/or bank holidays.

One advantage of Laeven and Valencia database is that it identifies only systemic banking crises. Conversely isolated banking crises that do not spread to the whole system are not considered. This is important for our study since minor crises can be too limited to affect the political regime.

In Laeven and Valencia's (2012) dataset, a systemic banking crisis ends in year t when both real GDP growth and real credit growth are positive for two consecutive years for year *t+1*. In case real GDP growth and real credit growth are positive for the first two years, then the end and start years are identical. When information on credit growth is missing, only real GDP growth is considered in the definition. In all cases Laeven and Valencia (2012) limit the maximum duration of a banking crisis to 5 years, starting from the first year of the crisis.

Out of the 129 countries in our sample, 79 experienced at least one systemic banking crisis over the period 1975-2010, for a total number of 94 systemic banking crises. Table A1 in appendix lists the countries considered in our study as well as the banking crises they experienced during our period of study.

Since we want to analyze the democracy level 10 years after a banking crisis started, we have to delete observations 10 years at the beginning of our sample (T=10) for the

estimations. Though we restrict our sample period to 1975-2010, we can use Laeven and Valencia's dataset to check the absence of any systemic banking crisis during the period 1970-1975. The first banking crises in that database happened in 1976 (in Central African Republic and Chile). But we cannot use Laeven and Valencia's dataset to go back further in time. We thus make use of Reinhart and Rogoff (2009) dataset to investigate the occurrence of banking crises over the period 1965-1970. This widely used dataset of banking crises is less restrictive than the previous one as it considers all banking crises, including non-systemic ones.

Reinhart and Rogoff's (2009) dataset informs that there were no banking crises over the period 1965-1970[3]. Therefore, requiring that T=10 is equivalent to deleting 10 years of observations at the beginning of our sample period, while analyzing long-term consequences at the same time.

Some countries experienced several systemic banking crises over the sample period. In our dataset, this accounts for 15 countries. For these countries, it is confusing to define the pre-crisis and post-crisis periods, especially after the first systemic banking crisis episode. The problem arises only for countries whose time span between the start dates of two successive crises is less than 10 years. In fact, the post-crisis period exceeds 10 years for these countries. The choice of T=10 allows us to reduce this problem. However, as a robustness check, , we will present the results for a sample that excludes countries with more than one systemic banking crisis so that we make sure that this coding logic for these countries does not affect our results.

A country that needs a particular discussion is Brazil. According to the Laeven and Valencia (2012) dataset, this country experienced two successive banking crises: from 1990 to 1994 and from 1994 to 1998. In our analysis, we consider this country as having a single banking crisis that lasts from 1990 to 1998.

Though we measure democracy until 2010, we do not consider the impact of the 2009 Global Financial Crisis due to data limitations. We cannot observe the democracy index for the following 10 years.

We will estimate specifications (1) and (2) using the dataset we describe above by ordinary least squares with robust standard errors clustered at the country level. Alternative estimation techniques including a dynamic dependent variable estimator, an instrumental variable estimator, and treatment effect models, are used as robustness checks.

[3] We have to stress that considering a larger T requires further hypotheses about the realization of systemic banking crises over the period before 1970.

3. Results

3.1 Baseline Results

Table 1 reports our baseline results. We perform three estimations. We first estimate the equation (1) for the whole sample. We then exclude countries that experienced several episodes of banking crises since the post-crisis period may exceed 10 years for this subgroup of countries. Finally we exclude the Global Financial Crisis by considering only years until 2007.

The key finding is the positive and significant coefficient for the banking crisis variable. This result is observed in all regressions. Therefore, it stands when we consider the full sample but also when we exclude countries that several banking crises and when we exclude the Global Financial Crisis, showing robustness to this result. Thus our main conclusion is that the occurrence of a banking crisis contributes to favor democracy in the ten following years. A banking crisis is thus beneficial for democratic change. In other words, our findings support the view from Acemoglu and Robinson (2001), according to which negative economic shocks create windows of opportunity to contest autocratic regimes and thus favor democratic improvement. It accords with the results by Burke and Leigh (2010) and Brückner and Ciccone (2011) that economic recessions encourage domestic change.

The size of the coefficient varies from 0.196 to 0.219 across the estimations, with 0.196 for the whole sample. It means that the annual increase in democracy index following a banking crisis is about 0.20.

Furthermore we observe that the lagged dependent variable for the democracy index is positive and significant at the 1% level. The coefficient is lower than one. Therefore the democracy index is persistent and converges.

3.2 Robustness Checks

We check the robustness of our results with a battery of tests.

3.2.1 Alternative specifications

First, we exclude former socialist countries from the sample. These countries have experienced both a change in their political system and a wave of democratic improvements. In parallel, they also experienced a change in their economic system which was followed by a period of turbulence in their banking system. One can thus question whether that the improvement in democracy that we observed in our baseline results is driven by former

socialist countries, but that it is not a general consequence of banking crises. In our baseline specification, time and country fixed effects take into account this consideration. Nonetheless, we check the robustness of our results by excluding this sub-sample of countries. The results are reported in Column (1) of Table 2. We observe that the coefficient of the banking crises dummy is still positive and significant. This finding confirms that our baseline results is not driven by the sample of former socialist countries.

Second, we control for the international aspect of democratic improvements to make sure that we capture domestic pressures towards democratic reforms. The reason is that many democratic changes appear in the form of waves where a country's democratic change follows that of a cohort. The Arab Spring is an illustration of this movement. To control for the international aspect of democratic change, we compute a regional index of democracy as follows. We start by identifying geographic regions in our sample based on the World Bank's classification. Then, for a *i* country located in a given region *j*, we compute the average democracy score for the remaining countries in that region. We call this variable "regional democracy" and we introduce it among explanatory variables. Column (2) of Table 2 displays the corresponding results. We find that the regional democracy variable is significantly positive. This means that a country's democracy moves in the same direction as the democracy of its region, which implies that democratic changes occur in waves. However the key finding is that the coefficient of the banking crises dummy remains positive and significant, confirming our baseline results. In other words, the improvement in democracy that we observe in the 10 years following the start of a banking crisis mainly comes from domestic pressures.

Third, we use an alternative measure of democracy as a robustness check. We rely on the V-Dem (varieties of democracy) project (Coppedge et al., 2016) which provides a multi-dimensional and disaggregated dataset on democracy measures. Democracy measures compiled in the V-Dem project are based on not only factual information from official documents, but also on subjective assessments on topics like political practices and the compliance with de jure rules.

We use the five high level principles of democracy (electoral democracy, liberal democracy, participatory democracy, deliberative democracy, egalitarian democracy) and civil liberties as alternative measures of our democracy of V-Dem. The exact definition of each principle is given in Appendix A. The results are reported in the five last columns of Table 2 by considering alternatively each aspect of democracy.

We find that among the different aspects of democracy, only egalitarian and civil

liberties are affected by banking crises. On the contrary, there is no systematic effect of banking crises on electoral, liberal, participatory and deliberative democracies. So this robustness check suggests that banking crises would exert an influence on democratic change for some aspects of democracy.

Fourth, we control for other income shocks. Namely one can question our identification strategy in the sense of what we interpret as the effect of a financial crisis is in fact the outcome of other income shocks. To this end, we consider three types of shocks: large income shocks, sovereign debt crises, and currency crises. For each type of shock, we consider a different treatment: the treatment for large income shocks refers to the treatment group for countries that experienced large income shocks, and so it is for sovereign debt crises and currency crises.

By defining a different treatment for each type of income shocks, we can check if one of these three types exerts an influence on the relation between the occurrence of a banking crisis and the level of democracy. If banking crises have an effect on democracy which is independent of other income shocks, their statistical significance will still be observed even after controlling for these other shocks. The treatment of each shock follows the same logic of the banking crisis dummy. That is, we first identify the date of their occurrence. Then we code a dummy variable that takes the value of one in the 10 years that follow their occurrence. Otherwise it takes the value of zero.

To identify large income shocks, we rely on Bluhm et al. (2018): they characterize economic slumps as "interruptions of a positive growth regime by a sharp downward shift coinciding with a sequence of two trend breaks". The identification of sovereign debt crises and currency crises is based on Laeven and Valencia (2012). They define sovereign debt crises as years of a sovereign default to the private sector, while currency crises are defined as a nominal depreciation of the currency of at least 30% that is also at least a 10% increase in the rate of depreciation compared to the previous year.

The results are reported in Table 3 with each of the three columns adding the dummy variable for one type of income shocks. We observe that the coefficient of banking crises is still significant and positive in all three specifications. Therefore banking crises have an independent effect on democracy which is not captured by other income shocks. Interestingly we also point out that only sovereign debt crises also have a positive impact on the level of democracy, while the effect of large income shocks and currency crises is not significant.

Thus these estimations show that banking crises have an impact on democracy that is independent of other income shocks.

3.2.2 Alternative Estimation Techniques

We now turn to robustness checks focusing on alternative estimation techniques. We successively use a dynamic dependent variable estimator, an instrumental variable estimator, and treatment effect models.

First, we utilize a dynamic dependent variable estimator. The presence of the lagged dependent variable among the explanatory variables combined with country fixed effects makes the within estimators inconsistent. This is the Nickell bias in dynamic panel data model. However this bias is expected to be limited in our case because we have a large time horizon. Nonetheless we deal with this issue by using a dynamic panel data estimator, the Blundell and Bond estimator (SYS-GMM). We only perform it as a robustness check rather than in the baseline estimations since the SYS-GMM estimator is ill-adapted to dummy variables. Given that we have a large time period, we collapse instruments to avoid the issue of instruments proliferation pointed out by Roodman (2009). We use the two-step estimator with small sample correction.

The results are displayed in the first three columns of Table 4. We test the three same specifications than in the baseline estimations: on the whole sample, by excluding countries with several episodes of banking crises, and by excluding the Global Financial Crisis.

We observe that the banking crises dummy is significant and positive in the three specifications. We thus confirm our baseline results according to which the democracy index improves in the ten years following the start of a banking crisis.

We also report the test related to first-order serial correlation in level by looking at the second-order correlation in differences. We cannot find any evidence of second serial correlation in the error term. We also report the Sargan/Hansen test of instruments' exogeneity. They all point to the exogeneity of the instrument (with the exception of the Sargan test in the specification excluding the Global Financial Crisis).

Second, we address the potential endogeneity concern by re-estimating our baseline specification using an instrumental variable strategy. Our baseline results lead unbiased estimates if banking crises are exogenous. Our analysis of the timing in the next section does not provide evidence of anticipation effects, which suggests that endogeneity is not a major concern in our case. However, it is difficult to rule out endogeneity completely. One may

argue that banking crises are not exogenous since they can be predicted. In line with this view, Acemoglu et al. (2003) and Blum et al. (2018) provide evidence that weak institutions are associated to economic crises.

To tackle this problem, we instrument the banking crises dummy variables with capital inflow surges. This variable was used as an instrument in Gutmann, Pfaff and Voigt (2017) and it is found to be a significant predictor of systemic banking crises (Reinhart and Reinhart, 2013; Reinhart and Rogoff, 2008; Caballero, 2014).

We extract data on capital flow bonanzas from Reinhart and Reinhart (2008). The authors code episodes of sudden increase in international capital inflows, also called bonanzas, for 181 countries from 1980 to 2007. When a country experiences a deterioration of current account that is higher than a 20^{th} percentile threshold, it is classified as undergoing a capital flow bonanza.

The validity of capital flows bonanzas as instrument relies on the assumptions that they are correlated with banking crises, and that they affect democracy level only via their impact on banking crises. Caballero (2012) shows that capital flows bonanzas increase the probability that a country experiences a financial crisis through the vulnerabilities it induces in the macroeconomic environment due either to excessive lending or to surges in portfolio-equity. Once we identify the date of capital flow bonanzas, our instrumental variable takes the value of one in the ten years that follow the start of a capital flow bonanza and zero otherwise.

The last three columns of Table 3 report the results for the two stage least squares estimator using capital flows bonanzas as instrument. We find again that the coefficient of the banking crises variable is positive and significant. Hence the estimations with instrumental variables corroborate our baseline results.

Third, we rely on treatment effect models to assess the link between banking crises and democracy. Specifically, the average treatment effects on the treated (ATET) where the treatment is ending a banking crisis and the outcome is democracy level can be written as follows:

$$\beta = E\left(Demo_{it}(1) - Demo_{it}(0) \mid BC = 1, X_{it}\right) \quad (2)$$

where *Demo(1)* denotes the democracy level of a country that experienced a banking crisis, and *Demo(0)* denotes the democracy level of the same country if it did not experience a banking crisis; X is a set of covariates.

Treatment effects estimators of β rely on the assumption of conditional independence. The latter stipulates that conditional on the selection of observables in the vector X, ending a

banking crisis is independent of the level of democracy. Put it differently, any other factors that affect the treatment must be independent of the potential levels of democracy and any other factors that affect the potential levels of democracy must be independent of the treatment.

The main difficulty in estimating β in the above expression is that we cannot observe *Demo(0)* for a country that has been subject to the treatment. The idea of potential outcomes framework is to build counterfactual level of democracy for countries that received the treatment so as to compare them to their observed democracy level after the treatment. This will produce a consistent estimate of β when the conditional independence holds.

Conceptually, since the potential outcomes are not observable, the estimation of treatment effects consists of a missing data problem. We solve it using different methods namely regression adjustment (RA), Inverse Probability Weighting (IPW), Doubly-robust (IPWRA), nearest-neighbor matching (NNMatch) and Propensity score matching (PSM) estimators. With these methods, we compute the counterfactual democracy as using lagged-values of democracy and time dummies. We use all these five methods and report the results in Table 5.

We start by using *regression adjustment* estimator *(RA)*. The idea of this estimator is to construct potential outcomes based on a regression analysis. The goal is to compare the democracy level of a country that experienced a banking crisis to its democracy level if it did not experience the banking crisis. Described by Jordà (2005), this method has been used by Acemoglu et al. (2014), Lacroix et al. (2017) among others. It proceeds in two steps: first, it fits a regression line of the outcome on a set of covariates. In our case, we construct potential democracy levels as a function of lagged-democracy and time dummies for the treated:

$$Demo_{it+s} = \alpha + \sum_{j=1}^{5} \gamma_j Demo_{it-j} + \lambda_t + \beta BC_{it} + \varepsilon_{it} \quad (3)$$

Then, the average treatment effects on the treated is the average of the difference between observed democracy and predicted democracy over the sample of the treated.

The results corresponding to regression adjustment are displayed in Column (1) of Table 5. They show that the estimated treatment effects banking crisis on democracy is positive and significant. It equals to 0.207, which means that on average, democracy level improves by 0.207 point in the 10 years that follow the occurrence of a banking crisis.

We continue with the *inverse probability weighting* estimator *(IPW)*. Its idea is to construct a treatment-effects estimator using weighted means with the weights given by the inverse probability of the treatment status. For each observation, we use a logit model to

predict the probability of experiencing and ending a banking crisis (P_{it}) based on lagged-democracy and time dummies. That is, we estimate the following equation:

$$P_{it} = \rho_0 + \sum_{j=1}^{5} \rho_j Demo_{it-j} + \lambda_t + \varepsilon_{it} \quad (4)$$

For countries who received the treatment, the weight is equal to the reciprocal of the predicted probability of treatment. For subjects who did not receive treatment, the weight is equal to the reciprocal of the predicted probability of not receiving the treatment.

Column (2) of Table 5 reports the estimated treatment effects when we use the inverse probability weighting method. The estimated treatment effect of banking crises on democracy is significantly positive with a point estimate of 0.201.

Next, we use the *doubly robust* estimator (IPWRA): it combines elements of both RA and IPW estimators. We use a linear regression model to predict the democracy levels of each country and a logit model to predict treatment status. Column (3) of Table 5 reports the results of Doubly Robust estimator. The estimated coefficient of treatment effects is again positive and significant with a point estimate equal to 0.202.

Finally, we use matching estimators. They are based on the comparison of the outcomes of subjects that are similar as possible with the sole exception of their treatment status. In our case, we could select a country which experienced a banking crisis and another country which has similar characteristics as the previous one but has not experienced a banking crisis. The data of each country serve as the potential outcome for the other comparable country. We can estimate the treatment effects by comparing both countries. The last 2 columns of Table 5 report the results of nearest-neighbor matching (NN Match) and propensity scores matching (PSM). The NN Match estimator shows that the treatment effect on the treated is significantly positive. Concerning the PSM estimator, we cannot find any systematic evidence of banking crises on democracy at conventional statistical significance levels.

Thus our results with treatment effect models overall confirm our baseline results that the democracy level improves in the 10 years that follow the start of banking crises.

4. Extensions

We have found that the occurrence of a banking crisis contributes to enhance democracy. However we can question whether this impact is the same in all cases. To this end, we extend our baseline results in three directions. First, we check if this impact is influenced by the severity of the banking crisis. Second, we investigate if it is conditional to

the initial level of democracy of the country. Third, we analyze the timing of the impact to see if it varies over time.

4.1 Do severe banking crises affect more democracy?

So far, we have treated all banking crises in the same way insofar as they have the same effect on democracy. However banking crises can differ in their severity (Claessens et al., 2012). Our baseline results point to the fact that banking crises bring about improvements in democracy index by opening a window of opportunity for democratic reforms. Yet, the literature on the political economy of reforms stressed that because the reforms may be associated with a reduction in the incumbents' benefits, there could be cases where the incumbent politician opposes the reform agenda (Haggard and Webb, 1993). In line with Anderson (2016), we now test the assumption that severe banking crises are more likely to breakdown the opposition to political reforms.

Therefore we check the conditional effect of a banking crisis on democracy, depending on the severity of the crisis. We do so by considering two alternative definitions of the severity of banking crises. With the first definition, a banking crisis is severe if it lasts more than three years, else it is moderate. With the second definition, the severity of a banking crisis refers to the peak of non-performing loans during the crisis. In that case, if the share of non-performing loans to the total assets of the banking system exceeds 40%, then the banking crisis is severe. Otherwise it is considered as moderate.

To compare the impact of severe banking crises relative to moderate ones, we consider two sets of dummy variables coding separately severe and moderate crises in the following equation:

$$Demo_{it} = \alpha_i + \mu_t + \gamma Demo_{it-1} + \beta_1 BC_{it}^{Severe} + \beta_2 BC_{it}^{Moderate} + \sum_{j=1}^{J} \theta_j X_{jit} + \varepsilon_{it} \quad (5)$$

BC_{it}^{Severe} is a dummy variable that is equal to one in the 10 years that follow the start of a banking crisis if that banking crisis was severe, and zero otherwise; likewise, $BC_{it}^{Moderate}$ is a dummy variable that is equal to one in the following 10 years of a banking crisis if that banking was moderate, and zero otherwise.

The reference group in Equation (5) still includes countries that did not experience a systemic banking crisis. We are interested in coefficients β_1 and β_2: they respectively measure the annual impact of severe and moderate banking crises on democracy level.

The first two columns of Table 6 report the results devoted to estimating Equation (5). In the first column, we define severity with respect to the duration; in the second column, we define it according to the peak of non-performing loans.

When using the definition of severity based on the duration of the banking crisis, we observe that the coefficient for severe banking crises is significant and positive while the coefficient for moderate banking crises is not significant. This result means that banking crises that last more than three years have a significant effect on democracy while banking crises that last less than three years do not affect democracy.

When considering the definition of severity related to the peak of non-performing loans, the coefficients of both severe and moderate banking crises are positive and significant. In other words, banking crises characterized by a peak of non-performing loans higher that 40% have the same impact on democracy as those characterized by a peak of non-performing loans lower than 40%.

Hence we find limited support for the hypothesis that severe banking crises are more likely to breakdown the opposition to political reforms and thus contribute to greater democratic improvements. It is observed for more severe banking crises in terms of duration but not in terms of the peak of non-performing loans.

4.2 Does the level of democracy matter?

Our baseline estimations consider all countries with no distinction regarding the country characteristics. Yet the success of a reform agenda may be conditional several factors including the willingness of the government and its ability to mobilize political support among others. These factors in turn characterize the type of political regimes to some extent. It is therefore of importance to analyze whether our result is influenced by the level of democracy of the country.

There are two opposing views on the impact of banking crises on democracy conditional to the level of democracy. On the one hand, there are three arguments supporting the view that this impact would be higher for non-democratic countries. First, non-democratic countries have more room for democratic improvements. Second, pressures for democratic improvements in non-democratic countries are not only internal, but also external, reinforced by international institutions. In line with that, Haggard and Webb (1993) stress that international institutions seek to influence domestic policies through loan conditions in developing countries, most of them being non-democracies. Third, authoritarian regimes can

be more successful in initiating reforms than democratic countries thanks to the time horizon of the politicians. Because reforms are usually associated with short term costs and long-term benefits, authoritarian leaders might find it easier to take a longer-term perspective because they face weaker interest-groups and electoral constraints (Haggard and Webb, 1993).

On the other hand, this view is challenged by several arguments according to which democratic countries would be more successful than non-democratic countries in improving democracy after a banking crisis. First, non-democratic countries are characterized by some limitations in freedom to contest the power, which is necessary for triggering democratic reforms (Andersson, 2016). Second, finding the necessary coalition to approve the reform agenda may take time in democratic countries compared to non-democratic countries (Haggard and Webb, 1993).

Thus the direction of the role played by the initial level of democracy for the impact of banking crises on democracy is theoretically unclear and remains an empirical matter. To investigate that role, we expand Equation (1) to include an interaction between the banking crisis dummy variable and the lagged democracy index. That is, we estimate the following equation:

$$Demo_{it} = \alpha Demo_{it-1} + \beta BC_{it} + \theta BC_{it} * Demo_{it-1} + \sum_{j=1}^{J} \theta_j X_{jit} + \delta_i + \mu_t + \varepsilon_{it} \quad (6),$$

Where BC_{it}*$Demo_{it-1}$ denotes the interaction between the banking crises dummy variable and the lagged democracy. In this way, Equation (6) allows the effect of banking crises to depend on democracy via the coefficient θ. The latter represents the incremental effect of banking crises on democracy due to the previous level of democracy.

Column (3) of Table 6 reports the results of the estimation for the Equation (6). We still observe that the coefficient of the banking crises dummy is positive and significant. We also point out that the coefficient of the interaction between this dummy and the lagged democracy is negative and significant. This means that the higher the previous level of democracy, the lower the positive effect of banking crises on democracy. We consequently find that the impact of banking crises on democracy is conditional to the level of democracy and we support the view that the democratic improvement following a banking crisis is greater for non-democratic countries.

4.3 Timing of the Impact

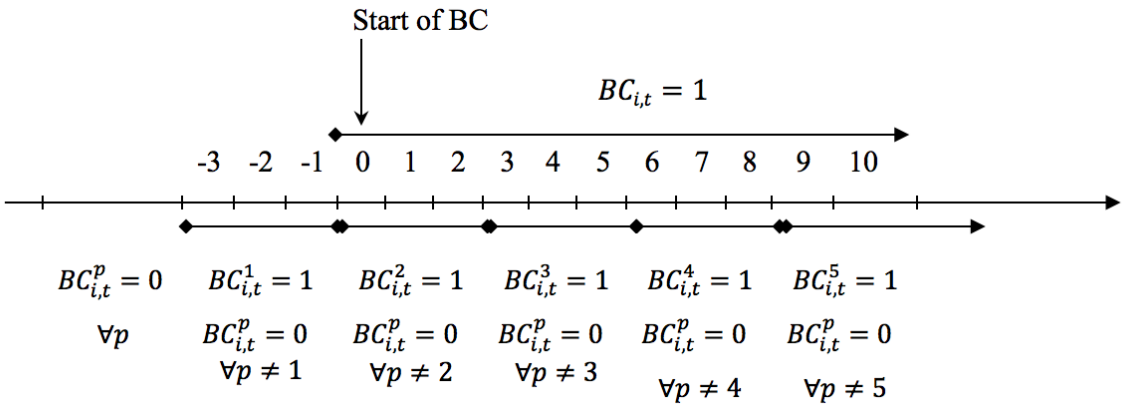
We now analyze the timing of the impact of a banking crisis on democracy level to check whether this effect varies over time. In the baseline estimations, we analyse this influence for the ten years following the beginning of a banking crisis. We can investigate the timing of this impact by decomposing the period following the banking crisis.

To this end, we do so by estimating Equation (7) below:

$$Demo_{it} = \alpha Demo_{it-1} + \sum_{j=1}^{5} \beta_j * BC_{it}^{j} + \sum_{k=1}^{K} \theta_j X_{kit} + \delta_i + \mu_t + \varepsilon_{it} \qquad (7)$$

All variables are defined like in Equation (1) except the variables coding the timing of the effect of a financial crisis, BC_{it}^{j} (j = 1,..,5). These variables are dummy variables defined as follows: BC_{it}^{1} takes the value 1 in the three years preceding the start of a banking crisis, and 0 elsewhere; BC_{it}^{2} takes the value 1 in the year in which a banking crisis starts, and in the 1st and 2nd years that follow, and 0 elsewhere; BC_{it}^{3} takes the value 1 during years 3, 4 and 5 that follow the year a banking crisis started, and 0 elsewhere; BC_{it}^{4} takes the value 1 in the 6th, 7th, and 8th years that follow the start of a banking crisis, and 0 elsewhere; BC_{it}^{5} takes the value 1 in the 9th, the 10th, the 11th and the 12th years following the start of a banking crisis and zero otherwise. Figure (1) below summarizes the specification that looks at the timing in more detail.

Figure 1: Definition of banking crisis dummies



The estimation of equation (7) thus allows the effect of a banking crisis on the level of democracy to vary over time. Moreover it allows checking the existence of anticipation

effects, by looking at the coefficient β_1. A significant β_1 would signal that countries that experienced a banking crisis followed a different trajectory before they indeed start it.

The coefficient β_2 measures the impact of banking crises on democracy from the start of a banking crisis to the following two years; β_3 measures the impact of banking crises on democracy from the third to the fifth year that follow the start of banking crises; β_4 measures the impact of banking crises on democracy between the sixth and the eight years that follows the start of banking crises; and β_5 measures the impact of banking crises on the democracy between the 9^{th} and the 12^{th} year after the banking crisis starts . Altogether, the β_j $(j=2,\ldots,5)$ describe the timing of the change in democracy after the crisis started.

Equation (7) therefore allows a finer analysis of the timing of the impact of banking crises on democracy than Equation (1) by differentiating between the time horizons. However, this is done at the cost of imposing more structure on the model.

Table 7 provides the results of estimating Equation (7) for the three specifications of our baseline results: for the whole sample, after excluding a sub-sample of countries that experienced multiple banking crises, and after excluding the Global Financial Crisis.

Several conclusions emerge. First, β_1 is not significant, meaning that there is no anticipation effect. This means that countries covered in our study were on the same trajectory in terms of democracy before the occurrence of a banking crisis. Second, β_2 is not significant, which implies no impact of banking crises on democracy in the 2 years that follow the start of a banking crisis. Given that the average banking crisis lasts 2 to 3 years in our sample, this result means that there is democracy index is not affected during the crisis. Third, β_3, β_4, and β_5 are all positive and significant. This result implies that democracy improves from the 3^{rd} to the 12^{th} years that follow the start of a banking crisis.

Overall the results show that on average the bulk of the improvement in democracy that we observe in our baseline results materializes from the 3^{rd} year after the start of a banking crisis while there is no effect in the first two years. This conclusion can come from the fact that governments and policymakers are more concerned about resolving the crisis in the following 2 years of its start. But once the crisis is resolved, reforms are proposed for sound political institutions. But the implementation of the reform agenda may take some time, at least three years.

5. Conclusion

This paper examines the impact of banking crises on the level of democracy. We find that banking crises contribute to improve democracy. We interpret this result by the fact that banking crises are major negative shocks for the economy which serve as a triggering event to contest power in autocracies in line with Acemoglu and Robinson (2001). This result is robust to a battery of robustness checks controlling among others for the definition of democracy and the other income shocks and using instrumental variables, treatment effect models, and dynamic panel data estimators. We observe that the impact of a banking crisis on democratic change is greater in non-democratic countries and when the banking crisis is severe in the sense that it lasts more than three years. Finally, we find that the bulk of the improvement in democracy that we observe in our baseline results materializes from the third year after the start of a banking crisis while there is no effect in the first two years.

Our paper therefore contributes to a better understanding of the aftermath of banking crises. Next to their detrimental economic consequences, these events can favor democratization. This conclusion does not mean that a banking crisis is required for democratic improvement but it suggests that autocrats fearing political changes should have incentives to prevent any banking crisis.

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Appendix A:

Varieties of Democracy

Electoral Democracy: "The electoral principle of democracy seeks to embody the core value of making rulers responsive to citizens, achieved through electoral competition for the electorate's approval under circumstances when suffrage is extensive; political and civil society organizations can operate freely; elections are clean and not marred by fraud or systematic irregularities; and elections affect the composition of the chief executive of the country. In between elections, there is freedom of expression and an independent media capable of presenting alternative views on matters of political relevance. In the V-Dem conceptual scheme, electoral democracy is understood as an essential element of any other conception of (representative) democracy – liberal, participatory, deliberative, egalitarian, or some other."

Liberal Democracy: "The liberal principle of democracy emphasizes the importance of protecting individual and minority rights against the tyranny of the state and the tyranny of the majority. The liberal model takes a "negative" view of political power insofar as it judges the quality of democracy by the limits placed on government. This is achieved by constitutionally protected civil liberties, strong rule of law, an independent judiciary, and effective checks and balances that, together, limit the exercise of executive power. To make this a measure of liberal *democracy*, the index also takes the level of electoral democracy into account."

Participatory democracy: "The participatory principle of democracy emphasizes active participation by citizens in all political processes, electoral and non-electoral. It is motivated by uneasiness about a bedrock practice of electoral democracy: delegating authority to representatives. Thus, direct rule by citizens is preferred, wherever practicable. This model of democracy thus takes suffrage for granted, emphasizing engagement in civil society organizations, direct democracy, and subnational elected bodies."

Deliberative democracy: "The deliberative principle of democracy focuses on the process by which decisions are reached in a polity. A deliberative process is one in which public reasoning focused on the common good motivates political decisions—as contrasted with emotional appeals, solidary attachments, parochial interests, or coercion. According to this principle, democracy requires more than an aggregation of existing preferences. There should also be respectful dialogue at all levels—from preference formation to final decision—among informed and competent participants who are open to persuasion."

Egalitarian democracy: "The egalitarian principle of democracy holds that material and immaterial inequalities inhibit the exercise of formal rights and liberties, and diminish the ability of citizens from all social groups to participate. Egalitarian democracy is achieved when 1) rights and freedoms of individuals are protected equally across all social groups;2) resources are distributed equally across all social groups; and 3) access to power is equally distributed by gender, socioeconomic class and social group."

Civil liberties: "Civil liberty is understood as liberal freedom, where freedom is a property of individuals. Civil liberty is constituted by the absence of physical violence committed by government agents and the absence of constraints of private liberties and political liberties by the government."

Table 1: Baseline Results

Dependent Variable: Democracy	Before/After		
	Whole Sample	Multiple crises countries excluded	Subprime crisis period excluded
Demo (t-1)	0.874	0.871	0.873
	(74.69)***	(66.75)***	(73.21)***
BC 10 years after the start	0.196	0.209	0.219
	(2.130)**	(2.083)**	(2.223)**
Constant	-0.431	-0.311	-0.424
	(-2.234)**	(-1.900)*	(-2.208)**
Observations	4,132	3,618	3,800
Number of countries	129	114	128
Adjusted R-squared	0.845	0.837	0.835
Country FE	YES	YES	YES
Year FE	YES	YES	YES

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Robustness Checks

VARIABLES	Democracy : Polity2		Democracy : V-Dem					
	Former Socialist countries excluded	Controlling for Regional Democracy	Electoral Democracy	Liberal Democracy	Participatory Democracy	Deliberative Democracy	Egalitarian Democracy	Civil liberties
Demo (t-1)	0.876	0.845	0.952	0.958	0.961	0.960	0.959	0.950
	(73.45)***	(48.78)***	(148.1)***	(151.9)***	(156.9)***	(172.0)***	(150.9)***	(148.9)***
BC 10 years after the start	0.224	0.214	0.00423	0.00203	0.00173	0.00406	0.00347	0.00459
	(2.390)**	(2.256)**	(1.400)	(0.885)	(0.986)	(1.535)	(1.712)*	(1.727)*
Regional Demo		0.204						
		(5.538)***						
Constant	-0.415	-0.111	0.0187	0.0142	0.0104	0.0107	0.0140	0.0214
	(-2.153)**	(-0.619)	(4.095)***	(4.179)***	(3.718)***	(2.757)***	(4.345)***	(6.127)***
Observations	4,037	4,132	3,998	3,998	3,998	3,998	3,998	3,975
Number of countries	124	129	117	117	117	117	117	116
Adjusted R-squared	0.847	0.849	0.890	0.901	0.903	0.904	0.900	0.922
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Controlling for Other Income Shocks

Dependdent Variable : Democracy	Large Income Shocks	Before/After Sovereign Debt Crises	Currency Crises
Demo (t-1)	0.874	0.874	0.873
	(74.71)***	(73.89)***	(74.45)***
BC 10 years after the start	0.189	0.169	0.193
	(2.051)**	(1.893)*	(2.046)**
10 years after Large Income Shocks	0.178		
	(1.210)		
Sovereign Debt crisis 10 years after		0.298	
		(1.884)*	
Currency crisis 10 years after			0.0331
			(0.416)
Constant	-0.433	-0.428	-0.433
	(-2.246)**	(-2.236)**	(-2.251)**
Observations	4,132	4,132	4,126
Number of countries	129	129	129
Adjusted R-squared	0.845	0.845	0.844
Country FE	YES	YES	YES
Year FE	YES	YES	YES

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Other Estimation Techniques

VARIABLES	Dynamic Panel Data			Instrumental Variable		
	Whole Sample	Multiple crises countries excluded	Subprime crisis excluded	Whole Sample	Multiple crises countries excluded	Subprime crisis excluded
Demo (t-1)	0.928	0.905	0.946	0.863	0.860	0.860
	(122.7)***	(119.0)***	(124.3)***	(54.18)***	(48.81)***	(48.56)***
BC 10 years after the start	0.161	0.141	0.220	1.618	1.679	1.736
	(5.462)***	(4.713)***	(7.421)***	(1.985)**	(1.788)*	(1.950)*
Constant	0.716	-0.369	-0.291	-0.145	-0.207	-0.456
	(7.816)***	(-4.589)***	(-2.913)***	(-0.579)	(-0.777)	(-1.929)*
Arellano and Bond AR(1)	0.000	0.000	0.000			
Arellano and Bond AR(2)	0.586	0.727	0.998			
Sargan test of Overidentification	0.374	0.237	0.049			
Hansen test of Overidentification	0.122	0.319	0.301			
F-stat First Stage				8.20	4.04	7.675
P-value				0.005	0.038	0.006
Observations	4,132	3,618	3,800	4,083	3,575	3,777
R-squared				0.948	0.949	0.947
Number of countries	129	114	128	129	114	128
Country FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Average Treatment Effects on the Treated

Dependent Variable : Democracy	(1) RA	(2) IPW	(3) IPWRA	(4) NN Match	(5) PSM
BC 10 years after the start	0.202	0.201	0.202	0.327	0.357
	(2.634)***	(2.141)**	(2.543)**	(2.329)**	(1.492)
Observations	3,563	3,563	3,563	3,563	3,563
Country FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Robust z-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Conditional Effects of Banking Crises and Democracy

Dependent Variable : Democracy	Severity		By Degree of democracy
	Duration	NPL	
Demo (t-1)	0.875	0.880	0.883
	(75.58)***	(78.52)***	(71.91)***
Severe BC	0.247		
	(2.076)**		
Moderate BC	0.0756		
	(0.599)		
Severe BC		0.433	
		(2.213)**	
Moderate BC		0.337	
		(2.497)**	
BC 10 years after the start			0.246
			(2.352)**
Demo(t-1)*BC			-0.0343
			(-2.925)***
Constant	-0.416	-0.392	-0.423
	(-2.160)**	(-2.009)**	(-2.195)**
Observations	4,176	3,777	4,132
Number of countries	129	129	129
Adjusted R-squared	0.845	0.852	0.845
Country FE	YES	YES	YES
Year FE	YES	YES	YES

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Timing of the Impact

Dependent Variable : Democracy	Periods around the start of a banking crisis		
	Whole Sample	Multiple crises countries excluded	Subrpime crisis period excluded
Demo (t-1)	0.873	0.869	0.872
	(72.71)***	(65.15)***	(70.80)***
BC 3 years before the start	0.0331	-0.0235	0.0430
	(0.214)	(-0.127)	(0.274)
BC 0 to 2 years after the start	0.0522	0.0831	0.0716
	(0.332)	(0.405)	(0.439)
BC 3 to 5 years after the start	0.355	0.341	0.380
	(2.224)**	(2.021)**	(2.278)**
BC 6 to 8 years after the start	0.265	0.303	0.295
	(2.374)**	(2.481)**	(2.475)**
BC 9 to 12 years after the start	0.197	0.231	0.222
	(2.220)**	(2.113)**	(2.203)**
Constant	-0.431	-0.311	-0.423
	(-2.240)**	(-1.904)*	(-2.218)**
Observations	4,132	3,618	3,800
Number of countries	129	114	128
Adjusted R-squared	0.845	0.837	0.836
Country FE	YES	YES	YES
Year FE	YES	YES	YES

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A1: List of countries and banking crises

Countries	Banking crises
Full Democracies	
Australia	
Austria	
Belgium	
Canada	
Colombia	1982, 1998-2000
Costa Rica	1987-1991
Cyprus	
Denmark	
Fiji	
Finland	1991-1995
France	
Greece	
India	1993
Ireland	
Israel	1977
Italy	
Jamaica	1996-1998
Japan	1997-2001
Kosovo	
Luxembourg	
Mauritius	
Montenegro	
Netherlands	
New Zealand	
Norway	1991-1993
Pakistan	
Solomon Islands	
Sri Lanka	1989-1991
Sweden	1991-1995
Switzerland	
Trinidad and Tobago	
Turkey	1982-1984, 2000-2001
United Kingdom	
United States	1988
Democracies	
Argentina	1980-1982, 1989-1991, 1995, 2001-2003
Armenia	1994
Azerbaijan	1995
Belarus	1995
Botswana	
Brazil	1990-1998
Burkina Faso	1990-1994
Comoros	
Croatia	1998-1999
Dominican Republic	2003-2004
Ecuador	1982-1986, 1998-2002
El Salvador	1989-1990
Eritrea	1993
Estonia	1992-1994
Georgia	1991-1995
Guatemala	
Guyana	1993
Honduras	
Lebanon	1990-1993
Liberia	1991-1995
Madagascar	1988
Malaysia	1997-1999
Mexico	1981-1985, 1994-1996
Myanmar	
Namibia	
Papua New Guinea	
Portugal	
Senegal	1988-1991
Sierra Leone	1990-1994
Singapore	
South Africa	
Spain	1977-1981
Suriname	
Tanzania	1987-1988
Thailand	1983, 1997-2000
Zimbabwe	1995-1999
Non Democracies	
Afghanistan	
Albania	1994-1994
Algeria	1990-1994
Angola	
Bahrain	
Bangladesh	1987
Benin	1988-1992
Bhutan	
Bulgaria	1996-1997
Burundi	1994-1998
Cambodia	
Cameroon	1987-1991, 1995-1997
Central African Republic	1976, 1995-1996
Chad	1983, 1992-1996
Chile	1976, 1981-1985
China	1998
Cuba	
Djibouti	1991-1995
Equatorial Guinea	1983
Gabon	
Ghana	1982-1983
Guinea	1985, 1993
Guinea-Bissau	1995-1998
Haiti	1994-1998
Hungary	1991-1995
Indonesia	1997-2001
Iraq	
Jordan	1989-1991
Kenya	1985, 1992-1994
Kuwait	1982-1985
Lao PDR	
Lesotho	
Libya	
Malawi	
Mali	1987-1991
Mauritania	1984
Morocco	1980-1984
Mozambique	1987-1991
Nepal	1988
Nicaragua	1990
Niger	1983-1985
Nigeria	1991-1995
Oman	
Panama	1995
Paraguay	1995
Peru	1983
Philippines	1983-1986, 1997-2001
Poland	1992
Qatar	
Romania	1990-1992
Rwanda	
Saudi Arabia	
Somalia	
Swaziland	1995-1999
Togo	1993-1994
Tunisia	1991
Uganda	1994
Uruguay	1981-1985, 2002-2005
Zambia	1995-1998