Do National Numerical Fiscal Rules Really Shape Fiscal Behaviours in Developing Countries? A Treatment Effect Evaluation

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Abstract:

This paper analyses the effect of National Numerical Fiscal Rules (*FRs*) upon fiscal discipline in 74 developing countries over the period 1990-2007. It is the first study that assesses the impact of *FRs* on budgetary outcomes while controlling for the self-selection problem. It finds that the effect of *FRs* on structural fiscal balance is significantly positive, robust to a variety of alternative specifications, and varies with the type of *FRs*. It also finds that the treatment effect differs according to countries characteristics: number of *FRs*, time length since *FRs* adoption, presence of supranational *FRs*, government fractionalisation and government stability.

JEL Codes: H11, H61, H62.

Keywords: Fiscal rules, Fiscal discipline, Treatment effect, Propensity scores-matching, Developing countries.

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1. Introduction

The recent global recession and financial crisis, along with the policy actions taken to buffer their effects, have eroded fiscal positions in several countries, raising concerns about the sustainability of public finances. Policymakers are therefore urged to undertake appropriate measures to put back public finances on a sustainable path. To this end, the establishment of fiscal rules appears *inter alia*, as a good candidate. Indeed, fiscal rules –*FRs* hereafter- are "a permanent constraint on fiscal policy, expressed in terms of a summary indicator of fiscal performance" (Kopits and Symansky, 1998). Well designed and implemented, they are able to strengthen fiscal credibility and fiscal discipline, in that they place a durable constraint on the discretion of fiscal authorities (Alesina and Perotti, 1995; Debrun et *al*., 2007; Eichengreen and Wyplosz, 1998; von Hagen, 1992; von Hagen and Harden, 1995; Inman, 1996; and Poterba, 1996). First started in the developed countries, the new wave of *FRs* has gained the developing world.[1] To date, twenty-five low-income and middle-income countries have adopted *FRs* at the national level to frame the conduct of their fiscal policy (IMF, 2009).

Parallel to this growing appetite for *FRs* in developing countries, a few papers attempted to evaluate their effectiveness in shaping fiscal behaviours in these countries. Alesina et *al*. (1999) are the first study to assess the impact of *FRs* in developing countries, namely in Latin America and the Caribbean. Thereafter, Dabla-Norris et *al*. (2010), Gollwitzer (2011), Hallerberg et *al*. (2009), Poter and Diamond (1999), and Prakash and Cabezon (2008) analysed the effect of *FRs* and found that they improve fiscal discipline in developing countries. But a drawback, common to all these existing studies, is that they ignore the *self-selection* problem in policy adoption, which might bias the estimate of the effect of *FRs* in

[1] The new wave of *FRs*, whose implementation is accompanied by a greater transparency contrary to the older ones, started in New Zealand in 1994 (Kopits, 2001).

these early studies. A more formal re-evaluation of the impact of *FRs*, taking into account the *self-selection* problem in policy adoption is therefore necessary.

The aim of this paper is therefore to assess the effect of *FRs* on fiscal developments, by addressing carefully the issue of *self-selection* this time. To this end, we make use of a variety of *propensity scores-matching* and a wide panel of 74 developing countries, of which 22 have introduced rule-based fiscal frameworks by the end of 2007, to evaluate the *treatment* effect of *FRs*. In the literature related to monetary policy, such methods have been used to evaluate the impact of inflation targeting, a monetary policy framework where *self-selection* is potentially also at work (Lin and Ye, 2007). But to the best of our knowledge, our paper is the first study to take into account this *self-selection* problem while investigating the impact of *FRs*. More precisely, throughout the paper, we aim to answer the following questions: do national numerical *FRs* improve fiscal discipline as measured by the cyclically-adjusted primary fiscal balance (CAPB), after controlling for *self-selection*? Does the *treatment* effect vary with the types of rules (Budget Balance Rules, Expenditure Rules and Debt rules)? Finally, is there heterogeneity in the *treatment* effect of *FRs*, depending on countries structural characteristics? We explore five possible sources of heterogeneities: number of *FRs* in place, time length since *FRs* adoption, presence of supranational *FRs*, government fractionalisation and government stability.

The rest of the paper is organised as follows: the second section describes the econometric methodology and introduces the dataset. Section 3 shows the *propensity scores-matching* results while section 4 considers some robustness checks. In section 5, we explore the heterogeneity feature of the *treatment* effect of *FRs* using a control function regression approach. Section 6 briefly concludes and draws some policy recommendations.

2. The data

Our dataset consists of 74 developing countries examined over the period 1990-2007.[2] The panel is unbalanced because of missing observations. The time coverage of the sample is 1990-2007 because it is a common feature that reliable fiscal data exist only from early 1990s to 2007 at most, especially in developing countries. The sample is composed of 22 countries that have adopted *FRs* at the national level by the end of 2007 (called *FRers*) and 52 *non-FRers.* To make sure that the *control* group is a good counterfactual of the *treatment* group, that is the two groups are reasonably comparable, we include in the *control* group only *non-FRers* developing countries that have a real GDP per capita at least as large as that of the poorest *FRer* and with a population size at least as large as that of the smallest *FRer*.[3] The 22 *treated* countries and the 52 *control* countries that satisfy these criteria are listed in the first two columns and the last three columns of *Appendix 1* respectively.

The *FRers* along with their starting dates have been taken from the Fiscal Rules Database by the IMF's Fiscal Affairs Department, Fiscal Policy and Surveillance Division (2009) which gives a comprehensive overview on *FRs* experiences around the world at the national level as well as at the supranational level.[4] Nevertheless, we choose to focus on national *FRs* for two reasons. First, supranational rules generally suffer from a problem of insufficient enforcement and compliance so that the member countries frequently violate these rules without any sanctions. The most obvious examples are some countries from the European Union, the WAEMU and the CEMAC (Prakash and Cabezon, 2008).[5] These rules therefore look like simple ornaments. Accordingly, it seems better to focus on *FRs* experiences at the national level which most of time result from a real political commitment. Second, by

[2] The developing countries category considered here refers to the World Bank classification, thus including low-income as well as middle-income countries.
[3] The poorest *FRer* in our sample is Kenya with a real per capita GDP of 2025.179 in 2007 while the smallest *FRer* in terms of population size is Cape Verde (with 424395 inhabitants in 2007).
[4] The database is available at www.imf.org/external/np/pp/eng/2009/121609.pdf.
[5] West African Monetary and Economic Union (WAEMU) involves eight countries of West Africa, while the Central African Economic and Monetary Community (CEMAC) is composed of six countries of Central Africa.

distinguishing the national *FRs* from the supranational ones, as in Debrun et *al*. (2008) and IMF (2009), we are able to analyse in the *probit* estimates of the *propensity scores*, the influence of having supranational rules on the decision to introduce national *FRs*. Better, this allows us to explore whether or not the presence of rule-based fiscal frameworks at the supranational level influences the *treatment* effect of the national *FRs*.

It is worthnoting that in the existing literature, the effectiveness of *FRs* is assessed using not only the presence of numerical targets or limits on fiscal aggregates, but also employing other aspects related to the strength or intensity of these rules. These include their statutory basis, the sanctions for breaking the rules, the procedures required to modify or amend the rules, the share of government finances covered by rules and fiscal transparency. However, the *propensity-scores matching* used in this paper allows building a binary measure of *FRs* only, indicating the presence or not of rule, but not to build a synthetic *FRs* index summarising the other aspects mentioned above. Furthermore, even though it would be possible to analyse the importance of the strength of the rules when we will explore the heterogeneity feature of the *treatment* effect of *FRs* using a control function regression approach, most of these aspects related to the strength of *FRs* are missing for many countries in our sample. Attempting to analyse the influence of the strength of the rule on the *treatment* effect of *FRs* will imply a significant reduction in our sample size. Accordingly, we choose to use only the simple binary measure of *FR*.

Table 1 displays the 22 *FRers* along with their starting dates. Excepted Indonesia which has implemented *FRs* in 1967, most countries started at the end of 1990s or early 2000s. 60 per cent of *FRers* have adopted Budget Balance Rules (*BBR*), 36% have adopted Expenditure Rules (*ER*) and 55 per cent have opted for Debt Rules (*DR*). Only two countries, namely Kenya and Nigeria have enacted Revenue Rules (*RR*).[6]

[6] Madagascar (2006), Mexico (2006), Equatorial Guinea (2007) and Mauritius (2008) adopted *FRs*, but given that our sample ends up in 2007, we still treat them as *non-FRers.* The Union of the Comoros (2001), Namibia (2001), Liberia (2004) and

Table 1: Fiscal Rules countries along with their starting dates

Countries	Starting Dates				
	National Numerical Fiscal Rule				
		BBR	*ER*	*RR*	*DR*
Angola	2005		2005		
Argentina	2000	2000	2000		2000
Bulgaria	2003				2003
Brazil	2000	2000	2000		2001
Botswana	2003		2003		
Cape Verde	1998	1998			1998
Chile	2000	2000			
Costa Rica	2001		2001		
Czech Republic	2005		2005		
Ecuador	2003	2003			2003
Estonia	1993	1993			
India	2004	2004			
Indonesia	1967	1967			2004
Israel	1992	1992	2005		
Kenya	1997			1997	1997
Lithuania	1997				1997
Nigeria	2004			2004	
Pakistan	2005	2005			2005
Panama	2002	2002			2002
Peru	2000	2000	2000		
Poland	1997				1997
Sri Lanka	2003	2003			2003

Source: Fiscal Affairs Department, IMF (2009). www.imf.org/external/np/pp/eng/2009/121609.pdf BBR=Budget Balance Rule; ER=Expenditure Rule; RR=Revenue Rule; DR: Debt Rule.

In the econometric analysis below, *FR* is a dummy variable equalling one, if in a given country at a given year a numerical constraint exists on any fiscal aggregate at the national level (budget balance, spending, revenue or debt). *BBR*, *ER*, *RR* and *DR* are dummies variables equalling one, if in a given country at a given year a numerical constraint is placed only on budget balance, expenditure, revenue and debt respectively.[7]

Timor-Leste (2005) also adopted *FRs*, but due to lack of available data on fiscal balances, they are not included in our sample.

[7] The numerical constraints are generally expressed as a ceiling or a target. For instance, the *BBR* may target a specific budget balance in nominal terms, a specific budget balance as a percentage of GDP or a specific budget balance as a percentage of GDP in cyclically-adjusted or structural terms. The *DR* may target a specific amount of debt in nominal terms; a specific debt-to-GDP ratio or may establish a ceiling for the Government debt in level or as a % of GDP. The *ER* may specify a ceiling on the expenditure-to-GDP ratio, a ceiling on current expenditure growth or expenditure limits inserted in a medium-term expenditure framework. Finally, the *RR* may specify desired developments of the tax base, a target for revenue-to-GDP ratio or a ceiling on the use of oil revenues.

Fiscal data come from the IMF World Economic Outlook (2010). Our measure of fiscal discipline is the Structural or Cyclically-Adjusted Primary Fiscal Balance, as GDP percentage (CAPB). It is the difference between General Government revenues and expenditures excluding interest payments, adjusted for the effect of business cycle fluctuations. This is a measure of discretionary fiscal behaviour, that is fiscal policy changes really imputable to current fiscal policymakers, in that it not only excludes the effects of past fiscal policy decisions (interest payments) but also filters out the impact of automatic stabilisers on the primary balance. To compute the CAPB, in line with the so-called "residuals" approach (Fatás and Mihov, 2006; 2003) we estimate the following fiscal policy reaction function adapted from Fatás and Mihov (2003; 2006), on a country-by-country base[8]:

$$PB_t^i = \alpha + \beta PB_{t-1}^i + \lambda GAP_t^i + \delta W_t^i + \varepsilon_t^i, \quad \forall i \tag{1}$$

where PB_t^i is the primary fiscal balance, for country i at year t, GAP_t^i the output gap, and W_t^i a set of control variables. The output gap is calculated as the difference between the logarithm of real GDP and the logarithm of a Hodrick-Prescott filtered trend of real GDP (with 100 as smoothing parameter).[9] Control variables include inflation and a time trend. The λ coefficient measures the cyclical response of fiscal policy to business cycle fluctuations, and the error term ε_t^i measures the unsystematic component of fiscal policy. The estimated value of this latter catches the part of primary fiscal balance unexplained by economic conditions and is our measure of fiscal discipline (CAPB).[10] To correct for a potential endogeneity of output gap in equation (1), we use the two stages least squares (2SLS) method and instrument the

[8] In the literature, the CAPB is also calculated using a three-step procedure, especially in several international organisations (OECD, IMF or European Commission, see Girouard and André, 2005). First, they calculate a measure of potential GDP. Second, to estimate the budget balance that is due to business cycle fluctuations, they apply the elasticity of government revenues and expenditures, to the deviation between the effective GDP and the potential GDP. Third, they deduct the CAPB by subtracting the budget balance estimated in the second step, from the primary fiscal balance actually observed. Although very attractive, this methodology is very intensive in detailed data, namely in the estimation of revenue and expenditure elasticity. As such detailed information does not exist in developing countries we rather focus on the "residuals" approach.

[9] We have also used 6.25 as smoothing parameter, but this does not change significantly the estimation results.

[10] Note that Fatás and Mihov (2003; 2006) rather took the standard deviation of the error term (and not the error term itself) because they were interested in the volatility of fiscal policy (and not fiscal policy itself).

output gap with its lagged value.The 2SLS-based results (see *Appendix 10*) indicate that in 69% of cases (51 of 74 countries), the *F-statistics* associated with the instrumentation equations are above the rule of thumb of 10 (Staiger and Stock, 1997), suggesting that the lagged output gap is reasonably a strong instrument. This result is reinforced by the analysis of the *Shea's Partial* R^2 statistics, which shows that in more than 85% of cases (63 of 74 countries), the *Partial* R^2 are above the rule of thumb of 20.

We also use the same methodology displayed in (1) to calculate a measure of Cyclically-Adjusted-Primary Expenditure (CAPE). We use this latter to evaluate the impact of Expenditure Rule (*ER*) on a measure of government spending filtered out from the influence of economic conditions. Regarding the assessment of the impact of Debt Rule (*DR*) on government debt developments, we use a recent central government debt database collected by Ali Abbas et *al*. (2010). Descriptive statistics, definitions and sources of the other variables can be found in *Appendices 8* and *9*.

As depicted in *Figure 1* (see *Appendix 7*), the *treated* countries (*FRers*) improved their CAPB between the *pre-FR* period and the *post-FR* period, their CAPB passing from -0.8 (as GDP percentage) to 1 (as GDP percentage). Meanwhile, the CAPB in the *control* group (which is around 0, as GDP percentage) decreased slightly between the *pre-FR* period and the *post-FR* period.[11] This seems to give a first indication that *FRs* adoption improves fiscal discipline. A similar finding can be viewed in *Figures 2* and *3* (see *Appendix 7*) where we use the CAPE and government debt as alternative measures of fiscal performances respectively. Indeed, the CAPE decreased between the *pre-FR* period and the *post-FR* period in *treated* countries while it increased in the *control* group. Regarding public debt, it decreased between the *pre-FR* period and the *post-FR* in the *treated* countries as well as in the *control* group, but

[11] Note that as this is done in the literature related to inflation targeting, the *post-FR* period for the *non-FRers* has been defined as the mid between the first *FRs* adoption date in our sample (Indonesia started *FRs* in 1967, but given that our sample begins in 1990, this latter becomes therefore the starting date of *FRs* for Indonesia) and the ending date in our sample (2007), that is 1998.

more in the first group. Nevertheless, are these naive correlations corroborated by a more rigorous econometric analysis? In the next section, we assess the impact of *FRs* more formally, by controlling for the *self-selection* problem in policy adoption.

3. Methodology

Our objective is to evaluate the *treatment* effect of *FRs* on fiscal discipline. To this end, we consider the adoption of *FRs* by a country as a *treatment*, just as in the program evaluation literature in microeconomic studies. Consistently with this literature, we refer to the countries having adopted *FRs –FRers* hereafter- as the *treated* group, and to the *non-FRers* as the *control* group. Then, the average effect of being a *FRer* on fiscal discipline, the so-called Average Treatment effect on the Treated (*ATT*), can be expressed as follows:

$$ATT = E[(Y_{i1} - Y_{i0}) \mid FR_i = 1] = E[Y_{i1} \mid FR_i = 1] - E[Y_{i0} \mid FR_i = 1] \quad (2)$$

where FR_i is the fiscal rule dummy variable in country *i*. Y_{i1} is the value of the outcome variable when the country *i* has adopted *FRs* and Y_{i0} if not. $Y_{i0} / FR_i = 1$ is the outcome value that would have been observed if a *FRer* had not adopted *FRs* policy, and $Y_{i1} / FR_i = 1$ the outcome value really observed on the same *FRs* country. Equation (2) is telling us that a simple comparison between the outcome value (fiscal discipline in our case) observed in the *treatment* group and the outcome value observed in the same countries if they had not adopted *FRs* would give us an unbiased estimate of the *ATT*. Unfortunately, it is not possible to observe this latter outcome value since we cannot observe the fiscal performance a *FRer* had it not adopted *FRs*. We face here, as it is common in non-experimental studies, an identification problem.

A common approach to circumvent this difficulty is to compare the sample mean budgetary outcome of the *treatment* group with that of the *control* group if and only if assignment to the *treatment* is random. However, *FRs* adoption may be non-random, as *FRs*

may be correlated with a set of observable variables that also affects the outcome variable, leading to the so-called *self-selection* problem.[12] Simple comparison of the sample mean budgetary outcome between the two groups would then produce biased estimates of the *ATT*. As in Lin and Ye (2007), to address this problem of selection on observables, we make use of a variety of *propensity scores-matching* methods recently developed in the *treatment* literature.

3.1. Matching on Propensity-Scores

Propensity Scores-Matching (*PSM* hereafter) consists of pairing *FRers* with *non-FRers* which have similar observed characteristics, so that the difference between the outcome of a *FRer* and that of a matched counterfactual is attributable to the *treatment* (*FRs* adoption). A key assumption needed to apply *PSM* is "conditional independence" $(Y_0, Y_1 \perp FR|X)$ which requires that conditional on the observables (*X*), the outcome be independent of the *treatment* variable. Under this assumption, equation (2) can be rewritten as:

$$ATT = E[Y_{i1}|FR_i = 1, X_i] - E[Y_{i0}|FR_i = 0, X_i] \quad (3)$$

where we have replaced $E[Y_{i0}|FR_i = 1, X_i]$ with $E[Y_{i0}|FR_i = 0, X_i]$ which is observable. Yet, as the number of covariates in *X* increases, matching on *X* would be difficult to implement in practice; to overcome this high dimension problem, we follow Rosenbaum & Rubin (1983) and base the matching on the propensity scores (instead of *X*). The *propensity score* (PS hereafter) is the probability of adopting the IT regime, conditional to the observable covariates (*X*), namely

$$p(X_i) = E[IT_i|X_i] = Pr(IT_i = 1|X_i) \quad (4).$$

Under a final assumption needed for the validity of the PSM (the so-called "common support assumption" $p(X_i) < 1$, namely the existence of some comparable control units for each treated unit), we estimate the ATT as

$$ATT = E[Y_{i1}|FR_i = 1, p(X_i)] - E[Y_{i0}|FR_i = 0, p(X_i)] \quad (5).$$

12 It is worthnoting that the Propensity Scores-Matching method does not implicitly account for the unobservables; as a result, the issues it addresses differ from those related to selection on unobservables (omitted variables) as well as from a Heckman-type sample selection problem (see Dehejia and Wahba, 2002, and Heckman et al., 1998 for further details).

3.2. Estimating the propensity scores (*PS*)

We estimate the *PS* using a *probit* model with the binary variable *FR* as the dependent variable. Our baseline selection equation includes past fiscal development variables (Cyclically-Adjusted Primary budget Balance-CAPB- and Debt, both as GDP percentage, and lagged one ear), the real per capita GDP growth rate, dependency ratio, government stability, government fractionalisation, inflation and a dummy for the presence of a supranational *FR*.

We expect *FRs* to be introduced more likely in fiscally healthier countries, since the public credibility regarding the ability of government to meet its announced targets for fiscal aggregates is the cornerstone of *FRs* (Calderon and Schmidt-Hebbel, 2008; and IMF, 2009). Accordingly, we expect a positive correlation between the probability of *FRs* adoption and the lagged value of CAPB, but a negative correlation with the lagged value of public debt.[13] We also expect *FRs* to be adopted more likely in countries with good macroeconomic performances (IMF, 2009). As a result, the expected signs on the estimated coefficients of real per capita GDP growth rate and inflation are positive and negative respectively. Countries with higher dependency ratio, which implies generally higher public fiscal burden, are less likely to adopt rule-based fiscal frameworks (Calderon and Schmidt-Hebbel, 2008). We therefore expect a negative correlation between *FRs* adoption and dependency ratio, that is the ratio of dependents (people younger than 15 or older than 64) to working-age population (those ages 15-64). Regarding the politico-institutional factors, we expect a positive link between the probability of adopting *FRs* and the fragmentation of government. Indeed, according to the "tying their hands" approach, *FRs* introduction can be viewed as a mechanism to rule out the deficit bias originating from the so-called "common-pool" problem (Alesina and Perotti, 1995; and Debrun et *al*., 2008). The expected sign on government stability is ambiguous *a priori*. Indeed, on the one hand, greater government stability may

[13] It is worthnoting that the relationship might be non-linear for public debt, with the likelihood of *FRs* introduction increasing below a give threshold, while decreasing above that threshold. We check for such a non-linear effect in section 4.

lead to lower *deficit bias*, which in turn should be associated positively with *FRs* adoption. On the other hand, government instability, that is the inability of the government to stay in office and carry out its declared programs, may encourage governments to tie their hands through *FRs* adoption in order to ensure fiscal discipline despite the succession of different executive teams. In this spirit, greater government stability might be less conducive to *FRs* adoption. Finally, we expect a positive link between the supranational fiscal rule dummy and *FRs* adoption at the national level, as the presence of supranational fiscal rule may catalyse the introduction of the national ones (Debrun et *al*., 2008; and IMF, 2009). *Table 2* below reports the *probit* estimates of the *PS*.[14]

Table 2: Probit estimates of the propensity scores

Dependent Variable	**Fiscal Rule (FR) Dummy Variable**					
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
CAPB (GDP%) lagged one year	0.045**	0.047***	0.045**	0.044**	0.044**	0.047**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Public Debt (GDP %) lagged one year	-0.005**	0.002	-0.005*	-0.004*	-0.005**	-0.005**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Real per capita GDP growth rate	0.032**	0.028**	0.033**	0.041***	0.034***	0.032**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Dependency ratio	-0.007	-0.007	-0.007	-0.003	-0.008	-0.007
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Government stability	-0.121***	-0.125***	-0.121***	-0.104***	-0.123***	-0.118***
	(0.031)	(0.031)	(0.032)	(0.033)	(0.032)	(0.031)
Government fractionalisation	0.383*	0.386*	0.384*	0.345*	0.367*	0.343*
	(0.205)	(0.204)	(0.205)	(0.207)	(0.203)	(0.206)
Inflation	-0.023***	-0.023***	-0.023***	-0.022***	-0.025***	-0.021***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Supranational FR Dummy	0.049	0.006	0.046	0.055	0.124	0.048
	(0.178)	(0.180)	(0.183)	(0.183)	(0.184)	(0.179)
Squared public debt (lagged one year)		-0.178**				
		(0.081)				
Logarithm of Real per capita GDP			0.011			
			(0.130)			
Quality of the bureaucracy				0.251***		
				(0.069)		
Trade Openness					-0.002	
					(0.002)	
International Official Reserves to GDP						-0.001
						(0.004)
Number of observations	772	772	772	772	772	757
Pseudo R^2	0.100	0.107	0.100	0.114	0.102	0.100

Note: Robust standard errors are reported in brackets. Constants included (but not reported).*, **, and ***: significance level of 10%, 5%, and 1% respectively.

[14] According to the *conditional independence* assumption, omitting in the *probit* model, variables that systematically affect the probability of enacting *FRs* but do not affect budgetary outcomes, has little influence on results (Persson, 2001). In other words, an estimate bias occurs only if we omit an explanatory variable that simultaneously affect fiscal discipline and the probability of adopting *FRs*. We give much attention to this issue when selecting variables into the *probit* model.

Column [1] displays the probit results with *FR* as the dependent variable. Recall that *FR* is a dummy variable equalling one, if in a given country at a given year a numerical constraint exists on any fiscal aggregate (budget balance, expenditure, or debt). Most coefficients are significant and have the expected signs. Lagged CAPB, real per capita GDP growth rate, government fractionalisation and supranational *FR* dummy are correlated positively with *FRs* adoption. Note however that the estimated coefficient on supranational *FR* dummy is not significantly different from zero. Lagged government debt, dependency ratio and inflation are negatively associated with the probability of adopting *FRs*. Finally, the sign of the estimated coefficient on government stability is negative, suggesting that *FRs* in our sample are introduced for "tying their hands" reasons. *Column* [1] of *Appendices 2*, *3* and *4* display the *probit* results for Budget Balance Rule (*BBR*), Expenditure Rule (*ER*) and Debt Rule (*DR*) respectively. The results remain almost identical to those of *Table 2*, except in some cases.[15]

3.3. Results from matching on propensity scores.

Based on the *PS* estimated above, we employ four commonly used methods to match each *FRer* with *non-FRers*, depending on the closeness of their scores to that of the *FRer*.[16] First, the *nearest neighbour matching* with replacement, which matches each *treated* country to the N *control* countries that have the closest *PS* (we use $N=1$, $N=2$ and $N=3$). Second, the *radius matching*, which performs the *matching* based on *PS* falling within a certain *radius* or "caliper" R (we use a small *radius R=0.005*, a medium *radius R=0.01* and a wide *radius R=0.05*). The third method is the *regression-adjusted local linear matching* developed by Heckman et *al*. (1998). Fourth, we consider the *kernel matching*, which matches a *FRer* to all *non-FRers* weighted proportionally to their closeness to the *treated* country. As the matching

[15] The estimated coefficient on lagged public debt becomes positive with *BBR* and *DR*, but remains negative (although not statistically significant) with *ER*. The estimated coefficient on the supranational dummy becomes negative with *BBR* and *ER* but proved to be not significantly different from zero.

[16] While matching *FRers* to *non-FRers*, we employ the "*common support*" option. With this option, we exclude the *treated* countries whose the *PS* is higher than the maximum or less than the minimum *PS* of the *untreated* countries.

estimator presents no analytical variance, we compute standard errors by bootstrapping (that is by re-sampling the observations of the *control* group, see Dehejia and Wahba, 2002).

The upper panel of *Table 3* (line [1]) reports the estimated *ATT* of *FRs* on the CAPB. Irrespective of the *matching* method, the estimation results show that *FRs* adoption does improve fiscal discipline, as the estimated *ATT* is positive and statistically significant. The amplitude of the estimated *ATT* ranges from 0.642 (*kernel* matching) to 1.180 percentage points of GDP (*1-Nearest-neighbour*), suggesting that on average, *FRs* adoption enhances the CAPB by 0.642 and 1.180 percentage points of GDP respectively. Does the discipline-enhancing effect of *FRs* vary depending on the type of rule (*BBR*, *ER*, *DR*)? The upper panel (line [1]) of *Tables 4, 5* and *6* address this issue.[17]

Panel [1] of *Table 4* reports the *ATT* with *BBR* as the *treatment* variable and the CAPB as the budgetary outcome. The *ATTs* still are positive, suggesting that placing numerical constraints on the budget balance allows enhancing the CAPB. But the statistical significance and the magnitude of the estimated *ATTs* decrease slightly with respect to those estimated with *FR* as treatment variable (*Panel* [1] of *Table 3*). *Panel* [1)] of *Table* 5 reports the estimated *ATT* with this time, *ER* as the *treatment* variable, and Cyclically-Adjusted Primary Expenditure (CAPE) as the budgetary outcome. Irrespective of the *matching* estimator, the estimation results show that *ER* adoption does reduce the CAPE. The amplitude of the *ATT* is even higher than that estimated in *panel* [1] of *Table 3* and *4*: it extends from -0.866 (local linear regression matching) to -1.612 (*1-Nearest-neighbor*) percentage points of GDP. Finally, panel [1] of *Table 6* displays the *matching* results for *DR*. The estimated *ATTs* are not statistically different from zero, suggesting that in our sample, countries having enacted *DR* do not perform better than countries that did not introduce *DR*, in terms of government debt developments. But one might be cautious in interpreting this last result. Indeed, the lack of

[17]We do not assess the effect of revenue rule (*RR*) because only two countries (Kenya and Nigeria) enacted *RR* in our sample.

significance of the estimated *ATT*, and to a lesser extent the fact that in some cases the sign of the *ATT* of *DR* is even positive, might be due to the inability of the simple binary measure of *DR* to account for the other important aspects –enforcement, monitoring, transparency, sanctions- necessary for the success of any rules.

Table 3: Matching results (With FR Dummy as Treatment Variable)

Treatment Variable	1-Nearest-Neighbour Matching	2-Nearest-Neighbour Matching	3-Nearest Neighbour Matching	Radius Matching			Local Linear Regression Matching	Kernel Matching
				r=0.005	r=0.01	r=0.05		
Fiscal Rule (FR)	***Dependent variable: Cyclically-Adjusted Budget Balance (CAPB, GDP %)***							
[1]: Average Treatment on the Treated (ATT)	1.180***	0.828**	0.820**	0.772***	0.691***	0.644***	0.685***	0.642***
	(0.424)	(0.389)	(0.357)	(0.289)	(0.263)	(0.244)	(0.259)	(0.246)
Number of Treated observations	128	128	128	125	127	128	128	128
Number of Control observations	640	640	640	640	640	640	640	640
	Robustness Checks							
[2]: Adding squared public debt (lagged)	1.250*	0.762*	0.750**	0.675**	0.690***	0.715***	0.737***	0.698***
	(0.697)	(0.409)	(0.364)	(0.294)	(0.260)	(0.241)	(0.262)	(0.239)
[3]: Adding Logarithm of Real per capita GDP	1.405**	1.373**	1.248**	1.170**	1.099**	1.138**	1.206***	1.144**
	(0.594)	(0.549)	(0.510)	(0.505)	(0.466)	(0.463)	(0.440)	(0.458)
[4]: Adding Quality of the bureaucracy	1.218*	1.365*	1.355**	1.416***	1.248**	1.225***	1.222***	1.224***
	(0.708)	(0.704)	(0.646)	(0.538)	(0.502)	(0.446)	(0.471)	(0.448)
[5]: Adding Trade Openness	1.980***	1.760***	1.567***	1.333***	1.375***	1.200***	1.344***	1.215***
	(0.672)	(0.593)	(0.543)	(0.493)	(0.482)	(0.465)	(0.485)	(0.463)
[6]: Adding International Official Reserves to GDP	1.872***	1.590***	1.455**	1.330***	1.276**	1.259***	1.297***	1.270***
	(0.570)	(0.571)	(0.568)	(0.478)	(0.497)	(0.474)	(0.490)	(0.475)

Note: bootstrapped standard errors (via 500 replications) in brackets. *, **, and *** indicate the significance level of 10%, 5%, and 1%.

Table 4: Matching results (With BBR Dummy as Treatment Variable)

Treatment Variable	1-Nearest-Neighbour Matching	2-Nearest-Neighbour Matching	3-Nearest Neighbour Matching	Radius Matching			Local Linear Regression Matching	Kernel Matching
				r=0.005	r=0.01	r=0.05		
Budget Balance Rule (BBR)	***Dependent variable: Cyclically-Adjusted Budget Balance (CAPB, GDP %)***							
[1]: Average Treatment on the Treated (ATT)	0.707	0.810*	0.676*	0.588*	0.486*	0.465*	0.417*	0.478**
	(0.488)	(0.441)	(0.403)	(0.345)	(0.247)	(0.260)	(0.272)	(0.243)
Number of Treated observations	80	80	80	78	78	80	80	80
Number of Control observations	688	688	688	688	688	688	688	688
	Robustness Checks							
[2]: Adding squared public debt (lagged)	0.826*	1.051**	1.137**	1.049***	1.070***	1.083***	1.034***	1.083***
	(0.493)	(0.459)	(0.453)	(0.379)	(0.365)	(0.359)	(0.360)	(0.360)
[3]: Adding Logarithm of Real per capita GDP	0.640*	0.437	0.789**	0.816**	0.942**	0.965**	0.901**	0.961**
	(0.326)	(0.411)	(0.394)	(0.376)	(0.367)	(0.377)	(0.369)	(0.377)
[4]: Adding Quality of the bureaucracy	0.791*	0.909**	1.020**	0.897**	0.923**	0.919**	0.830**	0.910**
	(0.469)	(0.436)	(0.425)	(0.394)	(0.394)	(0.384)	(0.394)	(0.384)
[5]: Adding Trade Openness	1.283***	1.188***	1.213***	1.204***	1.212***	1.242***	1.157***	1.240***
	(0.469)	(0.427)	(0.388)	(0.345)	(0.347)	(0.353)	(0.345)	(0.350)
[6]: Adding International Official Reserves to GDP	1.112**	1.177***	1.146***	1.187***	1.168***	1.220***	1.164***	1.142***
	(0.453)	(0.434)	(0.424)	(0.381)	(0.377)	(0.364)	(0.355)	(0.360)

Note: bootstrapped standard errors (via 500 replications) in brackets. *, **, and *** indicate the significance level of 10%, 5%, and 1%.

Table 5: Matching results (With ER Dummy as Treatment Variable)

Treatment Variable	1-Nearest-Neighbour Matching	2-Nearest-Neighbour Matching	3-Nearest Neighbour Matching	Radius Matching			Local Linear Regression Matching	Kernel Matching
				r=0.005	r=0.01	r=0.05		
Expenditure Rule (ER)	***Dependent variable: Cyclically-Adjusted Primary Expenditure (CAPE, GDP %)***							
[1]: Average Treatment on the Treated (ATT)	-1.612**	-1.148*	-1.147*	-0.936**	-0.874**	-0.966**	-0.866**	-0.957**
	(0.794)	(0.672)	(0.600)	(0.431)	(0.437)	(0.433)	(0.440)	(0.430)
Number of Treated observations	40	40	40	40	40	40	40	40
Number of Control observations	722	722	722	722	722	722	722	722
	Robustness Checks							
[2]: Adding squared public debt (lagged)	-1.322***	-1.262***	-1.227***	-1.002***	-0.983***	-0.925***	-0.911***	-0.933***
	(0.492)	(0.453)	(0.404)	(0.355)	(0.317)	(0.285)	(0.296)	(0.285)
[3]: Adding Logarithm of Real per capita GDP	-0.891*	-0.757*	-0.784*	-0.730**	-0.753**	-0.777**	-0.868***	-0.779**
	(0.514)	(0.448)	(0.406)	(0.355)	(0.352)	(0.318)	(0.329)	(0.319)
[4]: Adding Quality of the bureaucracy	-1.085**	-0.805*	-0.897**	-0.554	-0.782**	-0.926***	-0.838**	-0.917***
	(0.491)	(0.478)	(0.447)	(0.375)	(0.367)	(0.329)	(0.340)	(0.325)
[5]: Adding Trade Openness	-1.158**	-1.040**	-1.167***	-1.037***	-1.092***	-1.037***	-1.079***	-1.050***
	(0.514)	(0.471)	(0.451)	(0.322)	(0.322)	(0.299)	(0.323)	(0.301)
[6]: International Official Reserves to GDP	-1.310***	-0.735*	-0.800**	-0.807**	-0.853***	-0.947***	-0.929***	-0.938***
	(0.493)	(0.420)	(0.377)	(0.336)	(0.326)	(0.271)	(0.282)	(0.271)

Note: bootstrapped standard errors (via 500 replications) in brackets. *, **, and *** indicate the significance level of 10%, 5%, and 1%.

Table 6: Matching results (With DR Dummy as Treatment Variable)

Treatment Variable	1-Nearest-Neighbour Matching	2-Nearest-Neighbour Matching	3-Nearest Neighbour Matching	Radius Matching			Local Linear Regression Matching	Kernel Matching
				r=0.005	r=0.01	r=0.05		
Debt Rule (DR)	*Dependent variable: Public Debt (GDP %)*							
[1]: Average Treatment on the Treated (ATT)	-2.788	-2.613	-0.371	-0.549	1.546	0.520	3.406	0.964
	(8.638)	(6.757)	(6.757)	(6.757)	(6.757)	(6.757)	(6.757)	(6.757)
Number of Treated observations	73	73	73	72	73	73	73	73
Number of Control observations	686	686	686	686	686	686	686	686
	Robustness Checks							
[2]: Adding squared public debt (lagged)	0.601	1.456	0.0732	-0.858	-0.790	-3.322**	-0.734	-2.806**
	(2.695)	(2.587)	(2.349)	(1.302)	(1.237)	(1.290)	(1.286)	(1.251)
[3]: Adding Logarithm of Real per capita GDP	-0.839	1.736	1.199	-0.259	-0.811	-4.269***	-0.425	-3.762***
	(3.194)	(2.753)	(2.544)	(1.451)	(1.347)	(1.281)	(1.309)	(1.253)
[4]: Adding Quality of the bureaucracy	-0.263	1.631	0.253	-0.906	-0.770	-3.848***	-1.018	-3.338**
	(3.758)	(3.167)	(3.122)	(1.845)	(1.603)	(1.353)	(1.457)	(1.322)
[5]: Adding Trade Openness	-7.097*	-5.461*	-4.526	-2.490	-2.470	-4.583***	-2.588	-4.503***
	(3.821)	(3.170)	(2.790)	(1.818)	(1.743)	(1.418)	(1.581)	(1.430)
[6]: International Official Reserves to GDP	-0.557	-1.415	-1.085	-0.672	-0.319	-3.181***	-0.569	-3.022***
	(3.925)	(3.213)	(2.861)	(1.660)	(1.402)	(1.181)	(1.316)	(1.147)

Note: bootstrapped standard errors (via 500 replications) in brackets. *, **, and *** indicate the significance level of 10%, 5%, and 1%.

4. Robustness Checks

To make sure that we filter out sufficiently any possible polluting effect resulting from observables known to affect both fiscal performances and the probability of adopting *FRs*, we augment the *probit* model by controlling respectively for the lagged value of the squared public debt (for a possible non-linearity in the effect of public debt), the logarithm of real per capita GDP (proxy for the level of economic development), quality of the bureaucracy (proxy for institutional quality), trade openness, and international official reserves to GDP.[18] *Columns* [2], [3], [4], [5] and [6] of *Table 2* show the *probit* results when using *FR* dummy as the selection variable, and their corresponding results for the *ATT* are depicted in *Table 3* (*lines* [2] to [6]). The *probit* results when using *BBR*, *ER* and *DR* dummies as the selection variables are depicted in columns [2] to [6] of *Appendices 2*, *3* and *4* respectively, while their corresponding *ATT* results are depicted in *lines* [2] to [6] of *Tables 4*, *5*, and *6* respectively. The results remain robust to these new specifications: the *probit* results as well as the estimated *ATT* do not change qualitatively and quantitatively.

5. Exploring the heterogeneity in the treatment effect

Even though developing countries share some common features, there exists however some differences between them, including *inter alia*, their socio-political and institutional contexts. They may even differ in some aspects related to the way they apply *FRs*. Given that these factors may make different, the *ATT* of *FRs* on fiscal discipline, we explore in this section the presence of heterogeneity in the *treatment* effect of *FRs*. We test five possible

[18] These variables are considered in the literature as possible determinants of the probability of *FRs* adoption (see, *e.g.*, Calderon and Schmidt-Hebbel, 2008; and IMF, 2009).

sources of heterogeneity: the number of *FRs* in place, the time length since *FRs* introduction, the presence of a supranational *FR*, government fractionalisation and government stability.[19]

For this purpose, we use a control function regression approach, adapted from Lin and Ye (2009) and described as follows. We perform, within the *common support* from the *matching* in previous section, the simple following OLS regression:

$$CAPB_{it} = \alpha + \beta FR_{it} + \gamma Pscore_{it} + \phi X_{it} + \theta FR_{it} * X_{it} + \varepsilon_{it} \quad (6).$$

$Pscore_{it}$, the estimated *propensity scores* from our baseline *probit* model, is included as a control function. X_{it} is the set of possible sources of heterogeneity variables. The coefficient of the interactive term between the *FR* dummy and Xit, θ, catches the heterogeneity feature of the *treatment* effect of *FRs.*

Table 7 below reports the estimated *treatment* effect of *FRs* on the CAPB, based on the control function regression approach. The first column shows a simple OLS regression linking the *FRs* dummy to the CAPB within the *common support*. The estimated coefficient of *FRs*, which catches the difference in mean CAPB between *FRers* and *non-FRers*, is positive and significantly different from zero. Then, in the second column, we include $Psocre_{it}$ as a control function. Its estimated coefficient is positive and significantly different from zero, indicating that *self-selection* bias is at work in the evaluation of the *treatment* effect of *FRs* upon fiscal discipline in our sample. This justifies *a posteriori* the use of *propensity scores-matching* in the previous section. The estimated coefficient of *FR* is still significantly different from zero but smaller in magnitude. The estimated average *treatment* effect of *FR* on the CAPB as GDP percentage, after controlling for *self-selection* is about 0.689, which is close to the *ATT* obtained from *matching*. The last five columns of *Table 7* are devoted to

[19] It would be interesting to check for possible heterogeneity due to factors related to the strength of *FRs* (enforcement, monitoring, transparency and sanctions). However, the lack of such detailed data for a sufficient number of countries in our sample prevents us from doing so.

possible heterogeneity of the *treatment* effect of *FRs*, through the estimated coefficients of the interactive terms as described above.[20]

In column [3], the estimated coefficient of the interaction of *FR* and the number of *FRs* in place is positive and significantly different from zero, suggesting that the more the number of rules in place, the larger the discipline-enhancing effect of *FRs*. The adoption of a constraint on an additional fiscal aggregate increases the *treatment* effect by 0.645 percentage point. It is worthnoting that even though this is not a real measure of the intensity of *FRs*, this result seems to indicate that the intensity in the use of *FRs* matters, in accordance with the existing literature (Debrun et *al*., 2008).[21]

Column [4] shows that time length since *FRs* adoption reduce the disciplinary effect of *FRs*, as the estimated coefficient of the interaction term between *FR* and *Time* is negative and significantly different from zero. This suggests that the credibility component of *FRs* comes more from the "signals" they send to the public and financial markets rather than from any reputation acquired due to length of time in the use of *FRs*. Results in columns [5], [6] and [7] show that the *treatment* effect of *FRs* is reduced by the presence of supranational rules and government fragmentation whereas it is enhanced by government stability. The first result of this set of three may be due to the fact that supranational *FRs* are weakly enforced in developing countries, as documented in Prakash and Cabezon (2008), so that this may result in negative externalities onto the national rules, leading to an overall smaller *treatment* effect. Regarding the role of government fragmentation, it is in accordance with Alesina and Perotti (1995) who argue that the *common pool* problem is expected to be stronger in fragmented and heterogeneous government coalitions. Finally, the enhanced *treatment* effect of *FRs* in more stable governments suggests that the ability of government to stay in power and carry out its

[20] Note that as this appears in equation (6), normally, both interacted variables, *FR* and X_{it}, should be included in the regression individually. But in column [3] of *Table 3*, we do not include *Number of FRs* in the regression because the interaction term is the same as *Number of FRs.* Similarly, *Time* is not included in the regression of column [4], because the interaction term is the same as *Time*.

[21] It would have been more relevant to use for example the share of government finances covered by rules. But the lack of availability of such data in developing countries prevents us from using this measure.

declared programs, including the fulfilment of the announced targets for fiscal aggregates, is a key element of the success of *FRs*.

We also explore the heterogeneity in the treatment effect of *ER* on the *CAPE*. The results (see *Table 8*) confirm the previous conditional discipline-enhancing effects of *FR*. Indeed, it appears that while the reducing-effect of *ER* on the CAPE decreases with the existence of supranational *FRs* and the degree of fractionalisation of the government, it increases with the degree of stability of the government. However, the number of rules in place as well as the time length since the introduction of an *ER* does not influence any more significantly the reducing-effect of *ER* on the CAPE.

Table 7: Heterogeneity in the treatment effect of Fiscal Rules (FRs) on Structural Primary Fiscal Balance

Dependent Variable: CAPB (GDP %)	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**
FR Dummy	0.828***	0.689**	-0.294	0.975**	0.797**	1.306***	-2.820*
	(0.317)	(0.291)	(0.603)	(0.400)	(0.311)	(0.483)	(1.696)
Propensity Score		1.570*	1.690	1.719*	1.800*	2.476**	1.993*
		(0.801)	(1.038)	(1.027)	(1.084)	(1.094)	(1.043)
FR * Number of FRs			0.645*				
			(0.358)				
FR * Time				-0.0549*			
				(0.0309)			
Supranational Dummy					0.443		
					(0.654)		
FR * Supranational					-1.239*		
					(0.681)		
Gov. Fractionalisation						-0.419	
						(0.502)	
FR*Gov. Fractionalisation						-2.304**	
						(0.922)	
Government stability							-0.022
							(0.072)
FR*Government Stability							0.434**
							(0.219)
Observations	768	768	768	768	768	768	768
R^2	0.009	0.011	0.014	0.013	0.013	0.021	0.017

Note: in brackets the bootstrapped standard errors (with 500 replications). *, **, and *** indicate the significance level of 10%, 5%, and 1%, respectively. Constant terms are included but not reported.

Table 8: Heterogeneity in the treatment effect of ER on the CAPE (GDP %)

Dependent Variable: CAPE (GDP %)	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**
ER Dummy	-1.023*	-0.933**	-1.414	-0.911	-1.057**	-2.142***	3.501*
	(0.522)	(0.470)	(1.136)	(0.689)	(0.519)	(0.667)	(2.008)
Propensity Score		-4.381*	-4.335*	-4.368*	-4.712*	-4.863*	-4.678*
		(2.305)	(2.281)	(2.298)	(2.480)	(2.559)	(2.386)
ER * Number of FRs			0.256				
			(0.497)				
ER * Time				-0.00599			
				(0.117)			
Supranational Dummy					0.153		
					(0.335)		
ER * Supranational					1.295*		
					(0.690)		
Gov. Fractionalisation						-0.0327	
						(0.400)	
ER *Gov. Fractionalisation						4.539***	
						(1.124)	
Government stability							0.0764*
							(0.040)
ER *Government Stability							-0.543**
							(0.259)
Observations	762	762	762	762	762	762	762
R^2	0.005	0.007	0.007	0.007	0.008	0.018	0.012

Note: ER=Expenditure Rule; CAPE: Cyclically-Adjusted Primary Expenditure. In brackets the bootstrapped standard errors (with 500 replications). *, **, and *** indicate the significance level of 10%, 5%, and 1%, respectively. Constant terms are included but not reported.

Finally, for the sake of robustness check, we carried out the same control function regressions, but controlling for country-fixed and year-fixed effects this time. The main results remain robust to these new specifications (see *Appendices 5* and *6* for the effect of *FR* on the CAPB and the effect of *ER* on the CAPE respectively).

6. Conclusion

In this paper, we analyse the relationship between national fiscal rules (*FRs*) and fiscal discipline in developing countries. Based on a wide panel data of 74 developing countries over the period 1990-2007, this paper is the first, to the best of our knowledge, to take into account the *self-selection* problem in policy adoption while evaluating the effect of *FRs* on fiscal performances. Relying on a variety of *propensity scores matching* methods, which allows us controlling for *self-selection*, this paper therefore reassesses more formally the impact of *FRs* on budgetary outcomes. It finds that the Average Treatment effect (*ATT*) of *FRs* on the Cyclically-Adjusted Primary fiscal Balance (CAPB) is significantly positive and

robust to a variety of alternative specifications.The magnitude of the contribution of *FRs* to the CAPB is rather important, as *FRs* enhance the CAPB by at least 0.642 and up to 1.180 percentage points of GDP. We also find that the *treatment* effect varies with the type of *FRs*: while Budget Balance Rules and Expenditure Rules have significant discipline-enhancing effects, the effect of Debt Rules appears mixed and not significantly different from zero. Last but not the least, we show that there is heterogeneity in the *treatment* effect of *FRs*, depending on countries characteristics: number of *FRs* in place, time length since *FRs* adoption, presence of supranational *FRs*, government fractionalisation and government stability.

In terms of policy implications, this paper suggests that the introduction of rule-based fiscal frameworks remains a credible remedy for governments in developing countries against fiscal indiscipline. This is particularly important in the current context, where the implementation of massive stimulus plans has eroded fiscal positions in many countries, which commands to undertake credible measures to put back public finances on a sustainable path. Nevertheless, it is important to keep in mind that the simple adoption of *FRs* is not sufficient to guarantee fiscal credibility and fiscal discipline. Their adoption must be accompanied with a set of other measures, beyond the scope of this study, but essential to the success of *FRs*. Such measures include *inter alia*, fiscal transparency, fiscal responsibility, enforcement mechanisms, sanctions and independent fiscal institutions (fiscal councils).

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Appendices

Appendix 1: Country List

Treatment **Group**		***Control*** **group**		
Angola	Panama	Albania	Georgia	Philippines
Argentina	Peru	Algeria	Guatemala	Romania
Bulgaria	Poland	Azerbaijan	Hungary	Russian Federation
Brazil	Sri Lanka	Bangladesh	Iran, Islamic Rep.	Serbia
Botswana		Bahrain	Korea, Republic	Slovak Republic
Cape Verde		Belarus	Lesotho	Slovenia
Chile		Bolivia	Malaysia	South Africa
Costa Rica		Chad	Mauritius	Sudan
Czech Republic		Cameroon	Mexico	Swaziland
Ecuador		China	Mongolia	Syrian Arab Rep.
Estonia		Colombia	Morocco	Thailand
India		Congo, Republic	Mozambique	Trinidad & Tobago
Indonesia		Côte d'Ivoire	Jamaica	Tunisia
Israel		Croatia	Jordan	Turkey
Kenya		Dominican Republic	Kazakhstan	Ukraine
Lithuania		Egypt	Latvia	Uruguay
Nigeria		Fiji	Paraguay	Venezuela
Pakistan		Gabon		

Appendix 2: Probit estimates of the propensity scores (With BBR as dependent variable)

Dependent Variable	**BBR Dummy Variable**					
	[1]	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**
CAPB (GDP%) lagged one year	0.063***	0.070***	0.062***	0.064***	0.059***	0.076***
	(0.022)	(0.022)	(0.021)	(0.022)	(0.020)	(0.023)
Public Debt (GDP %) lagged one year	0.003	0.016***	0.004*	0.004	0.003	0.002
	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
Real per capita GDP growth rate	0.013	0.001	0.017	0.023	0.015	0.016
	(0.016)	(0.016)	(0.016)	(0.017)	(0.016)	(0.017)
Dependency ratio	-0.024***	-0.025***	-0.016**	-0.019***	-0.028***	-0.032***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)
Government stability	-0.153***	-0.156***	-0.146***	-0.138***	-0.157***	-0.138***
	(0.036)	(0.037)	(0.037)	(0.039)	(0.036)	(0.036)
Government fractionalisation	0.781***	0.790***	0.799***	0.756***	0.747***	0.833***
	(0.249)	(0.256)	(0.247)	(0.251)	(0.248)	(0.260)
Inflation	-0.039***	-0.041***	-0.039***	-0.037***	-0.046***	-0.041***
	(0.010)	(0.009)	(0.010)	(0.010)	(0.011)	(0.010)
Supranational FR Dummy	-0.534*	-0.722**	-0.624**	-0.564*	-0.325	-0.557**
	(0.283)	(0.291)	(0.281)	(0.295)	(0.285)	(0.280)
Squared public debt (lagged one year)		-0.334***				
		(0.089)				
Logarithm of Real per capita GDP			0.298*			
			(0.155)			
Quality of the bureaucracy				0.311***		
				(0.084)		
Trade Openness					-0.005**	
					(0.002)	
International Official Reserves to GDP						-0.035***
						(0.010)
Number of observations	772	772	772	772	772	757
Pseudo R^2	0.143	0.174	0.151	0.163	0.154	0.169

BBR=Budget Balance Rule. Robust standard errors are reported in brackets. Constants included (but not reported). *, **, and ***: significance level of 10%, 5%, and 1%.

Appendix 3: Probit estimates of the propensity scores (With ER as dependent variable)

Dependent Variable	ER Dummy Variable					
	[1]	[2]	[3]	[4]	[5]	[6]
CAPB (GDP%) lagged one year	0.072**	0.074***	0.073**	0.072**	0.068**	0.071***
	(0.029)	(0.028)	(0.029)	(0.029)	(0.023)	(0.028)
Public Debt (GDP %) lagged one year	-0.002	0.002	0.002	-0.002	-0.003	-0.001
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Real per capita GDP growth rate	0.023	0.020	0.032*	0.027	0.029	0.023
	(0.017)	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)
Dependency ratio	-0.012*	-0.011*	0.010	-0.009	-0.014*	-0.012*
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Government stability	-0.092**	-0.095**	-0.079	-0.083*	-0.095**	-0.107**
	(0.047)	(0.046)	(0.051)	(0.048)	(0.047)	(0.046)
Government fractionalisation	-0.003	0.005	0.064	-0.020	-0.070	0.006
	(0.312)	(0.311)	(0.313)	(0.317)	(0.303)	(0.314)
Inflation	-0.010**	-0.010**	-0.011**	-0.010**	-0.013**	-0.010**
	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Supranational FR Dummy	-0.138	-0.159	-0.345	-0.147	0.108	-0.142
	(0.273)	(0.281)	(0.284)	(0.276)	(0.268)	(0.273)
Squared public debt (lagged one year)		-0.111				
		(0.106)				
Logarithm of Real per capita GDP			0.847***			
			(0.171)			
Quality of the bureaucracy				0.130		
				(0.090)		
Trade Openness					-0.006**	
					(0.002)	
International Official Reserves to GDP						0.008**
						(0.004)
Number of observations	772	772	772	772	772	757
Pseudo R^2	0.077	0.080	0.136	0.081	0.093	0.087

Robust standard errors are reported in brackets. Constants included (but not reported).*, **, and ***: significance level of 10%, 5%, and 1%

Appendix 4: Probit estimates of the propensity scores (With DR as dependent variable)

Dependent Variable	DR Dummy Variable					
	[1]	[2]	[3]	[4]	[5]	[6]
CAPB (GDP%) lagged one year	0.032*	0.029	0.034*	0.030*	0.029*	0.038**
	(0.017)	(0.018)	(0.018)	(0.017)	(0.017)	(0.019)
Public Debt (GDP %) lagged one year	0.004**	-0.018**	0.003	0.004**	0.004**	0.003
	(0.002)	(0.008)	(0.002)	(0.002)	(0.002)	(0.002)
Real per capita GDP growth rate	-0.0005	0.010	-0.004	0.003	0.001	0.002
	(0.012)	(0.014)	(0.012)	(0.012)	(0.012)	(0.012)
Dependency ratio	-0.017***	-0.019***	-0.025***	-0.014**	-0.022***	-0.024***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Government stability	-0.109***	-0.113***	-0.114***	-0.099***	-0.115***	-0.093***
	(0.033)	(0.034)	(0.034)	(0.035)	(0.034)	(0.034)
Government fractionalisation	0.277	0.256	0.266	0.282	0.226	0.290
	(0.212)	(0.217)	(0.215)	(0.215)	(0.207)	(0.214)
Inflation	-0.023***	-0.022***	-0.023***	-0.022***	-0.029***	-0.022***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)
Supranational FR Dummy	-0.047	0.060	0.020	-0.044	0.146	-0.046
	(0.211)	(0.221)	(0.217)	(0.214)	(0.223)	(0.213)
Squared public debt (lagged one year)		0.628***				
		(0.174)				
Logarithm of Real per capita GDP			-0.238*			
			(0.132)			
Quality of the bureaucracy				0.157**		
				(0.068)		
Trade Openness					-0.006***	
					(0.002)	
International Official Reserves to GDP						-0.028***
						(0.010)
Number of observations	772	772	772	772	772	757
Pseudo R^2	0.074	0.111	0.079	0.079	0.090	0.094

DR=Debt Rule. Robust standard errors are reported in brackets. Constants included (but not reported).*, **, and ***: significance level of 10%, 5%, and 1%

Appendix 5: Heterogeneity in the treatment effect of FR on the CAPB (Country & Time Fixed Effect Estimations)

Dependent Variable: CAPB (GDP %)	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**
FR Dummy	2.660**	2.430**	-0.266	2.281**	2.607**	3.041**	-1.931
	(1.208)	(1.133)	(2.199)	(1.019)	(1.134)	(1.415)	(2.866)
Propensity Score		4.513*	4.355*	4.398*	4.660*	5.354*	6.053**
		(2.302)	(2.222)	(2.243)	(2.817)	(2.813)	(2.912)
FR * Number of FRs			1.671*				
			(0.852)				
FR * Time				0.0709			
				(0.279)			
Supranational Dummy					1.199*		
					(0.612)		
FR * Supranational					-1.875*		
					(0.956)		
Gov. Fractionalisation						-0.730	
						(0.887)	
FR *Gov. Fractionalisation						-3.893*	
						(1.986)	
Government stability							0.093
							(0.159)
FR *Government Stability							0.531*
							(0.271)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	768	768	768	768	768	768	768
R^2	0.133	0.141	0.152	0.141	0.145	0.152	0.150

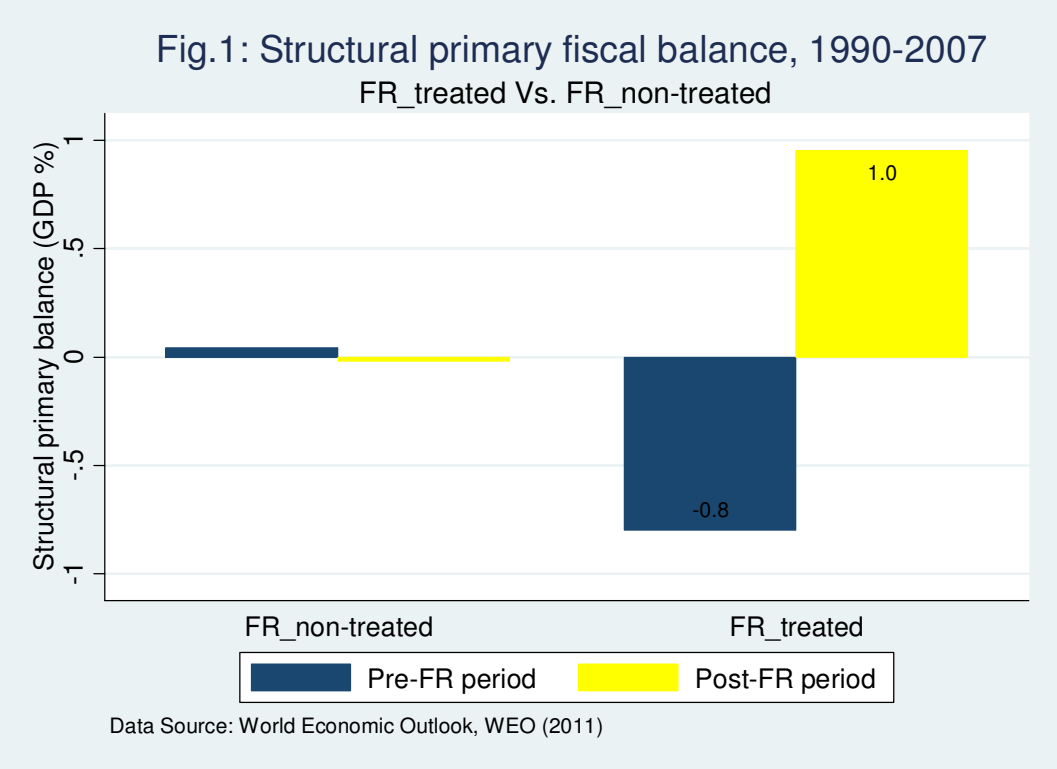
Note: FR=Fiscal Rule; CAPB: Cyclically-Adjusted Primary Balance. In brackets the bootstrapped standard errors (with 500 replications). *, **, and *** indicate the significance level of 10%, 5%, and 1%, respectively. Constant terms are included but not reported.

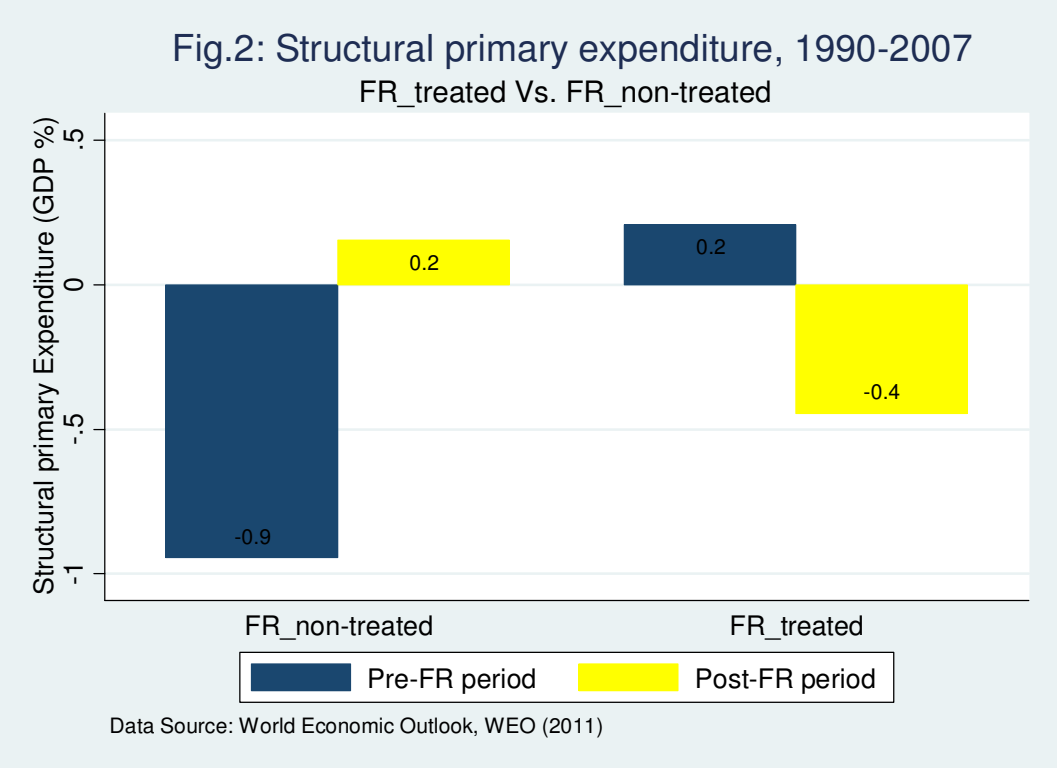
Appendix 6: Heterogeneity in the treatment effect of ER on the CAPE (Country & Time Fixed Effect Estimations)

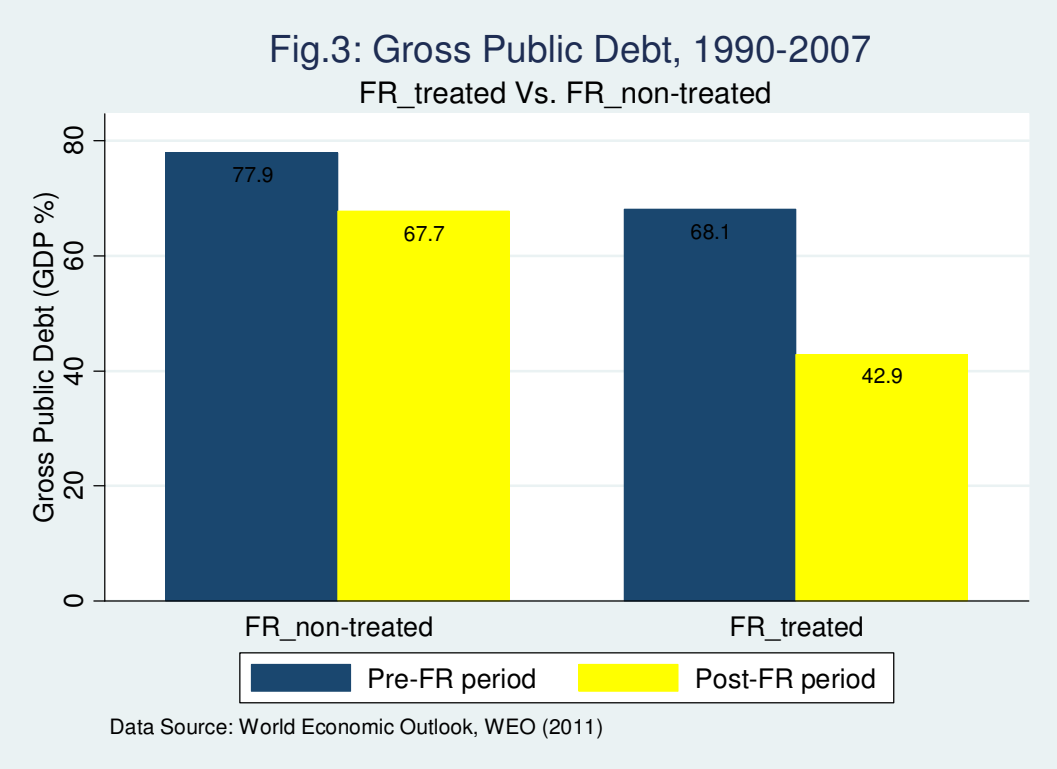
Dependent Variable: CAPE (GDP %)	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**
ER Dummy	-3.048**	-2.523*	-3.376	-2.470	-2.766*	-4.873***	3.728
	(1.456)	(1.418)	(2.491)	(1.748)	(1.603)	(1.436)	(2.706)
Propensity Score		-21.55***	-21.77***	-21.51***	-21.23**	-21.78***	-20.62**
		(8.240)	(8.303)	(8.242)	(8.258)	(8.365)	(8.128)
ER * Number of FRs			0.486				
			(1.582)				
ER * Time				-0.015			
				(0.215)			
Supranational Dummy					0.642		
					(0.808)		
ER * Supranational					1.996*		
					(1.018)		
Gov. Fractionalisation						-0.070	
						(0.826)	
ER *Gov. Fractionalisation						8.986***	
						(2.263)	
Government stability							0.209*
							(0.107)
ER *Government Stability							-0.751**
							(0.319)
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	762	762	762	762	762	762	762
R^2	0.127	0.144	0.144	0.144	0.147	0.170	0.152

Note: ER=Expenditure Rule; CAPE=Cyclically-Adjusted Primary Expenditure. In brackets the bootstrapped standard errors (with 500 replications). *, **, and *** indicate the significance level of 10%, 5%, and 1%, respectively. Constant terms are included but not reported.

Appendix 7: Figures







Appendix 8: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Cyclically-Adjusted Primary Fiscal Balance (GDP %)	1074	0.016	3.492	-18.344	24.618
Cyclically-Adjusted Primary Expenditure (GDP %)	1075	-0.188	3.660	-26.182	25.411
Government Debt (GDP %)	1195	56.838	41.593	3.742	454.864
Fiscal Rule (FR)	1332	0.131	0.337	0.000	1.000
Budget Balance Rule (BBR)	1332	0.092	0.289	0.000	1.000
Expenditure Rule (ER)	1332	0.035	0.185	0.000	1.000
Revenue Rule (RR)	1332	0.013	0.112	0.000	1.000
Debt Rule (DR)	1332	0.068	0.251	0.000	1.000
Supranational Dummy	1314	0.066	0.249	0.000	1.000
Real per capita GDP growth rate	1280	2.670	5.803	-32.935	44.281
Dependency ratio	1332	64.997	16.581	38.100	106.900
Government stability	1118	8.008	2.003	1.000	12.000
Government Fractionalisation	1197	0.224	0.273	0.000	0.893
Inflation	1279	55.109	340.794	-11.686	7481.664
Number of FR	1332	0.191	0.543	0.000	3.000
Time since FR adoption	1332	0.748	2.894	0.000	28.000
Real per capita GDP	1292	7223.490	4777.399	955.786	26306.430
Quality of the bureaucracy	1118	2.023	0.845	0	4
Trade Openness	1292	80.787	41.214	10.094	222.288
International Official Reserves to GDP	1254	15.047	14.009	0	114.448

Appendix 9: Sources and definitions of data

Variables	Definition	Sources
Fiscal Rule (*FR*)	Dummy Variable taking the value 1 if in a given year a country has in place, a national numerical constraint on government buget aggregates.	Fiscal Rules Database by the IMF's Fiscal Affairs Department, Fiscal Policy and Surveillance Division (2009), available at: www.imf.org/external/np/pp/eng/2009/121609.pdf
Budget Balance Rule (*BBR*)	Dummy Variable taking the value 1 if in a given year a country has in place, a national numerical constraint on government Fiscal Balance.	
Expenditure Rule (*ER*)	Dummy Variable taking the value 1 if in a given year a country has in place, a national numerical constraint on government Expenditure.	
Revenue Rule (*RR*)	Dummy Variable taking the value 1 if in a given year a country has in place, a national numerical constraint on government Revenue.	
Debt Rule (*DR*)	Dummy Variable taking the value 1 if in a given year a country has in place, a national numerical constraint on government Debt.	
Supranational Dummy	Dummy Variable taking the value 1 if in a given year a country has in place, a supranational numerical constraint on government fiscal aggregates.	
Structural Primary Fiscal Balance	Difference between General Government revenue and expenditure (excluding interest payments), adjusted for business fluctuations, as GDP percentage.	World Economic Outlook (WEO, 2010) and Own calculations
Structural Primary Expenditure	General government expenditure excluding interest payments, adjusted for business fluctuations, as GDP percentage.	
Government Debt	Gross General government debt, as GDP percentage	Ali Abbas et al. (2010)
Real per capita GDP growth rate	Annual growth rate of real output per capita	Penn World Table (PWT6.2)
Trade Openness	Sum of imports and exports divided by GDP	
Real per capita GDP	GDP per capita at constant prices.	
Inflation	Annual growth rate of average CPI	World Economic Outlook (WEO, 2010)
Government stability	Index ranging from 0 to 12 and measuring the ability of government to stay in office and to carry out its declared program(s).The higher the index, the more stable the government is.	International Country Risk Guide (ICRG, 2009)
Quality of the bureaucracy	Index ranging from 0 to 4 and measuring the institutional strength and expertise that the bureaucracy has to govern without drastic changes in policy or interruptions in government services.	
Government Fractionalisation	Index measuring the Probability that two deputies picked at random among from the government parties will be of different parties.	World Bank Database of Political Institutions (2010)
Dependency Ratio	Ratio of dependents (people younger than 15 or older than 64) to working-age population (those ages 15-64)	World Development Indicators (WDI, 2010)
Total Official reserves	Total reserves comprise holdings of monetary gold, special drawing rights, reserves of IMF members held by the IMF, and holdings of foreign exchange under the control of monetary authorities.	

Appendix 10: Strength of the instruments in the 2SLS estimations of the CAPB (key test statistics)

Country	1st step F-Statistic	Shea's Partial R^2	Country	1st step F-Statistic	Shea's Partial R^2
Albania	4,026	20,609	Jamaica	16,110	38,874
Algeria	13,495	40,149	Jordan	7,185	96,424
Angola	29,271	66,217	Kazakhstan	6,949	78,133
Argentina	19,860	39,684	Kenya	30,856	55,671
Azerbaijan	15,210	47,783	Korea, South	4,845	20,089
Bahrain	12,149	12,679	Latvia	54,175	85,632
Bangladesh	17,237	33,684	Lesotho	8,621	20,203
Belarus	14,547	68,915	Lithuania	15,126	39,706
Bolivia	38,734	58,501	Malaysia	25,590	42,945
Botswana	34,775	58,553	Mauritius	16,772	34,126
Brazil	14,882	32,015	Mexico	14,064	23,990
Bulgaria	30,449	68,911	Mongolia	21,202	51,623
Cameroon	67,185	73,437	Morocco	5,481	8,164
Cape Verde	5,934	2,560	Mozambique	5,291	8,672
Chad	13,706	27,429	Nigeria	19,133	34,812
Chile	11,241	24,787	Pakistan	24,702	27,497
China	29,767	87,034	Panama	14,167	32,855
Colombia	46,529	58,981	Paraguay	4,440	88,059
Congo	18,401	1,795	Peru	25,213	59,021
Costa Rica	12,219	49,645	Philippines	4,010	29,161
Cote d'Ivoire	38,633	53,911	Poland	6,978	29,281
Croatia	42,304	82,674	Romania	6,466	20,803
Czech Republic	14,792	62,529	Russia	33,342	41,799
Dominican Republic	14,208	48,548	Serbia	4,305	14,683
Ecuador	10,121	53,928	Slovakia	12,194	55,197
Egypt	16,883	28,922	South Africa	25,328	69,019
El Salvador	15,579	49,139	Sri Lanka	23,466	44,097
Estonia	18,083	74,633	Sudan	6,305	12,588
Fiji	4,012	2,602	Swaziland	14,378	29,410
Gabon	6,688	9,525	Syria	4,977	10,862
Georgia	8,767	41,462	Thailand	17,639	40,194
Guatemala	7,506	25,355	Trinidad & Tobago	6,130	35,457
Hungary	85,210	45,385	Tunisia	6,358	3,690
India	20,087	51,373	Turkey	7,325	23,807
Indonesia	338,076	98,888	Ukraine	97,600	98,079
Iran	17,732	41,941	Uruguay	23,414	47,324
Israel	4,005	53,022	Venezuela	10,001	35,960