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

ABSTRACT Do crises really lead to more institutional reforms? This paper explores the connection between financial crises and one type of reform frequently advocated during the recent global financial crisis, namely, fiscal institutional reforms. Some authors expect that crises lead to reforms, but we demonstrate that the relationship is not so straightforward. Using a data set of Latin American countries that experienced several crises and also several periods of reform in the period from 1990 to 2005, we find that the type of crisis and its duration matter. We argue that reforms are less likely during a banking crisis, whereas fiscal crises are most likely to lead to fiscal reforms. This means that the type of economic crisis is important for explaining the likelihood of reforms. We explore other possible explanations for reform, such as the partisanship of the president and whether a country is under an IMF program, and do not find confirming evidence for alternative explanations.

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As countries recover from the worldwide financial crisis, much of the industrialized world has been facing pressure to initiate fiscal consolidation. The pressure is not only for policy reform (that is, expenditure cuts and revenue increases), but also for fundamental institutional reform. A variety of actors, including heads of international economic organizations, presidents of central banks, and prime ministers of countries, have called for fiscal institutional reforms. The European Union, in particular, strengthened its fiscal framework in 2011 and 2012, and some countries are now under joint European Union and International Monetary Fund (IMF) programs.

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. Fiscal institutional reforms are those that reduce the size of the common pool resource problem that is endemic in fiscal decisionmaking. Latin America is interesting in the context of the current crisis. Unlike other regions, 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 major 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 concur that better fiscal management before the crises allowed 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 Were these apparently successful reforms the result of learning from previous crises?

Three major financial crises hit the region in the past twenty years: the Mexican (or 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 were also a number of financial crises that were concentrated in specific countries. Latin American governments introduced several fiscal institutional reforms in the same period. Fiscal responsibility laws, which usually combine numerical spending 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.³

In this paper, we explore why countries implemented fiscal institutional reforms in the first place. In particular, when a financial crisis hits a country,

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.” Ecuadorian President Rafael Correa in a speech given at the Fifth Summit of the Americas, held at Port of Spain in Trinidad and Tobago, 17–19 April 2009. Available online at www.taringa.net/posts/info/2454954/Ecuador_-_Discurso-de-Correa-en-la-Cumbre-de-las-Américas.html.

2. For example, an IMF publication from the period states that “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” (IMF, *Regional Economic Outlook—Western Hemisphere: Crisis Averted—What’s Next?*, October 2009 [available online at www.imf.org/external/pubs/ft/reo/2009/WHD/eng/wreo1009.pdf]). Elsewhere, the IMF further recognized the role of the countries’ economic policies for coming out of the crisis unscathed, for example, praising the role of fiscal responsibility in Brazil (see “FMI elogia sólida estructura de la política económica de Brasil,” *América Economía*, 8 June 2010 [www.americaeconomia.com/economia-mercados/fmi-elogia-solida-estructura-de-politica-economica-de-brasil-en-revision-de-2010]).

3. For example, Filc and Scartascini (2007); Eslava (2012).

under what conditions does the government initiate fiscal reforms? What are the political conditions that make reforms more or less likely?

The paper begins with the conceptualization of our dependent variable, which is whether there is fiscal institutional reform, and how to operationalize it. The second part considers how financial crises may be connected to reforms and explains how we measure crises. We then look at additional catalysts for reform before providing our empirical analysis, including 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 need credibility with markets, so they introduce fiscal reforms in later years. Moreover, if the crisis transforms into a true sovereign debt crisis, fiscal reforms become much more likely.

Fiscal Institutional Reforms in Latin America

Conceptually, we are interested in changes in rules and institutions. The core theoretical model on why fiscal institutional reforms that centralize the budget process are important assumes that all policymakers 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 everyone pays taxes, a constituency's 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 part of the cost can be shifted to other sectors of the economy, then the minister may be more willing to ask for higher resources for the sector. Similarly, a congressperson in Argentina may care most about expenditures in his or her home province. If the entire country pays for the spending, the burden on the province is smaller than if the province had to bear the full tax burden.

There is an established literature on the effects of fiscal institutions that draws on this underlying model. Much of the work focuses on developed countries and indicates that centralization of the budget process leads to tighter fiscal discipline in Europe and the United States.⁴ Increases in the transparency of the process have similarly led to healthier budget balances.⁵ Another strand

4. See, for example, Hallerberg and von Hagen (1999); Fabrizio and Mody (2006); Baldacci, Kumar, and Schaechter (2010); Alt and Lowry (1994); Poterba and Reuben (2001).

5. Alt and Lassen (2006).

of the literature attempts to explain the introduction of these fiscal institutional reforms in different developed countries and regions.⁶

For Latin America, an analogous literature explores the effects of fiscal institutions on fiscal outcomes.⁷ Summarizing this body of work, Eslava concludes that “the finding that good budget institutions increase budget discipline is quite robust.”⁸ What is missing, however, is a consideration of why some Latin American countries implemented fiscal institutional reforms while others did not. This paper is the first to focus on this question.⁹

We are interested in explaining reforms that centralize the budget process and reduce the scope of the CPR problem, in particular by setting limits on the outcomes of budget negotiations. Our data set codes reforms for seventeen Latin American countries for the period 1990–2005. We begin in 1990 because, as Pérez-Liñan and Mainwaring indicate, it is the first year in which all the countries in our sample have truly competitive political systems, with the exceptions of Cuba and Haiti.¹⁰ The fiscal institutions we care most about are institutional changes to decisionmaking processes in democracies. Consequently, it does not make sense to go earlier in time when some of the countries in the sample did not have elected presidents. We set the end point at 2005 to define a period of relative political calm that includes several crises. This means that we can exploit the variance across periods and across countries.

There are three broad types of fiscal institutional reforms.¹¹ First, a numerical rule establishes *ex ante* constraints on debts, deficits, or expenditures (or all three). A balanced budget requirement is an example of such a rule. Second, a procedural rule specifies the norms and prerogatives of actors in the budget process. This would include, for example, a restriction on the type of amendments the legislature can make to the budget proposal submitted by

6. On Europe, see Hallerberg, Strauch, and von Hagen (2009) and Fabrizio and Mody (2010); on the G-20, see Debrun and others (2008) and IMF (2009); on the United States, see Alt, Lassen, and Rose (2006).

7. For example, Alesina and others (1999); Filc and Scartascini (2007); Caceres, Corbacho, and Medina (2010).

8. Eslava (2012, p. 515).

9. The nuanced literature on the neoliberal reforms in Latin America in the 1990s does not directly address fiscal institutional reforms (for example, Stokes, 2001; Weyland, 2002; Levitsky and Murillo, 2005; Wibbels, 2005).

10. Pérez-Liñan and Mainwaring (2010); see also Mainwaring, Brinks, and Pérez-Liñan (2007).

11. Von Hagen (1992); von Hagen and Harden (1995); Alesina and others (1999); Filc and Scartascini (2007).

the executive. Finally, a transparency rule makes it easier to follow what the government is doing on the budget. An increase in the comprehensiveness of budget documents and the identification and even the closing of extra-budgetary funds would constitute a transparency rule according to our definition. When we classify episodes in which governments introduced any of these measures, we find that a reform occurred almost twice per country in the period. Some countries had almost no reforms: Guatemala, for example, introduced a change only in 2000. Other countries had multiple reforms: Argentina and Ecuador both introduced reforms in six of the fifteen years in the period. While there are several reforms from 2000 on, a total of sixteen reforms were passed in the 1990s.

These rules, in turn, were often integrated in packages known as fiscal responsibility laws. The best-known example is also the most successful, namely, the fiscal responsibility law in Brazil. This law extends restrictions to all levels of governments, not just to the national level. In terms of subnational finance, Brazil has twenty-six states plus the federal district of Brasília. 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 audit both state and municipal finances.¹² There is also a clear punishment mechanism. Once the caps are in place, any subnational 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 data set of seventeen countries, eight introduced a fiscal responsibility law in the period under analysis.¹³ Table 1 lists the countries and years of reform, together with the type of reform: numerical rules, a new counter-cyclical fund, a multi-year framework, a fiscal responsibility law, a single account for the state budget, and reforms that increased the transparency of

12. The *Tribunal de Contas*; see Melo, Pereira, and Figueiredo (2009).

13. See Alston and others (2009) for more details on the Brazilian case. Hallerberg, Scartascini, and Stein (2009) compare the Argentine and Brazilian experiences with fiscal responsibility laws.

TABLE 1. Fiscal Reforms in Latin America, 1990 to 2005^a

Country	1992	1993	1994	1995	1996	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 Republic												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
Peru			U					R(N,P,C,T)		r(n),C,T			
Uruguay					U								
Venezuela						C,U		P			R(N)		

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

a. There were no fiscal reforms in the sample countries in 1990, 1991, and 1997. N: Numerical rules; C: Countercyclical fund; P: Multi-year framework; R: Fiscal responsibility law: 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.

the budget.¹⁴ The year marks the approval of the law. In total, 16 percent of years in the data set are reform years.¹⁵ As the table shows, all countries experienced at least one reform year, with Guatemala having the fewest reform years (one) and Argentina the most (six). The median number of reform years per country is about 2.5.

As we explain in more detail below, we have theoretical expectations about what explains reforms that centralize the budget process. We do not have a theory, however, to explain why one country would introduce expenditure limits while another put most accounts on-budget. Similarly, the literature on the effects of budget institutions on fiscal outcomes also groups together the different types of reforms. Our dependent variable, therefore, is a dummy variable that takes the value of one for any change in fiscal institutions that centralizes the budget process, and zero otherwise.

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.¹⁶ The argument is that there are multiple streams at work at any given 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 Emanuel'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.¹⁷

14. Filc and Scartascini (2007) assembled the data set through visits and interviews with government officials, surveys, and analysis of legislation. They checked the accuracy of the survey and interview answers by distributing the results to government officials from the budget office in each country. We updated the data set for this paper.

15. In our database, we do not include one reform that appears in table 1 and that Filc and Scartascini (2007) collect, namely, the increase in the power of the finance ministry. This is a subjective assessment for which we have no evidence of actual legislation that increased the minister's power.

16. See, for example, Kingdon (1984, 1997); Zahariadis (2003).

17. U.S. President Barack Obama's first Chief of Staff made this comment shortly after the November 2008 election, but the correct attribution is to the economist Paul Romer in 2004. See Jack Rosenthal, "A Terrible Thing to Waste," *New York Times*, 31 July 2009.

Alesina and Drazen focus on domestic opposition to reforms, using an inflation crisis to model the response of different interest groups.¹⁸ There are different groups in society whose approval is required before action can be taken. The political representation of each group, however, does not want to bear the costs of the adjustment process to get inflation under control. In normal times, various interest groups block reforms so as to avoid paying their share of the costs. As the crisis escalates, this process becomes a “war of attrition” with each group trying to wait out the others, until one of the groups concedes and accepts a disproportionate share of the cost burden. Drazen and Grilli suggest that only when the crisis hits, such that all groups are bearing high costs anyway, is it politically feasible for the government to take the steps necessary to address the crisis.¹⁹

There is an important difference between reforms in trade, monetary policy, and taxation, as discussed in the above papers, and the reform of fiscal institutions. While fiscal institutional reforms have distributional effects because they lead to tighter fiscal discipline, who suffers and who benefits from these reforms is not clear-cut. In contrast, in the case of trade reforms, the structure of the economy leads to fairly straightforward predictions about who benefits and who loses out.²⁰ The adjustment story from Alesina and Drazen thus depends on the nature of the underlying crisis.

Other key factors are the type of crisis and their sequence. In this paper, we focus on financial (or banking) crises. Sovereign debt crises sometimes develop during or after the financial crisis, for several reasons.²¹ These two are clearly related—reforms to address a banking crisis are expensive. At the beginning of a banking crisis, there have rarely been concurrent sovereign debt defaults.²² Pressure on government fiscal policy builds up, however: in their historical study of eight centuries of crises, Reinhart and Rogoff find that the debt burden grew 82 percent, on average, in the first two years after a banking crisis.²³ There are three reasons why the government increases spending to deal with the negative effects of the crisis on economic activity. The first is the cost of the bailout itself. The second is due to lost economic output, which causes the crisis to translate into more social spending and lower tax collections, thereby generating a direct effect on the budget. In their study of 147 banking crises

18. Alesina and Drazen (1991).

19. Drazen and Grilli (1993).

20. See Rodrik (1994) on the role of crises in advancing trade reforms.

21. In related work, we examine other measures of crisis, such as so-called sudden stops. The data sets are available for only a subset of countries, however.

22. Laeven and Valencia (2012).

23. Reinhart and Rogoff (2009).

over the period 1970–2011, Laeven and Valencia find that the average crisis costs a government a little over 13 percent of gross domestic product (GDP) and, at the extreme, up to 55 percent of GDP.²⁴ The final reason is the implementation of an 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 a banking crisis can be beneficial: Baldacci, Gupta, and Mulas-Granados find that banking crises are shorter in countries that use such fiscal stimulus measures.²⁵

These arguments are based on a worldwide data set, but the patterns are similar in Latin America in 1990–2005. The greatest economic loss was in Argentina in 2001, when the economy is thought to have shrunk almost 43 percent after the crisis. The greatest fiscal cost was to Ecuador in 1998, at almost 22 percent of GDP.²⁶

These debt dynamics create 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 counterproductive—the whole point is to spend more money to address the crisis. As the debt piles up, however, governments lose the confidence of markets, so they are motivated to introduce more centralized fiscal institutions to signal that the run-up in debt should end. As Jácome 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.”²⁷ We therefore expect that reforms are *less* likely during the initial phase of a banking crisis.

In terms of our predictions of fiscal reform, however, a sovereign debt crisis—whether in connection to a financial crisis or as a separate, unrelated event—has an effect on the probability of reform for three reasons.²⁸ First, the

24. Laeven and Valencia (2008, p. 24). This is their estimated cost of the Argentine banking crisis in 1980.

25. Baldacci, Gupta, and Mulas-Granados (2009).

26. Laeven and Valencia (2008, pp. 32–49).

27. Jácome (2008, p. 14).

28. A sovereign debt crisis exists formally when a country defaults. This is a government decision that is hard to predict—some countries, like Japan, have debt-to-GDP ratios approaching 250 percent, while Argentina defaulted in 2001 with a debt-to-GDP ratio of less than 60 percent. In our data set, countries not in default have an average external debt equivalent to 261 percent of gross national income (GNI). Countries in default have an average external debt level of 282 percent of GNI, which is not appreciably higher. The *t* test of the difference in means is not close to statistical significance at the $p < 0.10$ level.

default may force the different interest groups finally to act on the problem, and a new fiscal rule may be part of any agreement among the various parties.²⁹ Second, the default means that 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 are no longer able to borrow, and they need tools to restrict spending.

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, in which the government is unable to borrow on international credit markets. In the midst of a sudden stop, the government may thus experience not only pressure to reassure markets, but also a very practical need to implement 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.

So far we have focused on domestic crisis, but crises in other countries in the region could 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 demonstrate to the markets that a given country is not in the same position 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.

Finally, crises often lead to requests for international help from organizations such as the International Monetary Fund (IMF). The IMF sometimes attaches requirements for fiscal outcomes to its aid programs, including expectations or even stipulations 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 pressuring a government to implement reforms. At the same time, a government may ask the IMF to include certain reforms in the program requirements so that it can blame the IMF for the new policies. Causation is thus a real issue: did IMF pressure lead to reform, or are the stipulations of the country

29. This is consistent with Alesina and Drazen (1991).

TABLE 2. Crises in Latin America^a

Country	Systemic banking crisis	Currency crisis	Debt crisis through debt restructuring
Argentina	1990, 1991, 1995, 2001 , 2002, 2003	2002	2001 , 2002, 2003, 2004 , 2005
Bolivia	1994		1992
Brazil	1990, 1991, 1992, 1993, 1994, 1995, 1996, 1997, 1998	1992, 1999	1994
Chile			1990
Colombia	1998, 1999, 2000		
Costa Rica	1994, 1995	1991	1990
Dominican Republic	2003, 2004	1990, 2003	1994, 2003, 2004 , 2005
Ecuador	1998, 1999 , 2000, 2001, 2002	1999	1999, 2000
El Salvador	1990		
Guatemala			
Mexico	1994, 1995, 1996	1995	1990
Nicaragua	1990, 1991, 1992, 1993, 2000	1990	1995
Panama			1996
Paraguay	1995	2002	1992
Peru			1996
Uruguay	2002, 2003, 2004, 2005	1990, 2002	1991, 2002, 2003
Venezuela	1994, 1995, 1996, 1997, 1998	1994, 2002	1990

Source: Laeven and Valencia (2012).

a. This table provides the incidence of different types of crisis in Latin America. Years in bold correspond to a fiscal institutional reform the same year. The last column lists years from any debt crisis through a debt restructuring, which is a key independent variable in the empirical results that follow. There were several cases of restructurings without an initial ex ante debt crisis, namely, in Argentina (2002), the Dominican Republic (2003), Ecuador (1999), and Uruguay (2002).

program a signal that the government had already decided to undertake certain reforms? Nonetheless, we do include a variable for whether a country is under an IMF program. Biglaiser and DeRouen find that IMF involvement increases the likelihood of some types of economic reforms.³⁰ We therefore test explicitly whether countries reacted differently to crises given whether they were under an IMF program or not.

Table 2 presents the country years for which there is a crisis in our data set, with a breakdown into banking, currency, and sovereign debt crises. With regard to the latter, Laeven and Valencia consider both the year of sovereign debt defaults to private lending and the year of debt rescheduling.³¹ In our sample, true defaults are rare, occurring in only four country years. Sovereign debt restructurings, however, are more common, with sixteen cases that include all the countries but Colombia, El Salvador, and Guatemala. Table 2 lists the years from any default through a restructuring. In the regression

30. Biglaiser and DeRouen (2011).

31. Laeven and Valencia (2012). They use Beim and Calomiris (2001, appendix to chap. 1), World Bank (2002), Sturzenegger and Zettelmeyer (2006), and IMF staff reports as sources.

analysis, we create one variable for fiscal crisis that considers the period from the initial debt default to debt restructuring. A country is receiving no funding from markets during this time, and the coding of this variable is analogous to the banking crisis variable that extends from the beginning to the end of a given crisis.³² To test for market pressure, we include the (lagged) interest costs on external debt as a percent of exports. The idea here is to capture the government's ability to continue to fund such costs. The next step is to consider the causes of fiscal reforms in a multivariate framework, in order to clarify the role of political variables in the reform process.

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 make reforms more or less likely. Moreover, governments may have overt political reasons for introducing reforms, over and above the presence of a crisis. We explore several hypotheses here and discuss how to operationalize them for the empirical section. A first variable concerns the electoral cycle. A pre-electoral period may affect the likelihood of reform. Brender and Drazen suggest that fiscal cycles are especially prevalent in young democracies.³³ Barberia and Avelino, however, find no effects of the age of democracy in their Latin American sample, and they argue that political business cycles are common in the region.³⁴ 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 government's ability to run fiscal cycles. Following Franzese, we measure electoral periods according to the proportion of the current year that is part of a pre-electoral year.³⁵ For example, an election on 1 July 2000 would be measured as 0.5 in 2000 and 0.5 in 1999.

32. Laeven and Valencia (2012) also discuss the timing of currency crises and sovereign debt crises. They define a currency crisis (following Frankel and Rose, 1996) as a nominal depreciation of the currency of at least 30 percent and an increase in the depreciation rate of at least 10 percent over the previous year. In our sample, such crises were less frequent than banking crises, at about 3.7 percent of the time. For fiscal crises, this coding means that the following countries and periods have such crises: Argentina 2001–05, the Dominican Republic 2003–05, Ecuador 1999–2000, and Uruguay 2002–03.

33. Brender and Drazen (2005).

34. Barberia and Avelino (2011).

35. Franzese (2002).

There is an ongoing debate about whether the presence of “veto players” accelerates or retards reform. Tsebelis expects that increasing the number of veto players that do not share the same preferences should decrease the possible space for changes to the status quo.³⁶ In contrast, different analyses of economic reform in central and eastern Europe suggest that more veto players make it harder for special interests to block further reform, such that increasing the number of veto players should make reforms more likely rather than less.³⁷ Finally, Tommasi, Scartascini, and Stein find that when intertemporal bargains are included, the effects of the number of veto players is ambiguous.³⁸ In our empirical analysis, we use the variable “allhouse” from Beck and others, which takes the value of one if one party controls the relevant houses of congress and zero otherwise.³⁹ This picks up the contrast between countries with united versus divided government.⁴⁰

In addition to the number of parties needed to pass legislation, party ideology affects the likelihood of reform. There is a rich literature on the effects of partisanship on neoliberal reforms in Latin America. For example, Stokes finds that party labels are good predictors of rhetoric before elections, but not good predictors of whether presidents actually try to introduce neoliberal reforms.⁴¹ Her work suggests that there should be no association between partisanship and reform. Despite the general leftist tilt to electoral outcomes in the region in the 2000s, the changes in partisanship have not been as great as some presume, as the political orientation of elected presidents has shifted from center-right to center (rather than left) over the last decade.⁴² Leftist presidents have, however, made a subtle but important difference in policies—they have stalled or even reversed reforms consistent with the so-called Washington Consensus.⁴³ They also seem to have increased tax revenues.⁴⁴ To the extent that fiscal reforms are seen as part of the Washington Consensus and to the extent that leftist presidents act to roll back such reforms, fiscal reforms should be more common under right-leaning presidents. Yet

36. Tsebelis (2002).

37. Hellman (1998); Gehlbach and Malesky (2010).

38. Tommasi, Scartascini, and Stein (2014).

39. Beck and others (2001).

40. In robustness regressions, we also include the “checks” variable from Beck and others (2001).

41. Stokes (1999).

42. Baker and Greene (2011); Murillo, Oliveros, and Vaishnav (2010).

43. Baker and Greene (2011).

44. Stein and Caro (2013).

partisanship may be relevant for another reason related not to market reforms but to the markets themselves: leftist presidents may need to signal to markets that they are serious about the economy. In this case, liberal presidents should introduce more reforms than conservative presidents. Our measure for partisanship comes from Murillo, Oliveros, and Vaishnav and is coded on a five-point scale, with one representing the most left-leaning president and five the most right-leaning president.⁴⁵

We also include important economic variables. A deterioration in a country's terms of trade 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.⁴⁶ In additional regressions, we consider variations based on economic growth in terms of the average relative to five years previously, since pressure for reform may increase when average growth declines over time.⁴⁷ An interest rate shock may also push governments to respond with fiscal reforms; we define this variable as an episode in which the change in the spread is over one standard deviation from its trend.⁴⁸

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. For example, an IMF study on the economic determinants of fiscal reforms (only) considers both parametric and nonparametric hazard models and finds that countries introduce reforms under good economic conditions.⁴⁹ The paper also uses conditional logit modeling 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 study) drop out of the sample once they have had a reform. The analogy comes from medicine, where event history

45. Murillo, Oliveros, and Vaishnav (2010).

46. We use the net barter terms of trade index, with 2000=100.

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

48. Data for the economic indicators are from World Development Indicators.

49. IMF (2009).

techniques were initially developed 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 for dealing with this issue, it is more problematic to accommodate the fact that patients may “die” in consecutive years (or initiate reforms over consecutive years).⁵⁰ It is difficult to model how they reenter the sample.

We therefore start with a standard logit model with country-clustered standard errors, with the following form:

$$\begin{aligned} \text{FiscalReform}_t = & \text{BankCrisis}_t + \text{FiscalCrisis}_t + \text{PoliticalVars}_t + \text{EconVars}_t \\ & + \text{InterestExt}_{t-1} + \text{TermsOfTrade}_{t-1} + \text{IMF}_t \\ & + \text{NumberPreviousReforms}_t + \text{Year}. \end{aligned}$$

Year dummy variables are included to pick up exogenous effects that may affect all countries at the same time. We anticipate that there may be time dependence. We include a count variable that refers to the time since the last fiscal reform.⁵¹ This variable makes the model equivalent to a Cox proportional hazard model, with the dummy variables marking the length of the spell. It also means that reforms beyond just the past year are theoretically relevant. We include country-clustered standard errors. While one robustness check in the next section uses country fixed effects, we have few fiscal crises in the data set and expect that country fixed effects mask the relevance of this variable. Additional robustness checks in the next section include a lagged dependent variable instead of the variable for the time since the last reform to address serial correlation; other measurements of independent variables, such as crises and partisanship; and possible omitted variables.⁵²

50. We thank Erik Wibbels for extensive discussions about how best to model the data.

51. According to Beck, Katz, and Tucker (1998), this variable addresses possible temporal dependence (or serially correlated errors).

52. Another approach worth consideration is a conditional frailty model (Box-Steffensmeier, de Boef, and Joyce, 2007), which allows the simultaneous modeling of 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 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, but the model had difficulties computing when more than three variables were included.

TABLE 3 . Mean and Standard Deviation of Key Variables in the Sample

<i>Variable</i>	<i>Mean</i>	<i>Standard deviation</i>
Fiscal reform	0.19	0.39
Banking crisis	0.18	0.38
Fiscal crisis	0.05	0.22
Banking crisis in other countries, GDP weighted	0.07	0.17
Fiscal crisis in other countries, GDP weighted	0.01	0.03
Presidential election year	0.23	0.31
United government	0.24	0.43
Ideology of president	3.69	1.04
Interest rate shock	0.09	0.29
Average growth	0.09	0.14
Interest on external debt (lag)	10.43	6.53
Terms of trade (lag)	98.40	12.40
IMF	0.57	0.50

Our dependent variable has a maximum of 272 observations, which corresponds to whether there were fiscal reforms during the period 1990–2005 in seventeen countries. We include lags of some variables, however, and there were no reforms in 1991, so the inclusion of year dummy variables means that the effective period covered in the analysis is 1992–2005. Table 3 includes the mean and standard deviation for the dependent and independent variables in our sample. The results are then presented in table 4 and include the coefficients and standard errors, as well as the probability of a reform under no crisis and a crisis (respectively) while holding the other variables at their means.⁵³ Our focus is on the effects of banking and fiscal crises. Column A presents the results for the model described above. Because the effects of a banking crisis could be conditional on whether they lead to a sovereign debt crisis or, alternatively, the effects of the two types of crises could be separate, column B reports results with an interaction variable.

The results suggest that banking crises do make reforms less likely. The straight probability of a reform in a noncrisis year is 0.15 in table 4. If one sets the independent variables at their means, the probability of a fiscal reform during a banking crisis but no fiscal crisis is 0.03 and is not statistically significantly different from zero. The probability then falls effectively to zero when there is a banking crisis. The coefficient on debt crises, however, is positive and significant at the one percent level, and the probability of a reform

53. We use the Clarify software to calculate marginal effects of moving from zero to one for the crisis variables with the other variables set at their means.

TABLE 4 . Logit Results for Banking Crisis, Fiscal Crisis, and the Probability of Reform^a

<i>Explanatory variable</i>	<i>(A)</i>		<i>(B)</i>	
	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>
Baseline probability: No crises		0.15**		
Banking = 0 Fiscal = 0				
Banking crisis	-1.85***		-1.79***	
Banking = 1 Fiscal = 0	(0.63)	0.03	(0.65)	
Fiscal crisis	2.45***		2.85**	
Banking = 0 Fiscal = 1	(0.71)	0.67**	(1.25)	
Banking = 1 Fiscal = 1		0.24*		
Banking*Fiscal			-1.64	
Banking = 0 Fiscal = 0			(1.80)	0.15**
Banking = 1 Fiscal = 0				0.03
Banking = 0 Fiscal = 1				0.75**
Banking = 1 Fiscal = 1				0.22
Presidential election year	0.86		0.87	
	(0.54)		(0.56)	
United government	0.53		0.52	
	(0.43)		(0.43)	
Ideology of president	0.12		0.13	
	(0.18)		(0.19)	
Interest rate shock	-0.90		-0.85	
	(1.06)		(1.15)	
Average growth	-1.45		-1.36	
	(1.52)		(1.45)	
Interest on external debt (lag)	0.11***		0.12***	
	(0.03)		(0.03)	
Terms of trade (lag)	-0.07***		-0.07***	
	(0.02)		(0.02)	
IMF program	-0.37		-0.39	
	(0.52)		(0.54)	
Time since previous reform	0.25***		0.25***	
	(0.08)		(0.09)	
Number of previous reforms	0.13		0.11	
	(0.15)		(0.15)	
Year dummy variables	Yes		Yes	
No. observations	224		224	

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. This table reports logit results, as well as the estimated probability of fiscal reform when moving from a noncrisis to a crisis, holding the other variables at their means. The model includes year fixed effects and a constant term (not reported) and computes country-clustered standard errors.

when there is a fiscal crisis but no banking crisis is much higher, at 0.67 (or in two out of every three years, on average).

We are also interested in the case in which banking and fiscal crises are concurrent. Technically, we could use a model without an interaction term to calculate the expected probability. Logit results are already based on the effects of other variables. If one does not include an explicit interaction term, one is assuming that the relevant variables have an S-shaped relationship. That is, the effects are greater in the middle of the distribution than in the tails. Indeed, the probability of a fiscal reform given both types of crises in column A is 0.24. However, the two independent variables we care most about—banking and fiscal crises—are coded as dummy variables, so it is hard to envision a middle that would be S-shaped. We therefore report a specification in column B that has an interaction term, and we report the expected probabilities of fiscal reform given crisis conditions. As before, the probability drops from 0.15 to 0.03 if one moves from no banking crisis to a banking crisis in the absence of a fiscal crisis. This jumps to 0.22 if the banking crisis is combined with a fiscal crisis, but it is not statistically significant. A fiscal crisis without a banking crisis has a much greater effect—the probability of 0.75 represents an almost three in four chance of a fiscal reform. These results indicate that banking crises have a strongly negative impact on fiscal reforms initially, with no reforms when the crisis remains a banking crisis. If this banking crisis develops into a sovereign debt crisis, the probability increases. Most important, however, are fiscal crises: the probability of a fiscal reform is high when countries are largely cut off from capital markets.

Consistent with this argument about the increasing pressure of debt is that higher interest payments on external debt, as a percent of exports, intensify the pressure for fiscal reforms. The variable is statistically significant at the one percent level in all specifications, with virtually the same coefficient. When the other variables are set at their means, a move from the fifth percentile country (Panama, 1994) in terms of debt interest to the ninety-fifth percentile country (Brazil, 2000) in the specification in column A increases the likelihood of a fiscal institutional reform from 0.05 to 0.57. This reinforces the finding that fiscal pressure is most important in triggering fiscal reforms.

Deteriorating terms of trade also appear to increase the pressure on the government to take fiscal institutional steps. In contrast, some variables we expected to influence reforms are not relevant. For example, an IMF program does not increase the likelihood of reform. In any case, as we suggested in the theoretical section, whether an IMF program can be interpreted 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. Similarly, united government is not significant, 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.

To explore whether we chose the correct empirical model, we conduct robustness tests on our results, as presented in the next section.

Robustness Checks

There are two ways to approach the robustness of the results—one based on the statistical model and one using additional variables for which there are reasonable grounds to consider their inclusion. In terms of the modeling approach, we consider alternatives for addressing serial correlation, as well as the possibility of spatial correlation. We rerun the analysis with a lagged dependent variable instead of a count since the last set of reforms; the results are shown in table 5 (column A). The key findings on the negative effects of a banking crisis and the contrasting positive effects of increasing debt on reform do not change,

One could speculate that there is also spatial correlation affecting our results, which would represent some sort of diffusion across countries. In particular, crises in other parts of Latin America could have an effect on fiscal reforms at home. One can imagine that a country might want to initiate a reform to convince markets that it is different from a neighboring country that is in either a banking or fiscal crisis. We create a variable that captures the share of regional GDP (not including the country under examination) that is experiencing either a banking or a fiscal crisis, with the expectation that markets, and hence governments, pay more attention when a big country gets into trouble.⁵⁴ This type of specification should not include year dummy variables, so to be clear about the comparison, column B in table 5 presents the standard model without year dummies, while column C includes whether other countries are experiencing a banking or fiscal crisis weighted by GDP. Neither of the new variables is significant, while the effects of the core variables remain the same.

Another possibility is that markets do not differentiate among countries in default, but only know that a country from a given region is in default. We

54. Basinger and Hallerberg (2004) study of tax competition in member countries of the Organization for Economic Cooperation and Development (OECD); they find that governments pay more attention to tax changes in countries with larger shares of OECD GDP.

TABLE 5. Robustness Tests for Serial and Spatial Correlation^a

<i>Explanatory variable</i>	(A)		(B)		(C)		(D)		(E)
	<i>Coefficient</i>		<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>
Baseline probability: No crises									
Banking = 0 Fiscal = 0				0.13***		0.14***		0.13***	
Banking crisis	-1.63***	-1.58***			-1.58**		-1.58**		-1.39*
Banking = 1 Fiscal = 0	(0.63)	(.68)		0.03	(0.70)		(0.68)		(0.76)
Fiscal crisis	2.73***	1.69***			1.64***		1.70***		1.74
Banking = 0 Fiscal = 1	(0.89)	(0.62)		0.46***	(0.60)		(0.61)		(1.15)
Banking = 1 Fiscal = 1				0.15*		0.14		0.15*	
Lagged dependent variable	-1.42***								
	(0.44)								
Banking crisis in other countries, GDP weighted					0.54				
					(0.96)				
Fiscal crisis in other countries, GDP weighted					-2.34				
					(5.77)				
Default year							-0.16		
							(0.28)		
Presidential election year	0.81	0.64			0.65		0.66		0.40
	(0.52)	(0.50)			(0.54)		(0.51)		(0.78)
United government	0.07	0.34			0.34		0.34		0.53
	(0.48)	(0.35)			(0.34)		(0.34)		(0.76)
Ideology of president	0.28	0.03			0.03		0.04		-0.34
	(0.18)	(0.16)			(0.17)		(0.16)		(0.38)

Interest rate shock	-0.57 (1.12)	-1.06 (1.16)	-1.05 (1.18)	-1.06 (1.17)	-2.04 (1.18)
Average growth	-1.79 (1.63)	-2.30 (1.98)	-2.42 (1.95)	-2.46 (1.92)	-2.89 (2.95)
Interest on external debt (lag)	0.12*** (0.03)	0.10*** (0.03)	0.10** (0.04)	0.10** (0.04)	0.11** (0.05)
Terms of trade (lag)	-0.07*** (0.02)	-0.06*** (0.02)	-0.05*** (0.02)	-0.06*** (0.02)	-0.05 (0.03)
IMF program	-0.76 (0.47)	-0.24 (0.49)	-0.22 (0.52)	-0.24 (0.49)	-0.35 (0.66)
Time since previous reform		0.36*** -0.08	0.37*** (0.08)	0.36*** (0.08)	0.26** (0.12)
Number of previous reforms	-0.09 (0.14)	0.37*** (0.11)	0.39*** (0.12)	0.38*** (0.11)	-1.26*** (0.48)
Year dummy variables	Yes	No	No	No	Yes
Country fixed effects	Clustered	Clustered	Clustered	Clustered	Dummy
No. observations	224	256	256	256	256

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. This table reports logit results for (A) a lagged dependent variable; (B) the core specification without year fixed effects; (C) a spatial correlation, based on whether there are crises in other countries weighted by the GDP of the country; (D) a spatial correlation, based on whether a government in the region defaulted in a given year; and (E) both year and country fixed effects. All specifications include a constant term (not reported). For columns A and E, marginal probabilities are difficult to compute and are excluded.

create an alternative variable that is a simple dummy variable if there is a country that defaults in a given year, which appears in column D. (Note that this last specification cannot include year fixed effects.) As table 5 indicates, none of the specifications for spatial correlation are statistically significant. The result for the effects of fiscal crises does weaken in the specification with fiscal crises in other countries weighted by GDP, but the spatial diffusion variables themselves are not statistically significant and the dampening effect of a banking crisis remains.

Finally, column E explores the effects of country dummy variables instead of clustering the variance by country. The fiscal crisis variable weakens. This is to be expected once country dummy variables are included, given that only a few countries had such crises in practice. Similarly, the effect of banking crises declines somewhat, with a statistical significance level of $p < 0.07$. The positive effects of increasing debt on reform remain, as before.

There are several alternative arguments to consider in the core model. We begin with modifications to the coding of the crisis variable and then discuss additional political 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. Countries that previously had fixed exchange rates are the most likely to experience this type of crisis.⁵⁵ A currency crisis 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 A of table 6 reports the results for exchange rate crises, using the data and classification from Laeven and Valencia.⁵⁶ The substantive results do not change, and exchange rate crises do not affect the likelihood of fiscal reforms.

A second consideration is that the effects of a crisis may appear in the future and not in the same year that a crisis occurs. In the model, we consider the effects of crises as contemporaneous, that is, we assume that governments react immediately to a crisis with fiscal reform legislation in the same year. However, fiscal reforms may come well after the crisis has begun. An IMF study suggests that one type of reform—namely, fiscal rules—is introduced to lock in fiscal adjustment gains.⁵⁷ The analogy is to inflation targeting in the central banking literature, which points out that several central banks 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

55. Fischer (2001).

56. Laeven and Valencia (2012).

57. IMF (2009).

TABLE 6. Robustness Tests: Crisis Variables^a

Explanatory variable	(A)		(B)		(C)	(D)	
	Coefficient	Probability	Coefficient	Probability	Coefficient	Coefficient	Probability
Data set for banking crises	Laeven-Valencia		Laeven-Valencia		Laeven-Valencia	Reinhart-Rogoff	
Baseline probability: No crises							
Banking = 0 Fiscal = 0		0.14**		0.15**			0.15**
Banking crisis	-1.75***		-1.92***			-1.64**	
Banking = 1 Fiscal = 0	(0.53)	0.03	(0.72)	0.03		(0.75)	0.03
Fiscal crisis	2.71***		2.47***		2.47***	1.83**	
Banking = 0 Fiscal = 1	(0.83)	0.71**	(0.68)	0.67***	(0.76)	(0.76)	0.53***
Banking = 1 Fiscal = 1		0.31*		0.24*			0.18
Banking crisis (LV), one-year lag			0.09				
			(0.55)				
Banking crisis (LV), two-year lag			0.17				
			(0.49)				
Banking crisis (LV), year of crisis					-1.50***		
					-0.44		
Banking crisis (LV), year squared					0.20***		
					-0.07		
Exchange rate crisis	-1.32						
	(1.49)						
Presidential election year	0.79		0.87*		0.72	0.62	
	(0.55)		(0.53)		(0.65)	-0.61	
United government	0.55		0.52		0.67	0.6	
	(0.44)		(0.43)		(0.45)	-0.46	
Ideology of president	0.10		0.13		0.18	0.17	
	(0.19)		(0.18)		(0.20)	-0.18	
Interest rate shock	-0.76		-0.85		-1.15	-0.75	
	(1.15)		(1.11)		(1.03)	-0.97	

(continued)

TABLE 6. Robustness Tests: Crisis Variables^a (Continued)

<i>Explanatory variable</i>	<i>(A)</i>		<i>(B)</i>		<i>(C)</i>		<i>(D)</i>	
	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Coefficient</i>	<i>Probability</i>	
Average growth	-1.49 (1.46)		-1.14 (1.82)		-2.39 (1.59)	-0.35 (1.78)		
Interest on external debt (lag)	0.11*** (0.03)		0.11*** (0.03)		0.11*** (0.03)	0.11*** (0.03)		
Terms of trade (lag)	-0.07*** (0.02)		-0.07*** (0.02)		-0.08*** (0.02)	-0.06*** (0.02)		
IMF program	-0.41 (0.52)		-0.40 (0.47)		-0.39 (0.53)	-0.48 (0.50)		
Time since previous reform	0.25*** (0.08)		0.25*** (0.08)		0.27*** (0.08)	0.21** (0.10)		
Number of previous reforms	0.12 (0.15)		0.11 (0.14)		0.20 (0.16)	0.06 (0.16)		
No. observations	224		224		224	224		

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. This table reports logit results for different configurations of crisis variables, including an exchange rate crisis, lagged banking crises using a count of the year of the crisis, and the Reinhart-Rogoff (2009) coding of crises instead of Laeven-Valencia. All logits include a constant term (not reported) and country-clustered standard errors.

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 B of table 6. 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 third consideration is to use a “count” version of a banking crisis, which is coded per year that the crisis lasts, beginning at one. We expect that each consecutive year of a banking crisis affects the probability of reforms, so that the negative impact moves toward zero and then becomes positive. The logit analysis with a simple dummy variable cannot capture whether such effects exist. Column C considers the financial crisis as a count, and it includes the squared term as well to capture nonlinear effects. As before, we are interested in the effects of a banking crisis both with and without 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. The coefficients are all significant in the expected direction—bank crises initially depress the likelihood of a reform, but the squared term indicates that the effect reverses over time. An issue, however, is that there are few crises that last more than four years. While outer years become clearly positive, they are based on very few observations, and based on point estimates for specific years, they have very high standard errors and are not statistically different from year to year of the crisis. The coefficients, which indicate that the probability of a reform increases after the initial dip to zero in the first year of the crisis, can therefore only be suggestive.⁵⁸

The final possibility is that there are alternative ways of coding banking crises. Column D considers the measure from Reinhart and Rogoff, who document several centuries of crisis using a somewhat broader definition of banking crisis.⁵⁹ In practice, this means that 19 percent of the country-years are in a banking crisis, as opposed to 16 percent according to Laeven and Valencia.⁶⁰ The results remain substantively the same under the Reinhart-Rogoff measure, with banking crisis having the same negative and statistically significant coefficient.

The additional political variables that we consider are reported in table 7. The first is a measure for the size of the common pool resource (CPR) problem, which is the source of fiscal indiscipline that institutional reforms are meant to

58. Detailed results available upon request.

59. Reinhart and Rogoff (2009). Their data set, in part, considers Laeven and Valencia (2008), which mostly overlaps with Laeven and Valencia (2012).

60. Laeven and Valencia (2012).

TABLE 7. Additional Political Variables^a

<i>Explanatory variable</i>	<i>(A)</i>		<i>(B)</i>		<i>(C)</i>		<i>(D)</i>		<i>(E)</i>	
	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>	<i>Coefficient</i>	<i>Probability</i>
Baseline probability: No crises										
Banking = 0 Fiscal = 0		0.15**		0.15**		0.15**		0.15**		0.15**
Banking crisis	-2.08		-1.79***		-1.82***		-1.73***		-1.79***	
Banking = 1 Fiscal = 0	(0.66)	0.02	(0.65)	0.03	(0.65)	0.03	(0.62)	0.03	(0.63)	0.03
Fiscal crisis	2.90***		2.42**		2.44**		2.22**		2.22**	
Banking = 0 Fiscal = 1	(0.84)	0.76**	(0.77)	0.66***	(0.76)	0.67***	(0.64)	0.62***	(0.66)	0.62***
Banking = 1 Fiscal = 1		0.28**		0.25*		0.24*		0.23*		0.21**
Personal vote	1.98**									
	(0.79)									
New president			-0.45							
			(0.52)							
New presidential party					-0.30					
					(0.61)					
Checks							-0.04			
							(0.11)			
Partisanship (DPI)									0.24	
									(0.21)	
Presidential election year	0.91		1.13*		1.03*		0.76		0.85	
	(0.59)		(0.60)		(0.60)		(0.52)		(0.54)	

United government	0.88* (0.53)	0.50 (0.43)	0.51 (0.43)		0.54 (0.48)
Ideology of president	0.25 (0.19)	0.11 (0.18)	0.11 (0.18)	0.09 (0.18)	
Interest rate shock	-0.59 (1.03)	-0.93 (1.06)	-0.95 (1.10)	-0.84 (1.09)	-0.70 (1.01)
Average growth	-2.18 (1.60)	-1.36 (1.56)	-1.47 (1.59)	-1.32 (1.53)	-0.88 (1.36)
Interest on external debt (lag)	0.10*** (0.03)	0.11*** (0.03)	0.11*** (0.03)	0.11*** (0.03)	0.11*** (0.03)
Terms of trade (lag)	-0.07*** (0.02)	-0.07*** (0.02)	-0.07*** (0.02)	-0.07*** (0.02)	-0.06*** (0.02)
IMF program	-0.21 (0.58)	-0.41 (0.53)	-0.39 (0.52)	-0.31 (0.52)	-0.22 (0.46)
Time since previous reform	0.28*** (0.10)	0.25*** (0.09)	0.25*** (0.09)	0.24*** (0.08)	0.27*** (0.09)
Number of previous reforms	0.09 (0.16)	0.13 (0.16)	0.13 (0.16)	0.11 (0.16)	0.15 (0.17)
No. observations	224	224	224	221	224

* Statistically significant at the 10 percent level.

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. This table reports logit results for additional political variables or for different codings of political variables. All logits include a constant term (not reported) and country-clustered standard errors.

reduce. In the previous analyses, we implicitly assume that the size of the CPR problem is constant across countries. However, political institutions may affect the overall size of the problem: the *potential* CPR problem is greater when decisionmakers think they can improve their political future by worrying about a narrow slice of the population when making spending and taxation decisions. 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 of the personal vote first introduced by Hallerberg and Marier.⁶¹ 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.⁶² Candidate-centered electoral systems encourage congresspersons to think about a very narrow slice of the common pool, while party-centered systems lead to a consideration of the party's constituency. These scores indicate the potential size of the CPR problem in congress. We expect that the greater the size of the personal vote, the greater the theoretical size of the CPR problem. This implies that the costs from *not* reforming fiscal institutions are great. There should be a positive relationship between the extent of the personal vote and the likelihood of fiscal reforms.

The results in column A are broadly consistent with the argument that countries with potentially greater CPR problems in their legislatures are more likely to reform. This finding is seemingly counter to what one would expect from the literature on neoliberal reform, which suggests that stronger parties lead to more reform.⁶³ The result suggests that this pressure counterbalances, and even exceeds, the need for stronger parties (at least in this group of countries). An issue with this variable, however, is that it rarely changes within countries, so it could be picking up other factors associated with the countries. Other identification strategies that are beyond the purview of this paper, such

61. Hallerberg and Marier (2004).

62. For more details about the calculation of this variable, see Hallerberg and Scartascini (2011). The general 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 consider these factors in the context of the district magnitude of a given country, which we measure as the size of the median electoral district. 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. Data for this variable are from Hallerberg and Marier (2004), which in turn are updated (and sometimes corrected) with data from Payne, Zovatto, and Mateo Díaz (2007) and from a data set posted on John Carey's website (www.dartmouth.edu/~jcarey/Data%20Archive.html).

63. See, for example, Haggard and Kaufman (1994).

as process tracing to get at the causal mechanism and more micro-evidence, would be needed to evaluate this variable in more detail.

The policy stream literature suggests that changes in policy might be especially likely after changes in leadership, when the political stream has come more in sync with the other two streams. We therefore check whether changes in the president or changes in the partisanship of the president affect the results (see columns B and C). Another possibility is that our definition of veto players is too narrow; we thus include the “checks” variable from Beck and others as an alternative to the united government variable (column D).⁶⁴ This variable counts each chamber of a legislature unless the president’s party controls it and there is a closed list electoral system in place. Finally, there are different data sets for measuring partisanship. A popular alternative measure of presidential partisanship comes from the same source for the “checks” measure of veto players.⁶⁵ The coding of this variable is ambiguous for several countries in this data set, however, so we use data from Hallerberg and Scartascini to provide the additional codes and complete the data set.⁶⁶

As table 7 indicates, the new variables introduced in these remaining specifications are not statistically significant. This means that changes in the president, the president’s partisanship, the measure of veto players, and the measure of partisanship do not make fiscal reforms more likely. At the same time, the effects of the two crisis variables remain the same as in the base model.

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. Under a banking crisis, fiscal pressure to find more money quickly restricts the government’s ability to initiate fiscal reforms. This need clearly trumps the demand to signal to markets that the country will be solvent in the future. These results are consistent with others who suggest that crises do not always lead to reform.⁶⁷ We thus find that the relationship between crisis and reform is more complex than simply “a crisis is a terrible thing to waste.” At the same time, fiscal crises do

64. Beck and others (2001).

65. Beck and others (2001).

66. Hallerberg and Scartascini (2011).

67. For example, Hugh-Jones (2014).

lead to big jumps in the probability of fiscal reforms. It is the type of crisis, and not just the existence of one, that is most crucial.

This paper also takes our understanding of fiscal rules and institutions forward. A wide body of literature considers their effectiveness, but the endogeneity question hangs a shadow over most (though not all) of that literature—why did countries improve 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 strengthening fiscal discipline. Moreover, reforms can be reversed, and the reversals can take several forms, from outright repeal of the relevant law to a choice not to enforce it. Incorporating reversals into a model of institutional effectiveness would require a more detailed examination of specific cases and a typology of what reversals really represent. A research strategy that builds on this paper's focus on the introduction of fiscal reforms could examine the form such reversals take, as well as their timing.

Our results are also interesting for what they do not reveal. We find no evidence that crises lead 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.

Our findings point to what further research might reveal. In our robustness section, we consider two variations, namely, including a variable for the year of the banking crisis (and not just for the presence of a crisis) and a measure for the personal vote, which captures the extent of the common pool resource problem. In the first case, there were not enough observations of crises that lasted more than four years to generate any confidence that the probability of reform increases in future years. In the second, there is simply not enough variation across countries. The strategy for exploring the first would be to look at other crises in other regions and at other points of time; for the second, simply adding data from more countries will probably not suffice, as it is unlikely that the personal vote varies greatly within countries. Other identification strategies are needed to get a better sense of the importance of this variable for reforms.

Our results on the dynamics between different types of crisis and the introduction of fiscal reforms are relevant beyond Latin America. They help explain why reforms have been so hard to pass in the developed world during the ongoing global financial crisis. Governments initially need money both to wind down troubled banks and to address the drop in economic output. This applies to governments in Greece, Ireland, Spain, and so on. Thoughts of fiscal institutional reform arose only after these countries needed money to finance their mounting debt burdens.

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