Economic Crisis and Fiscal Reforms in Latin America

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Abstract[*]

The recent financial crisis has initiated pressures for not only policy reform but also fundamental institutional fiscal reforms. This paper explores the connection between economic crises and fiscal institutional reforms in a region that has experienced plenty of both in recent years, namely Latin America. For that purpose it reviews the literature and provides five hypotheses about why, and under what circumstances, crises would promote reforms. The empirical evidence shows that debt crises make reforms more likely but banking crises on their own, if anything, reduce the pressure for fiscal institutional reforms. Political institutions are also important. If the electoral system encourages the personal vote, the country is more likely to reform. This evidence may become useful for predicting the likelihood of reforms in the developed world.

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1. Introduction

The financial crisis that struck in 2008 may be leading to a sovereign debt crisis in several countries in 2011. Recent attention focuses on the fiscal situation in Greece and Ireland, but much of the industrialized world is facing pressure to initiate fiscal consolidations. The pressure is for not only policy reform (i.e., cuts in expenditures) but also fundamental institutional reform in an attempt to prevent future crises. A variety of actors, be they heads of international economic organizations, presidents of central banks, or prime ministers of countries, have called for fiscal reforms, and the European Union is strengthening its fiscal framework in an attempt to prevent future sovereign debt crises.

This paper examines the connection between crisis and fiscal reforms in a region of the world that has experienced plenty of both, namely Latin America. This region is also interesting in the context of the current crisis. Unlike other regions of the world, such as Eastern Europe or the United States, it seems to have done well economically relative to the rest of the world in the last few years. This is a reversal of previous world shocks, such as the East Asian crisis in the late 1990s, when Latin America was susceptible to contagion. As Ecuadorean President Correa recently claimed, the crisis did not hit the Latin American economies because of the economic policies implemented there.[1] Most leaders in Latin America could say much the same thing.[2] The whole point of institutional reform is to promote the implementation of better policies; are such reforms a reason why the region has performed well? Did governments introduce these reforms during crises?

In the past 20 years, three major crises have hit the region: the Tequila Crisis, the Brazilian crisis, and the Argentine crisis. While each of these hit one or more of the largest countries especially hard, they affected the entire region. In addition to regional crises, there have also been country-specific crises. Latin American governments have introduced several fiscal reforms during the same period. Fiscal responsibility laws, which usually combined numerical spending and/or budget balance targets with measures to increase transparency, were particularly common in the late 1990s and early 2000s, although, as we shall see, governments reversed many of these reforms.

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[1] "Con legítimo orgullo podemos decir que si la crisis no ha golpeado con crudeza, con extrema fiereza a nuestra economía, es por las medidas oportunas, inteligentes, honestas, que hemos tomado" Presidente Correa speech at the America's Summit. Available at: http://www.taringa.net/posts/info/2454954/Ecuador_-Discurso-de-Correa-en-la-Cumbre-de-las-Americas.html

[2] Not only did the leaders promote this idea, but the IMF also recognized the role of the country's economic policy for coming out of the crisis unscathed, for example by stressing the role that fiscal responsibility had in Brazil. Available at: http://www.americaeconomia.com/economia-mercados/fmi-elogia-solida-estructura-de-politica-economica-de-brasil-en-revision-de-2010

In order to explore whether fiscal reforms had any effect on why Latin American countries seem to do well today, one needs to understand why countries experienced fiscal reforms in the first place. In addition to addressing this endogeneity question, we are also interested in what sorts of lessons the region may hold for other regions today. Were crises the catalysts for these reforms? Does the exit from a crisis situation explain the collapse of reforms? Are neighboring countries more likely to reform if others are facing difficulties in order to demonstrate both to domestic populations and to markets that the country is not in the same situation as the other countries facing crisis?

This paper begins with a discussion of our dependent variable, which is whether there is fiscal reform. The second part considers why crises may be connected to reforms, and it provides five testable hypotheses. The third part of the paper considers how crises are measured in the literature. The fourth part adds additional, more political explanations for reforms. The final part of the paper provides the empirical analysis. While we examine several different types and different measures of crises, we find that only fiscal crises in the year the crisis hit (not later) make it more likely that countries reform. Moreover, other more political variables seem to be relevant as well. Countries that experience more frequent general strikes and that have electoral institutions that encourage a personal instead of a party vote also are more likely to reform. We expect that these variables are more likely symptoms of broader problems. The strikes occur at least partly because the political system is not functioning well. The electoral institutional variable is one indicator of the scale of the common pool resource problem. The greater the problem, the more likely there are fiscal issues unresolved in the first place.

2. Fiscal Reforms in Latin America

Conceptually, we are interested in changes in rules and institutions. In this paper, we focus on fiscal institutions that affect the budgetary decision-making process.[3] The underlying assumption is that decision-makers face a potential common pool resource (CPR) problem. This arises when politicians care only about the spending and revenue implications of their decisions on their constituencies and their tax burden is smaller than the full tax implications of their spending. An agriculture minister, for example, may worry most about how farmers benefit from spending programs and how much tax they pay. Similarly, a Congressperson in Argentina may care most about expenditures in her home province. She will want more spending if the entire country pays for it, so the share of her province is smaller than if her province had to bear the full tax burden.

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[3] In future versions of this research agenda we will extend the analysis to tax reforms, which are changes in rules on the revenue side of the budget, and to fiscal federal reforms.

The severity of the potential size of the CPR problem depends significantly on the underlying political institutions. Consider a budget that requires first congressional and then presidential approval. To begin with, a potential source of CPR problems in Congress is the electoral system. As argued elsewhere (e.g., Hallerberg and Marier, 2004; Hallerberg and Scartascini, 2010), some electoral systems encourage a personal vote while others lead to a party vote. For example, under the Brazilian system of open-list proportional representation where voters determine the order of the candidates on the list, congresspersons have an incentive to appeal to very narrow segments of a given electoral district's population (see Ames, 1995). In Mexico, in contrast, with closed-list proportional representation, which means that parties determine where a candidate appears in the ballot, there are greater incentives for candidates to care about party representation. To the extent the party represents a greater share of the overall population, the party internalizes more of the costs of its decisions than an individual would.

The same exercise can be done for the executive. The more the electoral system as well as the party system encourages the president to think about the tax burden for the entire country, the smaller the CPR problem. In the case of Latin America, however, this is not usually a problem, as most presidents are directly elected, and presidents have to win either a plurality or outright majority. Cabinets are an additional consideration. The more cabinet members there are, the greater the potential CPR problem in the executive, particularly when they have spending prerogatives (e.g., Perotti and Kontopoulos, 1999; Wehner, 2010).

An additional determinant of the CPR problem is the amount of the country's budget that passes through the national budget process. One can imagine a system that is centralized at the national level but where most of the spending takes place at another level of government. To the degree that this is the case, the fiscal rules that structure the ability of sub-national governments to make policy are relevant, and the nature of fiscal federalism deserves more scrutiny.

In this paper, we are interested in reforms that centralize the budget process and that should reduce the scope of the CPR problem. In the dataset, there are several classes of possible reforms we have coded for 17 countries for the period 1990-2005. We start in 1990 because this is the date when all countries in the region have elective presidential democracies. The fiscal institutions we care most about are institutional changes to decision-making processes in democracies, and it does not make sense to go much earlier in time when some of the countries in the sample did not have elected presidents. The end point of 2005 is useful because it includes several crises over the period but is also

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a period of "relative" political and economic calm. This means that we can exploit the variance across periods and across countries.

There are three broad types of reforms (von Hagen, 1992; von Hagen and Harden, 1995; Alesina et al., 1998). A numerical rule establishes *ex ante* constraints on debts, deficits, and/or expenditures. A balanced budget requirement, for example, is such a rule. The second type is a procedural rule, which establishes the norms and prerogatives of actors in the budget process. A transparency rule makes it easier to follow what the government is doing on the budget. An increase in the comprehensiveness of budget documents as well as the identification and even the closing of extra-budgetary funds would constitute a transparency rule according to our definition. If one examines episodes where governments introduced any of these measures, a reform occurred in approximately one out of every eight years in the sample. Some countries had almost no reforms; Guatemala, for example, introduced a change only in 2000. Other countries had multiple reforms; Argentina and Ecuador both had six reform episodes over the 15-year period.

These rules, in turn, often appeared in Latin America in packages known as "fiscal responsibility laws." The best-known is also the most successful, namely the fiscal responsibility law in Brazil. The law extends restrictions to all levels of governments, not just to the national level. In terms of sub-national finance, there are 27 states (26 states plus the federal district of Brasilia). The states negotiate budget balance and expenditure caps with the central government, and the national Senate approves them. Any new expenditure in the budget requires full information on costs in the initial year and the following two years. Independent bodies exist in each state that audit both state and municipal finances (the *Tribunal de Contas*). There is also a clear punishment mechanism. Once the caps are in place, any sub-national government that exceeds the spending/debt provisions is identified publicly and placed on a list, which is updated monthly. Lower levels of government that continue to exceed the caps are denied federal transfers in the following year if they do not correct them. Moreover, the law is connected to criminal law in the Brazilian system. Politicians who break the law are subject to a lifetime ban from politics and possible jail time. Hundreds of municipal politicians have faced such bans, and a few have served behind bars. In our dataset of 17 countries, eight introduced a fiscal responsibility law.[4]

Based on this template, Table 1 lists the types of reforms in our dataset since 1990 which one of the co-authors has collected (Filc and Scartascini, 2007). Note that the date is for the date of the approval of the law. For the analysis that comes later in the paper, the dependent variable is any change

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[4] See Alston et al. (2009) for more details on the Brazilian case.

in fiscal institutions, coded dichotomously, with 16.5 percent of years in the dataset being reform years.[5]

Table 1.Fiscal Reforms in Latin American Countries, 1990-2005

	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005
Argentina			N		U						R(N,C,T),S,P,r(c),S,r(n),S				R(N,S,C)	
Bolivia			S								U					
Brazil									N		R(N,S,T),P					
Colombia						C					N	S		R(N,P,T)		
Chile											R(N,C)			T		
Costa Rica												U,A				
Ecuador						U			N	C			R(N,P,C,T)		T	r(n)
El Salvador				U			A									
Guatemala											P,N,U					
Honduras														P,U		
Mexico									C				C,P,T			
Nicaragua													S,A			P
Panama									U				R(N),S,T		r(n)	P,U
Paraguay											U			P		p
Perú				U							R(N,P,C,T)		r(n),C,T			
Dom Rep															T	
Uruguay							U									
Venezuela									C,U		P			R(N)		
	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005

Notes
N: Numerical rules; C: Contra-cyclical Fund; P: Multiyear framework; R: Fiscal Responsibility Law; S:Subnational Governments; U: Single account;
T:Transparency, and; A: Principles of transparency.
Italic lower case means that the previously established reforms were reversed or the restrictions weakened.
R(X,Y) means that the Fiscal Responsibility Law included restrictions to X and Y.

Source: Filc and Scartascini (2007), updated by authors.

3. Why Expect Crisis and Reforms?

There is an expectation that economic crises and fiscal reforms go together. The following section considers several explanations for this expectation. One can differentiate the arguments according to the type of reform that matters as well as the timing of the reform, and we present five testable hypotheses that we examine in the empirical section.

The first explanation focuses on the possible link between the origins of the crisis and fiscal policy—something about the state's management of its finances caused the crisis, and the only way to exit the crisis is to address the problem. One version of this argument is Alesina and Drazen's (1991) "war of attrition" model. Their focus is on "crisis" in the form of an inflation crisis. There are different groups in society whose approval is required for action to be taken. The political representation of each group, however, would not like to bear the costs of the adjustment process of getting inflation under control. As Drazen and Grilli (1993) suggest, only when the crisis hits and when all groups are bearing

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[5] We do not include an increase in the power of the finance ministry, which may be a more subjective assessment.

high costs anyway is it politically feasible for the government to take the steps necessary to address the crisis. In their model, the "pain" comes in the form of fiscal adjustments.

While this model explains why policy reforms may be introduced, our focus in this paper is on change in how fiscal policy is made, and this model alone does not explain such institutional change. Crises may nevertheless represent a chance to change the institutional framework under which governments make policy. The public policy literature speaks of "windows of opportunity" that open during crisis periods and make even radical reforms possible (e.g., Kingdon, 1984 and 1997; Zahariadis, 2003). The argument is that there are multiple streams at work at any one point in time. The first stream is the perception that something is a problem the government should address. A second stream is a policy stream, which is the discussion in the policy community about the desirability of some policies over others. The third stream is political, with shifts in national mood moving this stream. When all three come together, there is an opportunity to introduce real change. Rahm Emmanuel's statement that "you never want a serious crisis to go to waste'" is very much in the spirit of the "window of opportunity" literature.[6] In this case, there does not have to be any direct connection to fiscal policy and institutions for there to be reform. Empirically, one would expect changes in policy especially after changes in leadership, or when the "political" stream has come more in sync with the other two streams.

Related to the argument about windows of opportunity, crises often lead to requests for international help. The IMF sometimes attaches requirements for fiscal outcomes to their programs. These may also include either expectations or even requirements for fiscal reforms. There are several problems associated with measuring IMF influence, however. Using the data on banking crises in Laeven and Valencia (2008), which consider only whether an IMF program was in place or put in place during a crisis, their sample suggests that programs were less common than one may first think—they were either in place or put in place in only about half the cases.[7] But the IMF and other international organizations may have less public ways of suggesting reforms, and their pressure may be more instrumental in crisis years than in non-crisis years.

These arguments suggest the first two hypotheses. According to the windows of opportunity literature,

H1: Fiscal reforms should coincide with (any type of) domestic crises.

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[6] American President Obama's first Chief of Staff made this comment in November 2008.

[7] Argentina 1990 (actually December 1989), 1995, and 2001 (all Y), Brazil 1990 and 1994 (N, but in place in 1990, N), Colombia 1998 (N), Dominican Republic 2003 (Y), Ecuador 1998 (Y), Mexico 1994 (Y 1995), Paraguay 1995 (N), Uruguay 2002 (N, already in place), and Venezuela 1994 (N, already in place).

If this issue is one of endogeneity, that is, if it was a fiscal problem that caused the crisis in the first place, then

H2: Fiscal reforms should coincide with domestic sovereign debt crises.

Note that when we test this argument later in the paper we will look at two operationalizations of fiscal crises. One is the year of the default. In this case, and consistent with Alesina and Drazen (1991), it takes a default to get the different interests finally to act on the problem. The dynamics of sovereign debt restructurings are probably different.

A second reason why reforms may be connected to crises is that fiscal policy was not the direct cause of the problem, but any fix will be expensive, and reforms may be introduced in tandem in an attempt to control spending on other items. In their historical study of eight centuries of crises, Reinhart and Rogoff (2009) find that debt burdens grow on average 82 percent in the first two years after a banking crisis. This then provides a fiscal shock to the system, and reforms may be part of government efforts to exit the crisis. To take an OECD example, the German government combined a fiscal stimulus in early 2009 with the introduction of a "debt brake" in the German Constitution. At the same time, one might expect the pressure to undertake reform to be proportional to the extent of fiscal need. A fiscal stimulus to stabilize the economy will not increase the overall debt level as much as a bailout of most of a country's banking sector. This line of argument suggests that reforms coincide with banking crises. There may also be some delay before the onset of the banking crisis and the fiscal reform, however, as the fiscal implications of the bailouts may not be evident at first.

H3: Fiscal reforms follow domestic banking crises.

So far the emphasis has been on domestic crisis, but there are reasons to believe that crises in the region may have an effect. Markets may become spooked with fiscal policies in the region in general, not just in the country or countries experiencing a crisis. The introduction of fiscal reforms would represent an attempt to reassure markets that a given country is not the same "type" as the countries already in trouble. Stronger fiscal institutions are meant to reassure markets about the government's future behavior. The fiscal rules make it more likely that the government will have lower debt than initially feared because of two factors. First, the application of the fiscal reform is intended to lower debt. Second, the introduction of the reform may serve as a signal of future government intentions on debt policy.

The threat that a country will not have access to capital markets may be quite real. In the early 2000s countries in Latin America experienced what are known as "sudden stops," which means that it is not possible for the government to borrow on international credit markets. In addition to an attempt to reassure markets, a sudden stop could lead to a very practical need for deep cuts in public spending because international capital to finance previous levels of spending is simply no longer available. Fiscal reforms would make it easier to implement the necessary changes in the budget.

This then leads to the fourth hypothesis:

H4: fiscal reforms are more likely when neighbors are experiencing crises.

Finally, fiscal reforms may come well after the crisis has passed. IMF (2009) suggests that one type of reform, namely fiscal rules, is introduced to lock in fiscal adjustment gains. The analogy is to inflation targeting in the central banking literature, where several central banks seem to have announced an inflation target after the inflation rate had fallen below the new target. They argue that countries that reduced their public debt ratio by more than 2 percentage points in the three years before the introduction of a fiscal rule had twice the probability of introducing the rule. The argument is that the adjustment makes the rule more credible to markets so governments are more likely to introduce it in the first place.

H5: Fiscal reforms come only several years after the recovery.

4. Political Explanations of Reform

The previous section focused on the connection between crises and reforms, but one would expect that variables of a more political nature would also be a cause of reforms. While this paper has so far focused on the effects of crises on the introduction of fiscal reforms, there may be more overt political reasons why governments introduce reforms. We explore several hypotheses here.

First, following up on the discussion above of the common pool resource (CPR) problem as a source of fiscal indiscipline, one could expect that countries where the potential problem is high would be more likely to introduce fiscal reforms. The more a decision-maker thinks that she can improve her political future by worrying about a narrow slice of the population when making spending and taxation decision, the greater the *potential* CPR problem. Fiscal institutional changes, however, can address the problem by centralizing the budget process. For example, the goal of greater transparency in the budget is to make it harder for decision-makers to approve funding only for narrow interests. Similarly, changes to the decision-making process itself, such as the introduction of medium-term fiscal
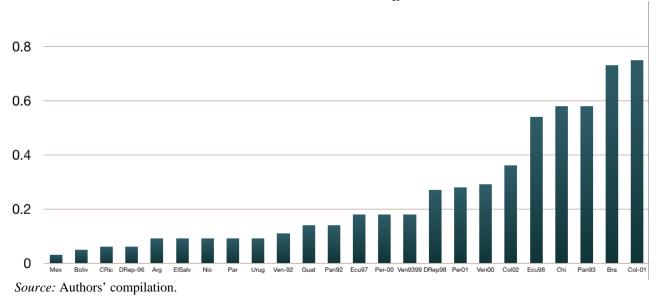
framework that force decision-makers to think about the medium-term effects of today's decisions, also reduce the CPR problem. Countries with larger potential CPR problems benefit the most from such reforms, and they may be more likely to introduce them.

To measure the potential CPR problem, we use a measure for the personal vote first introduced in Hallerberg and Marier (2004). The index considers the extent to which the electoral system for the lower house of the legislature encourages candidates for office to appeal to a vote for themselves over a vote for a given political party. More details about the calculation of this variable appear in Hallerberg and Scartascini (2010), but the idea, following Carey and Shugart (1995), is to look at the construction of the ballot (whether one votes for a person or party), whether votes are pooled across the party level, and the number of votes cast, and to look at these factors in the context of the district magnitude of a given country, which we measure as the size of the median electoral district. If a country has a closed ballot, which means that people vote only for a party, increasing district magnitude decreases the personal vote. If the country has an open ballot, then increases in district magnitude mean that a candidate has to appeal to an ever-smaller proportion of the population to get elected. The index runs theoretically from approximately 0, where there is a complete party vote, to 1, where there is a complete personal vote. As Figure 1 indicates, the country with the lowest index score is Mexico (entire time period) at 0.03 while the highest scores are for Colombia (1990-2001) at 0.78 and Brazil (entire time period) at 0.73.[8] These scores indicate the potential size of the CPR problem in Congress, and we expect that the greater the size the greater the impetus for institutional reform. There should be a positive relationship between the extent of the personal vote and the likelihood of fiscal reforms.

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[8] Data for this variable are from Hallerberg and Marier (2004), which in turn is updated (and sometimes corrected) by Payne, Zovatto and Mateo Díaz (2007) and by a dataset posted on John Carey's website (http://www.dartmouth.edu/~jcarey/Data%20Archive.html)

**Figure 1. Comparison of the Personal Vote in Latin American Countries,
Lower House of Congress**

Source: Authors' compilation.

While our prediction is that weakening parties leads to more (fiscal) reform because the size of the CPR problem is greater, one should note that there is a literature looking at other types of reform that would argument the reverse relationship. Haggard and Kaufman (1994) would anticipate that strong parties are needed to push through reforms after a transition to democracy. Similarly, Ames (2001) contends that the personalistic nature of the political system made reforms in Brazil especially difficult. The mechanism in the two arguments is different; we are suggesting that the problem is much bigger under personalistic systems, and that this pressure makes reforms that are meant to relieve this pressure more likely.

A second, related variable is the size of the cabinet. As Wehner (2010) has indicated, countries with larger cabinets generally have higher spending. He takes this as evidence of the CPR problem in practice within the executive. In our dataset, the average cabinet had 17.6 members, with the smallest (Argentina 190-95) having 11 and the largest (Ecuador 1996) having 31.[9]

Additional political variables include partisanship, elections, general strikes, and the competency of the bureaucracy. There is a rich literature that considers the effects of partisanship on neo-liberal reforms in Latin America. However, as Stokes (1999) has suggested in her study of the introduction of neo-liberal reforms, party labels are good predictors of rhetoric before elections but not good predictors of whether presidents actually try to introduce such reforms. Her work suggests that there should be no association between partisanship and reform. At the same time, there may be a

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[9] Data from Banks (2009).

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greater need for "left" presidents to signal to markets that they are serious about the economy, and they may be more likely to introduce fiscal reforms, so we include a variable for partisanship of the executive from the Beck et al. (2000) World Bank dataset, which the authors have updated several times. There is one particularity in that data worth noting—if the coders cannot determine the partisan shade of a given executive, they code it as a "0." In our dataset, 41 of 272 observations receive this coding, and they include some countries like Panama and Venezuela for at least half of the sample.[10] We will therefore run some models that code the "0" term as missing and others that exclude this variable. The variable, which runs from 1 (right) to 3 (left), has an average of 1.6 over the period, which means that the "average" president is politically to the center-right.

A pre-electoral period may affect the likelihood of reform, although *a priori* the expected direction of effect is hard to predict. On the one hand, a president may want to introduce a reform to illustrate what he or she is doing in a pre-electoral period. This suggests a positive relationship between elections and reform. On the other hand, it may be difficult to pass legislation as elections approach. This suggests a negative relationship. We measure electoral periods according to the proportion of the current year that is part of a pre-electoral year. This means that an election on July 1, 2000, would be measured as 0.5 in 2000 and 0.5 in 1999.

Public protest against the government is another way that citizens can influence policy-making. General strikes in particular may push policymakers to take action they would not otherwise undertake. Whether strikes lead to specifically *fiscal* reform is not clear. To guard against a simultaneity problem, we include the lag of strikes in the equations. In our dataset, there was an average of 0.44 general strikes per year, with many countries having no strikes in a given year and one country (Uruguay 1990) having six.

The final variable to consider is the overall quality of the bureaucracy. Countries with higher bureaucratic capacity may be able to implement reforms better than countries with low capacity and may be more likely to adopt them in the first place. The average of this variable, which runs on a 0 to 6 scale when including most countries in the world, is 1.8 in our sample, ranging from 0 (e.g., Guatemala and Panama in the early 1990s) to 3 (e.g., Argentina and Brazil in the early 2000s).[11]

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[10] The countries that receive a "0" coding on partisanship are Colombia (2003-05), Ecuador (2003-05), Guatemala (1992-95), Nicaragua (1991-96, 2002-05), Panama (1995-99, 2005), and Venezuela (1994-2008).

[11] Data for the last two variables come from the Cross National Time Series Archive dataset (Banks, 2009), which is described at http://www.databanksinternational.com/ .

5. Patterns of Crises in Latin America

This section considers the patterns of different kinds of crises. Latin America has experienced many of them, and there are four basic types. The first is an inflation crisis of some sort, which is usually a hyper-inflation. The second is a banking crisis. The third is a currency crisis. While one can separate the two types of crises, banking and currency crises often go together. If the country has a fixed exchange rate and a currency crisis forces it to abandon the peg, both borrowers and banks can be left exposed if they have significant debts in foreign currencies. Even without an explicit peg, the costs to cleaning up the banking crisis may be so great that markets come to doubt the credibility of the currency. The fourth is a sovereign debt crisis.

During a financial crisis, there have rarely been concurrent sovereign debt crises (Laeven and Valencia, 2008). The danger to the public fisc usually comes some years afterward. As Reinhart and Rogoff (2009) are quick to point out, sovereign debt crises often follow banking crises. The reason mentioned above is that policies to assist banks and debtors have large fiscal costs. Moreover, lost economic output because of the crisis translates into more social spending and lower tax collections and has a direct effect on the budget. In their study of 42 banking crises over the period 1970-2007, Laeven and Valencia (2008: p. 24) find that the average crisis costs a government a little over 13 percent of GDP and, at the most extreme, up to 55 percent of GDP.[12] During our sample period, and within our Latin American set of cases, the greatest economic loss was in Argentina in 2001, when the economy is thought to have shrunk almost 43 percent after the crisis, while the greatest fiscal cost was to Ecuador in 1998, when the fiscal cost was almost 22 percent of GDP (Laeven and Valencia, 2008: pp. 32-49).

There are additional complications in our set of countries, however. In some, the public sector is an owner of one or more banks. In others, the banking sector is mostly in private hands. For example, according to Laeven and Valencia (2008), 41 percent of assets in Argentina were in a government-owned bank the year before the 1995 crisis, while the percentage was almost 32 percent in Brazil the year before its 1994 crisis. In contrast, there were no publicly-owned banks in Bolivia in 1994 during its crisis at roughly the same time. This makes causation a bit tricky to untangle in places where publicly owned banks play a prominent role. The crisis may begin because the government cannot credibly back the banks it owns. Moreover, it has direct exposure when such crises do occur, which suggests that its anticipated fiscal losses will be larger.

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[12] This is the estimated cost of the Argentine banking crisis in 1980.

Similarly, currency crises often arise when governments can no longer credibly back their policy, which (in cases where there are crises) usually involves some sort of peg. Credibility, in turn, is related to a country's reserves and its willingness to devote more of its money to defend a given policy.

Laeven and Valencia (2008) identify 124 systemic banking crises over the period 1970-2007.[13] Their definition of such crises is that "a large number of defaults and financial institutions and corporations face great difficulties repaying contracts on time." They combine both a quantitative filter as well as some subjective judgments. Note importantly that they provide data only on the year of an onset of a crisis, which means that there is no information about the overall length. In our sample, almost 5 percent of "country years" are crisis years according to their dataset.

A somewhat different definition of a banking crisis comes from Reinhart and Rogoff (2009), which does not require the large number of defaults discussed in Laeven and Valencia (2008) and therefore provides a broader definition of crises. As Reinhart and Rogoff explain on page 11,

> "We mark a banking crisis by two types of events: (1) bank runs that lead to the closure, merging, or takeover by the public sector of one or more financial institutions and (2) if there are no runs, the closure, merging, take-over, or large-scale government assistance of an important financial institution (or group of institutions) that marks the start of a string of similar outcomes for other financial instruments".

We use their data appendix (Reinhart and Rogoff, 2009: pp. 349-92) to code a variable for banking crises. Unlike Laeven and Valencia (2008), they include additional years beyond the beginning of the crisis. This means, for example, that Mexico has a banking crisis from 1981 to 1992 and again from 1994 to 1997, while Nicaragua has a banking crisis in 10 of 16 years from 1990 to 2006. More generally, about 18 percent of the 272 country-years for which they have data in our Latin American sample are marked by banking crises. As one can see from Table 2, most of the banking crises that appear in the Laeven and Valencia dataset appear in the Reinhart and Rogoff list as well, but the broader coverage in the latter's dataset means that there are more of them. In the empirical analysis in the main body of the paper, we use the Reinhart-Rogoff coding, but in the robustness section we also compare results using the more narrow definition that appears in Laeven and Valencia (2008).

Laeven and Valencia (2008) also discuss the timing of currency crises and sovereign debt crises. Their definition of currency crises comes from Frankel and Rose (1996), and it is a nominal depreciation of the currency of at least 30 percent and an increase in the rate of depreciation of at least

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[13] This, in turn, is "an updated, corrected, and expanded version of the Caprio and Klingebiel (1996) and Caprio, Klingebiel, Laeven, and Noguera (2005) banking crisis databases." (p. 5)

10 percent over the previous year. In our sample, such crises were less frequent than banking crises, or about 3.7 percent of the time.

Rothenberg and Warnock (2006) provide a potential dataset for testing Hypothesis 4, which includes countries that experience a sudden flight and/or a true sudden stop.[14] About 20 percent of country years have a sudden flight or stop according to their definition over the time period for which they have data, or 1989-2005. At the same time, they have fewer countries in their dataset, or only seven of the 17 in our dataset, so we will not be able to use this measure in the multi-variate analysis that follows. We will instead examine crises in other countries as an indirect test.

For sovereign debt crises, Laeven and Valencia (2008) consider both the year of sovereign debt defaults to private lending as well as the year of debt rescheduling. They rely on Beim and Calomiris (2001), World Bank (2002), Sturzenegger and Zettelmeyer (2006), and IMF staff reports as sources. In our sample, true defaults are rare, or only initiated in four country years. Moreover, they are clearly correlated with exchange rate crises—all of the sovereign debt crises are either in the same year or in the year following an exchange rate crisis. Sovereign debt restructurings, however, are more common, with 16 cases that include all the countries but Colombia, El Salvador, and Guatemala. Note that these restructurings follow defaults that occurred in the period before 1990, or the beginning of our dataset.[15] The question we will ask is whether countries introduce reforms the same year as they default or whether the reforms come when they reschedule their debts with creditors.

We also investigate an alternative way of considering a fiscal crisis. Hallerberg, Strauch, and von Hagen (2009) find in their study of European Union countries that fiscal reforms often followed periods where the budget deficit was greater than the 95 percent confidence interval for a given country. The expectation is that the level of "crisis" varies across countries depending upon their past behavior. What may seem like a "crisis" in a country with small swings in its budget balance may not be a crisis for a country that experiences larger swings. We are interested in deficits that are in the 5 percent extreme of the tail for a given country, which in practice means the worst year in terms of fiscal performance among the 16 years of data. The dataset come from the IMF's International Financial

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[14] Their exact definition is as follows: "We term an episode a sudden flight if it owed primarily to local residents sending their money abroad. Specifically, a sudden flight is one in which the increase in gross financial outflows from te = -3,-2,-1,0 to te = 1,2,3,4 is greater than the decrease in gross financial inflows over the same period. In contrast, in a true sudden stop, which owes primarily to the actions of global investors; the decrease in gross inflows exceeds the increase in gross outflows."

[15] Peru, for example, defaulted in 1978 but did not reschedule its debt until 1996.

Statistics.[16] As Table 2 indicates, the level of the deficit that is in this 5 percent range varies widely—Ecuador's worst fiscal performance was a budget deficit of almost 18 percent of GDP, while Chile's worst performance was a small budget surplus of 0.4 percent of GDP. Once again, empirical results using this definition appear in the robustness section.

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[16] We also considered Rancière et al. (2006), who are concerned with the "twin" crises of bank and currency crises that occur at the same time. They have fewer countries in Latin America in their study, however, and they only cover the time period 1980-2002.

Table 2. Comparison of Crisis Years

Country	Bank (LV)	Bank (RR)	Exchange	Sudden Stop	Debt Crisis	Debt Restructuring	Budget Deficit
Argentina	1995, 2001	1990, 1995, 2001	2002	1995, 1999, 2001-2	2001	2005	3.3, 2001
Bolivia	1994	1994		[missing]		1992	9.4, 2002
Brazil	1990, 1994	1990, 1994-96	1992, 1999	1993, 1995, 1997, 1999		1994	2.3, 1993
Chile				1991-2, 1998-9, 2004-05		1990	[.4], 1998
Colombia	1998			1998-2000			6.1, 2000
Costa Rica	1994	1994-97	1991	[missing]		1990	5.1, 1994
Dom Rep	2003	1996, 2003	1990, 2003	[missing]	2003	1994, 2005	.38, 1994
Ecuador	1998	1994, 1996, 1998-9	1999	[missing]	1999	2000	17.9, 1995
El Salvador		1998		[missing]			4.1, 1992
Guatemala		1991, 2001		[missing]			2.8, 1999
Mexico	1994	1994-97	1995	1994-5		1990	2.5, 1990
Nicaragua	2000	1990-6, 2000-02	1990	[missing]		1995	19.8, 1990
Panama				[missing]		1996	5.2, 2004
Paraguay	1995	1995-99, 2002	2002	[missing]		1992	.30, 2000
Peru		1990, 1999		1997-8		1996	3.2, 1999
Uruguay	2002	2002	2002	[missing]	2002	1991, 2003	4.7, 2002
Venezuela	1994	1993-95	1994	2000-01		1990	5.6, 2001

Source: Authors' compilation.

Table 3. Kappa Measures of Agreement among Different Measures of Crises After 1989

	Banking (LV)	Banking (RR)	Exchange (LV)	Sudden Stops (RW)	Debt (LV)	Debt Restructuring (LV)
Banking (LV)	1					
Banking (RR)	.38**	1				
Exchange (LV)	.24**	.15*	1			
Sudden Stop (RW)	.19**	.06	.13			
Debt (LV)	.09**	.12**	.32**	.07*	1	
Debt Restructuring (LV)	-.02	-.01	-.07	-.10	-.02	1

*p<.05, ** P<.01
Source: Authors' estimation.

While the raw data appear in Table 2, Table 3 provides a kappa measure of agreement scores across the different measures of crises. This measure computes the hypothetical chance that any two values would be the same given their overall probability distribution, then considers the extent to which the level of agreement differs from a chance outcome. It is a more useful statistic than a more standard correlation matrix when the variables are dichotomous. Consistent with the raw data provided in Table 2, there is a statistically significant relationship between the two measures of bank crises as well as among the measure for exchange rate and banking crises. Debt crises tend to be associated with other types of crises, while debt restructurings do not frequently coincide with other types of crisis. Reinhart and Rogoff suggest that sovereign debt crises generally follow banking crises with a two-year lag, and indeed debt restructurings could be symptoms of other crises. We therefore look at the measure of agreement between the two-year lag of the beginning of the crisis and both debt crisis and restructuring, but the measure of agreement for the former is only -0.001, while for the latter it is 0.02.

The next step is to consider causes of fiscal reforms in a multivariate framework.

6. Do Crises Explain the Introduction of Fiscal Reform?

Modeling the data presented above entails several challenges. As a first cut, it would seem that an event history analysis would be the most appropriate technique. The dependent variable is dichotomous and there is a clear time element. IMF (2009), for example, which also examines determinants of fiscal reforms, considers both parametric and nonparametric hazard models. The paper also uses conditional logits to predict whether a given fiscal rule is in place.

There is, however, an issue with using standard event history analysis. Standard models in this tradition assume that cases (or countries in our case) drop out of the sample once they have had a reform. The analogy comes from medicine, which is a field where event history techniques were initially developed and where they were applied to predict the onset of a disease or the mortality of patients. Once a patient dies, the patient is out of the sample. In our sample, however, the "patients" can "die" multiple times, that is, they may have reforms again at a future time, and they certainly do not leave the sample. No less problematic is that patients may "die" in consecutive years. It is difficult to model how they re-enter the sample.

For this reason, we use two techniques. The first is a conditional logistic regression with fixed effects. Von Hagen, Hallett and Strauch (2001), as well as Mierau, Jong-A-Pin and de Haan (2007), have used such models to predict the timing of fiscal adjustments in either European or OECD countries, and an extension to consider fiscal reforms is straightforward. An advantage of logit models

is that they are fairly easy to interpret in terms of relative odds. As a robustness check, we also consider a BTSCS (Binary Time-Series Cross Section data) model that includes time splines, which is described in more detail in Beck, Katz, and Tucker (1998). This is especially appropriate for variables that do not vary much within countries because the conditional logistic regression with fixed effects becomes problematic. We report results from both model specifications.

There remain modeling issues with the logit construction, and with the standard event history models more generally. First, they do not directly model subject event dependence. Whether a country introduces a reform may depend upon what reforms it has introduced before. Second, they do not model heterogeneity. One can easily imagine, for example, that Latin American leaders "learn" from the examples different lessons and at different rates. There are some ways to try to address these issues directly in the logit model. To deal with subject event dependence, we have models where we include what we speculate are the most important determinants of the dependence. Our Hypothesis 4 examines whether a country is more likely to reform if others are in crisis. To deal with repeated events, we include a variable for the time since the last reform.[17],[18]

Our dependent variable has 272 observations, which corresponds to whether there were fiscal reforms during the time period 1990-2005 in 17 countries. We lose observations, however, when we run the most complete model, due especially to missing values in the variables "partisanship" and "checks and balances." We therefore present one version of the results that maximizes the number of variables and a second that maximizes the number of observations.

To test the first two hypotheses, Table 4 shows the results from the conditional logit models with fixed effects as well as a BTSCS logit for the "maximum variables" equation, which has only 190 observations. There are few significant variables—only the personal vote and the time since the last reform seem to have any effect—so it seems reasonable to drop the variables for which we have fewer observations. Table 5 therefore presents the core data to test the hypotheses in this paper.

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[17] A future paper may also want to consider not only the onset of the reforms but also their overall duration. This would be appropriate especially for fiscal responsibility laws, where there are data on how long the law remained in place. A Markov transition model in this case allows one to include covariates that specifically affect event onset, event duration, and both onset and duration. A challenge, however, is the number of observations. Given relatively few degrees of freedom, such an analysis would only be suggestive.

[18] A second, and more direct, way to consider the data is to use a conditional frailty model (Box-Steffensmeier, De Boef and Joyce, 2007). They allow one simultaneously to model both subject event dependence and heterogeneity. A frailty model assumes that some units are more or less prone to "failure" over time. They then "treat individual effects as random draws from a specific parametric distribution" (Box-Steffensmeier, De Boef and Joyce, 2007: p. 240). Frailty models alone, however, do not control for event dependence. The conditional frailty model combines the random component to estimate the frailty portion as well as estimates for event-specific baseline hazards. Preliminary results using the R-statistics program either reinforce the results for the conditional logics or have difficulties computing, so the results of the conditional logit models with fixed effects provide the basis for the analysis that follows.

Table 4. Analysis with All Political Variables

	(1) Conditional logit	(2) BTSCS
Banking Crisis	-1.813	-1.909
	(1.193)	(1.047)
Debt Crisis	2.962	3.150
	(1.645)	(1.539)*
Restructuring	-1.122	-0.869
	(1.177)	(1.153)
Presidential Election	0.362	0.477
	(0.711)	(0.688)
Personal Vote	10.476	2.856
	(4.864)*	(1.201)*
Cabinet Size	-0.114	-0.197
	(0.138)	(0.075)**
Lag General Strikes	-0.121	0.269
	(0.307)	(0.276)
One-party	-0.776	1.048
	(1.045)	(0.681)
Checks	0.182	0.308
	(0.253)	(0.210)
Partisanship	0.227	0.767
	(0.412)	(0.279)**
Bureaucracy	-0.446	0.030
	(0.515)	(0.356)
N Prev R	0.415	-1.022
	(0.130)**	(1.114)
_Time Since R	-0.213	0.295
	(0.304)	(0.205)
_spline1		-1.076
		(0.790)
_spline2		0.766
		(0.542)
_spline3		-0.127
		(0.090)
Constant		-2.740
		(1.475)
Observations	190	198

Standard errors in parentheses
* significant at 5%; ** significant at 1%
Source: Authors' estimations.

In terms of the results, the effects of crises vary according to the type of crisis and its timing. Banking crises seem to retard reforms, and significantly so in the equation with more observations. Debt crises, however, make reform more likely, in the same year but not when lagged. This is a curious result because if governments react to a crisis, and given that it usually takes some time to pass legislation, one would anticipate that the lag of these crises would be significant, or significant as well (even though it is true that under severe crises those times may shorten). At the same time, one should be aware of the data limitations we face—Table 2 indicates that there was a banking crisis every year there was a debt crisis, but that the reverse was not true. A linear combination of the two variables is therefore appropriate if one wants to understand the effects of "crises" in a given year. The linear combination coefficient is 1.47 with a standard error of 1.28. Taken together, the picture that emerges is that fiscal reforms are more unlikely during banking crises, but when coupled with fiscal crises this effect goes away.[19]

Table 5. Comparison of Results

	(1) Cond Logit	(2) BTSCS	(3) Cond Logit	(4) BTSCS	(5) Cond Logit	(6) BTSCS
Banking crisis	-2.271 (0.899)*	-2.276 (1.140)*	-2.314 (0.900)*	-2.265 (1.106)*	-2.553 (0.997)*	-2.347 (1.196)*
Debt crisis	3.750 (1.465)*	3.715 (1.579)*	3.516 (1.522)*	3.657 (1.529)*	3.309 (1.538)*	3.308 (1.693)
Restructuring	-1.619 (1.195)	-0.872 (1.231)	-1.662 (1.186)	-0.847 (1.237)	-1.420 (1.238)	-0.728 (1.233)
Lag Banking	0.054 (0.588)	0.812 (0.604)	0.253 (0.604)	0.829 (0.660)	0.538 (0.647)	0.970 (0.608)
Lag Debt	0.810 (1.417)	-0.129 (1.740)	0.542 (1.441)	-0.237 (1.799)	0.150 (1.481)	-0.513 (1.674)
Lag Restruct	-0.465 (0.895)	0.106 (0.858)	-0.190 (0.969)	0.476 (0.887)	-0.274 (0.996)	0.312 (0.877)
Personal Vote	7.781 (3.905)*	2.015 (0.82)*	9.520 (4.276)*	1.969 (0.843)*	7.188 (4.029)	2.216 (0.85)**
Cabinet Size	-0.167 (0.094)	-0.087 (0.051)	-0.136 (0.101)	-0.086 (0.051)	-0.133 (0.098)	-0.096 (0.051)
Lag Strikes	-0.119 (0.263)	0.216 (0.223)	-0.091 (0.264)	0.219 (0.220)	0.038 (0.290)	0.390 (0.257)

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[19] We also examined exchange rate crises in unreported results, which are correlated with debt crises, but this variable is never significant on its own.

Table 5., continued

	(1) Cond Logit	(2) BTSCS	(3) Cond Logit	(4) BTSCS	(5) Cond Logit	(6) BTSCS
One-Party	-0.003	-0.000	-0.004	0.000	-0.004	-0.000
	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)
Bureaucracy	-0.397	-0.057	-0.456	-0.055	-0.439	-0.074
	(0.383)	(0.276)	(0.400)	(0.282)	(0.393)	(0.281)
N Prev R	0.375	0.410	0.398	0.606	0.334	0.438
	(0.097)**	(0.920)	(0.101)**	(0.938)	(0.097)**	(0.964)
Time Since R	-0.099	0.362	-0.163	0.424	-0.434	0.216
	(0.228)	(0.157)*	(0.250)	(0.162)**	(0.262)	(0.187)
Time spline1		-0.385		-0.239		-0.248
		(0.612)		(0.615)		(0.656)
Time spline2		0.395		0.295		0.274
		(0.415)		(0.417)		(0.451)
Time spline3		-0.120		-0.106		-0.089
		(0.067)		(0.067)		(0.075)
Lag 2 Banking			-0.964	-0.116		
			(0.634)	(0.562)		
Lag banking in others					-2.601	-1.402
					(1.113)*	(1.208)
Lag debt in others					10.553	6.268
					(4.905)*	(4.831)
Constant		-2.047		-2.196		-1.550
		(1.099)		(1.113)*		(1.115)
Lag2 restruc			0.178	0.348		
			(0.823)	(0.656)		
Lag2 Debt			-14.414			
			(1,612.879)			
Observations	253	253	253	249	253	253

Standard errors in parentheses
* significant at 5%; ** significant at 1%
Source: Authors' estimations.

The political variables are also of particular interest. As the first conditional logit indicates, most of the political variables have no discernible effect. There is no clear trend, for example, for the effects of pre-electoral periods, partisanship, the lag of general strikes, checks and balances, and bureaucratic quality. Three of the political variables have fewer observations,

and we drop them for the second equation, with the results generally staying the same when the number of observations increases from 190 to 253.

The personal vote, however, is an exception for the political variables—it is consistently significant and in the expected direction. The more the electoral system favors a personal vote at the expense of a party vote, the more likely there is fiscal institutional reform. In terms of marginal effects and setting the other variables at their means, a move from the 5 percent value of the personal vote to the 95 percent value increases the expected probability of a reform 0.20 with a standard error of 0.09.[20] Given that this is an increase in the probability of reform every year, this is a substantial jump.

Hypothesis 3 considers whether reforms follow banking crises, with the expected lag to be two years based on Reinhart and Rogoff's work, while Hypothesis 5 predicts that reforms come well after crises have passed. As we already noted above, however, fiscal crises when they happen occur in the same year as a banking crisis rather than with delay. Nevertheless, Table 5 provides the results, which indicate that no measure of crisis is significant when the banking crisis variable is lagged one and two years.

Hypothesis 4 considers whether crises in other parts of Latin America have an effect on fiscal reforms at home. We create variables that capture the share of regional GDP (not including the country under examination) that is experiencing a given type of crisis. The idea is that if big regional neighbors are having problems then this puts pressure on the domestic government, while if a "small" country in terms of GDP has a crisis that will have less of an effect. Conditional logits with contemporaneous measures did not yield interesting results, but there is a strong finding when one considers one-year lags of these more regional measures. Pressure from regional crises has some effect at home—when the share of regional GDP of other countries caught in debt crises in the previous year increases, domestic reforms are more likely. Similarly, banking crises in other countries the year before decreases the chances of domestic reform.

7. Conclusion

This paper considered the connection between economic crises in Latin America and fiscal institutional reforms. It reviewed five hypotheses about why, and under what circumstances, crises would promote reforms. It is clear that some types of crises cluster, and that there are

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[20] The routine Clarify (Tomz, Wittenberg and King, 2001) under Stata 11.1 was used to compute marginal effects.

moments when banking, exchange rate, and fiscal crises happen at the same time. Banking crises on their own if anything reduce the pressure for fiscal institutional reforms. This could be because the government is otherwise preoccupied or because there simply is not the same pressure on state budgets that then encourages more fundamental institutional reforms, but the empirical evidence is quite clear. If a debt crisis happens domestically at the same time, however, then the negative effect of banking crises disappears. The results are stronger for crises in other countries, however, as debt crises in countries with a larger share of regional GDP make it more likely that the home country reforms.

Ideally, it would be useful to have other measures to pick up this type of pressure on leaders. We have only actual crises in the dataset, and one can imagine that a very successful reform may have prevented a crisis from breaking out in the first place. We did examine possible data sources for market data in the expectation that market movements may provide us some guidance, but the extant datasets do not cover all of our countries and often start in 1999, so we have data for less than half of our observations. At the same time, we believe that the measure for crises in other countries as well as the personal vote variable, which is a measure for the potential pressure on government, picks up much of this.[21]

In terms of political variables, the most important is whether the electoral system encourages a personal vote. If it does so, the country is more likely to reform. This finding is counter to what one would expect from the literature on neo-liberal reform, where one expects that stronger parties lead to more reform. Note that we anticipate that greater CPR problems lead to a greater likelihood of crisis. The results suggest that this pressure counterbalances, and exceeds, the need for stronger parties at least in this group of countries. Readers should note that at this stage we can only predict that reforms were more likely under some conditions. Moreover, the personal vote variable is functional in nature—it simply predicts when a reform would have a higher payoff for an actor. One would want to know more about the individual policymaking process and the actors under it to have a better understanding of when reforms actually occurred.

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[21] We also explored Heckman models where we would predict first the crisis then whether there is reform. As we noted before, however, different types of crises are correlated, and we did not have satisfactory results.

Appendix: Robustness Checks

Table A1. Comparison of Results Using Narrow and Broad Measures of the Banking Crisis Variable

	Narrow (LV) Bank Crisis Measure		Broad (RR) Bank Crisis Measure	
	Conditional Logit (fixed effects)	Conditional Logit (fixed effects)	Conditional Logit (fixed effects)	Conditional Logit (fixed effects)
Banking Crisis (Laeven-Valencia)	-.99 (1.08)	-.20 (.82)		
Banking Crisis (Reinhart-Rogoff)			-2.14** (.82)	-1.26* (.05)
Exchange Rate Crisis		-.66 (1.11)		-.23 (1.15)
Debt Crisis (Laeven-Valencia)	2.09 (1.43)		3.41** (1.37)	
Debt Restructuring	-1.31 (1.10)	-1.42 (1.10)	-1.46 (1.13)	-1.54 (1.12)
Number of Years Since Last Reform	.33** (.08)	.33** (.08)	.35** (.08)	.34** (.08)
Number of Previous Reforms	.14 (.15)	.17 (.14)	.08 (.15)	.12 (.15)
Likelihood Ratio	25.88	23.86	34.50	28.45
p	0.00	0.00	0.00	0.00
N	272	272	272	272

*p<.05, ** P<.01

Note: If one looks at the strict definition of banking crisis, the results on the crisis variables weaken—it appears that no crisis leads to more reform. The results with the less strict measure of banking crises from Reinhart and Rogoff, however, suggest effects.

	Narrow (LV) Bank Crisis Measure		Broad (RR) Bank Crisis Measure	
	Conditional Logit (fixed effects)	Conditional Logit (fixed effects)	Conditional Logit (fixed effects)	Conditional Logit (fixed effects)
Banking Crisis (Laeven-Valencia), 2-Year Lag	-.97 (.81)	-1.03 (.81)		
Banking Crisis (Reinhart-Rogoff), 2-Year Lag			-.80 (.51)	-.89 (.51)
Exchange Rate Crisis		-.75 (1.11)		-90 (1.11)
Debt Crisis (Laeven-Valencia)	1.28 (1.08)		1.23 (1.08)	
Debt Restructuring	-1.29 (1.08)	-1.40 (1.10)	-1.39 (1.11)	-1.51 (1.11)
Number of Years Since Last Reform	.35** (.08)	.34** (.08)	.35** (.08)	.35** (.08)
Number of Previous Reforms	.15 (.15)	.17 (.14)	.12 (.15)	.12 (.15)
Likelihood Ratio	26.51	25.71	27.45	27.01
p	0.00	0.00	0.00	0.00
N	272	272	272	272

*p<.05, ** P<.01

	Conditional Logit
Banking Crisis (Reinhart-Rogoff),	-2.31*
	(.92)
Debt Crisis (Laeven-Valencia)	3.08*
	(1.37)
Debt Restructuring	-1.14
	(1.13)
Banking Crisis (Reinhart-Rogoff), 1-Year Lag, Other Countries, GDP Weighted	-2.52* (1.02)
Debt Crisis (Laeven-Valencia), 1-Year Lag, Other Countries, GDP	10.23* (4.64)
Number of Years Since Last Reform	.32**
	(.08)
Number of Previous Reforms	-.26
	(.19)
Likelihood Ratio	45.73
p	0.00
N	272

Table A2. BTSCS Logistic Regression with Political Variables

	Conditional Logit
Banking Crisis (Reinhart-Rogoff),	-1.60
	(.87)
Debt Crisis (Laeven-Valencia)	3.14*
	(1.40)
Debt Restructuring	-0.44
	(1.04)
Presidential Pre-Electoral Period	.82
	(.59)
Personal Vote	2.30*
	(.97)
Cabinet Size	-.09
	(.05)
General Strikes	.56**
	(.21)
Quality of Bureaucracy	-.19
	(.32)
Number of Years Since Last Reform	.87
	(.86)
Number of Previous Reforms	.39
	(.15)
Time Spline 1	.05
	(.60)
Time Spline 2	.07
	(.42)
Time Spline 3	-.06
	(.07)
Wald	34.94
p	0.00
N	236

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