Financial Crisis and Fiscal Reform in Latin America: When Do Governments Improve Fiscal Institutions?

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Abstract

This paper explores the connection between financial crises and fiscal institutional reforms in a region that has experienced plenty of both in recent years, namely Latin America. It also tests a series of political arguments. Some authors expect that crises lead to reforms, but we demonstrate that the relationship is not so straight-forward. The initial policy response to a financial crisis includes fiscal expansions for two reasons: One, the government is often the only actor who can bail out troubled banks; and two, governments use fiscal expansions to stabilize output when consumption declines. In this environment of a demand for more spending, fiscal institutional reforms are less likely, not more. If the crisis lasts, however, pressure on public finances increases. If the crisis evolves into a sovereign debt crises, there should be an additional increase in the probability of reform. The empirical evidence confirms this argument. Banking crises reduce fiscal reform efforts to zero in the early years of a crisis. As the crisis lasts, however, pressure for reform builds. In cases where a sovereign debt crisis also develops, fiscal institutional reforms are more likely, increasing from a zero probability to 33% in the initial year of the banking crisis.

Introduction

Since 2008, many countries have experienced a rolling set of crises. After the worldwide financial crises that began in 2007 and 2008, recent attention has focused on the fiscal situation in the US, where agreements about fiscal consolidation remain to be made, and in the eurozone, where some countries are now under joint European Union and International Monetary Fund programs. While these cases have received the most attention, much of the industrialized world is facing pressure to initiate fiscal consolidations. The pressure is not only for policy reform (i.e., cuts in expenditures and/or increases in

revenues) but also for fundamental institutional reform. A variety of actors, be they heads of international economic organizations, presidents of central banks, or prime ministers of countries, have called for fiscal institutional reforms, and the European Union in particular has strengthened its fiscal framework in 2011 and 2012.

This paper examines the connection between financial crisis and fiscal institutional 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 and to have avoided financial crises. 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. Latin American politicians have expressed exuberance claiming that their policies have worked.[1] Many analysts have concurred that better fiscal management before the crises allowed for these countries to respond swiftly to the negative shock so that the effects of the crisis in the developed world were minor in Latin America.[2][3] Were these apparently successful reforms the result of learning from previous crises?

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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] See, for example, this statement from the *IMF Regional Economic Outlook 2009:* "Policy frameworks in many LAC countries have improved substantially during the last decade, particularly among the largest economies. Countries in the financially integrated commodity exporting group, for example, adopted inflation targeting and more flexible exchange-rate regimes. Several countries also have adopted fiscal frameworks that establish fiscal and debt sustainability rules." Available at: http://www.imf.org/external/pubs/ft/reo/2009/WHD/eng/wreo1009.pdf

[3] 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; see, for example, the IMF's praise of the role that fiscal responsibility had in Brazil.

In the past 20 years, three major financial crises have hit the region: the Tequila Crisis, the Brazilian (or "caipirinha") crisis, and the Argentine (or "Tango") 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 financial crises that have been concentrated on specific countries. Latin American governments have introduced several fiscal institutional 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. After these reforms, countries in Latin America have in general fared much better in terms of their fiscal results than before the reforms. Several authors contend that the improved fiscal institutional frameworks directly contributed to higher levels of fiscal discipline (e.g., Filc and Scartascini 2007; Eslava 2012).

In this paper, we want to understand why countries experienced fiscal institutional reforms in the first place. In particular, when a financial crisis hits a country, under what conditions does the government initiate fiscal reforms? What are the political conditions that make reforms more or less likely?

This paper begins with a discussion of our dependent variable, which is whether there is fiscal institutional reform. The second part considers how financial crises may be connected to reforms and it explains how crises are measured in the literature. The third part focuses on additional catalysts for reform. The fourth part of the paper provides the empirical analysis. The final section includes robustness tests. We find that financial crises initially retard fiscal reforms so much that they simply do not occur. As the crisis continues, however, governments do need some credibility with markets and they introduce fiscal reforms in later years. Moreover, if the crisis transforms itself into a true sovereign debt crisis, there is a jump in the probability of reform.

Available at: http://www.americaeconomia.com/economia-mercados/fmi-elogia-solida-estructura-de-politica-economica-de-brasil-en-revision-de-2010

Fiscal Institutions Reforms in Latin America

Conceptually, we are interested in changes in rules and institutions. The core theoretical model why fiscal institutional reforms that centralize the budget process are important assumes that all policy-makers face a common pool resource (CPR) problem. This arises when actors care only about the spending and revenue implications of their decisions on their constituencies, but, because taxes are paid by everyone, their constituency tax burden is smaller than the full tax implications of the spending. An agriculture minister, for example, may worry most about how farmers benefit from spending programs and how much tax they pay. If she can shift part of the cost to other sectors of the economy then she may be more willing to ask for higher benefits for her sector. 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 burden on her province is smaller than if her province had to bear the full tax burden.

Based on this underlying model, there is an established literature on the effects of fiscal institutions. Much of it focuses on developed countries and indicates that centralization of the budget process leads to tighter fiscal discipline in European and OECD countries (e.g. Hallerberg and von Hagen 1999; Fabrizio and Mody 2006; Baldacci et. al 2010) and in American states (e.g., Alt and Lowery 1994; Poterba and Rueben 2001), while increases in transparency of the process have similarly led to healthier budget balances (e.g., Alt and Lassen 2006). There is also a literature that attempts to explain the introduction of these fiscal institutional reforms, and it focuses on either the European Union member states (Hallerberg, Strauch, and von Hagen 2009; Fabrizio and Mody 2010) the G-20 (Debrun et. al 2008; IMF 2009), or American states (Alt, Lassen, and Rose 2006).

Specifically for Latin America, there is an analogous literature on the effects of fiscal institutions on fiscal outcomes (e.g., Alesina et. al 1999; Filc and Scartascini 2007; Caceres et. al, 2010). Summarizing this body of work on Latin America, Eslava (2012) concludes that "the finding

that good budget institutions increase budget discipline is quite robust (p. x).” Given this background, what is missing is a consideration found in the literature on the developed world why there are fiscal institutional reforms in some countries and not others in Latin America. To our knowledge, this paper is the first to focus on this question.[4]

We are interested in explaining reforms that centralize the budget process and reduce the scope of the CPR problem, in particular by setting limits to the outcomes of budget negotiations. Our dataset codes reforms for 17 Latin American countries for the period 1990-2005. We begin in 1990 because this is the date when countries in the region with the exception of Cuba and Haiti have elective presidential democracies. Pérez-Liñan and Mainwaring (2010) indicate that 1990 is the first year where all countries in our sample have truly competitive political systems (see also Mainwaring, Brinks, and Pérez-Liñan 2006). The fiscal institutions we care most about are institutional changes to decision-making processes in democracies, and it does not make sense to go 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 a period of “relative” political calm. This means that we can exploit the variance across periods and across countries.

There are three broad types of fiscal institutions reforms (von Hagen, 1992; von Hagen and Harden, 1995; Alesina et al., 1998, Filc and Scartascini, 2007). 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. It establishes the norms and prerogatives of actors in the budget process. A restriction on the type of amendments the legislature can make to the proposal submitted by the executive is an example of such a rule. 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

[4] This is, of course, a nuanced literature that examines neo-liberal reforms in Latin America in the 1990s (e.g., Stokes 2001; Weyland 2002; Levitsky and Murillo 2005; Wibbels 2005). It does not directly address fiscal institutional reforms.

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 reforms in 6 years out of the 15-year period. While there are several reforms from 2000 on, there are sixteen reforms in total in the 1990s.[5]

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 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*; see Melo et. al 2009). 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 liable to a lifetime ban from politics and to 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.[6]

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[6] See Alston et al. (2009) for more details on the Brazilian case. Hallerberg et al (2009) compares the success of the Argentine and Brazil experiences with the fiscal responsibility law.

Based on this template, Table 1 lists the reforms in our dataset since 1990.[7] 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 in fiscal institutions, coded dichotomously, with 16.5 percent of years in the dataset being reform years.[8] The empirical section will provide more detail about how to model cross-section and time elements**,** but a few comments are in order here. First, one should model whether a country has had previous reforms. Second, we include time splines to capture the time element.[9] Third, the apparent dependencies across time and space may also be due to diffusion. Weyland (2002), for example, finds that ideas about social security reform moved from Chile onto other countries in the region over time. We also consider explicitly whether other countries in the region have introduced reforms weighted by various factors, such as the size of the economy of whether the other country is contiguous.

[7] Filc and Scartascini (2007) assembled the dataset through visits and interviews with government officials, surveys, and analysis of legislation. They distributed answers to government officials from the Budget Office in each country to check its accuracy. We updated the dataset for this paper.

[8] In our database we don't include a reform that has been coded in Filc and Scartascini (2007), namely the increase in the power of the finance ministry, because it may be a more subjective assessment.

[9] Other specifications in the robustness section include year dummies; one then loses observations from 1991 and 1992 because there are no reforms in those years. As the last section will show, these additional restrictions do not affect the results.

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				
Dominican Rep															T	
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			
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: Subnational Govts; 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.

When Do Financial Crises Accelerate or Retard Reforms?

The literature suggests that crises more generally can 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.[10]

Consistent with the "windows of opportunity" literature, both Rodrik's (1994) discussion of the role of crises in advancing trade reforms as well as Alesina and Drazen's "war of attrition" model focus on domestic opposition to reforms. In "normal" times, various interest groups block reforms. Alesina and Drazen's (1991) "war of attrition" model focuses 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 to get inflation under control. Drazen and Grilli (1993) suggest that only when the crisis hits and when all groups are bearing high costs anyway is it politically feasible for the government to take the steps necessary to address the crisis.

This leads to an important difference between the type of reforms discussed above and the reform of fiscal institutions. While fiscal institutional reforms have distributional effects because they usually lead to tighter fiscal discipline, who hurts and who benefits from such reforms is not so clear-cut. This situation is different than under trade, where the structure of the economy leads to fairly

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

straight-forward predictions about who benefits and who gets hurt. The adjustment story from Alesina and Drazen depends upon the nature of the underlying crisis.

It is also important to pay attention to the type of crisis as well as their sequence. As we explain in more detail below, while we focus on financial, or banking, crisis, there are several reasons why sovereign debt crises sometimes develop during, and after, the financial crisis.[11] These two are clearly related--reforms to address a banking crisis will be expensive.

At the beginning of a banking crisis, there have rarely been concurrent sovereign debt crises (Laeven and Valencia, 2012). The danger to the public fisc usually comes some years afterward. 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. There are three reasons for why debt goes up so dramatically. The first is the costs of the bailout itself. The second is due to 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 147 banking crises over the period 1970-2011, 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] The final reason is the costs of active fiscal policy in the form of a fiscal stimulus to stabilize the overall economy during a banking crisis. Such use of fiscal policy during banking crises can be beneficial--Baldacci et. al (2009) find that banking crises are shorter in countries that have such fiscal stimuli. During our sample period as opposed to Laeven and Valencia's worldwide dataset, 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).

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[11] In other work (see Hallerberg and Scartascini 2012), we examine other measures of crisis, such as so-called "sudden stops." The datasets are available for only a subset of countries, however.

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

These debt dynamics implications for the pressure on governments to undertake fiscal reforms. Countries with low levels of debt may be more likely to introduce stimulus packages. Reforms meant to tighten fiscal discipline are seen as counter-productive—the whole point is to spend more money to address the crisis. As the debt piles up, however, governments lose the confidence of markets, and there is reason to introduce more centralized fiscal institutions to signal that the run-up in debt should end. As Jácome (2008, p. 14) notes, "highly indebted countries were generally unable to raise money in—domestic or international—capital markets during periods of financial stress, thereby hindering governments' capacity to cope with banking crises using non-inflationary means. In these circumstances, tightening fiscal policy may be the only alternative countries have…" This line of argument suggests that reforms are less likely during the initial phase of a banking crisis. As the crisis continues, the need to signal to markets future fiscal behavior increases as well, and the probability of reforms also increases. To test this, we model market pressure on governments directly by including the (lagged) interest costs on external debt as a percent of exports. The idea here is to capture the ability of the government to continue to fund such costs.

Note that the pressure from a banking crisis over time should exist whether or not a country enters a sovereign debt crisis. Such a crisis exists formally when a country defaults. This decision, in turn, is a government decision and one that is hard to predict—some countries like Japan have debt to GDP ratios approaching 250%, while Argentina defaulted in 2001 with its debt to GDP ratio less than 60%. In our dataset, if one looks at external debt as a percent of gross national income (a more narrow definition of economic output than GDP), countries not in default have an average debt amounting to 261% of GDP. Countries in default have an average external debt level of 282%, which is not appreciably higher.[13]

In terms of our predictions of fiscal reform, however, a sovereign debt crisis has an additional effect on the probability of reform for three reasons. First, and consistent with Alesina and Drazen

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[13] The t-test of the difference in means is not close to statistical significance at the $p<.1$ level.

(1991), the default ,may force the different interests finally to act on the problem. A new fiscal rule may be part of any agreement among the interests. Second, the default means governments have lost credibility with markets. A government may intend for a fiscal reform to be part of a more general process of rebuilding its relationship with investors. Third, in cases like Argentina where market relations remain bad, governments may have to impose fiscal rules because they cannot borrow anymore and they need tools to restrict spending.

In terms of the measurement of sovereign debt crises, Laeven and Valencia (2012) 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. Sovereign debt restructurings, however, are more common, with 16 cases that include all the countries but Colombia, El Salvador, and Guatemala. Note that several of these restructurings follow defaults that occurred in the period before 1990, or the beginning of our dataset.[14] In the regression analysis, we create one variable that considers periods from debt default to debt restructuring, which is analogous to the banking crisis variable that extends from the beginning to the end of a given crisis, for the sovereign debt crises in our dataset.[15]

So far the emphasis has been on domestic crisis, but there are reasons to believe that crises in the region may also 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

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

[15] Laeven and Valencia (2012) 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 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.

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 mean 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.

Finally, and 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. Most importantly, the IMF and other international organizations may have less public ways of suggesting reforms. Even if a country is under an IMF program, it may ask the Fund to include certain reforms in the program so that it can blame the Fund for the new policies. Causation is then a real issue—did Fund pressure lead to reform, or are statements in country programs mainly signals that the government has decided to undertake certain reforms anyway? Nonetheless, we do include a variable for whether a country is under any IMF program. Biglaiser and DeRouen (2011) find that IMF involvement increases the likelihood of some types of economic reforms. We will therefore test explicitly whether countries reacted differently to crises given whether they were under an IMF program or not.

Table 2: Crises in Latin America

Country	*Systemic Bank Crisis*	*Currency Crisis*	*Debt Crisis*	*Debt Restructuring*
Argentina	1990-91, 1995, 2001-03	2002	2001	2005
Bolivia	1994			1992
Brazil	1990-98	1992, 1999		1994
Chile				1990
Colombia	1998-2000			
Costa Rica	1994-95	1991		1990
Dom Rep	2003-04	1990, 2003	2003	1994, 2005
Ecuador	1998-2002	1999	1999	2000
El Salvador	1990			
Guatemala				
Mexico	1994-96	1995		1990
Nicaragua	1990-93, 2000	1990		1995
Panama				1996
Paraguay	1995	2002		1992
Peru				1996
Uruguay	2002-05	1990, 2002	2002	1991, 2003
Venezuela	1994-98	1994, 2002		1990

Source:Laeven and Valencia 2012

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

Political and Economic 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 will interact with the crisis and make reforms more or less likely. Moreover, 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.

A first set of variables models the size of the common pool resource (CPR) problem, which is the source of fiscal indiscipline that institutional reforms are meant to reduce. Studies of the CPR problem generally presume that it is a constant in all government settings. But political institutions may affect its overall size—the more a decision-maker thinks 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. Countries with institutions that create 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 (2011), 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. 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.[22] 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 because of the risk that implies not reforming. There should be a positive relationship between the extent of the personal vote and the likelihood of fiscal reforms.

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 argue in favor of 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 CPR problem is much bigger under personalistic systems, and that this pressure makes reforms that are meant to relieve this pressure more likely.

A pre-electoral period may also affect the likelihood of reform. Brender and Drazen (2005) suggest that fiscal cycles are especially prevalent in young democracies. Barberia and Avelino (2011), however, find that the age of democracy in their Latin American sample is not

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

relevant, and that political business cycles are common in the region. Both articles suggest a negative relationship with reforms prior to an election--the point of reform is to centralize the budget process, which may restrict the ability of the government to run fiscal cycles. Following Franzese (2002), we measure electoral periods according to the proportion of the current year that is part of a pre-electoral year. This means, for example, that an election on July 1, 2000, would be measured as 0.5 in 2000 and 0.5 in 1999.

There is an ongoing debate about whether the presence of "veto players" accelerates or retards reform. The classic text is Tsebelis (2002), who would expect that increasing the number of veto players that do not share the same preferences should decrease the possible space of changes to the status quo. Hellman (1998) as well as Gehlbach and Malesky (2010) a decade later, in their analyses of economic reform in Central and Eastern Europe, suggest that more veto players make it harder for special interests to block further reform. This means that increasing the number of veto players should make reforms more likely rather than less. Scartascini and Tommasi (2012) suggest that, when intertemporal bargains are included, that the effects of the number of veto players is ambiguous. In our empirical analysis, we use the variable "allhouse" from the Beck, et. al (2010) dataset, which is coded as "1" if one party controls the relevant houses of Congress and "0" otherwise. This picks up the contrast between countries with "united" government and "divided" government.[24]

In contrast to the number of parties, one can also consider their ideology. There is a rich literature that considers the effects of partisanship on neo-liberal reforms in Latin America. As Stokes (1999) has suggested in her study of the introduction of such 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

[24] In robustness regressions, we also include the "checks" variable from Beck et. al. 2010.

between partisanship and reform. Thinking about developments over a long span of time, including over the 2000s when there was a general leftist tilt to electoral outcomes in the region, Baker and Greene (2011) as well as Murillo, et. al (2010) using somewhat different measures of partisanship contend that the changes in partisanship have not been as great as some presume--there has been a general move the last decade from center-right to center (rather than left) presidents. Leftist presidents have, however, made a subtle but important difference in policies—they have stalled or even on occasion reversed reforms consistent with the so-called "Washington Consensus" (Baker and Greene 2011). To the extent fiscal reforms are seen as part of the Washington Consensus and to the extent that Left presidents act to roll back such reforms, fiscal reforms should be more common under Right presidents. Yet partisanship may be relevant for another reason related not to market reforms but to the markets themselves--there may be a greater need for "leftists" presidents to signal to markets that they are serious about the economy. In this case, Left Presidents should introduce more reforms than Right Presidents. Our measure for partisanship comes from Murillo et al. (2010) and is on a five-point scale, with 1 the left-most and 5 the right-most president.

We also include important economic variables. A deterioration in the terms of trade of a country means that more exports would be needed to finance the same debt level. The (lagged) terms of trade should therefore have a negative sign; countries that are doing better should face less pressure to reform.[25] In additional regressions, we consider variations based on economic growth in term of the average relative to five years previously; pressure for reform may increase when average growth declines over time.[26] An interest rate shock may also push governments to respond with fiscal reforms; we measure this as…

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[25] We use the net barter terms of trade index, with 2000=100.

[26] An alternative measure is growth the previous year. In neither case did inclusion of the variables substantively change the core results.

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 considers economic determinants of fiscal reforms (only) and finds that countries introduce reforms under good economic conditions, considers both parametric and nonparametric hazard models. Their paper also uses conditional logits to predict whether a given fiscal rule is in place.

There are, however, issues with using standard event history analysis given the distribution of our dependent variable. 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. While there are some techniques to deal with this issue, more problematic is that patients may "die" in consecutive years (or initiate reforms over consecutive years).[28] It is difficult to model how they re-enter the sample.

For this reason, we use two techniques. Table 4 presents results from a BTSCS (Binary Time-Series Cross Section data) model with time splines.[29] This is equivalent to a random effects logit. As Clark and Linzer (2012) note based on their extensive simulations, random effects are superior to fixed effects when there is relatively little within-unit variation for independent

[28] We thank Erik Wibbels for extensive discussions about how best to model the data.

[29] See Beck, Katz, and Tucker (1998) for more detail. We also reran the analysis with country year dummies; those results are reported in the robustness section

variables, which is the case for some of our political variables, and when there are relatively few observations per unit, which is again the case in our dataset. For robustness checks in the next section we also estimate several additional specifications, including a conditional logistic regression (which incorporates 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. We use the Clarify software to calculate marginal effects of moving from the 1st to the 99th percentile for a given significant variable with the other variables set at their means.

Some modeling issues remain with the logit construction, and with the standard event history models more generally. First, whether a country introduces a reform may depend upon what reforms it has introduced before. Second, leaders may "learn" from the examples set in different countries. To deal with repeated events, we include a variable that counts the time since the last reform. To deal with subject event dependence, we have models to capture diffusion, although because the diffusion variable is not statistically significant we exclude it in this first analysis. [30,31]

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[30] Ideally, one would also consider not only the onset of the reforms but also their overall duration. This would be appropriate especially for fiscal responsibility laws, where there is data for some countries on how long the law remained in place. A Markov transition model in this case would allow one to include covariates that specifically affect event onset, event duration, and both onset and duration. A challenge, however, is the number of observations as well as certain knowledge about when a reform was no longer in place. Given few degrees of freedom, such an analysis would only be suggestive.

[31] Another approach worth consideration is a conditional frailty model (Box-Steffensmeier, De Boef and Joyce, 2007). This model allows one simultaneously to model both subject event dependence and heterogeneity. It assumes that some units are more or less prone to "failure" over time. One should 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

Our dependent variable has maximum of 272 observations, which corresponds to whether there were fiscal reforms during the time period 1990-2005 in seventeen countries (in practice models the number of observations is somewhat lower because of missing values in one or more of the independent variables). Results are presented in Table 3., with columns 1 and 3 presenting the raw numbers while columns 2 and 4 present the marginal effects given that the other variables are at their means.[32] Our focus is on the effects of financial crisis, and columns 1 and 2 present results where the financial crisis is coded as a dummy variable while 3 and 4 have results for when it is coded as a count, with the expectation that initially financial crises retard reform but over time they encourage reform.

The results suggest that financial crises do make reform less likely. If the variables are set at their means except for banking and debt crises, which are set at 0, the overall probability of a reform is .12. Column 2 indicates that this probability then falls effectively to zero when there is a banking crisis.[33] This change in probability is statistically significant at the .01 level. The coefficient on fiscal crises, however, is positive and significant at the $p<.05$ level, and the marginal effect is large at .43 if the others are set at the means. In practice, however, we are also interested in the case where banking crises are concurrent, not where such crises are at their means; the Table also displays the effects of a fiscal crisis (defined as the time from default to debt restructuring) given the presence of a banking crisis. In this case, the marginal effect from moving from no fiscal crisis to a fiscal crisis is an increase in the probability of .20, which is also

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component to estimate the frailty portion as well as estimates for event-specific baseline hazards. Preliminary results using the R-statistics program were substantively similar to the results for the conditional logics, but the model had difficulties computing when more than three variables were included. The BTSCS results provide the basis for the analysis that follows.

[32] The marginal effects are calculated with the statistical software package Clarify; see

[33] That is, the baseline is .11 while the banking crisis marginal effect is -.11, so there are then no reforms.

statistically significant.[34] These results therefore fit our theoretical expectations—financial crises have a negative impact on fiscal reforms initially, but if they turn into a sovereign debt crisis the effect reverses.

We also expect that each consecutive year of a banking crisis affects the probability of reforms, so that the negative impact moves towards zero and then becomes positive. The logit analysis with a simple dummy variable cannot capture whether such effects exist. The remaining columns consider the financial crisis as a count, and it includes the squared term as well to capture non-linear effects. As before, we are interested in the effects of a banking crisis without a fiscal crisis and with a fiscal crisis. The prediction again is that a banking crisis without a sovereign debt crisis depresses the chances of reform, but a sovereign debt crisis increases the chances of reform. Interpretation of the straight coefficients is difficult, so Figure 1 graphs the predicted probability of reform given the number of years of a financial crisis both for banking crises without, and with, a simultaneous debt crisis.[35] Looking at the expected probability of reform for banking crises without fiscal crises first, one sees that the effect drops from .12 to close to zero for the first five years of the banking crisis. It then picks up and approaches .4 by the eight year of the crisis. As expected, there is a shift in the curve if there is also a sovereign debt crisis at the same time, with the shift especially large in the first year of a banking crisis, to a probability of .33. The curve then drops but stays consistently above the curve for a banking crisis without a fiscal crisis, and it also stays above the non-crisis year prediction in all years but Year 4.

Consistent with this argument about the increasing pressure of debt is that increases in interest payments as a percent of exports on external debt increase pressure for fiscal reforms.

[34] This result cannot be read directly from the logit results table; it is calculated using Clarify, where all variables are set to their means but banking crisis, which is set to 1.

[35] The remaining variables are set at their means.

The variable is statistically significant at the .01 level and a move from the 1st percentile country (Panama 1994) in terms of debt interest to the 99th percentile country (Argentina 2001) increases the likelihood of a fiscal institutional reform .30 percentage points.

Table 3: Banking Crisis, Fiscal Crisis, and the Probability of Reform

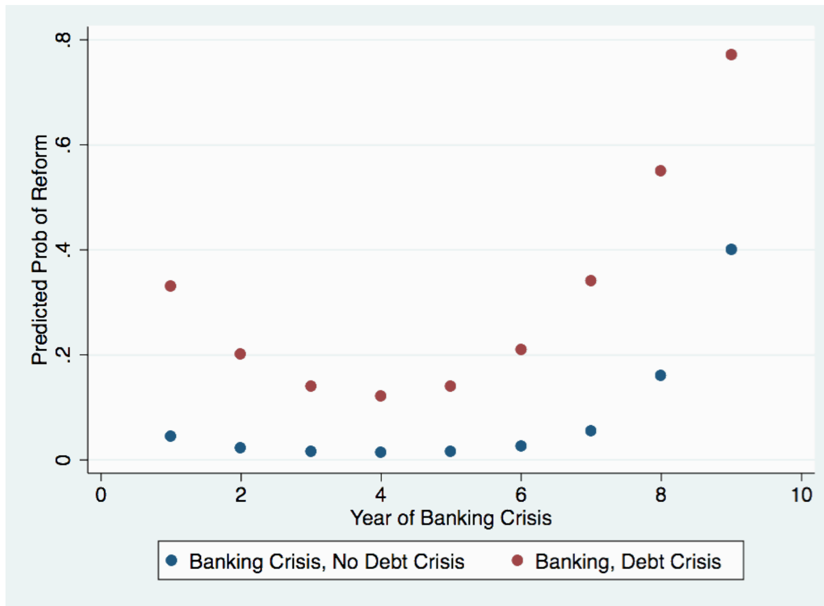
VARIABLES	Fiscal Reform	Move from 1th to 99th percentile	Fiscal Reform	Move from 1th to 99th percentile
Baseline Probability		0.11		
Banking Crisis	-1.79***	-0.11		
	-0.68			
Banking Crisis--Yearly Count			-1.35***	
			(0.39)	
Banking Crisis--Yearly Count (sq)			0.17***	
			(0.05)	
From Default through Restructuring	2.36**	0.43	2.37**	
	-1.05		(1.00)	
Default Given Banking Crisis		0.20		
Presidential Election Year	0.72		0.65	
	-0.57		(0.58)	
Personal Vote	2.01**	0.19	1.87*	
	-0.96		(1.00)	
United Government	0.64		0.76	
	-0.46		(0.47)	
Ideology of President	0.15		0.19	
	-0.22		(0.23)	
Interest Rate Shock	-0.66		-0.78	
	-0.78		(0.78)	
Average Growth	-3.00*		-3.65**	
	-1.77		(1.84)	
Interest on External Debt (lag)	0.08***	0.31	0.08***	
	-0.03		(0.03)	
Terms of Trade (lag)	-0.06***	-0.44	-0.07***	
	-0.02		(0.02)	
IMF Program	-0.06		-0.06	
	-0.44		(0.45)	
Time Since Last Reform	1.32		1.38	
	-0.89		(0.88)	

Number of Previous Reforms	0.32**	0.36**
	-0.14	(0.14)
Constant	0.59	1.37
	-1.6	(1.68)
Observations	256	256

Robust standard errors in parentheses. Time splines included but not reported.

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

Figure 1: Predicted Probability of Reforms According to Year of Banking



Explanation: This graph compares the predicted probabilities based on the consecutive year of a banking crisis on reform given no concurrent fiscal crisis and given such a crisis.

While we will evaluate the robustness of these core findings in the next section, there are additional results of interest.

An increase in the personal vote, which serves as a measure of the size of the CPR problem, makes fiscal reforms more likely. As such, an electoral system with the most personalistic system (Colombia 1990-93) would have an almost sixteen percentage point increase in the probability of reforming its fiscal institutions over the country in Latin America with the most party based electoral system (Mexico, entire time period). Given that this is an increase in the probability of reform every year, this is a substantial jump. To be convinced that

the mechanism is what we say it is, namely that the personal vote increases pressure on governments for reform because the consequences of failure to reform are greater, would require a process tracing exercise that is not the main point of this paper, but the results are stronger suggestive.

Two economic variables have expected effects. Deteriorating terms of trade appear to increase pressure on the government to take fiscal institutional steps. Similarly, while only statistically significant at the p=.1 level in the first logit, weaker economic growth than the past average also leads to more reform.

Some variables we expected to influence reforms are not relevant. An IMF program does not increase the likelihood of reform. As we suggested in the theoretical section, whether to interpret an IMF program as a signal that the government wanted to initiate the reform anyway or whether it represents true international pressure is unclear and difficult to untangle in any case. for the role of the IMF. None of these variables (neither one that considers whether there was a program or not nor one that counts the number of programs) is significant. Similarly, united government is not significant at the .05 level, although the sign suggests that the view that more veto players may facilitate reform is significant at the .17 level, so there is no support for the argument that divided government inhibits fiscal reform. Finally, the partisanship of the president is not relevant; left and right presidents are equally likely to initiate reform.

One can question whether we chose the correct empirical model, and the next section evaluates the robustness of our results.

Robustness Checks

There are two ways to approach the robustness of the results—one based on the statistical model and the second on additional variables for which there are reasonable grounds to consider their inclusion.

In terms of the modeling approach, we reproduce column 1 of Table 3 as the first column in Table 4. We rerun the analysis with country fixed effects (column 2), which is equivalent to a conditional logit, with year dummy variables instead of time splines (column 3), and fixed effects with time dummies (column 4). The key findings on the negative effect of banking crisis as well as the contrasting positive effects of increasing debt on reform do not change across

specifications. The effects of the personal vote, however, do weaken when additional spatial and time effects are included, which is to be expected. The fiscal crisis variable also weakens, which, given that only a few countries had such crises in practice, is also to be expected once country dummy variables are included.

Table 4: Robustness Tests Based on Alternative Model Specifications

VARIABLES	(1) Fiscal Reform	(2) Fiscal Reform	-3 Fiscal Reform	-4 Fiscal Reform
Banking Crisis	-1.79***	-1.62**	-2.08***	-1.61**
	(0.68)	(0.69)	(0.61)	(0.79)
Fiscal Crisis (Default through Restructuring)	2.36**	1.06	2.90**	1.67
	(1.05)	(1.05)	(1.23)	(1.15)
Presidential Election Year	0.72	0.64	0.91	0.50
	(0.57)	(0.65)	(0.57)	(0.79)
Personal Vote	2.01**	4.71*	1.98**	4.10
	(0.96)	(2.80)	(0.97)	(4.02)
United Government	0.64	0.07	0.88	0.36
	(0.46)	(0.73)	(0.56)	(0.77)
Ideology of President	0.15	-0.16	0.25	-0.28
	(0.22)	(0.32)	(0.24)	(0.39)
Interest Rate Shock	-0.66	-1.54*	-0.59	-2.03*
	(0.78)	(0.93)	(0.77)	(1.19)
Average Growth	-3.00*	-5.46**	-2.18	-3.67
	(1.77)	(2.36)	(1.98)	(3.11)
Interest on External Debt (lag)	0.08***	0.08**	0.10***	0.11*
	(0.03)	(0.04)	(0.03)	(0.06)
Terms of Trade (lag)	-0.06***	-0.04	-0.07***	-0.04
	(0.02)	(0.02)	(0.02)	(0.03)
IMF Program	-0.06	-0.09	-0.21	-0.23
	(0.44)	(0.54)	(0.46)	(0.67)
Time Since Last Reform	1.32	1.11	0.28***	0.25**
	(0.89)	(0.89)	(0.10)	(0.12)
Number of Previous Reforms	0.32**	-0.10	0.09	-1.44***
	(0.14)	(0.23)	(0.24)	(0.54)
Model	Random Effects Logit	Fixed Effects	Logit	Fixed Effects
Time	Splines	Splines	Year Dummies	Year Dummies
Observations	256	256	224	256
Number of countries		16		16

Notes: Robust standard errors. Constant as well as spatial and time effects (when used) are not displayed, but results available upon request.

There are several alternative arguments to consider in the core model, and we begin with modifications to the coding of "crisis," then discuss additional political variables, and finish with additional economic variables.

Our first extension in terms of crises is to add exchange rate crises to the analysis. Exchange rate crises represent a big loss in the value of a given country's currency, with countries that previously had fixed exchange rates the ones that experience such crises (Fischer 2001). This has a direct effect on the country's finances, and the loss in the currency's value

makes it more difficult for the government to repay debts. Column 1 reports results for exchange rate crises, which use the data and classification from Laeven and Valencia (2012). The substantive results do not change while exchange rate crises do not affect the likelihood of fiscal reforms. One could also consider alternative ways of coding of banking crises. Reinhart and Rogoff (2009) document several centuries of crisis. While their dataset in part considers Laeven and Valencia (2008), which mostly overlap with Laeven and Valencia (2012) for our years and countries, they have a somewhat broader definition of "banking crisis." In practice, this means that 19% of the country-years are in a banking crisis as opposed to 16% according to Laeven and Valencia (2012). Column 2 uses the Reinhart-Rogoff measure instead, and the results stay largely the same, with banking crisis having the same negative and statistically significant coefficient.

The third consideration is that we consider the effects of crises as contemporaneous. That is, we assume that governments react immediately to them through fiscal reform legislation. One could argue that one sees the effects of crises first in later years, and that crises should be lagged. Similarly, fiscal reforms may come well after the crisis has begun. 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. 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. We examine several different lag structures in unreported results but include a lag of one and two years in column 3. While the coefficient for the unlagged banking crisis variable weakens somewhat, there are no other substantive changes, and the lags themselves are not statistically significant.

A fourth possibility is that 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 both a banking and a fiscal crisis, and we report the results in column 4. The idea is that if big regional neighbors are having problems then this puts pressure on the domestic government to signal to markets and others that they have "fit" institutions, while if a "small" country in terms of GDP has a crisis that will have less of an effect. This variable is also not statistically significant while the rest of the model has almost the same substantive results.

Figure 5: Robustness Tests, Crisis Variables

	(1)	(2)	(3)	(4)
VARIABLES	Fiscal Reform	Fiscal Reform	Fiscal Reform	Fiscal Reform
Banking Crisis	-1.74**		-1.48*	-1.78***
	(0.69)		(0.86)	(0.68)
Banking Crisis (lag)			-0.62	
			(0.80)	
Banking Crisis (lag 2)			0.29	
			(0.51)	
Banking Crisis, Other Countries by GDP				1.11
				(1.03)
Banking Crisis, Reinhart-Rogoff		-1.43**		
		(0.72)		
Fiscal Crisis	2.59**	1.59	2.42**	2.29**
				-4.76
Fiscal Crisis, Other Countries				-6.82
	(1.16)	(1.07)	(1.04)	(1.09)
Exchange Rate Crisis	-1.01			
	(1.01)			
electP	0.70	0.62	0.73	0.75
	(0.57)	(0.56)	(0.58)	(0.57)
personalvotemedian	2.03**	1.10	2.05**	2.07**
	(0.97)	(0.93)	(0.96)	(0.97)
allhouse	0.67	0.53	0.66	0.62
	(0.47)	(0.46)	(0.46)	(0.48)
MOV_Ideology	0.14	0.11	0.17	0.15
	(0.23)	(0.22)	(0.23)	(0.23)
rate_spread_shock	-0.59	-0.70	-0.63	-0.62
	(0.77)	(0.80)	(0.78)	(0.78)
avg_gdpgrowth	-2.95*	-1.60	-3.11*	-3.27*
	(1.75)	(1.66)	(1.78)	(1.67)
L.extint_exp	0.08***	0.08**	0.08***	0.08**
	(0.03)	(0.03)	(0.03)	(0.03)
L.ToT	-0.06***	-0.04***	-0.06***	-0.06***
	(0.02)	(0.01)	(0.02)	(0.02)
IMF_any	-0.08	-0.31	-0.05	-0.01
	(0.45)	(0.41)	(0.43)	(0.44)
noreformyear	1.31	0.85	1.04	1.39
Observations	256	256	240	256

There are additional political variables that should be considered.
Related to the policy stream literature, one might expect changes in policy especially after changes in leadership, or when the "political" stream has come more in sync with the other two streams. We check whether changes in the president, and changes in the partisanship of the

president, affect the results in Columns' 1 and 2. One may also decide that our definition of veto players is too narrow; we include the "checks" variable from Beck et. al (2010) as an alternative. It counts each chamber of a legislature unless the president's party controls it and there is a closed list electoral system in place, and this variable appears in Column 3. An alternative measure of presidential partisanship comes from Beck, et al (2010) as well. There are several countries where the coding is ambiguous, however, and using Hallerberg and Scartascini (2011) we include the additional codes so that we have a complete dataset.

Table 6: Additional Political Variables

	(1)	(2)	(3)	(4)
VARIABLES	Fiscal Reform	Fiscal Reform	Fiscal Reform	Fiscal Reform
Banking Crisis	-1.757***	-1.800***	-1.714**	-1.700**
	(0.673)	(0.674)	(0.685)	(0.675)
Fiscal Crisis	2.348**	2.358**	2.106**	2.103**
	(1.072)	(1.049)	(1.001)	(0.984)
Presidential Election	0.870	0.689	0.634	0.702
	(0.659)	(0.682)	(0.588)	(0.569)
Personal Vote	2.000**	2.012**	1.628*	1.698*
	(0.961)	(0.962)	(0.871)	(0.939)
United Government	0.622	0.638		0.598
	(0.464)	(0.465)		(0.454)
Veto Players (checks)			0.0975	
			(0.169)	
Ideology of President	0.147	0.153	0.105	
	(0.223)	(0.224)	(0.219)	
New President	-0.275			
New Presidential Party		0.0674		
		(0.596)		
Partisanship (DPI)				0.216
				(0.215)
Interest Rate Shock	-0.666	-0.649	-0.623	-0.481
	(0.778)	(0.781)	(0.801)	(0.683)
Average Growth	-3.011*	-2.990*	-3.189	-2.472
	(1.760)	(1.793)	(1.968)	(1.576)
Interest on External Debt (lag)	0.0819***	0.0821***	0.0793**	0.0788**
	(0.0313)	(0.0313)	(0.0326)	(0.0310)
Terms of Trade (lag)	-0.0571***	-0.0571***	-0.0541***	-0.0541***
	(0.0181)	(0.0182)	(0.0178)	(0.0175)
IMF Program	-0.0742	-0.0582	-0.156	0.0604
	(0.437)	(0.440)	(0.455)	(0.432)
Time Since Last Reform	1.323	1.321	1.432	1.268
	(0.886)	(0.893)	(0.896)	(0.903)
Number Previous Reforms	0.317**	0.315**	0.291**	0.305**
	(0.140)	(0.139)	(0.139)	(0.142)
Observations	256	256	253	256

[Two more worth considering: interactions; try again with a Heckman two-stage, where we try to predict the crisis.]

Conclusion

This paper considered the connection between economic crises in Latin America and fiscal institutional reforms. Banking crises on their own reduce the pressure for fiscal institutional reforms to zero. Over the time frame of banking crises that last multiple years, however, the likelihood of reform increases. Moreover, there is a jump in the likelihood of reform if the banking crisis transforms itself into a further sovereign debt crisis. The relationship between crisis and reform is therefore more complex than simply "a crisis is a terrible thing to waste." Under banking crises, fiscal reforms restrict the ability of governments to respond. This need clearly trumps the demand to signal to markets that the country will be solvent in the future. This changes, however, as the crisis advances and as the debt burden builds up.

This paper also has implications for the literature on economic reform in Latin America. One important finding is that the extent of the personal vote affects the likelihood of future reforms. A key variable is one that measures the extent to which institutions increase the common pool resource problem, namely whether the electoral system encourages a personal vote. If it does so, the country is more likely to reform. This finding is seemingly counter to what one would expect from the literature on neo-liberal reform (e.g., Haggard and Kaufman 1994), where one expects that stronger parties lead to more reform. There is preliminary evidence 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. At the same time, one-party government *does* promote reform. If one combines these two sets of results, divisions within parties facilitate fiscal reforms while divisions across parties do not. This suggests that there is a political logic to fiscal reforms—one role they play is to help parties coordinate themselves. When the benefits of coordination potentially go to other parties, however, reforms are less desirable. These results are highly preliminary, however, and more examination of the exact causal mechanisms, which is beyond the scope of this paper, is needed.

This paper also takes our understanding of fiscal rules and institutions forward. There is a wide body of literature that considers their effectiveness. But the endogeneity question has hung

a shadow over most (though not all) of that literature—why have countries improved their fiscal institutions in the first place? A future step would be to take the results in this paper as background for a two-stage model on the effectiveness of fiscal institutions in improving fiscal discipline.

Our results are also interesting for what they do not reveal. We find no evidence that crises in other countries leads to reforms in other countries in future years. We also find no evidence of an active and important role of the IMF in promoting this type of reform, at least in coincidence with an adjustment program.

There is no reason to think that these results are relevant only for Latin America. As banking crises turn into sovereign debt crises in Europe, there should be more fiscal reforms.

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