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Evaluating the Impact of IMF Programs:
A Comparison of Matching and Instrumental-Variable Estimators

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Abstract

We examine the impact of IMF programs on economic performance in 95 developing countries over the period 1993-2002. Three macroeconomic measures of economic performance are considered: the real per capita economic growth rate, the ratio of the fiscal surplus to GDP, and the ratio of the current account surplus to GDP. Three estimation techniques are used: censored-sample, full-sample instrumental-variable, and matching.

Substantively, we find little statistical support that IMF programs contemporaneously improve real economic growth in participating countries, but stronger evidence of an improvement in economic growth in years following a program. We find that both the fiscal ratio and the current-account ratio improve contemporaneously with IMF participation relative to the counterfactual, with effects in succeeding years differing little from the impact effects.

We conclude that the program-effect estimates of matching and other estimators will differ largely because of the sample included in estimation. Matching by its nature excludes country episodes associated with extreme values of the propensity score, while the instrumental-variable estimator includes those. If there is heterogeneity of performance response in extreme vs. moderate cases, the estimates differ systematically between the two techniques.

JEL classification: F33, F34, C34

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1. Introduction

Participation in IMF programs is more popular in recent years than at any other time in history, if the percentage of members participating is a guide. While only 5 percent of members participated in programs in 1976, and 29 percent participated in 1985, in 2004 nearly 45 percent of a larger member pool was participating in IMF programs.[1]

At the same time that participation in IMF programs has greatly expanded, there has been a vigorous debate about the efficacy of such participation in achieving desired economic outcomes. The debate over economic-growth effects of such programs is illustrative: while some researchers (e.g., Khan (1990), Conway (1994), Przeworski and Vreeland (2000), Vreeland (2004)) have concluded that participation in IMF programs singnificantly reduces economic growth in the short term, other researchers (e.g., Dicks-Mireaux et al. (2000)) have found strongly positive economic-growth effects. Hardoy (2003) and Hutchison (2004), using a matching technique, conclude there are no significant effects on economic growth. Ul Haque and Khan (1998) provides an exhaustive summary of estimates of IMF impact for this and other performance variables for the period up to 1998, and Vreeland (2004) reports a summary of results from more recent research.

It is not surprising when different researchers reach different empirical conclusions, but in this case the policy question – the effectiveness of IMF programs in stimulating economic growth – is of critical importance to developing countries. There are three possible sources for this divergence of results. First, the researchers may have considered different time periods – IMF programs in the 1970s may have been quite different in economic-growth impact than programs in the 1990s. Second, the researchers may have investigated different types of IMF programs: the impact of stand-by arrangements, for example, could be quite different from structural adjustment facilities. Third, the researchers may have used different econometric techniques to reach their conclusions

In this paper, we examine the participation and not-participation of 95 countries in a recent period of IMF programs: 1993 to 2002. We consider together all types of programs: Stand-by Arrangements, Extended Fund Facilities, Structural and Enhanced Structural Adjustment Facilities, and Poverty Reduction and Growth Facilities. Given this common time period and set of programs, we investigate the degree to which different econometric techniques will yield different conclusions on IMF effectiveness. The policy evaluation parameter of interest is the average treatment effect, as this has been the parameter estimated in each of the preceding analyses.[2]

We consider the average treatment effects on three key variables in IMF deliberations: the real per capita economic growth rate, the ratio of the fiscal surplus to GDP, and the ratio of the current account surplus to GDP. We employ three different approaches to

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[1] These figures are drawn from Annual Reports of the IMF for these years. This 45 percent is not in fact the peak – during 1995-1997, over 50 percent of members were participating in programs.

[2] Heckman and Vytlacil (2005) provides a useful overview of the various alternative policy evaluation parameters.

evaluation of the participation effect of IMF programs: censored-sample, instrumental-variable, and matching.

While there are several approaches to estimating these "participation effects", each represents an attempt to create a counterfactual. For each country participating in an IMF program, we would like to compare the outcome after participation with the outcome for the same country had it not participated. As this is not observable, we resort to econometric methods to approximate this. Such approximation is complicated by the fact that economic performance also influences the decision to participate in IMF programs. This selection bias implies that for any two countries, if one is a participant and the other a not-participant, there will likely be a systematic difference in initial economic conditions. If this difference between participant and not-participant is not accounted for in estimation the resulting estimates will confound the effect of the IMF program with the initial imbalance.

We find that the three estimators give similar aggregate estimates for the impact of IMF participation. All techniques indicate that IMF participation improves the two macroeconomic measures, the fiscal surplus and the current account surplus ratios, relative to not participating. We also find that there are no significant contemporaneous effects of IMF participation on economic growth. In our analysis, we extend existing uses of the matching technique by controlling for systematic deviations in exogenous variables even after performing the matching exercise.

We conclude based on our empirical results that the program-effect estimates of the different estimators will differ largely because of the sample included in estimation. Matching by its nature excludes "extreme" country episodes (with the term defined precisely in what follows), while the instrumental-variable estimator includes those. If there is heterogeneity of performance response in extreme vs. moderate cases, the estimates will differ systematically between the two techniques.

2. Selection bias in theory.

The problem of estimating average treatment effects (here called participation effects) in the presence of selection bias has been studied carefully since Heckman (1979) in the applied econometric literature: Madalla (1983) and Heckman and Vytlacil (2005) provide an earlier and more recent summary of techniques, respectively. Annex A provides a derivation of the potential biases associated with creation of a counterfactual.

Authors in the existing literature have taken three alternative routes to estimating the participation effect. All involve initial calculation of an estimate $p(Z_c)$ of the propensity score.

- The first undertakes separate estimations of the structural macroeconomic model for the participant and not-participant samples. The estimate $p(Z_c)$ is used to create inverse Mills ratios appropriate to the two samples. These are included as separate regressors in the appropriate estimation equation to control for selection bias. The difference in predicted values from the two regressions (excluding the terms in inverse Mills ratios) is the participation effect. Przeworski and Vreeland (2000) and Vreeland (2004) use this approach.

- The second estimates the structural model over the entire sample. In analogous fashion to two-stage least squares, the $p(Z_c)$ is found and selection corrections are derived. Then the structural model is estimated with selection correction variables and with $p(Z_c)$ in place of the participation variable. The coefficient on $p(Z_c)$ is then the estimate of the participation effect. Conway (1994) used this approach.

- The third is to match observations by their estimated values of $p(Z_c)$. If two observations have near-identical propensity scores but different participation decision, then they are matched and the difference in outcome calculated. Averaging over many such matches provides an estimator of the participation effect.[3] Hardoy (2003) and Hutchison (2004) use this approach.

Given the common derivation of the three approaches, it is curious that the results of research as reported in Conway (1994), Przeworski and Vreeland (2000), Hardoy (2003) and Hutchison (2004) are so different in conclusions drawn. In this paper we will apply the three methodologies to the same set of programs in a common time period to identify the crucial differences that the estimation technique brings to this exercise.

We argue that the differences observed are due to local differences in participation effects. Analogously to the difference between local average treatment effects and average treatment effects in Imbens and Angrist (1994), the participation effect differs systematically among countries with the propensity score. The matching approach, with its use only of that subset of observations with a common support (Rosenbaum and Rubin, 1983), will only match the estimates of local participation effects in that range. Local participation effects for more extreme values of propensity score will differ from these.

3. Empirical Framework

In this paper, we examine the participation effect of IMF-supported programs approved in the period 1993-2002. Our attention is focused on the impact of program participation on macroeconomic aggregates during the year immediately following the approval of the IMF program. Three complementary empirical strategies exploit data from 913 (country/year) periods characterized by 181 participation periods and 732 periods of non-participation.[4] For the purpose of consistency, we limit our sample of countries to the pool of transition and developing economies. The data on historical outcomes are drawn from the "World Economic Outlook" (WEO) database of the IMF. Information on the IMF program participation is obtained from the Annual Reports of the IMF. The data are

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[3] In the matching approach, the non-zero means due to censored sampling are assumed to be zero.

[4] Our definition of non-participation period implies that there was no IMF-supported program initiated in the country in question within the four-year period before the considered year. The proportion of participation to non-participation periods is less than the 45 percent noted in the introduction because non-participation periods for these countries can be constructed from overlapping periods.

redefined in each case to be relative to the initial program year: denoted as "year T" of the program.[5] The year prior to "year T" is denoted as T-1. The horizon-T data are observed changes in macroeconomic aggregates from period T-1 to period T.

We focus our attention on three macroeconomic aggregates – specifically, on the real per capita economic growth rate, (g_{ct}), the ratios of fiscal balance to GDP (y_{ct}) and the ratio of current-account balance to GDP (c_{ct}). Each program is treated as an independent observation in what follows. However, it is important to note that the database includes multiple programs for many participating countries.

We assume that the underlying structural model of variables g_{ct}, y_{ct} and c_{ct} in the absence of IMF participation can be represented by a vector autoregression. With appropriate substitution, this vector autoregression can be rewritten in error-correction form as equations (1) through (3).[6]

$$\Delta g_{ct} = \Delta\gamma_{ct} = a_o + a_1\,\Delta g_{ct-1} + a_2\,\Delta y_{ct-1} + a_3\,\Delta c_{ct-1} + a_4\,\Delta g_{ct-2}\;\; a_5\,\Delta y_{ct-2} + a_6\,\Delta c_{ct-2} + a_7\,g_{ct-1} + a_8\,y_{ct-1} + a_9\,c_{ct-1} + \varepsilon_{yct} \quad (1)$$

$$\Delta y_{ct} = \Delta\psi_{ct} = b_o + b_1\,\Delta g_{ct-1} + b_2\,\Delta y_{ct-1} + b_3\,\Delta c_{ct-1} + b_4\,\Delta g_{ct-2} + b_5\Delta y_{ct-2} + b_6\,\Delta c_{ct-2} + b_7\,g_{ct-1} + b_8\,y_{ct-1} + b_9\,c_{ct-1} + \varepsilon_{yct} \quad (2)$$

$$\Delta c_{ct} = \Delta\omega_{ct} = c_o + c_1\Delta g_{ct-1} + c_2\Delta y_{ct-1} + c_3\,\Delta c_{ct-1} + c_4\Delta g_{ct-2} + c_5\Delta y_{ct-2} + c_6\,\Delta c_{ct-2} + c_7 g_{ct-1} + c_8\,y_{ct-1} + c_9\,c_{ct-1} + \varepsilon_{cct} \quad (3)$$

The econometric effects modeled here can be divided into two groups. The first group, represented by the terms in $\Delta g_{ct-k,}$ Δc_{ct-k} and Δy_{ct-k} for k=1,2, captures the autoregressive structure of the system. The second group, represented by the terms in g_{ct-1}, y_{ct-1} and c_{ct-1}, captures the adjustment of these variables in response to deviations from the "normal" relationship between the two macro aggregates. These are the error-correction effects.

A. The Censored-Sample Approach.

In the censored-sample approach, the complete sample is split into two: one with $D_{ct} = 1$, and the second with $D_{ct} = 0$. The equations are estimated for each sub-sample, while including a variable representing the selection correction depending on the sub-sample. The difference in coefficients from one sub-sample to the other is the participation effect.

For comparability with the other techniques, we modify this approach by estimating participation effect using the complete sample, while including a dummy variable for

[5] The "year T" of each program is defined by IMF staff to be that fiscal year (as defined by the country) in which the program is approved. Programs are typically not approved at the beginning of year T, but rather at some point within the year.

[6] We refer to the "error-correction form" as one that includes both lagged differences and lagged levels of the two variables as explanatory variables for the current differenced variables. Details on derivation of this reduced form representation from a general AR specification of the two variables and specification tests for the appropriate lag length are discussed in Atoian, Conway, Selowsky, and Tsikata (2004).

participating observations ($D_{ct} = 1$). The variable λ_{ct} is the inverse Mills ratio to control for selection. This strategy allows us to restrict all coefficients but the intercepts to be equal across sub-samples.[7] The coefficients a_{11}, b_{11} and c_{11} are estimates of the participation effect in the three variables.

$$\Delta g_{ct} = \Delta\gamma_{ct} + a_{10}\,\lambda_{ct} + a_{11}\,D_{ct} + \varepsilon_{yct} \tag{4}$$

$$\Delta y_{ct} = \Delta\psi_{ct} + b_{10}\,\lambda_{ct} + b_{11}\,D_{ct} + \varepsilon_{yct} \tag{5}$$

$$\Delta c_{ct} = \Delta\omega_{ct} + c_{10}\,\lambda_{ct} + c_{11}\,D_{ct} + \varepsilon_{cct} \tag{6}$$

B. The Complete-Sample Instrumental-Variable (IV) approach.

With appropriate substitution, the system (1) through (3) can be rewritten:

$$\Delta g_{ct} = \Delta\gamma_{ct} + a_{20}\,\lambda_{ct} + a_{21}\,p_{ct} + \varepsilon_{yct} \tag{7}$$

$$\Delta y_{ct} = \Delta\psi_{ct} + b_{20}\,\lambda_{ct} + b_{21}\,p_{ct} + \varepsilon_{yct} \tag{8}$$

$$\Delta c_{ct} = \Delta\omega_{ct} + c_{20}\,\lambda_{ct} + c_{21}\,p_{ct} + \varepsilon_{cct} \tag{9}$$

The λ_{ct} is once again the selection adjustment, and p_{ct} is the estimated propensity score for country c in year t. The IV technique replaces the actual decision to participate with its predicted value (the propensity score) from the first-stage probit analysis over the entire sample. Our instruments in that first stage include lagged first and second differences of the variables and their levels in the year prior to the considered period. These terms will capture the impact of the variability in a country's macroeconomic conditions on its probability to participate in an IMF-supported program. Additionally, our instruments include a variable that describes the nature of the relationship between member countries and the IMF - the cumulative time spent in an IMF-supported program (denoted as $last10yr_{ct}$). This variable is constructed as the number of years a country has spent in the program mode within the last ten years prior to the year considered. This is believed to be related to the probability of program initiation since it captures the extent of a country's financial involvement with the IMF. However, it is assumed not to affect systematically a country's economic performance in the considered period.[8]

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[7] Przeworski and Vreeland (2000) use a slightly different methodology. They estimate the regressions of the two samples without constraining the coefficients, and then calculate the average difference between the predicted values from the two equations for each country in each period. They find, however, that the preponderance of difference is found in the intercept term. Our approach captures that effect.

[8] While this is a common assumption, it is not necessarily a good one. Recent work on duration and recidivism among participants in IMF programs indicates a relatively long-lasting effect of participation on economic performance. See Conway (2005) for a more detailed discussion. We plan to investigate the importance of these long-term effects in future research.

C. The Matching approach.

The matching approach to treatment effect identification uses estimated values p_{ct} of the propensity score to generate matches between observations drawn from pools of participating and non-participating periods. Since formed matches are conditioned on (approximately) the same propensity scores, this technique controls for the selection in observables and the estimate of the treatment effect is independent of the participation decision.[9]

We construct the matches with a methodology that is optimal in the sense that the not-participant observation of country c in period s is selected as a match for a participant observation of country i in period t only if it is the closest to this particular participant in terms of the absolute distance between their propensity scores, subject to the goal of minimizing the sum of all distances (DIS_{ic}) over all possible sets of matches. Moreover, the match is only made if the absolute distance in propensity score is less or equal to the chosen tolerance level (δ):[10]

$$\min DIS_{ic} = \Sigma\left(\mid p_{it}^{p} - p_{cs}^{np} \mid \right) \quad \text{s.t.} \;\; DIS_{ic} \leq \delta \;\; \forall \; i,c \tag{10}$$

The choice of tolerance level poses an obvious trade-off. While trying to maximize exact matches, many participating observations may be excluded due to incomplete matching. On the other hand, if a researcher tries to maximize the number of participants, inexact matching may result. Results discussed in this section are obtained with δ=0.025, which implies that a match would only be made if the propensity score of a not-participant was within a 2.5 percentage-point neighborhood from the participant in question. To maximize the number of matches we allow up to five not-participant matches for each participant.

Once the match is made, we consider two estimators of the impact of IMF programs. The first is the mean difference in the endogenous variable g_{ct}, y_{ct} or c_{ct} between participant and not-participant observations. Since allowing for multiple matches may bias the estimate of the participation effect due to "double-counting", we first create an average across all the matches for a single participant and then we use this average to calculate the difference.

Even after controlling for potential selection bias, however, comparisons of macroeconomic performance using simple averages of differences in matched observations can be misleading since they mask a great deal of the variability in the data. Each country starts from different initial economic conditions and is subject to a variety of external and internal shocks that influence macroeconomic outcomes. Thus, a second econometric methodology controls for these effects. We address this issue by exploiting the error-correction form of the previous section.

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[9] Matching by propensity score is the traditional approach, but is not the only one. Augurzky and Kluve (2004) provides a useful summary and comparison of propensity score, index score and Mahalanobis metric matching.

[10] The matching exercise is carried out using user-written SAS macros available for download from the Mayo Foundation for Medical Education and Research web page.

$$
\begin{aligned}
(\Delta g_{it}^{p} - \Delta g_{cs}^{np}) = {} & d_o + d_1 (\Delta y_{it-1}^{p} - \Delta y_{cs-1}^{np}) + d_2 (\Delta g_{it-1}^{p} - \Delta g_{cs-1}^{np}) + d_3 (\Delta c_{it-1}^{p} - \Delta c_{cs-1}^{np}) \\
& + d_4 (\Delta y_{it-2}^{p} - \Delta y_{cs-2}^{np}) + d_5 (\Delta g_{it-2}^{p} - \Delta g_{cs-2}^{np}) + d_6 (\Delta c_{it-2}^{p} - \Delta c_{cs-2}^{np}) \\
& + d_7 (y_{it-1}^{p} - y_{cs-1}^{np}) + d_8 (c_{it-1}^{p} - c_{cs-1}^{np}) + d_9 (g_{it-1}^{p} - g_{cs-1}^{np}) + \varepsilon_{gm} \qquad (11)
\end{aligned}
$$

$$
\begin{aligned}
(\Delta y_{it}^{p} - \Delta y_{cs}^{np}) = {} & e_o + e_1 (\Delta y_{it-1}^{p} - \Delta y_{cs-1}^{np}) + e_2 (\Delta g_{it-1}^{p} - \Delta g_{cs-1}^{np}) + e_3 (\Delta c_{it-1}^{p} - \Delta c_{cs-1}^{np}) \\
& + e_4 (\Delta y_{it-2}^{p} - \Delta y_{cs-2}^{np}) + e_5 (\Delta g_{it-2}^{p} - \Delta g_{cs-2}^{np}) + e_6 (\Delta c_{it-2}^{p} - \Delta c_{cs-2}^{np}) \\
& + e_7 (y_{it-1}^{p} - y_{cs-1}^{np}) + e_8 (c_{it-1}^{p} - c_{cs-1}^{np}) + e_9 (g_{it-1}^{p} - g_{cs-1}^{np}) + \varepsilon_{ym} \qquad (12)
\end{aligned}
$$

$$
\begin{aligned}
(\Delta c_{it}^{p} - \Delta c_{cs}^{np}) = {} & f_o + f_1 (\Delta y_{it-1}^{p} - \Delta y_{cs-1}^{np}) + f_2 (\Delta g_{it-1}^{p} - \Delta g_{cs-1}^{np}) + f_3 (\Delta c_{it-1}^{p} - \Delta c_{cs-1}^{np}) + \\
& f_4 (\Delta y_{it-2}^{p} - \Delta y_{cs-2}^{np}) + f_5 (\Delta g_{it-2}^{p} - \Delta g_{cs-2}^{np}) + f_6 (\Delta c_{it-2}^{p} - \Delta c_{cs-2}^{np}) + \\
& f_7 (y_{it-1}^{p} - y_{cs-1}^{np}) + f_8 (c_{it-1}^{p} - c_{cs-1}^{np}) + f_9 (g_{it-1}^{p} - g_{cs-1}^{np}) + \varepsilon_{cm} \qquad (13)
\end{aligned}
$$

With this specification, the treatment effect of IMF program participation will be captured by the intercept coefficients.

4. Results.

We begin our results with attention to estimation of the propensity score, and then turn to the three macroeconomic variables.

Propensity score.

Table 1 presents our estimation results for the propensity score. Estimated propensity scores indicate that the decision to participate in an IMF-supported program is significantly influenced by the macroeconomic state of the economy. Past economic growth proves to be a significant predictor of participation: the past level takes a coefficient with negative sign, as expected, while the past difference in growth rates enters significantly with a positive sign. The coefficient on the level of the external balance in the previous year is statistically significant and of the expected sign. A strong predictor of the decision to participate in a program this year appears to be the number of years of the last ten that the country spent in IMF programs. The positive sign on the coefficient implies that countries with a prolonged history of IMF involvement are substantially more likely to participate in a new program.

The last row of Table 1 and the contingency table reported in Table 2 indicates the numeric breakdown of model-predicted participation versus actual participation.[11] Overall, our model is capable of predicting approximately 88 percent of the actual participation choices with better success rates observed for not-participants.

The distribution of propensity scores differs significantly by decision to participate. The two panels in Figure 1 illustrate this difference. As might be expected, the distribution of the propensity scores for non-participating observations is heavily skewed toward low values of the propensity score: of 732 observations for non-participating countries, 567

[11] A country is predicted to participate in a given year if the estimated propensity score for that year is greater than 0.325. This cut-off value reproduces the number of participants observed in the sample.

have $p_{ct} < 0.10$ and 76 of these have $p_{ct} < 0.01$. Only 28 of non-participating observations have $p_{ct} > 0.50$. By contrast, the distribution characterized by participation does not exhibit a heavily pronounced skew toward high propensity scores. Out of 181 observations, 82 have $p_{ct} < 0.50$ and only 31 observations have $p_{ct} > 0.90$. As is evident from this breakdown, there is likely to be a certain degree of heterogeneity in the measured impact of participation according to propensity scores in the data. We will address this issue shortly by creating sub-samples from the whole based upon propensity score.

Effect of IMF Programs on Economic Growth.

Does participation in IMF programs have an independent effect on real economic growth per capita? We investigate this using the abovementioned data sample and the three estimators outlined above. The estimation results are reported in Tables 3, 4 and 5.

Table 3 reports the results from the censored-sample and full-sample IV estimators. As comparison of equations (4) and (7) illustrate, these estimators differ only in their correction for the simultaneous determination of D_{ct}. While the censored-sample approach controls for the non-zero mean of the censored sample, it does not address the simultaneity of D_{ct}. The complete-sample IV approach includes adjustments for both biases.[12]

The two estimation techniques return almost identical estimates of the components of $\Delta\gamma_{ct}$. The underlying economic-growth model is characterized by an error-correction structure: the coefficients on the lagged differences in economic growth are negative, as expected, and at the first lag the coefficient is significantly different from zero. The coefficient on the lagged economic growth rate is negative and significant. With the value of -0.693, it indicates that these economies adjust rapidly to re-attain long-run growth paths after an economic shock. The lagged policy ratios also play a significant role in modeling the economic-growth path. Increases in the fiscal ratio (i.e., greater surplus) are associated with increased economic growth in the next period, while increases in the current-account ratio (i.e., a greater surplus) are associated with declines in the change in economic-growth rates.

In the censored-sample approach, the impact effect of IMF programs on economic growth is derived as the change in the intercept derived for participation relative to not-participation. Its coefficient is 0.29, positive but insignificantly different from zero. The selection correction is denoted λ_{ct}. Its coefficient is negative, as expected, but is also insignificantly different from zero. The regression explains nearly 53 percent of variation in the yearly increase in economic growth rates.

For the full-sample IV approach, the estimated impact of participation in IMF-supported programs is once again insignificantly different from zero, but this time (-0.029) with

[12] These adjustments may not be complete. As a referee points out, it is reasonable to conjecture that the current fiscal ratio depends on a longer autoregressive structure than we are able to use, and that these longer lags are correlated with the "last 10 years" variable. If so, selection bias may still remain in the coefficient estimate.

negative sign. The coefficient on selection correction is once again negative in sign but insignificantly different from zero. The explanatory power of the equation is nearly identical to that of the censored-sample approach.

To evaluate the effect of IMF program participation using the matching approach we first look at the mean of the per-pair differences in the generated sub-sample. As it was described above, the multiple matches for a single participant were averaged before the difference was created. Table 4 reports descriptive statistics for computed differences.[13] While the mean and median values of this matching statistic indicate a small negative effect of IMF participation, it is evident from the standard deviation, and from the quantiles listed in the bottom of the table, that there is a great deal of dispersion around those central moments.

Given our concern that the matching technique ignores information about systematic variation in pre-determined variables and other country-specific factors, we re-consider the matching results through the lens of the error-correction model in equation (11). The results from that estimation are reported in Table 5, and illustrate the importance of differing systematic variation in pre-determined variables to the economic-growth outcomes observed. The error-correction structure of economic-growth determination that was evident in the regressions of Table 3 is evident here as well. The lagged changes in fiscal ratio and current-account ratio are also significant determinants, indicating that these systematic variation in pre-determined variables are important in explaining the differences in economic growth between matched country-year observations. Once this systematic variation has been accounted for, the independent effect of IMF participation from this matching sample is 0.209, positive but insignificantly different from zero.

Note the small sample used in the matching exercise. There are 76 participants, matched with 126 observations from not-participating countries. This represents the exclusion of a large number of observations that did not satisfy the requirements for matching. Table 6 reports the characteristics of these excluded observations. As expected, most of unmatched participants exhibit high levels of propensity scores while most of unmatched not-participants are characterized by extremely low values of this statistics. Another interesting feature is that unmatched observations drawn from the pool of IMF program participants are characterized by (on average) substantially higher levels of g_{ct}, y_{ct} and c_{ct} when compared with unmatched not-participants.[14]

For this sample, then, the three techniques lead to uniformly insignificant but differently signed coefficients for the impact of contemporaneous participation in IMF programs.

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[13] Our matching methodology is capable of finding at least one not-participant match for 76 out of 181 participating country/years. This, allowing for multiple matches, generates the 202 matched country-year pairs reported in Annex B. While for multiple matches the goodness of the match declines with the match's order, the overall quality is rather good: the mean absolute distances between the two propensity scores are 0.2, 0.5, 0.7, 1.0, and 1.2 percentage points for the first through the fifth matches respectively.

[14] Dehejia and Wahba (2002), in Proposition A (p. 19), point out that for propensity-score matching to be a valid estimation method the distribution of covariates should be similar for the same value of propensity score. This result suggests that the matching algorithm will not be unbiased here.

Effects of IMF programs on fiscal and current-account ratios. Recent analyses of IMF programs have examined the impact of participation on economic growth without considering the policy channels through which this impact might come about.[15] In the results of the previous section, we could conclude either that (a) the participating countries did not change behavior or (b) the participating countries did change behavior (in line with, for example, the conditionality of the program) but the changed behavior did not translate into improved economic growth. In this section we investigate whether two policy ratios -- the fiscal ratio (the fiscal surplus/GDP) and the current-account ratio (current account surplus/GDP) – are significantly affected by participation in IMF programs.

Tables 7 and 8 report the results of censored-sample and full-sample IV estimation of the impact of participation in IMF programs on the policy ratios. Once again, the underlying error-correction models $\Delta\psi_{ct}$ and $\Delta\omega_{ct}$ are precisely estimated and differ little by estimation technique. The orthodox explanation of IMF stabilization programs (as explained in e.g., Khan (1990)) is that participation will lead to an improvement in fiscal surplus as a share of GDP. The estimated coefficients in Table 7 illustrate that the fiscal ratio in this period followed an error-correction process in its own values, with significant lagged changes in the own ratio and a significant negative error-correction effect as coefficient to the lagged ratio. It is also evident that the fiscal and current-account ratios evolve in concert: positive movements in the lagged changes in current-account ratios lead to positive movements in the fiscal ratio.[16] The estimated coefficients for the current-account ratio are reported in Table 8. Unsurprisingly, given the results already reported, the current-account ratio is represented well by an error-correction model. Lagged changes and the lagged ratio all enter significantly, and with negative sign, as expected. Lagged positive changes to the economic growth rate have a negative and significant impact on Δc_{ct}, as expected. Lagged positive changes to the fiscal ratio have a positive impact.

Table 7 offers two estimates of the impact of IMF participation on the fiscal ratio. The censored-sample approach reports a positive and significant effect of 1.167. The full-sample IV estimator reports the significant and almost identical coefficient of 1.282. The correction for selection bias in both cases is also negative, as expected, but insignificantly different from zero. In Table 8, participation in IMF programs has a positive impact on the current account ratio in both techniques. This effect is similar in the two approaches, (0.98 for censored-sample, and 1.49 for full sample IV) but in each case is not significantly different from zero. The estimation techniques explain about 22 percent of the total variation in Δy_{ct} and 18 percent of the total variation in Δc_{ct}.

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[15] A recent exception is Dreher (2004). This examines the impact of participation on the fiscal ratio and the implementation of monetary policy. For the period in question here, monetary policy is not significantly affected by IMF participation while the fiscal ratio is significantly improved.

[16] This result is consistent with a simple flow-of-funds explanation of macro balances. An increase in the current-account ratio indicates less foreign saving available to the economy. As foreign saving is reduced, the economy must increase domestic saving. For given private saving, then, government saving must increase.

For comparison to these results, we report the results of an exercise using the sample of 202 pairs based upon propensity-score matching. Table 9 reports the results of simple matching (i.e., without controlling for systematic variation in pre-determined variables). Both the fiscal and current-account ratios are improved on average by participation in IMF programs. The range of individual differences, as reported under the quantiles section of that table, is quite large, leaving the average estimate insignificantly different from zero.

In Table 10, we use the error-correction methodology to control for differences in systematic variation in pre-determined variables. This proves to be especially important in the case of the fiscal surplus, but also has a significant impact on the results in the current-account regression. In both cases, the estimated impact of participation in IMF programs is positive and significantly different from zero. Participation in IMF programs is associated with roughly one percentage-point increase in each of these ratios.

Summary.

For this sample of countries, and for this time period, we have found that participation in IMF programs is associated with a significant improvement in both fiscal and current-account ratios. While the effect is not significantly different from zero in every method, it is fairly uniform in quantitative terms – participation is associated with approximately one percent improvement in both ratios. The simple matching technique is quite different from the others in results because of its lack of correction for systematic differences in systematic variation in pre-determined variables across participant and not-participant countries.

These significant results on policy ratios do not translate into significant contemporaneous effects on real economic growth. In no instance is it significantly different from zero. In some estimation techniques it is negative in sign and in others it is positive in sign.

5. Checking for robustness of results.

In this section we examine whether the results for our matching exercise are robust to the choices used in the matching algorithm.

Table 11 summarizes our findings for choice of the tolerance level δ ranging from 0.001 to 0.03 (which results in the number of matches varying from 89 to 205). Smaller levels of δ maximize the quality of the match in terms of a country's propensity score, while larger tolerance levels maximize the number of matches. Regardless of the choice of δ, the regression-based participation effect fluctuates around 1 percentage point for fiscal and current-account ratios. The regression-based impact on economic growth is roughly 0.3 except for the smallest tolerance level; at that smallest tolerance level the simple and regression-based estimates become quite similar at about -0.8.

Table 12 reports the changing values of matching participation effects when the number of matches allowed is varied from 1 to 9.[17] In the case of economic growth, allowing multiple matches makes a large difference: while the regression-based participation effect is a positive and significant 1.15 for one match, it declines continuously as the number of matches rises and becomes the much smaller 0.11 for 9 allowable matches. The simple matching estimator also declines with the number of matches, from 0.35 for 1 match to -0.26 for 9 matches. The impact of multiple matches is much less on the participation effect calculated for the fiscal ratio. For the current-account ratio, there is a large difference between estimates for 1 match and estimates for multiple allowable matches.

While propensity-score matching has been the standard since the exposition of Rosenbaum and Rubin (1983), it is also possible to match observations on other proxies for participation. In Table 13 we report the results of matching undertaken by either lagged economic growth rate or lagged current-account ratio. It is evident that the economic-growth results remain as insignificant under this approach as under propensity-score matching, while participation effects on fiscal surplus and current-account surplus are evident under these matching schemes as well.

6. Heterogeneity in Treatment Effects

While the results of different approaches are similar, the differences in approaches yield striking insights into the requisite sample for evaluating projects.

The first difference is in sample selection.

- The IV estimator uses the complete sample of observations available, including 732 observations of non-participation and 181 observations of participation. The philosophy: each is an independent observation of the underlying performance process, and the participation effect will be the average increase (or reduction) in performance for participants relative to not-participants once other exogenous factors and non-zero means of distributions have been controlled for. The counterfactual used for each participant is a synthetic one: an average of not-participant behavior that controls for the same level of exogenous variables observed in the participating country/year.
- The matching estimator insists upon a specific non-participating counterfactual country/year for each participating country/year. The propensity score is used as a summary statistic to identify that specific counterfactual. The philosophy: an unbiased estimate of participation effect must compare two nearly identical country/years. The average of these unbiased estimates will be the average participation effect. The result, in our case, is that many country/years cannot be matched. Many country/years of non-participation are excluded, but also 105 of the 181 participating country/years are laid aside as well. The sample used in estimation is thus a subsample of the total – a subsample drawn largely from the middle of the distribution of observations in terms of propensity score.

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[17] The row corresponding to five allowable matches includes the matching coefficients reported in earlier tables.

The second difference pertains to systematic variation in pre-determined variables.

- The IV estimator controls for systematic variation in pre-determined variables and other performance-determining exogenous variables.
- The naïve matching estimator, as reported in Tables 4 and 9, controls for those exogenous variables only through the propensity score. As equations (8)-(10) indicate, this is not sufficient – the estimation technique should control as well for exogenous variables determining performance directly.
- The error-correction matching approach we suggest and implement in Tables 5 and 10 incorporates this systematic variation. It provides a more precise indicator of the independent effect of participation in the IMF program.

The third difference pertains to non-zero sample means.

- The IV estimator and censored-sample estimator control for non-zero sample means through inclusion of the inverse Mills ratio in estimation.
- The matching estimator assumes that such corrections are not necessary.

The difference in samples used points up an interesting possibility in measuring participation effects: that of country heterogeneity leading to significantly different local participation effects. Angrist (2003) provides a useful classification of the observations in such a selection decision. He distinguishes three groups (with the names adapted to this application): non-participants; potential participants; and certain participants. The non-participants are those country/years for which the exogenous determinants are very strongly in favor of not participating. The certain participants are those for which the exogenous determinants strongly indicate participation. The potential participants are those for whom exogenous determinants could support participation or not. Only the potential participants are at the margin in this decision.

Our assessments of the effect of IMF program participation discussed in the sections above should be interpreted as average participation effects since they measure the impact of IMF programs as observed on average over the entire sample. We can also calculate local participation effects that measure the impact of IMF participation as observed on average only for sub-groups of the total observations. Specifically, we define three large subgroups of the sample: all observations characterized by propensity scores below 0.13, those with propensity scores between 0.13 and 0.27, and those with propensity scores above 0.27.[18] We'll refer to the first group as the "strong economy" group, and the last group as the "weak economy" group. Table 14 summarizes our estimates of the treatment effect for these three sub-samples obtained using the four methodologies employed in our paper.

In the estimates for economic growth, the lack of a correction for selection bias is evident in the strongly negative coefficients for the strong economies. The participants among the strong economies will be those with negative economic shocks. Only the IV approach corrects for that simultaneity, and only in that case is the coefficient positive (though insignificant). Among the weak economies there is a common positive effect:

[18] We choose these cut-off points to divide the sample into roughly equal subsamples.

here, the negative shocks are less important in triggering participation, and thus the degree of bias observed is less. The common tendency in these data is for the weak economies to benefit more from IMF participation than do the strong economies, although these differences are not statistically significant.

When the policy ratios are considered, there is a similar selection bias. Strong economies participating in IMF programs have positive shocks to fiscal and current accounts, due perhaps to budget constraints that led to unexpected reductions in expenditure. This triggered participation, thus giving a positive coefficient. The IV approach yields a negative effect of IMF participation on both fiscal and current accounts as the country's budget constraint is released. Weak economies experience an improvement in fiscal and current-account ratios.

We pursue this heterogeneity further by separating the sample into two different groups. The first group includes all country/years used in the matching calculations, while the second group includes all observations not used in the matching algorithm. The censored-sample and full-sample IV estimates for the two groups are reported in Table 15. In economic growth, for example, the complete-sample IV estimate is negative (-0.814) for those observations used in the matching exercise while positive (0.746) for those observations not used in matching. The censored-sample estimates, by contrast, are almost identical across samples and similar to the matching estimators. In the IV estimators, there is evidence of a local participation effect that is of opposite sign at the two extremes from that observed in the common support region used in matching. Results for the policy ratios indicate less extreme local participation effect differences in the fiscal ratio, but large differences in the local participation effects with the current-account ratio.

7. Different Time Horizons

To this point, we have focused our attention on the participation effect of IMF programs in the first year following the initiation of an IMF program (horizon T). However, the macroeconomic impact of IMF programs does not have to be limited to only one year. Conway (1994), for example, argues that the participation effects should more properly be examined in a dynamic context. Moreover, given the substantial policy lags and institutional inertia, one could argue that our failure to find statistically significant effects of IMF programs on economic growth is related to limiting the analysis to excessively short time span. To pursue this point, we repeat our exercise, while evaluating the macroeconomic impact of IMF program participation for horizons T+1, T+2, and T+3 (two, three, and four years after program initiation respectively). Similar to our previous strategy, we redefine the data in each case to be expressed in term of changes from the year before the program (T-1) to the corresponding period (T+1, T+2, and T+3). For consistency, we limit our analysis in this section only to those country/years for which non-missing observations are available for all four considered time horizons.

Table 16 summarizes our findings for IMF program effects for all of the four considered time horizons. Three conclusions are noticeable from this exercise:

- There is evidence that IMF programs have an increasingly positive impact on real economic growth in participating countries as the time horizon grows longer. All employed approaches uniformly report shifting towards a positive treatment effect of program participation when longer time horizons are considered. Moreover, our estimates for horizons T+2 and T+3 obtained using matching methodology, are statistically significant when differences in systematic variation in pre-determined variables are taken into account.
- The participation effect for fiscal ratio exhibits no clear pattern when different time horizons are considered.
- The participation effect for the current-account ratio appears to be declining over time.

This suggests that the time horizon examined is a crucial component in discovering a significant effect of IMF programs on economic growth. While the policy ratios may adjust immediately, the impact on economic growth is observed with a lag.

8. Conclusions

Matching and estimation provide two alternative methods for identifying the effect of IMF programs on participating countries. These generate the same theoretical prediction, and thus their different point estimates for the same sample are surprising. The reason for this divergence appears to be heterogeneity among country/years in the sample, and the different sampling strategies of the estimation and matching estimators.

In this paper we investigate these differences. We find the larger differences in local participation effects in the estimation of economic-growth effects, thus providing an explanation for the widely diverging estimates of average participation effects on real economic growth found in the literature. The discrepancies across techniques are smaller for participation effects on the fiscal ratio and the current-account ratio.

We demonstrate as well that one cause for the varying estimates is the restriction on the sample placed by the need to match participant and not-participant observations for the matching technique. This ensures that most observations with extreme values of the propensity score will be excluded. If program impact differs by propensity score, the matching technique will provide significantly different estimates from the complete-sample based estimates.

Why should the local participation effects differ by propensity score? One possible explanation is suggested by Garuda (2000) and Mody and Saravia (2003). In Mody and Saravia (2003), the effects of programs on participants differ by whether the country is in "crisis" or "non-crisis" economic straits. They model a non-linearity in response that they attribute to decision-making under crisis; this non-linearity may be evident in the sorting by propensity score. Garuda (2000) sorts by propensity score in a type of broad matching before estimating participation effects on income distribution; while he does not put forward a theoretical explanation, he does find significant differences across subgroups when sorted by propensity score. In both these cases, however, the authors do not correct for simultaneity bias: it is possible that the non-linearity that they derive is a manifestation of the simultaneity bias.

Should we prefer one method over the other when evaluating IMF programs? Matching has the advantage when our concern is with heterogeneity among countries, because the technique forces the investigator to consider "similar" countries. The definition of "similar" is a special one, however, as is clear in the matches reported in Annex B. The countries predicted to be most similar in decision to participate may differ substantially in their economic fundamentals: this is the rationale for the error-correction version of matching we propose and implement. The censored-sample and IV estimators have the advantage of including all available information distinguishing participants and not-participants. There is much to be learned from those countries unlikely to participate, just as there is from those countries almost surely to participate. These estimators also address explicitly the non-zero mean property associated with selection bias.

When will the censored-sample, full-sample IV and matching estimators yield exactly the same results? This should be the case when only one match is chosen per participating country/year, when the IV technique is used on that same sub-sample, and when the participants are chosen randomly from a single distribution of country/years. Our initial investigations (estimation results not reported, but available on request) support this conclusion, but in future work we plan to confirm this through a series of Monte Carlo experiments.

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Table 1: The Propensity Score Regression [1,2]

Variable	Estimate	Standard Error
Intercept	-2.395***	0.293
Δg_{ct-1}	0.039***	0.012
Δy_{ct-1}	0.014	0.015
Δc_{ct-1}	0.001	0.009
Δg_{ct-2}	0.016	0.011
Δy_{ct-2}	0.011	0.014
Δc_{ct-2}	0.001	0.007
g_{ct-1}	-0.052***	0.014
y_{ct-1}	0.006	0.012
c_{ct-1}	-0.019***	0.007
$last10yr_{ct}$	0.628***	0.040
Summary statistics:		
No. of observations	913	
Log-likelihood	-256.599	
Percent correctly predicted:		
Participation	70.72	
Non-participation	92.76	

Note: Here and in later tables, "*", "**", and "***" denote significance at 90, 95, and 99 percent confidence level respectively.

[1] Year dummy variables are included in all regressions.

[2] Results obtained using a probit model; parameter estimates are computed to reflect the propensity score of participation: P(Participation = 1) = F(X′β) where F is the normal cumulative distribution function. The last row reports percent correctly predicted for participation and non-participation periods if p=0.325 is the taken as the cutoff probability.

Table 2: Predicted Participation vs Actual Participation[1]

Actual Participation	Predicted Participation 0	Predicted Participation 1	Total
0	679 74.37 92.76 92.76	53 5.81 7.24 29.28	732 80.18
1	53 5.81 29.28 7.24	128 14.02 70.72 70.72	181 19.82
Total	732 80.18	181 19.82	913 100.00

[1] Each cell contains Frequency, Percent of Total, Row Percent, and Column Percent values. Chosen cut-off value of 0.325 reproduces number of participants observed in the sample.

Table 3: Measuring the Impact of IMF Programs on Economic Growth

Independent Variable	Censored-Sample Δg_{it}		Full-Sample IV Δg_{it}	
	Parameter Estimate	Standard Error[1]	Parameter Estimate	Standard Error[1]
Intercept	1.246***	0.342	1.228***	0.341
ΔIntercept (D=1)	0.291	0.533	---	---
Δy_{ct-1}	0.023	0.038	0.021	0.038
Δc_{ct-1}	0.028	0.022	0.028	0.022
Δg_{ct-1}	-0.191***	0.037	-0.190 ***	0.037
Δy_{ct-2}	0.042	0.033	0.041	0.033
Δc_{ct-2}	0.022	0.016	0.023	0.016
Δg_{ct-2}	-0.014	0.028	-0.013	0.028
y_{ct-1}	0.109***	0.029	0.110 ***	0.029
c_{ct-1}	-0.073***	0.016	-0.072***	0.016
g_{ct-1}	-0.693 ***	0.040	-0.694 ***	0.041
λ_{ct}	-0.894	1.955	-0.320	1.924
p_{ct}	---	---	-0.029	0.811
No. of observations	913		913	
R^2	0.529		0.529	

[1] Standard errors reported are consistent asymptotic standard errors of estimates. Greene (1981) provides technical details. Corrections were carried out using SAS macros developed by Sergiy Peredriy of SAS Institute, Cary.

Table 4: Simple Matching for Economic-Growth Effect (Δg_{it}^{p}-Δg_{cs}^{np})

Mean	Median	Std Dev
-0.107	-0.278	7.982

Quantiles (sorted by size of IMF effect)

Max 100%	99%	95%	90%	Q3 75%	Median 50%	Q1 25%	10%	5%	1%	Min 0%
24.98	24.98	13.45	9.57	4.86	-0.28	-4.79	-9.39	-10.48	-21.85	-21.85

Table 5: Matching for Economic Growth Effect, controlling for systematic variation in pre-determined variables

	Economic Growth (Δg_{it}^{p} - Δg_{cs}^{np})	
Independent Variable	**Parameter Estimate**	**Standard Error**
Participation Effect (intercept)	0.209	0.595
(Δg_{it-1}^{p} - Δg_{cs-1}^{np})	-0.248***	0.085
(Δy_{it-1}^{p} - Δy_{cs-1}^{np})	0.223**	0.107
(Δc_{it-1}^{p} - Δc_{cs-1}^{np})	-0.079	0.075
(Δg_{it-2}^{p} - Δg_{cs-2}^{np})	-0.066	0.077
(Δy_{it-2}^{p} - Δy_{cs-2}^{np})	0.286**	0.118
(Δc_{it-2}^{p} - Δc_{cs-2}^{np})	-0.117*	0.060
(g_{it-1}^{p} - g_{cs-1}^{np})	-0.803***	0.101
(y_{it-1}^{p} - y_{cs-1}^{np})	0.024	0.086
(c_{it-1}^{p} - c_{js-1}^{np})	0.003	0.057
No. of observations	202	
R^2	0.603	

Table 6: Summary Statistics for NON-matched Observations

	N	Mean	Std. Dev.	Minimum	Maximum
			IMF Program Participants		
p_{ct}	105	0.663	0.238	0.236	0.999
Δg_{ct}	105	1.625	6.688	-19.770	33.339
Δy_{ct}	105	1.106	3.545	-11.339	13.547
Δc_{ct}	105	1.246	5.235	-17.690	14.295
			IMF Program Non-Participants		
p_{ct}	530	0.037	0.024	0.000	0.104
Δg_{ct}	530	-0.694	6.805	-64.478	39.389
Δy_{ct}	530	0.245	6.410	-32.366	42.082
Δc_{ct}	530	0.055	9.038	-82.095	51.934

Table 7: Measuring the Impact of IMF Programs on the Fiscal Ratio

Independent Variable	Censored-Sample Δy_{it}		Full-Sample IV Δy_{it}	
	Parameter Estimate	Standard Error[1]	Parameter Estimate	Standard Error[1]
Intercept	-0.690**	0.310	-0.809***	0.310
ΔIntercept (D=1)	1.167**	0.483	---	---
Δy_{ct-1}	-0.109***	0.031	-0.111***	0.034
Δc_{ct-1}	0.059***	0.018	0.060***	0.019
Δg_{ct-1}	-0.006	0.033	-0.011	0.033
Δy_{ct-2}	-0.067**	0.030	-0.069**	0.030
Δc_{ct-2}	-0.005	0.014	-0.005	0.015
Δg_{ct-2}	0.025	0.025	0.023	0.025
y_{ct-1}	-0.276***	0.026	-0.277***	0.027
c_{ct-1}	-0.009	0.015	-0.007	0.015
g_{ct-1}	0.003	0.037	0.010	0.037
λ_{ct}	-2.311	1.772	-1.609	1.747
p_{ct}	---	---	1.282*	0.736
No. of observations	913		913	
R^2	0.223		0.221	

[1] Standard errors reported are consistent asymptotic standard errors of estimates. Greene (1981) provides technical details. Corrections were carried out using SAS macros developed by Sergiy Peredriy of SAS Institute, Cary.

Table 8: Measuring the Impact of IMF Programs on Current-account Ratio

Independent Variable	Censored-Sample Δc_{it}		Full-Sample IV Δc_{it}	
	Parameter Estimate	Standard Error[1]	Parameter Estimate	Standard Error[1]
Intercept	-0.587	0.460	-0.699	0.459
ΔIntercept (D=1)	0.976	0.717	---	---
Δy_{ct-1}	0.172 ***	0.051	0.172 ***	0.051
Δc_{ct-1}	-0.088 ***	0.029	-0.088 ***	0.029
Δg_{ct-1}	-0.166 ***	0.049	-0.173 ***	0.049
Δy_{ct-2}	0.042	0.044	0.042	0.044
Δc_{ct-2}	-0.122 ***	0.022	-0.122 ***	0.022
Δg_{ct-2}	-0.062 *	0.037	-0.065 *	0.037
y_{ct-1}	-0.052	0.039	-0.054	0.039
c_{ct-1}	-0.170 ***	0.022	-0.168 ***	0.022
g_{ct-1}	0.064	0.054	0.073	0.055
λ_{ct}	-1.879	2.629	-1.766	2.588
p_{ct}	---	---	1.486	1.091
No. of observations	913		913	
R^2	0.179		0.179	

[1] Standard errors reported are consistent asymptotic standard errors of estimates. Greene (1981) provides technical details. Corrections were carried out using SAS macros developed by Sergiy Peredriy of SAS Institute, Cary.

Table 9: Simple Matching

	Mean	Median	Std Dev
Δy_{it}^{p}-Δy_{cs}^{np}	0.654	0.022	4.539
Δc_{it}^{p}-Δc_{cs}^{np}	0.046	1.004	6.199

Quantiles

	Max 100%	99%	95%	90%	Q3 75%	Median 50%	Q1 25%	10%	5%	1%	Min 0%
Δy_{it}^{p}-Δy_{cs}^{np}	10.15	10.15	9.02	6.38	2.95	0.02	-1.38	-3.47	-8.65	-15.05	-15.05
Δc_{it}^{p}-Δc_{cs}^{np}	23.64	23.64	8.57	6.72	3.47	1.00	-3.58	-6.65	-9.32	-21.12	-21.12

Table 10: Matching while Controlling for Systematic variation in pre-determined variables

Dependent Variable:	**Fiscal Surplus (Δy_{it}^{p} - Δy_{cs}^{np})**		**Current Account Surplus (Δc_{it}^{p} - Δc_{cs}^{np})**	
Independent Variable	**Parameter Estimate**	**Standard Error**	**Parameter Estimate**	**Standard Error**
Participation Effect (intercept)	1.162***	0.316	1.017*	0.561
(Δg_{it-1}^{p} - Δg_{cs-1}^{np})	-0.026	0.045	-0.094	0.080
(Δy_{it-1}^{p} - Δy_{cs-1}^{np})	-0.134**	0.057	-0.015	0.101
(Δc_{it-1}^{p} - Δc_{cs-1}^{np})	-0.141***	0.040	-0.108	0.070
(Δg_{it-2}^{p} - Δg_{cs-2}^{np})	0.063	0.041	-0.069	0.072
(Δy_{it-2}^{p} - Δy_{cs-2}^{np})	0.069	0.062	-0.097	0.111
(Δc_{it-2}^{p} - Δc_{cs-2}^{np})	-0.032	0.032	0.061	0.056
(g_{it-1}^{p} - g_{cs-1}^{np})	0.033	0.054	0.073	0.096
(y_{it-1}^{p} - y_{cs-1}^{np})	-0.309***	0.045	0.194**	0.081
(c_{it-1}^{p} - c_{js-1}^{np})	0.053*	0.030	-0.248***	0.054
No. of observations	202		202	
R^2	0.374		0.202	

Table 11: Matching Approach: Robustness to the Choice of δ[1]

Variable:	Participation Effect						
	Economic Growth (Δg_{it}^{p} - Δg_{cs}^{np})		Fiscal Surplus (Δy_{it}^{p} - Δy_{cs}^{np})		Current Account Surplus (Δc_{it}^{p} - Δc_{cs}^{np})		No. of obs.
Tolerance Level	Differences in Means	Regression Based	Differences in Means	Regression Based	Differences in Means	Regression Based	
δ=0.030	-0.07	0.31	0.65	1.25***	-0.05	1.00*	205
δ=0.025	-0.11	0.21	0.65	1.16***	0.05	1.02*	202
δ=0.020	-0.12	0.25	0.66	1.17***	0.06	1.05*	200
δ=0.015	-0.09	0.28	0.65	1.19***	0.03	1.10*	198
δ=0.010	-0.13	0.37	0.64	1.22***	0.32	1.14**	189
δ=0.005	-0.01	0.29	0.58	1.16***	0.21	1.00*	178
δ=0.001	-0.79	-0.77	1.15	1.11**	1.10	0.84	89

[1] Reported estimates are obtained assuming that up to five non-participating matches are allowed for each participating observation.

Table 12: Matching Approach: Robustness to the Number of Multiple Matches Allowed [1]

Variable:	Participation Effects						No. of obs.
	Economic Growth (Δg_{it}^{p} - Δg_{cs}^{np})		Fiscal Surplus (Δy_{it}^{p} - Δy_{cs}^{np})		Current Account Surplus (Δc_{it}^{p} - Δc_{cs}^{np})		
# of Multiple Matches	Differ in Means	Regression Based	Differ in Means	Regression Based	Differ in Means	Regression Based	
1	0.35	1.15*	0.89	1.08**	0.09	0.55	104
2	-0.25	0.60	0.81	1.14***	0.12	0.76	131
3	-0.26	0.68	0.64	1.09***	0.09	0.89	156
5	-0.11	0.21	0.65	1.16***	0.05	1.02*	202
7	0.08	0.19	0.57	1.18***	-0.04	1.03**	241
9	-0.26	0.11	0.56	1.17***	-0.80	0.97**	267

Table 13: Matching Approach: Robustness to the Choice of Matching Criteria [1]

Variable:	Participation Effect: Economic Growth (Δg_{it}^{p} - Δg_{cs}^{np})		Fiscal Surplus (Δy_{it}^{p} - Δy_{cs}^{np})		Current Account Surplus (Δc_{it}^{p} - Δc_{cs}^{np})		No. of obs.
Tolerance Level [2]	Differences in Means	Regression Based	Differences in Means	Regression Based	Differences in Means	Regression Based	
Matching on the (T-1) level of the Current Account Ratio to GDP (c_{ct-1})							
δ=0.05	2.03	-0.41	0.99	0.99***	1.03	1.15***	318
δ=0.10	1.54	-0.14	0.77	0.92***	1.21	0.99***	413
δ=0.50	1.79	0.13	0.74	0.78***	0.65	0.73**	556
δ=1.00	1.81	0.07	0.62	0.65**	0.62	0.69*	593
Matching on the (T-1) level of the Economic Growth (g_{ct-1})							
δ=0.05	-0.17	-0.21	0.38	0.90***	-0.33	0.46	430
δ=0.10	0.01	-0.23	0.21	0.59**	0.05	0.55	540
δ=0.50	0.29	0.05	0.39	0.55**	0.33	0.63*	691
δ=1.00	0.52	0.08	0.67	0.63***	0.61	0.49	703

[1] Reported estimates are obtained assuming that up to five non-participating matches are allowed for each participating observation.
[2] For this table, the tolerance level is redefined to be expressed in percent of the current account ratio to GDP (upper panel) and in percent of the economic real per capita growth (lower panel).

Table 14: Heterogeneity in Participation Effects by Propensity Score [1]

		Sub-samples		
	Estimation Approach	$p_{ct} < 0.13$	$0.13 \le p_{ct} < 0.27$	$0.27 \le p_{ct}$
Economic Growth	**Censored-Sample Approach**	-1.618 N=617	1.144 N=82	1.251* N=214
	IV Approach [2]	0.677 N=617	-0.859 N=82	1.174 N=214
	Matching Approach: Differences in Means	-1.981 N=65	-0.197 N=68	0.443 N=69
	Matching Approach: Regression Based	-2.430*** N=65	-0.518 N=68	1.878*** N=69
Fiscal Balance	**Censored-Sample Approach**	0.998 N=617	1.132 N=82	1.148** N=214
	IV Approach [2]	-1.787 N=617	1.016 N=82	0.924 N=214
	Matching Approach: Differences in Means	1.460 N=65	0.188 N=68	0.590 N=69
	Matching Approach: Regression Based	1.283*** N=65	1.171* N=68	0.779 N=69
Current Account	**Censored-Sample Approach**	0.997 N=617	1.322 N=82	0.127 N=214
	IV Approach [2]	-1.025 N=617	6.176** N=82	0.394 N=214
	Matching Approach: Differences in Means	2.601 N=65	-0.726 N=68	-0.398 N=69
	Matching Approach: Regression Based	1.520* N=65	1.033 N=68	-0.660 N=69

[1] Each cell reports our estimate for the treatment effect of IMF programs.
[2] For the purpose of consistency of the treatment effect comparison across alternative approaches we rescale propensity scores for each sub-sample to vary between 0 and 1. Rescaling was implemented only for the IV approach. The IV approach was estimated excluding the λ_{ct} term.

Table 15: Heterogeneity in Participation Effects:
Potential Participants vs. Never and Always Participants [1]

	IV approach					
	Economic Growth		Fiscal Balance		Current Account	
Sub-samples	Complete Sample	Censored-Sample	Complete Sample	Censored-Sample	Complete Sample	Censored-Sample
Potential Participants (matched)	-0.814 (λ: -2.81)	0.549 (λ:-3.60)	1.034 (λ: 2.53)	1.037** (λ: 2.295)	2.861** (λ: 4.98)	1.059 (λ:5.63*)
	N=278					
Non and Certain Participants (not matched)	0.746 (λ: -0.40)	0.581 (λ:-1.08)	1.503 (λ: -3.06)	1.646* (λ:-5.90)	0.972 (λ: -1.36)	0.953 (λ:-2.84)
	N=635					

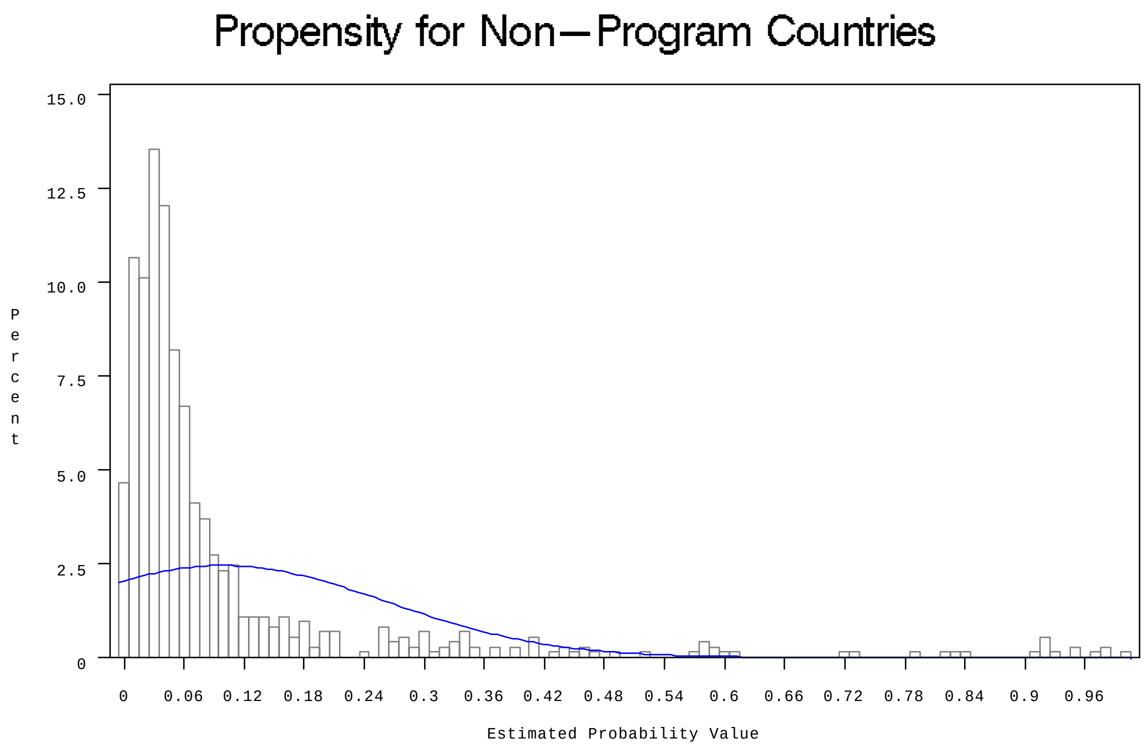
[1] Each cell reports our estimate for the treatment effect of IMF programs and the coefficient on the λ_{ct} term.

Table 16: Participation Effects for Different Time Horizons [1]

	Estimation Approach	T	T+1	T+2	T+3	N of obs.
Economic Growth	Censored-Sample Approach	-0.184	0.064	0.455	0.730	536
	IV Approach	-0.136	-0.497	-0.873	-0.053	536
	Matching Approach[2]: Differences in Means	-0.717	-0.090	0.023	0.478	116
	Matching Approach[2]: Regression Based	0.178	-0.468	1.671**	2.124***	116
Fiscal Balance	Censored-Sample Approach	0.993*	1.207*	0.726	0.783	536
	IV Approach	1.234	1.126	0.257	0.287	536
	Matching Approach[2]: Differences in Means	0.543	0.305	0.293	0.525	116
	Matching Approach[2]: Regression Based	0.667	0.311	-0.243	1.033*	116
Current Account	Censored-Sample Approach	0.804	1.180	0.776	0.388	536
	IV Approach	1.385	2.138	1.215	0.409	536
	Matching Approach[2]: Differences in Means	0.059	-0.415	-1.399	-1.203	116
	Matching Approach[2]: Regression Based	0.438	0.801	0.799	0.986	116

[1] Each cell reports our estimate for the participation effect of IMF programs.
[2] Reported estimated obtained using matching approach allow for up to five non-participating matches for each participating observation.

Figure 1


Propensity for Non-Program Countries
Percent
15.0
12.5
10.0
7.5
5.0
2.5
0
0
0.06
0.12
0.18
0.24
0.3
0.36
0.42
0.48
0.54
0.6
0.66
0.72
0.78
0.84
0.9
0.96
Estimated Probability Value

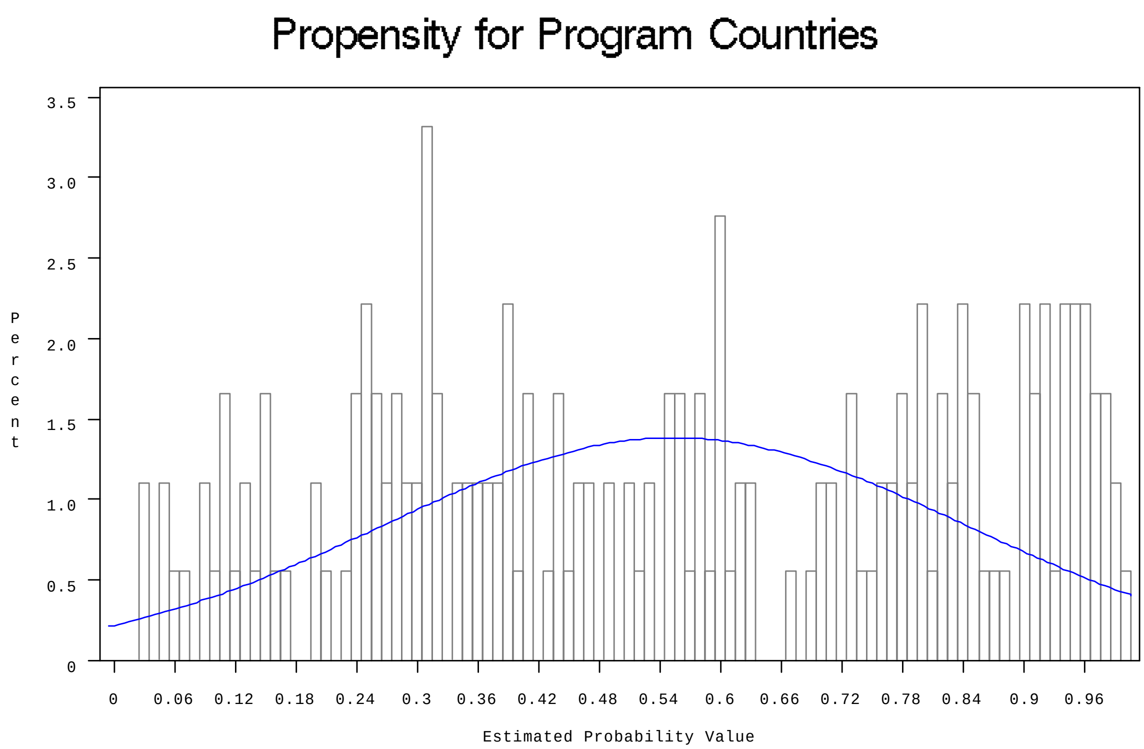
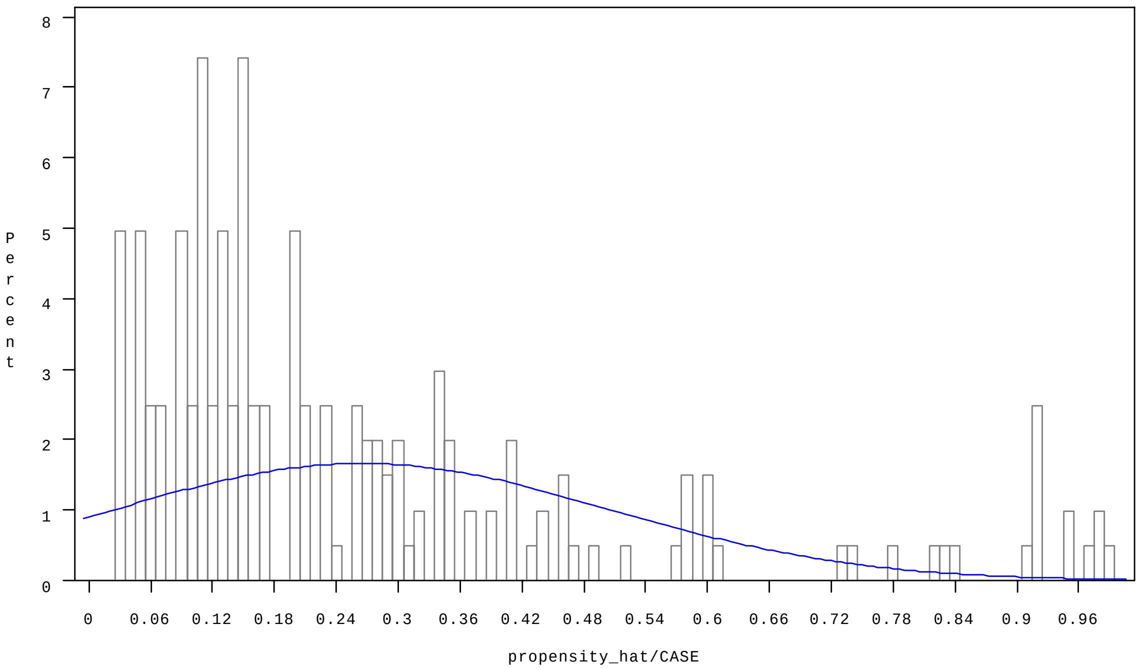

Propensity for Program Countries
Percent
3.5
3.0
2.5
2.0
1.5
1.0
0.5
0
0
0.06
0.12
0.18
0.24
0.3
0.36
0.42
0.48
0.54
0.6
0.66
0.72
0.78
0.84
0.9
0.96
Estimated Probability Value

Figure 2

Propensity Score for Matched Participants



ANNEX A:

Selection bias has been divided since Heckman (1979) into two parts: selection on observables, and selection on unobservables. Consider the selection problem for country c. The performance outcome is indicated by Y_c , with

$$Y_c = D_c Y_{1c} + (1-D_c)Y_{0c}. \qquad \text{(A1)}$$

The choice leading to Y_{1c} is defined by $D_c = 1$, while the choice leading to Y_{0c} is defined by $D_c = 0$. The choice $D_c = 1$ will be interpreted as the choice to participate in an IMF program; having done so, the country will have performance outcome Y_{1c}. The country's choice function is defined as

$$V_c = \mu_V(Z_c)+U_{Vc}. \qquad \text{(A2)}$$

Z_c are the observed factors determining the selection choice for country c while U_{Vc} are the unobserved factors. The country chooses to participate when $V_c > 0$.

$$D_c = 1 \text{ iff } V_c > 0 \qquad \text{(A3)}$$
$$D_c = 0 \text{ otherwise}$$

If we define the outcomes Y_{1c} and Y_{0c} as

$$Y_{1c} = \mu_1(X_c) + U_{1c} \qquad \text{(A4)}$$
$$Y_{0c} = \mu_0(X_c) + U_{0c} \qquad \text{(A5)}$$

X_c are the observed factors determining outcomes and U_{1c}, U_{0c} are the unobserved factors. The variables U_{jc} (j=0,1) are continuous random variables and all means are finite. Consider the parametric case of normally distributed errors: $(U_{1c},U_{0c},U_{Vc}) \sim N(0,\Sigma)$. Define $Var(U_j) = \sigma_j^2$ for $j\in\{1,0,V\}$ and $Cov(U_{kc},U_{jc}) = \sigma_{kj}$ for $k,j\in\{0,1\}$. Define as well $Cov(U_{kc},U_{Vc}) = \sigma_{kV}$. Let $\varphi(.)$ and $\Phi(.)$ be the probability density function (pdf) and cumulative density function (cdf) for a standard normal variable. If we further assume that the errors (U_{1c},U_{0c},U_{Vc}) are independent of the exogenous variables (X_c,Z_c), then

$$E(U_{1c}|X_c,Z_c,D_c=1) = E(U_{1c}|U_{Vc}>-\mu_V(Z_c)) = (\sigma_{1V}/\sigma_V)B_1(Z_c) \qquad \text{(A6a)}$$
$$E(U_{0c}|X_c,Z_c,D_c=0) = E(U_{0c}|U_{Vc}<-\mu_V(Z_c)) = (\sigma_{0V}/\sigma_V)B_0(Z_c) \qquad \text{(A6b)}$$

where $B_0(Z_c)$ and $B_1(Z_c)$ are the selection adjustment terms defined shortly.

Participation effect.

For any country c, our measure of the participation effect is $[\mu_1(X_c) - \mu_0(X_c)]$. Unfortunately, we only observe Y_{0c} (if D_c=0) or Y_{1c} (if D_c=1). Combining (A1) through (A6b) and taking expectations yield:

$$E(Y_{0c}|X_c,Z_c,D_c=0) = \mu_0(X_c) + (\sigma_{0V}/\sigma_V)B_0(Z_c) \qquad \text{(A7a)}$$

$$E(Y_{1c}|X_c,Z_c,D_c=1) = \mu_1(X_c) + (\sigma_{1V}/\sigma_V)B_1(Z_c) \quad \text{(A7b)}$$

$$E(Y_c| X_c,Z_c) = \mu_0(X_c) + E(D_c|Z_c)[\mu_1(X_c) - \mu_0(X_c)]+ E(D_c|Z_c) (\sigma_{1V}/\sigma_V)B_1(Z_c) - (1- E(D_c|Z_c)) (\sigma_{0V}/\sigma_V)B_0(Z_c) \quad \text{(A7c)}$$

While our interest is in deriving [$\mu_1(X_c) - \mu_0(X_c)$] from the data, it will be necessary to first derive estimates of $E(D_c|Z_c)$, $B_1(Z_c)$ and $B_0(Z_c)$. These all flow from the estimation of the propensity score.[19]

Propensity score.

The propensity score is defined $P(Z_c) = Pr(D_c=1|Z_c)$, and is the probability of choosing $D_c=1$ in (A2) and (A3). Its effect can be identified so long as a set of instruments Z_c exists that is significantly correlated with choice of D_c but uncorrelated with U_{1c} and U_{0c}. The propensity score is the summary statistic of $E(D_c|Z_c)$). Then the propensity score can be written

$$P(Z_c) = Pr(D_c=1|X_c,Z_c) = 1 – \Phi(-\mu_V(Z_c)/\sigma_V). \quad \text{(A8)}$$

The propensity score is also sufficient in this case to identify the terms $B_0(Z_c)$ and $B_1(Z_c)$.

$$B_1(Z_c) = [\varphi(\Phi^{-1}(1-P(Z_c)))/P(Z_c)] \quad \text{(A9a)}$$

$$B_0(Z_c) = [\varphi(\Phi^{-1}(1-P(Z_c)))/(1-P(Z_c))] \quad \text{(A9b)}$$

These terms are called the inverse Mills ratios and are transformations of the propensity score.

[19] The classic Heckman (1979) approach relied upon the use of the Inverse Mills Ratio to control for these biases. In a later section, we illustrate the link between that ratio and the propensity score.

ANNEX B: Matched Pairs (δ=0.025)

Obs	Participants	Non-Participants	#	DIS_{ic}	p^{p}_{it}	p^{np}_{cs}
1	Benin 1993	Tonga 1996	1	0.0001	0.0554	0.0555
2	Benin 1993	Antigua and Barbuda 1996	2	0.0003	0.0554	0.0557
3	Benin 1993	Malaysia 1996	3	0.0003	0.0554	0.0551
4	Benin 1993	Suriname 2001	4	0.0004	0.0554	0.0558
5	Benin 1993	Turkmenistan 1996	5	0.0004	0.0554	0.0550
6	Benin 1996	Guatemala 1996	1	0.0003	0.4146	0.4149
7	Benin 1996	Egypt 2001	2	0.0003	0.4146	0.4149
8	Benin 2000	Czech Republic 2000	1	0.0004	0.5680	0.5675
9	Brazil 1998	Tunisia 1994	1	0.0020	0.3219	0.3239
10	Bulgaria 1998	Jamaica 1994	1	0.0025	0.9932	0.9957
11	Burkina Faso 1993	Vanuatu 2000	1	0.0001	0.0536	0.0537
12	Burkina Faso 1993	Liberia 1998	2	0.0002	0.0536	0.0538
13	Burkina Faso 1993	Tonga 1998	3	0.0003	0.0536	0.0533
14	Burkina Faso 1993	Solomon Islands 2001	4	0.0004	0.0536	0.0532
15	Burkina Faso 1993	Cyprus 1996	5	0.0004	0.0536	0.0540
16	Burkina Faso 1996	Costa Rica 2001	1	0.0013	0.4445	0.4431
17	Cambodia 1994	Seychelles 1996	1	0.0000	0.0997	0.0996
18	Cambodia 1994	St. Lucia 1998	2	0.0006	0.0997	0.0990
19	Cambodia 1994	Poland 2002	3	0.0012	0.0997	0.0984
20	Cambodia 1994	Bhutan 1994	4	0.0014	0.0997	0.1011
21	Cambodia 1994	Slovak Republic 2001	5	0.0015	0.0997	0.1012
22	Cape Verde 1998	Sudan 1995	1	0.0034	0.1481	0.1447
23	Cape Verde 1998	Nepal 1994	2	0.0077	0.1481	0.1404
24	Cape Verde 1998	Haiti 2002	3	0.0080	0.1481	0.1401
25	Cape Verde 1998	Barbados 1999	4	0.0084	0.1481	0.1397
26	Cape Verde 1998	Barbados 1996	5	0.0104	0.1481	0.1377
27	Chad 1994	Dominica 1994	1	0.0002	0.1656	0.1658
28	Chad 1994	Grenada 1997	2	0.0023	0.1656	0.1633
29	Chad 1994	Jamaica 2002	3	0.0029	0.1656	0.1685
30	Chad 1994	Lithuania 1994	4	0.0037	0.1656	0.1693
31	Chad 1994	Sao Tome & Principe 2000	5	0.0098	0.1656	0.1558
32	Chad 1995	Tunisia 1996	1	0.0002	0.4110	0.4108
33	Chad 1995	Guatemala 1993	2	0.0003	0.4110	0.4113
34	Colombia 1999	Tunisia 1993	1	0.0003	0.1622	0.1619
35	Colombia 1999	Congo, Republic of 2002	2	0.0004	0.1622	0.1618
36	Colombia 1999	Barbados 1994	3	0.0032	0.1622	0.1591
37	Colombia 1999	Malawi 2002	4	0.0055	0.1622	0.1567
38	Colombia 1999	Qatar 1996	5	0.0062	0.1622	0.1560
39	Croatia 1994	Congo, Dem. Rep. of 1998	1	0.0014	0.2664	0.2678
40	Croatia 1994	Haiti 2001	2	0.0030	0.2664	0.2634
41	Croatia 2001	Hungary 2001	1	0.0041	0.4577	0.4537
42	Djibouti 1999	Costa Rica 2000	1	0.0024	0.4706	0.4730
43	Dominica 2002	Vanuatu 1998	1	0.0001	0.0695	0.0694
44	Dominica 2002	Solomon Islands 1998	2	0.0002	0.0695	0.0697
45	Dominica 2002	Sao Tome & Principe 2002	3	0.0003	0.0695	0.0698

46	Dominica 2002	Sierra Leone 2002	4	0.0004	0.0695	0.0692
47	Dominica 2002	Saudi Arabia 1997	5	0.0005	0.0695	0.0700
48	Egypt 1993	Gambia, The 1994	1	0.0027	0.3682	0.3654
49	El Salvador 1997	Argentina 1993	1	0.0021	0.9115	0.9093
50	El Salvador 1998	Jamaica 1993	1	0.0006	0.9833	0.9827
51	El Salvador 1998	Morocco 1994	2	0.0008	0.9833	0.9841
52	Equatorial Guinea 1993	Sierra Leone 2000	1	0.0003	0.2579	0.2582
53	Equatorial Guinea 1993	Dominican Republic 1999	2	0.0008	0.2579	0.2571
54	Estonia 2000	Jamaica 1996	1	0.0004	0.9526	0.9530
55	Ethiopia 1996	Sao Tome & Principe 1995	1	0.0003	0.1518	0.1520
56	Ethiopia 1996	Barbados 1998	2	0.0006	0.1518	0.1524
57	Ethiopia 1996	Trinidad and Tobago 1993	3	0.0007	0.1518	0.1524
58	Ethiopia 1996	Turkmenistan 1997	4	0.0011	0.1518	0.1507
59	Ethiopia 1996	Chile 1996	5	0.0039	0.1518	0.1557
60	Ghana 1995	Morocco 1999	1	0.0052	0.5154	0.5207
61	Ghana 1999	Malawi 2000	1	0.0059	0.3376	0.3317
62	Ghana 1999	Guatemala 1994	2	0.0074	0.3376	0.3302
63	Ghana 1999	Kazakhstan 1994	3	0.0077	0.3376	0.3299
64	Guatemala 2002	India 1996	1	0.0032	0.1489	0.1457
65	Guatemala 2002	Tunisia 1998	2	0.0038	0.1489	0.1451
66	Guatemala 2002	Liberia 1993	3	0.0068	0.1489	0.1421
67	Guatemala 2002	Bangladesh 1993	4	0.0125	0.1489	0.1363
68	Guatemala 2002	Tunisia 1997	5	0.0232	0.1489	0.1257
69	Guinea 1997	Sudan 1993	1	0.0012	0.3861	0.3873
70	Guinea-Bissau 2000	Trinidad and Tobago 1994	1	0.0017	0.3707	0.3724
71	Guyana 1998	Czech Republic 1998	1	0.0020	0.6020	0.6040
72	Haiti 1995	Congo, Dem. Rep. of 1997	1	0.0032	0.4584	0.4616
73	Haiti 1995	Poland 2001	2	0.0032	0.4584	0.4616
74	Honduras 1999	Guatemala 1997	1	0.0017	0.3462	0.3445
75	Honduras 1999	Nigeria 1993	2	0.0024	0.3462	0.3438
76	Indonesia 1997	Tonga 2001	1	0.0001	0.0296	0.0295
77	Indonesia 1997	Liberia 1995	2	0.0001	0.0296	0.0297
78	Indonesia 1997	Namibia 2000	3	0.0002	0.0296	0.0298
79	Indonesia 1997	Congo, Dem. Rep. of 2002	4	0.0002	0.0296	0.0294
80	Indonesia 1997	Bhutan 1998	5	0.0002	0.0296	0.0298
81	Indonesia 1998	India 1994	1	0.0001	0.1141	0.1140
82	Indonesia 1998	Sao Tome & Principe 1993	2	0.0003	0.1141	0.1138
83	Indonesia 1998	Yemen, Republic of 2002	3	0.0017	0.1141	0.1124
84	Indonesia 1998	Saudi Arabia 1996	4	0.0029	0.1141	0.1112
85	Indonesia 1998	Myanmar 1998	5	0.0047	0.1141	0.1094
86	Jordan 1996	Morocco 1996	1	0.0004	0.8338	0.8335
87	Jordan 1999	Morocco 1993	1	0.0021	0.9249	0.9270
88	Kazakhstan 1999	Morocco 1997	1	0.0012	0.7847	0.7859
89	Kenya 1996	Poland 2000	1	0.0022	0.7257	0.7235
90	Kenya 2000	Nigeria 1995	1	0.0008	0.5806	0.5814
91	Korea 1997	Antigua and Barbuda 1997	1	0.0001	0.0949	0.0951
92	Korea 1997	St. Vincent & Grens. 1999	2	0.0002	0.0949	0.0952
93	Korea 1997	Eritrea 2001	3	0.0009	0.0949	0.0940

94	Korea 1997	Barbados 1997	4	0.0013	0.0949	0.0962
95	Korea 1997	Bahamas, The 1998	5	0.0015	0.0949	0.0934
96	Lao People's Dem.Rep 1993	Maldives 1998	1	0.0001	0.0479	0.0478
97	Lao People's Dem.Rep 1993	Sri Lanka 1996	2	0.0002	0.0479	0.0478
98	Lao People's Dem.Rep 1993	St. Lucia 1994	3	0.0002	0.0479	0.0477
99	Lao People's Dem.Rep 1993	Bangladesh 1998	4	0.0003	0.0479	0.0476
100	Lao People's Dem.Rep 1993	Bahrain, Kingdom of 2001	5	0.0004	0.0479	0.0484
101	Lao People's Dem.Rep 2001	Dominican Republic 2001	1	0.0123	0.2266	0.2142
102	Lao People's Dem.Rep 2001	Gambia, The 1996	2	0.0132	0.2266	0.2134
103	Lao People's Dem.Rep 2001	Chile 1994	3	0.0145	0.2266	0.2121
104	Lao People's Dem.Rep 2001	Chile 1995	4	0.0237	0.2266	0.2029
105	Lao People's Dem.Rep 2001	Kuwait 1994	5	0.0239	0.2266	0.2026
106	Latvia 1996	Jamaica 1998	1	0.0010	0.8395	0.8405
107	Latvia 1997	Congo, Dem. Rep. of 1993	1	0.0026	0.9152	0.9178
108	Latvia 1999	Jamaica 1995	1	0.0001	0.9733	0.9733
109	Lithuania 2000	St. Kitts and Nevis 1995	1	0.0000	0.0317	0.0317
110	Lithuania 2000	Swaziland 1995	2	0.0001	0.0317	0.0318
111	Lithuania 2000	Chile 2001	3	0.0001	0.0317	0.0316
112	Lithuania 2000	Gambia, The 2001	4	0.0001	0.0317	0.0318
113	Lithuania 2000	Bahamas, The 2001	5	0.0001	0.0317	0.0318
114	Lithuania 2001	Comoros 1998	1	0.0011	0.1325	0.1314
115	Lithuania 2001	Sao Tome & Principe 1999	2	0.0017	0.1325	0.1308
116	Lithuania 2001	Nepal 1995	3	0.0022	0.1325	0.1303
117	Lithuania 2001	Honduras 1994	4	0.0024	0.1325	0.1301
118	Lithuania 2001	St. Vincent & Grens. 1996	5	0.0136	0.1325	0.1189
119	Macedonia, FYR 1995	India 1999	1	0.0004	0.1089	0.1085
120	Macedonia, FYR 1995	Bangladesh 1994	2	0.0004	0.1089	0.1084
121	Macedonia, FYR 1995	Liberia 1994	3	0.0013	0.1089	0.1075
122	Macedonia, FYR 1995	Bangladesh 1995	4	0.0016	0.1089	0.1072
123	Macedonia, FYR 1995	Lebanon 1999	5	0.0016	0.1089	0.1072
124	Macedonia, FYR 1997	Sierra Leone 2001	1	0.0004	0.2132	0.2128
125	Macedonia, FYR 1997	Samoa 1993	2	0.0030	0.2132	0.2102
126	Macedonia, FYR 1997	Dominican Republic 2000	3	0.0103	0.2132	0.2030
127	Macedonia, FYR 1997	Togo 2001	4	0.0163	0.2132	0.1970
128	Macedonia, FYR 1997	Angola 1994	5	0.0237	0.2132	0.1895
129	Madagascar 2001	Turkmenistan 1998	1	0.0028	0.2905	0.2932
130	Madagascar 2001	Sao Tome & Principe 1994	2	0.0046	0.2905	0.2951
131	Mali 1999	Jamaica 2001	1	0.0009	0.3406	0.3397
132	Mali 1999	Trinidad and Tobago 1996	2	0.0014	0.3406	0.3419
133	Mali 1999	Trinidad and Tobago 1999	3	0.0020	0.3406	0.3425
134	Mozambique 1996	Guinea-Bissau 1994	1	0.0036	0.2445	0.2409
135	Mozambique 1999	Czech Republic 2001	1	0.0010	0.2821	0.2832
136	Mozambique 1999	Dominican Republic 1998	2	0.0020	0.2821	0.2842
137	Mozambique 1999	Malawi 2001	3	0.0021	0.2821	0.2842
138	Mozambique 1999	Congo, Dem. Rep. of 1999	4	0.0026	0.2821	0.2847
139	Niger 2000	Morocco 2000	1	0.0012	0.2875	0.2887
140	Nigeria 2000	Sudan 1994	1	0.0010	0.3534	0.3524
141	Nigeria 2000	Congo, Republic of 2001	2	0.0021	0.3534	0.3513

142	Pakistan 1993	Burundi 1998	1	0.0055	0.1408	0.1353
143	Pakistan 1993	Chile 1998	2	0.0212	0.1408	0.1196
144	Pakistan 1993	India 1998	3	0.0213	0.1408	0.1196
145	Pakistan 1993	Belize 1994	4	0.0225	0.1408	0.1183
146	Pakistan 1993	Chile 1999	5	0.0249	0.1408	0.1159
147	Panama 1995	Algeria 2000	1	0.0001	0.4914	0.4916
148	Peru 1996	Burundi 1996	1	0.0010	0.3198	0.3187
149	Philippines 1994	Congo, Dem. Rep. of 1995	1	0.0008	0.9467	0.9475
150	Philippines 1998	Congo, Dem. Rep. of 1996	1	0.0012	0.8214	0.8203
151	Poland 1994	Morocco 1998	1	0.0053	0.6146	0.6093
152	Romania 1997	Poland 1999	1	0.0070	0.7419	0.7349
153	Rwanda 1998	Gambia, The 1995	1	0.0003	0.1345	0.1342
154	Rwanda 1998	Slovak Republic 1999	2	0.0012	0.1345	0.1333
155	Rwanda 1998	Barbados 2000	3	0.0063	0.1345	0.1282
156	Rwanda 1998	Lebanon 1996	4	0.0102	0.1345	0.1243
157	Rwanda 1998	Sudan 1996	5	0.0131	0.1345	0.1214
158	Slovak Republic 1994	Sao Tome & Principe 1996	1	0.0032	0.3033	0.3002
159	Slovak Republic 1994	Trinidad and Tobago 1995	2	0.0039	0.3033	0.2994
160	Slovak Republic 1994	Costa Rica 2002	3	0.0053	0.3033	0.2980
161	Slovak Republic 1994	Tunisia 1995	4	0.0060	0.3033	0.2973
162	Sri Lanka 2001	Bahamas, The 1996	1	0.0004	0.0865	0.0861
163	Sri Lanka 2001	Angola 1997	2	0.0006	0.0865	0.0871
164	Sri Lanka 2001	St. Kitts and Nevis 1996	3	0.0010	0.0865	0.0875
165	Sri Lanka 2001	Libya 1997	4	0.0012	0.0865	0.0878
166	Sri Lanka 2001	St. Kitts and Nevis 1997	5	0.0013	0.0865	0.0879
167	Tajikistan 1998	Czech Republic 1999	1	0.0001	0.5844	0.5845
168	Tajikistan 1998	Nigeria 1994	2	0.0012	0.5844	0.5832
169	Tanzania 1996	Guatemala 1995	1	0.0027	0.3113	0.3085
170	Turkey 1994	Trinidad and Tobago 1997	1	0.0013	0.2698	0.2710
171	Turkey 1994	Burundi 1995	2	0.0038	0.2698	0.2736
172	Turkey 2002	Sao Tome & Principe 1998	1	0.0046	0.2044	0.1998
173	Turkey 2002	Togo 2000	2	0.0111	0.2044	0.1933
174	Turkey 2002	Angola 1995	3	0.0197	0.2044	0.1847
175	Turkey 2002	Kazakhstan 1993	4	0.0221	0.2044	0.1823
176	Turkey 2002	Gambia, The 1993	5	0.0250	0.2044	0.1794
177	Uganda 1994	Uganda 2002	1	0.0001	0.1158	0.1157
178	Uganda 1994	Tajikistan 1993	2	0.0009	0.1158	0.1149
179	Uganda 1994	Grenada 1998	3	0.0015	0.1158	0.1143
180	Uganda 1994	Mauritania 2000	4	0.0063	0.1158	0.1095
181	Uganda 1994	Burundi 1997	5	0.0064	0.1158	0.1094
182	Uganda 1997	Chile 1993	1	0.0008	0.2591	0.2599
183	Uganda 1997	Turkmenistan 1999	2	0.0018	0.2591	0.2609
184	Uganda 1997	Sao Tome & Principe 1997	3	0.0033	0.2591	0.2624
185	Ukraine 1998	Jamaica 1997	1	0.0003	0.9213	0.9216
186	Ukraine 1998	Congo, Dem. Rep. of 1994	2	0.0011	0.9213	0.9224
187	Ukraine 1998	Morocco 1995	3	0.0017	0.9213	0.9231
188	Uruguay 1996	Jamaica 1999	1	0.0032	0.5963	0.5930
189	Uruguay 1996	Jamaica 2000	2	0.0045	0.5963	0.5918

190	Venezuela, Rep. Bol. 1996	Sierra Leone 1999	1	0.0026	0.4279	0.4305
191	Vietnam 1994	Comoros 1996	1	0.0000	0.1062	0.1062
192	Vietnam 1994	Venezuela, Rep. Bol. 2001	2	0.0002	0.1062	0.1064
193	Vietnam 1994	Slovak Republic 2000	3	0.0005	0.1062	0.1067
194	Vietnam 1994	Chile 1997	4	0.0009	0.1062	0.1071
195	Vietnam 1994	Barbados 2001	5	0.0016	0.1062	0.1047
196	Vietnam 2001	Sao Tome & Principe 2001	1	0.0140	0.1955	0.1816
197	Vietnam 2001	Burundi 1994	2	0.0141	0.1955	0.1814
198	Vietnam 2001	Samoa 1994	3	0.0167	0.1955	0.1788
199	Vietnam 2001	Togo 1999	4	0.0176	0.1955	0.1779
200	Vietnam 2001	Lithuania 1995	5	0.0249	0.1955	0.1706
201	Yemen, Republic of 1997	Trinidad and Tobago 1998	1	0.0034	0.3906	0.3940
202	Zambia 1999	Algeria 2001	1	0.0006	0.4376	0.4382