Does Transparency Pay?

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This paper studies whether transparency (measured by accuracy and frequency of macroeconomic information released to the public) leads to lower borrowing costs in sovereign bond markets. We analyze the data generated during 1999–2002 when the International Monetary Fund (IMF) instituted new ways for countries to increase their transparency—by publishing the IMF's assessment of their policies and committing to release more accurate data more frequently. The IMF's preexisting internal timetable for country reports introduced exogenous variation when countries were faced with the option to become more transparent. We exploit this time variation and construct instruments to estimate the impact of transparency on bond yields in a way that is free from endogeneity bias. We find that countries experience a statistically significant decline in borrowing costs (11 percent reduction in credit spreads on average) when they choose to become more transparent. The magnitude of the decline is inversely related to the initial level of transparency and the size of the debt market. [JEL F34, G14]

Agrowing literature examines the role of institutions in determining countries' economic performance. It has been argued that better institutions lead to more developed financial markets (La Porta and others, 1997 and 1998), greater resilience to shocks (Johnson and others, 2000), and better allocation of capital (Wurgler, 2000).

This literature has only limited policy implications, at least in the short run, because it examines the long-run consequences of institutions as a whole but does not isolate the impact of individual components. Countries seeking to design a reform program are faced with a long list of institutions that, as a bundle, promote economic performance but may be unimportant or even unhelpful individually.

It is in this context that greater transparency has been promoted as a practical policy option for countries seeking to improve their institutions. The idea is that transparency, in addition to improving the allocation of resources, can make governments more accountable, undermine the power of special interests, and thus lead to improved policies and institutions (Stiglitz, 2001; and Kaufmann and Bellver, 2005).

In testing the validity of this idea, there are two complications. First, transparency is defined in many different ways and is often measured based on subjective perceptions of interviewees that, as discussed by Glaeser and others (2004), may be inherently biased. Olken (2005), using data from Indonesia, shows how these biases can generate highly misleading results in cross-sectional studies. We avoid this problem by using an objective (and narrow) definition of transparency: the accuracy and frequency of economic information released to the public. This is similar to the concept of disclosure used in corporate finance.

The second complication is that the level of transparency across countries is highly correlated with other institutional characteristics that are, in the long run, likely to be codetermined. A number of cross-country studies have found a *correlation* between releasing more macroeconomic data and lower inflation (Chortareas, Stasavage, and Sterne, 2002), lower fiscal deficits (Alesina and others, 1999; and Alt and Lassen, 2006), more foreign direct investment inflows (Gelos and Wei, 2005), and lower sovereign risk premiums (Christofides, Mulder, and Tiffin, 2003). But, as the authors themselves acknowledge, these results are compatible with transparency being a by-product of good institutions and economic performance, rather than the other way around. So far, the literature has not convincingly resolved these omitted variable or reverse causality problems.

We overcome these difficulties by studying the data generated when the International Monetary Fund (IMF) introduced a series of reforms to promote transparency. The reforms involved (1) publishing an independent assessment of economic prospects and policies (Article IV report), (2) publishing detailed assessments (Reports on the Observance of Standards and Codes—ROSCs) of key economic institutions such as banking supervision, and (3) releasing more frequent, timely, and accurate macroeconomic statistics through the Special Data Dissemination Standard (SDDS).

The way the reforms were introduced provides us with an identification strategy for isolating the impact of transparency. Because different countries adopted the reforms at different times, we are able to use a panel estimation with country fixed effects and identify the impact of transparency using variation over time for a given country. This means our results are not subject to the standard omitted variable bias in cross-section studies, where more transparent countries tend to have more favorable institutional features in general.

More important, because the timing of reform adoptions by countries was primarily determined by internal IMF procedures that were unrelated to events in these countries, we can use these procedures to construct instruments for predicting when countries will adopt the reforms. The IMF has an internal timetable by which it reports on and discusses the economies of its members, with most countries discussed every 12 months with a 3-month grace period. This timetabling rule had been in place for many years. Thus, when the IMF decided to allow countries to release previously confidential Article IV reports to the public, there was a preordained order in which countries would face the decision about whether to release these reports. For example, in our sample, Colombia was the first country to publish an Article IV report, in December 1999, whereas Poland published its first report in March 2000. Given that both countries opted to publish their reports at their first opportunity, the timing of the publication of these two reports can be predicted entirely from the timing of the IMF's previous discussion of the confidential reports on these countries (in December 1998 and March 1999, respectively) and the simple 12-month rule. ROSCs were to be released according to the same standard IMF timetable, again introducing exogenous time variation.

Although the date on which a country would be faced with a decision about whether to publish an Article IV report and ROSC was determined exogenously, the adoption of the transparency reforms was voluntary, and hence we have to address the inherent selection bias. In other words, given a publication opportunity, countries are more likely to decide to publish an independent assessment of their economy if it contains good news. This good news can directly influence credit spreads, potentially creating bias in an ordinary least squares (OLS) estimate. To deal with this, we instrument for the probability of publishing the report using time-invariant factors that are by construction orthogonal to news about the country during our sample period.

Thus our instrument is the interaction of our measure when a country will be faced with the chance of publishing (based on the preexisting annual IMF timetable) and measures that predict whether a country will take that opportunity when it arises. We also control for the country fixed effect, which picks up long-run correlates of transparency, and for general time trends (using quarterly time dummies). We thus identify the impact of transparency by examining whether a particular country's bond spreads change more than those of other countries in the quarter in which the country is predicted to make the decision to become more transparent.

Our sample consists of emerging market economies where the issue of institutional reforms and transparency has been hotly debated since the crises in the 1990s. We measure the overall impact of the transparency reforms from the reaction of sovereign bond markets, because external borrowing costs are a readily available summary indicator of the market's perception of economic conditions and are an important element of emerging market public finance.

From quarterly sovereign credit spread data covering 23 emerging market economies, we find that, when countries choose to publish Article IV reports and ROSCs, and commit to the SDDS, their credit spreads fall by 11 percent on average. We show that our estimation results withstand various robustness checks.

There are two possible interpretations of these results. First, lack of transparency creates uncertainty and markets demand premiums for this additional uncertainty. Second, lack of transparency increases the cost of monitoring, and monitoring influences a country's behavior, which in turn affects its credit spreads. For example, if an IMF report concludes that a country's current fiscal deficit is unsustainable, the country will be more likely to take actions to reduce the deficit when this analysis is made public than when it is not. In this case, markets will reward those who opt to publish reports because they know that, on average, this will lead to a more sound policy. Similarly, knowing that an accurate level of international reserves will be released to the market on a regular basis is likely to act as a disciplining device and may reduce the likelihood of a government pursuing policies that cause a sudden reduction in reserves. Again, markets are likely to reward those that opt for this type of commitment device.

We further find that there is a diminishing marginal benefit of transparency; that is, countries that start out less transparent record larger gains from the reforms. We also find evidence that increased transparency is particularly beneficial for countries with smaller and less liquid debt markets, where the private sector has less incentive to do its own monitoring.

The link between transparency and borrowing costs has been extensively studied in corporate finance. Although the theoretical results from the disclosure literature suggest that transparency should lower the cost of capital (Diamond and Verrecchia, 1991; and Easley and O'Hara, 2004), the empirical counterparts have seldom gone beyond reporting a correlation between the two, again raising the concern that the correlation is driven by selection bias.[1] A few studies have sought to overcome selection bias by analyzing natural experiments. Greenstone, Oyer, and Vissing-Jorgensen (2006) study the impact of changes in mandatory disclosure requirements under the 1964 Securities Acts Amendments. They are able to use the firms not subject to these changes as their control. Leuz and Verrecchia (2000) focus on accounting standard reforms in Germany that gave firms the option

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[1]See Healy and Palepu (2001) or Verrecchia (2001) for a thorough literature review.

of signing up for a regulatory regime with more disclosure requirements. Both studies conclude that increased transparency indeed lowers the cost of capital.[2] More strikingly, they also conclude that the positive impact from increased transparency is inversely related to the initial level of transparency.

Unlike publicly listed corporations, sovereign nations are not overseen by regulators, and there is no episode comparable to the changes in mandatory disclosure that Greenstone, Oyer, and Vissing-Jorgensen (2006) study. In this context, the transparency reforms instituted by the IMF in the wake of the emerging market crises of the 1990s represent a unique opportunity to measure the effect of transparency untainted by endogeneity bias. Because the reforms created new information channels that did not exist until the late 1990s, countries' adoption was primarily influenced by their preexisting attitude to transparency rather than by their economic situation at the time. More important, the precise timing of adoption by different countries was primarily determined by internal IMF procedures that were not correlated with changing conditions in the country, thus providing an effective instrument. To the best of our knowledge, this paper is the first to exploit the data generated by these reforms and to document the causal effect of country-level transparency.

I. Description of Transparency Reforms

Lack of transparency was blamed for causing, or exacerbating, the emerging market crises of the 1990s. For example, many were unaware of the extent of nonperforming loans held by Korean banks prior to the crisis because these were effectively hidden on banks' balance sheets. Similarly, few in the markets were aware of the extent of intervention in the foreign exchange market in Thailand in the run-up to the crisis because of the way official reserves were reported.[3] In response, the IMF initiated a series of reforms to increase transparency in these economies, addressing the perceived lack of information available to market participants. The reforms gave birth to three new channels through which countries can feed the global financial market with economic information more accurately and frequently. This section describes the channels in detail.

Publication of Article IV Staff Reports

Although the IMF is best known for its lending operations, one of its key functions is to monitor and report on the policies and economic prospects of its member countries. As part of this process, Article IV reports are produced by IMF staff on a regular basis for all member countries. For most countries, they must be produced 12 months after the previous Article IV was discussed

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[2]Bushee and Leuz (2005) is another example.

[3]The narrow definition of reserves used by the Thai authorities (which excluded derivative transactions) was standard practice at the time, with many countries engaging in derivative transactions to influence reported reserves.

by the IMF Executive Board, with a three-month grace period. Article IV reports follow a standard format with a description of recent economic developments, short-term macroeconomic forecasts, standard tables of macro data, and policy suggestions. Until the late 1990s, these reports were considered highly confidential; even the publication of a two- to four-page summary was not permitted until 1996. These reports were considered highly confidential because, for example, they would question and provide alternatives to a government's forecasts for growth and tax revenues, and because they were based on private discussions with senior officials and internal government data. In March 1999, the IMF introduced a pilot program for voluntary publication of these Article IV reports, which was eventually made permanent in 2001. The first batch of Article IV reports were published toward the end of 1999. For more details on the procedures involved in writing and publishing Article IV reports, see Appendix I.

Article IV reports are closely followed by key market participants. Directors of country risk at major financial institutions indicate that Article IV reports are one of the first places they turn to in evaluating country risk (private communications).

Special Data Dissemination Standard

The SDDS was inaugurated following the 1994–95 Mexican crisis and was further strengthened in response to the 1997 Thai crisis. The Mexican and Thai central banks' failure to release transparent official reserve data in a timely manner was seen to have contributed to both crises.

The SDDS sets consistent definitions for macroeconomic data and, in particular, prescribes a very detailed template for compiling central bank reserve data—a key input into international investors' decisions about country risk. This was designed to put an end to the practice of manipulating reserve figures that occurred on a regular basis in advanced and emerging markets prior to the introduction of the SDDS. It also specifies timeliness and frequency standards for macroeconomic data releases. For example, to comply with the SDDS, countries must provide quarterly data on GDP and fiscal accounts that many emerging market economies had not previously released.

Countries were invited to subscribe to the SDDS between 1996 and 1998. The first countries to meet all the requirements were Canada and the United States, in February 1999. Major financial institutions use SDDS compliance as an explicit factor in determining sovereign credit rating and country risk (IMF, 2003).

Reports on the Observance of Standards and Codes

To address the perceived lack of information on institutional weaknesses of emerging market economies highlighted by the crises of the 1990s, ROSCs were introduced to assess countries' institutions against internationally accepted standards of good practice. They cover three broad categories:

transparency, financial market regulation, and corporate governance. The ROSC on banking supervision, for example, is written by banking experts and assesses the level of capital adequacy, the treatment of nonperforming loans on bank balance sheets, and the rules against lending to connected parties, as well as the extent to which written regulations are enforced in practice. The production and publication of ROSCs are voluntary for all IMF member countries. The first pilot reports were published in April 1999.[4] After this, the timing of ROSCs was designed to coincide with Article IV reports.

A survey of internationally active banks in G-7 countries reports that about 60 percent of the respondents use ROSCs in their investment decisions (IMF, 2003), although ROSCs are not as widely known or used as Article IV reports.

II. Estimation Strategy

This section examines whether the adoption of the aforementioned reforms led to lower borrowing costs in sovereign bond markets. We run panel regressions with country fixed effects to estimate the difference in countries' mean credit spreads before and after their implementation of the reforms, controlling for the changes in other countries' spreads over the same period. We use OLS, OLS on a restricted sample, and two-stage least squares.

Data

Sovereign credit spread data are taken from JPMorgan's Emerging Market Bond Index (EMBI). The EMBI tracks the value of country-specific portfolios at daily frequency. These portfolios are composed of dollar-denominated sovereign or quasi-sovereign debt instruments. The spread is defined as the sovereign yield over the U.S. zero-coupon curve, where the sovereign yield is set to equate the net present value of the sovereign portfolio cash flows to zero.

Our sample is chosen to cover the first big wave of transparency reform adoptions and runs from January 1, 1999, to June 30, 2002. A total of 23 countries are covered by the EMBI throughout this period and are included in our analysis.[5]

The data on these 23 countries' adoption of the reforms are available on the IMF website (http://www.imf.org). A reform event refers to the publication by the IMF of a report (Article IV report or ROSC) for the first time or the announcement by the IMF that a country has come into compliance with the SDDS. These reform events are distributed almost

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[4]The ROSC initiative is a joint one between the IMF and the World Bank with a division of responsibility for different subject areas between the two institutions. The IMF takes the lead in some areas (in particular, fiscal and monetary transparency) and the World Bank in others (such as corporate governance regulations).

[5]Data on nine other countries are available for parts of the sample period.

uniformly over our sample period. By the end of the sample period, 12 of the 23 countries had published at least one Article IV report, 14 had come into compliance with the SDDS, and 11 had published ROSCs. There is a conspicuous regional pattern in the adoption of the transparency reforms. In the sample, for example, no Asian country publishes an Article IV report, whereas all the European countries do. However, there is considerable time variation of adoption within Europe, as well as within other regions. See Table 1 for more details.

The official record pinpoints the dates of reform events. However, the decision to allow the IMF to publish a particular report is made in the weeks ahead of the event and may reach the market before the official date of publication. In addition, the internal rules that we use to instrument for the timing of the reforms cannot predict the timing to the day, but they narrow it down to intervals of a couple of months. Hence, we construct quarterly indicators of transparency based on whether a country had published an Article IV report or ROSC and had met the prerequisites for the SDDS. By modeling credit spreads at quarterly frequency, we can capture the effects of the reforms while obviating the arduous task of fully characterizing the higher-frequency credit spread process. As will become clear from Equation (1), almost no covariation between credit spreads and the transparency indicators is lost, even as they are time-aggregated into quarterly data.

In addition, we create an indicator for lack of transparency that reflects a country's decision to pass up an opportunity to publish an Article IV report. A telltale sign to the market of this decision is a publication of a short summary of the Article IV report that is not accompanied by the report itself. In our sample, 14 countries published such stand-alone summaries.

Possible Endogeneity in Transparency Adoption

As discussed in the introduction, our strategy is to identify the impact of transparency using time variation within countries, thus abstracting from the long-run correlates of transparency. For an OLS estimation of this type to be unbiased, the decision to become transparent and the timing of the decision need to be independent of other events that have a direct impact on the dependent variable (credit spreads). To the extent that countries with good news to tell are more likely to become transparent, this could introduce selection bias. And to the extent that events in the country influence when a particular report is released, this could introduce omitted variable bias. Here we discuss how we address these two potential sources of bias by constructing instruments that predict both whether a country will adopt the reforms and when they will do so. With these instruments, we run two-stage least squares (2SLS).[6]

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[6]Angrist (2001) and Kelejian (1971) recommend the use of 2SLS in models with dummy endogenous regressors because of its robustness in the presence of potential misspecification.

Article IV reports had been produced by the IMF staff long before the decision was made to allow countries, on a voluntary basis, to release them to the public. For most countries, these reports were produced every 12 months with a three-month grace period.[7] Once a policy of voluntary publication was adopted, the reports were to be released within 10 days of the IMF Executive Board discussion of them. This meant that there was a preset order in which countries would be faced with the decision about whether or not to release the reports, based on when their previous Article IV discussion had taken place. However, the preset timetable could be disturbed for a number of reasons, including (1) the country has a conditional loan from the IMF (the so-called program countries), and the Article IV discussion is delayed because certain target conditions have not been met; (2) domestic conditions, such as a recent or upcoming election, delay in visit to the country to collect information for a report; or (3) the IMF and the country fail to agree upon which sensitive information, if any, should be deleted prior to the publication (IMF, 2001). All of these exceptions are correlated with events in the country that could influence spreads directly, thus potentially biasing the OLS estimates.

Because deviations from the timetable occur more often for program countries than nonprogram countries, we can get a sense of the magnitude of the bias by comparing the OLS estimates in the full sample with those from a restricted sample of nonprogram countries.[8]

For the 2SLS, we use the IMF timetable to construct an instrument for when a country is likely to be faced with the decision to publish an Article IV report. More precisely, we use the time elapsed since the previous Article IV discussion and the average interval between Article IV consultations to predict when countries will face this decision.[9] Because the majority of countries followed the preset timetable, the months since the last Article IV report is a good predictor of when a country faced the opportunity to publish an Article IV report for the first time. More important, because the 12-month periodicity of reports and, hence, the order of future reports were established long before the decision to make the Article IV reports public, this predictor is uncorrelated with events in the country at the time.

But, although the preset timetable can predict when a country will face the decision about whether to become more transparent, it cannot predict whether a country will seize that opportunity. Here, we use a number of factors that are orthogonal to the changes in credit spreads during our sample period of 1999–2002 to predict who will publish. Because the

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[7]A few countries were on a longer cycle, but none of them are in our sample. More recently, program countries have been moved to a longer cycle, but this change falls outside our sample period.

[8]A nonprogram country is defined to be one that does not go through any IMF program during the sample period. There are seven such countries (Table 1).

[9]The average interval is calculated from data covering all emerging market economies in and out of the sample from 1999 to 2003.

Table 1. Implementation of Transparency Reforms, 1999–2002

Country	Article IV Report		Stand-Alone Summary	ROSC		SDDS Observance	Debt Market Size (US$ billions)	Governance Indicator		
	First publication	Months since last Article IV		First publication	Months since last Article IV			Law	Corruption	Voice
Argentina[1]	12/19/00	22	...	04/15/99	14	11/01/99	134.5	0.32	−0.27	0.49
Brazil[1]	...	...	12/22/00	12/06/01	12	03/14/01	184.7	−0.22	0.06	0.58
Bulgaria[1]	04/19/00	13	...	03/17/00	13	...	7.0	−0.15	−0.56	0.47
China	...	...	09/01/00	...	...	...	109.4	−0.04	−0.29	−1.29
Colombia[1]	12/29/99	13	...	...	...	05/09/00	24.4	−0.78	−0.49	0.15
Côte d'Ivoire[1]	10/02/01	13	09/08/00	...	...	...	11.3	−0.33	−0.08	−0.57
Croatia[1]	01/30/00	18	...	09/–/01	6	03/30/01	6.6	0.15	−0.46	−0.23
Ecuador[1]	...	...	09/07/00	...	...	07/14/00	11.0	−0.72	−0.82	0.27
Korea[1]	...	...	12/29/99	01/23/01	13	11/01/99	116.0	0.94	0.16	0.91
Lebanon	...	...	10/29/01	...	...	...	2.4	0.26	−0.40	−0.40
Malaysia	...	...	08/10/00	12/11/00	4	09/01/00	34.3	0.83	0.63	−0.09
Mexico[1]	10/21/01	19	03/22/00	10/25/01	18	06/29/00	144.6	−0.47	−0.28	0.11
Morocco[1]	11/13/01	16	09/01/00	...	...	...	11.6	0.68	0.13	−0.24
Nigeria[1]	08/06/01	20	...	...	...	...	20.0	−1.05	−0.95	−1.23
Panama[1]	02/20/01	13	02/28/00	...	...	...	4.7	−0.39	−0.46	0.66

Peru[1]	03/19/01	20	...	...	...	07/15/99	18.9	−0.52	−0.20	−0.69
Philippines[1]	...	...	03/13/01	09/−/01	6	01/17/01	29.9	−0.08	−0.23	0.63
Poland	03/31/00	15	...	12/11/00	9	03/02/00	34.5	0.54	0.49	1.12
Russia[1]	11/09/00	15	...	...	...	...	101.7	−0.72	−0.62	−0.19
South Africa	...	...	03/10/00	10/16/01	20	09/18/00	21.8	−0.35	0.30	0.99
Thailand[1]	...	...	02/10/00	...	...	05/16/00	76.0	0.41	−0.16	0.22
Turkey[1]	...	...	01/03/00	06/27/00	6	07/20/01	54.3	−0.01	−0.35	−0.88
Venezuela	...	...	...	...	...	...	28.6	−0.66	−0.72	0.15

Note: The second column lists the dates on which a country published an Article IV report for the first time. The months elapsed since the IMF Executive Board's discussion of the previous Article IV report are shown in the next column. A period of more than 15 months indicates that the Board's discussion had been postponed. Publication of a stand-alone summary after December 1999 is a sign of opacity: the country chose not to publish an Article IV report. The date of first Report on the Observance of Standards and Codes (ROSC) publication for each country is given in the fifth column, and the next column records the number of months by which the previous Article IV discussion by the Board preceded the ROSC publication. The seventh column presents the dates on which each country was declared in full compliance with the Special Data Dissemination Standard (SDDS) requirements. Debt market size is measured in 1998 U.S. dollars. These figures include short- and long-term external sovereign debt to banks, bonds, and export credits. The last three columns show 1997–98 governance indicators from Kaufmann, Kraay, and Mastruzzi (2003). A higher score indicates greater rule of law, less corruption, and more voice. The ellipsis (...) in the table indicates that there was no relevant publication during the sample period.

[1]Countries that went through any IMF program during the sample period.

opportunity to publish an Article IV did not exist prior to 1999, whether or not a country adopted the reform depended mainly on its existing taste for transparency. This makes it easier to predict which countries will adopt the reform on their first opportunity. A country's predicted probability of publishing when the time comes is proxied by GDP per capita and the size of the debt market in 1998 (in U.S. dollars); the rule-of-law indicators for 1997–98 formulated by Kaufmann, Kraay, and Mastruzzi (2003); and regional dummies. By construction, none of these variables are directly correlated with changes in credit spreads during the sample period.[10] Clearly, the level of GDP per capita in 1998 and the size of debt markets in 1998 are correlated with the *level* of spreads in 1999, but we are using the instruments here to predict the *changes* in credit spreads during, for example, the last quarter of 1999. Any relationship between the level of GDP and the level of spreads is picked up in our country fixed effects.

The instrument for Article IV publication in our 2SLS is the interaction of the months since the last Article IV report and the above predictors of whether the country is likely to publish at its first opportunity. Because the instruments predict both *who* will publish and *when*, both types of endogeneity are addressed. The first-stage results are reported in Table 2.

Let us describe what these instruments (fitted transparency indicators) for Article IV publication look like in practice. Poland, which had its last Article IV consultation before the transparency reform in March 1999, is predicted to have its first opportunity to publish in the second quarter of 2000. The predicted publication measure is therefore low through mid-2000, when it jumps to 98 percent. The big jump reflects the fact that Poland is rich, is European, and has good rule-of-law indicators that all suggest a high probability of reform adoption. The fitted transparency indicator stays high and does not jump again at the second opportunity to publish (predicted to be in the second quarter of 2001). This predicted pattern closely mirrors actual events, with Poland publishing at its first opportunity on March 31, 2000, which closely followed the standard timetable. There is no change in the actual transparency measure from then on because the decision has already been made. Russia, which had its last pre-reform consultation on July 29, 1999, is predicted to have its first opportunity to publish in the third quarter of 2000. The fitted transparency indicator jumps less than for Poland, reaching 75 percent probability of publishing. At the second predicted opportunity to publish, the fitted measure jumps again to 1. In reality, Russia did take its first opportunity to publish, and the timing again closely (though not exactly) followed the preset timetable, with Russia publishing in November 2000. Because the initial Board meetings for the countries in our sample were evenly distributed throughout the

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[10]We verified that, in our data, there is indeed no systematic relationship between these instruments and changes in credit spreads over the sample period.

year, the jumps in the fitted transparency indicators are similarly well spread out over the year.

ROSCs were designed to be written and released (with the country's permission) at the same time as Article IV reports. We can therefore use the same strategy for estimating the impact of releasing them as for Article IV reports. Because not all ROSCs were in fact released at the same time as the Article IV reports—for example, some were written by the World Bank, which followed its own schedule—the time since the previous Article IV report is a less accurate predictor of when a ROSC is released. This means that the instrument is somewhat weaker than is the case for Article IV reports, but the estimate is still unbiased.

The instrument for SDDS compliance is constructed in a way similar to that for Article IVs and ROSCs. In this case, we base our instrument on the time elapsed since a country made a commitment to comply with the requirements of the SDDS (the subscription date) as long as the subscription predated our sample period. Countries made this initial commitment at different points in time. There were also differences in how long it took countries to meet all the requirements after they had made a commitment to do so. Richer countries and ones that already produced more statistics tended to complete the process more rapidly. Our instrument for SDDS in the 2SLS is the interaction of the time since a country committed to come into compliance with the SDDS and time-invariant factors (such as income in 1998) that will predict how long the process will take. By construction, neither the timing of the original commitment (which predates our sample period) nor these time-invariant factors are directly correlated with changes in spreads during our sample period. Again, income in 1998 is of course correlated with the level of spreads, but our identification is based on changes over time.

Estimation

We run a series of fixed-effect panel OLS and 2SLS to estimate the difference in countries' mean credit spreads before and after the changes in their transparency level. Our baseline estimation equation is

$$\ln spread_{i,t} = \alpha_i + \beta_t Q_t + \gamma_1 Pub_{i,t} + \gamma_2 ROSC_{i,t}$$

$$+ \gamma_3 SDDS_{i,t} + \gamma_4 Pub_{i,t} \times ROSC_{i,t}$$

$$+ \gamma_5 Pub_{i,t} \times SDDS_{i,t} + \gamma_6 SDDS_{i,t} \times ROSC_{i,t} + \varepsilon_{i,t}. \tag{1}$$

Credit spreads are in basis points (1 percentage point = 100 basis points). In the data, countries with higher average spreads exhibit more volatility as well. In order to capture the effect of transparency on countries with low average spread and low volatility, and to contain the influence of possible outlier events, we choose to use log spreads as the dependent variable. As a robustness check, we also use the levels of spreads, and we

find that the overall estimation results do not change qualitatively—if anything, the estimated effect of transparency is more significant with levels than with logs. Estimation (6) in both Tables 3 and 4 is done with the level of spreads.

The functional form reflects our hypothesis that the transparency reforms have long-run effects on the level of credit spreads. Because there is no compelling theoretical basis for modeling a more complex transition trajectory, we opt for this simple and robust functional form.

The subscripts i and t, respectively, index countries and quarters. The country effect (α_i) picks up each country's characteristics that are constant through the sample period. Quarter dummies (Q_t) capture the common movements in credit spreads across all the countries in the sample.[11]

$Pub_{i,t}$ is a 0–1 dummy equal to 1 for the quarters following the first Article IV report publication for the country i. For the quarter in which a reform event takes place, the dummy is 1 if the event comes in the first half of the quarter, and 0 if in the second half. Of course, in the 2SLS, $Pub_{i,t}$ is replaced with the predicted probability of publishing in that quarter from the first-stage regression. $ROSC_{i,t}$ and $SDDS_{i,t}$ are similarly defined for the first publication of ROSCs and coming into compliance with the SDDS. Note that these 0–1 dummies start out as 0, and are nondecreasing over time. This construction is consistent with the fact that no country has adopted a reform and then reversed it. A decision to be transparent, the data suggest, represents a long-term commitment.

We also include interaction terms between the transparency indicators. These terms allow the impact of one transparency indicator to depend on the existing level of transparency. In the main specification, $Pub_{i,t}$, $ROSC_{i,t}$, and $SDDS_{i,t}$ represent the existing transparency for one another. In alternative specifications, the measure of existing transparency is broadened to include Kaufmann, Kraay, and Mastruzzi's (2003) rule-of-law indicators and Transparency International's Corruption Perception Index.

It is known that using inconsistent standard error estimates tends to exaggerate the significance of the treatment effects in many differences-in-differences settings (Bertrand, Duflo, and Mullainathan, 2004). When reporting the estimated coefficients, we use kernel-based standard errors that are robust to arbitrary forms of heteroscedasticity and autocorrelation at the country level.[12]

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[11]We also consider an alternative specification where we use quarter-region dummies, rather than just quarter dummies. Because we have four regions (Asia, Europe, Latin America, and Africa/Middle East) and 14 quarters, we lose quite a lot of degrees of freedom without much gain in explanatory power. Standard errors increase substantially, and we lose significance of some coefficients, although the overall conclusion does not change. All our reported estimation results (Tables 3, 4, and 5) allow for country fixed effects and quarter dummies.

[12]In practice, we use the Bartlett kernel (Newey-West) along with the "robust" option in Stata.

Table 2. Two-Stage Least Squares: First-Stage Regression

Regressor	*Pub*	*ROSC*	*SDDS*	*Pub* × *SDDS*	*Pub* × *ROSC*	*SDDS* × *ROSC*
$1P$	0.44 (0.43)	2.20 (0.49)‡	1.31 (0.67)†	0.34 (0.38)	0.28 (0.32)	1.20 (0.49)‡
$1P \times Eu$	1.22 (0.12)‡	−0.21 (0.23)	0.52 (0.18)‡	0.54 (0.15)‡	0.01 (0.20)	−0.32 (0.19)†
$1P \times LA$	0.56 (0.09)‡	−0.47 (0.11)‡	0.07 (0.13)	0.58 (0.10)‡	0.01 (0.04)	−0.36 (0.10)‡
$1P \times ME$	−0.23 (0.16)	−0.54 (0.20)‡	0.11 (0.25)	−0.09 (0.12)	−0.27 (0.11)‡	−0.30 (0.25)
$1P \times debt$	−0.05 (0.04)	−0.20 (0.05)‡	−0.12 (0.06)†	−0.03 (0.04)	−0.02 (0.03)	−0.09 (0.05)†
$1P \times Law$	0.21 (0.17)	0.16 (0.16)	−0.66 (0.28)‡	−0.01 (0.16)	0.32 (0.13)‡	0.11 (0.17)
$1P \times Vce$	−0.02 (0.09)	0.37 (0.12)‡	−0.05 (0.14)	−0.08 (0.08)	0.10 (0.05)‡	0.16 (0.10)
$1P \times Crpt$	−0.05 (0.21)	−0.20 (0.26)	1.47 (0.39)‡	0.47 (0.23)‡	−0.40 (0.19)‡	0.07 (0.24)
$2P$	−98.8 (14.8)‡	105. (21.5)‡	63.4 (18.8)‡	−27.9 (16.2)†	−38.2 (11.0)‡	73.9 (17.7)‡
$2P \times Eu$	−3.01 (0.67)‡	6.52 (0.68)‡	4.28 (0.60)‡	2.56 (0.64)‡	2.39 (0.72)‡	5.06 (0.64)‡
$2P \times As$	−6.42 (0.84)‡	4.30 (1.13)‡	2.39 (1.06)‡	−3.13 (0.96)‡	−3.72 (0.56)‡	2.79 (0.94)‡
$2P \times LA$	−0.67 (0.71)	5.45 (0.58)‡	3.24 (0.62)‡	3.77 (0.68)‡	4.09 (0.61)‡	4.25 (0.55)‡
$2P \times ME$	−8.07 (1.49)‡	2.80 (1.85)	2.14 (1.79)	−6.24 (1.69)‡	−7.60 (0.99)‡	1.75 (1.55)
$2P \times Law$	6.59 (1.22)‡	−2.34 (1.55)	−1.74 (1.48)	5.55 (1.39)‡	6.70 (0.81)‡	−1.78 (1.28)
$2P \times gdp$	26.8 (4.05)‡	−26.0 (5.82)‡	−15.7 (5.15)‡	8.76 (4.51)†	11.8 (2.92)‡	−18.3 (4.79)‡
$2P \times debt$	0.19 (0.08)‡	−0.53 (0.08)‡	−0.53 (0.06)‡	−0.11 (0.07)	0.03 (0.09)	−0.47 (0.08)‡
$2P \times Crpt$	−4.01 (0.57)‡	5.19 (0.85)‡	3.06 (0.77)‡	−1.67 (0.66)‡	−1.24 (0.44)‡	4.31 (0.71)‡
$2P \times Vce$	−1.27 (0.37)‡	−0.56 (0.39)	−1.08 (0.39)‡	−2.16 (0.40)‡	−2.09 (0.27)‡	−0.94 (0.34)‡
$2P \times debt^2$	0.62 (0.17)‡	0.34 (0.15)‡	−0.12 (0.17)	0.39 (0.18)‡	0.68 (0.12)‡	0.30 (0.13)‡
$2P \times gdp^2$	−1.83 (0.28)‡	1.62 (0.40)‡	1.02 (0.36)‡	−0.68 (0.32)‡	−0.94 (0.20)‡	1.15 (0.33)‡
$2P \times Law^2$	6.04 (1.44)‡	0.90 (1.58)	−0.43 (1.57)	6.42 (1.61)‡	6.97 (1.03)‡	1.07 (1.35)
$2P \times Vce^2$	1.13 (0.17)‡	−1.38 (0.26)‡	−0.74 (0.21)‡	0.41 (0.18)‡	0.34 (0.14)‡	−0.77 (0.20)‡
$2P \times Crpt^2$	−1.57 (0.93)†	−3.85 (0.90)‡	−1.72 (0.93)†	−5.03 (0.96)‡	−4.35 (0.79)‡	−2.77 (0.85)‡
$Tsub$	0.05 (0.04)	−0.25 (0.07)‡	−0.23 (0.08)‡	0.06 (0.03)†	−0.04 (0.03)	−0.22 (0.07)‡
$Tsub \times As$	0.01 (0.01)	−0.04 (0.02)†	0.02 (0.02)	0.01 (0.01)	−0.03 (0.01)‡	−0.05 (0.02)‡
$Tsub \times Eu$	0.01 (0.01)	0.00 (0.01)	−0.09 (0.02)‡	−0.01 (0.02)	0.03 (0.03)	−0.01 (0.03)
$Tsub \times Law$	−0.02 (0.01)†	0.02 (0.02)	0.04 (0.03)	0.00 (0.01)	0.01 (0.01)	0.04 (0.02)†
$Tsub \times debt$	0.00 (0.00)	0.02 (0.01)‡	0.03 (0.01)‡	0.00 (0.00)	0.00 (0.00)	0.02 (0.01)‡
$Tsub \times Vce$	−0.02 (0.01)‡	−0.02 (0.01)†	0.04 (0.02)‡	−0.01 (0.01)	0.00 (0.00)	0.00 (0.01)
$Tsub \times Crpt$	0.04 (0.02)‡	0.01 (0.03)	−0.11 (0.05)‡	0.01 (0.02)	0.01 (0.01)	−0.01 (0.02)
Observations	322	322	322	322	322	322
R^2	0.85	0.79	0.81	0.80	0.81	0.77
Joint F-statistic	17.7	22.1	37.1	61.7	16.4	21.9

Source: Authors' calculations.

Note: Dependent variable: the measures of transparency (publication of Article IV report, Report on Observance of Standards and Codes, and Special Data Dissemination Standard— *Pub*, *ROSC*, *SDDS,* respectively) and their interaction terms. $1P$: first opportunity to publish; $2P$: second opportunity to publish; *Eu, LA, ME, As*: regional dummies for Europe, Latin America, Middle East, and Asia, respectively; *Law, Vce, Crpt*: Kaufmann, Kraay, and Mastruzzi (2003) 1997–98 rule-of-law, voice, and corruption indicators; *debt*: log of outstanding sovereign debt stock in 1998 (in U.S. dollars); *gdp*: log of GDP per capita in 1998 (in U.S. dollars); *Tsub*: time elapsed since SDDS subscription. Heteroscedastic and autocorrelation-consistent standard errors are given in parentheses. † and ‡ denote significance at 10 and 5 percent, respectively. Interaction terms with low predictive power have been eliminated. The fitted values range from −0.3 to 1.3, but almost all of them fall between 0 and 1.

III. Empirical Results

Sovereign credit spreads fall with the implementation of all three transparency reforms, although the evidence for ROSCs is weaker than for the other two. The reduction in credit spreads owing to the implementation of the reforms is both statistically and economically significant, especially for those with low levels of initial transparency (Table 3). Because the dependent variable is in natural logs, the coefficient can be read as the percentage change in spreads owing to the adoption of the reforms. In the simplest specification, therefore, a country that starts publishing Article IV reports when it has adopted neither of the other reforms experiences a 25 percent reduction in spreads under the OLS specification and a 39 percent reduction in the 2SLS estimation. The numbers for SDDS adoption—assuming no other reform has yet been adopted—are reductions of 22 and 56 percent, respectively. Although all these coefficients for Article IV publication and SDDS observance are significantly different from 0, the standard errors are reasonably large, and thus the effect is not very precisely estimated. Also, because many countries had previously adopted other reforms before publishing their first Article IV report or meeting all the SDDS requirements, and because the interaction terms are uniformly positive, the mean effect of publishing Article IV and SDDS observance is less than these coefficients would suggest. For the mean country in our sample, once the interactions with other reforms are taken into account, adopting all three reforms would lead to an 11 percent decline in credit spreads under OLS and a 50 percent decline under 2SLS.[13]

To better gauge the practical significance of the coefficients, we do the following rough calculation. In the 2002 data, the median ratio of external debt to GDP for the countries in our sample is about 0.5. The median of the average credit spreads for these countries in 2002 is about 500 basis points. Eleven percent (a conservative estimate of the effect) of 500 basis points is 55 basis points. This reduction in the credit spread for such a median country, in 2002 figures, translates into an annual saving in external debt service tantamount to 0.275 percent of GDP. Note that the spreads on public debt serve as a reference point for those on private debt. In our sample, Morocco is the country closest to the median in both counts. In 2002 figures, Morocco's annual saving from transparency reforms would have been 0.224 percent of its GDP.

The positive interaction terms suggest there is a diminishing marginal benefit to becoming more transparent—in other words, the more transparent a country is to start with, the less benefit it gets from incremental

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[13]We use 2SLS because we want to control for possible endogeneity bias that would exaggerate the effect of the reforms. The actual estimation results, however, are to the contrary. The 2SLS estimates are bigger in magnitude than the OLS estimates. Although we do not have a convincing explanation for this, we would like to emphasize the fact that the 2SLS coefficients are much less precisely estimated than their OLS counterparts, and the difference between them is insignificant.

Table 3. Panel Estimation Results

Regressor	Panel Regression				2SLS			
	(1) ln *spread*	(2) ln *spread*	(3) ln *spread*	(4) ln *spread*	(5) ln *spread*	(6) *spread*	(7) ln *spread*	(8) ln *spread*
Pub	−0.25 (0.12)‡	−0.30 (0.11)‡	−0.38 (0.10)‡	−0.32 (0.19)†	−0.39 (0.20)†	−646 (279)‡	−0.47 (0.20)‡	−0.66 (0.18)‡
ROSC	−0.21 (0.19)	−0.46 (0.97)	0.26 (0.90)	−0.23 (0.14)	−0.84 (0.53)	−1197 (908)	−2.70 (1.65)	1.03 (1.75)
SDDS	−0.22 (0.08)‡	−2.79 (0.73)‡	−4.45 (0.78)‡	−0.18 (0.10)†	−0.56 (0.22)‡	−781 (294)‡	−1.57 (1.20)	−6.74 (1.63)‡
Pub × *SDDS*	0.30 (0.13)‡	0.44 (0.13)‡	0.35 (0.14)‡	0.67 (0.26)‡	0.23 (0.30)	862 (443)†	0.36 (0.29)	−0.01 (0.46)
Pub × *ROSC*	0.12 (0.21)	0.09 (0.22)	0.43 (0.33)	−0.27 (0.13)‡	0.22 (0.48)	587 (867)	0.29 (0.46)	1.13 (0.74)
SDDS × *ROSC*	0.15 (0.16)	−0.01 (0.15)	−0.25 (0.14)†		0.84 (0.37)‡	1296 (550)‡	0.16 (0.39)	−0.09 (0.45)
SDDS × *debt*		0.24 (0.07)‡	0.43 (0.08)‡				0.10 (0.11)	0.71 (0.17)‡
ROSC × *debt*		0.03 (0.09)	−0.07 (0.09)				0.22 (0.15)	−0.25 (0.22)
Sum			2.79 (0.57)‡					5.71 (1.00)‡
SDDS × *Sum*			−0.37 (0.17)‡					−1.16 (0.52)‡
ROSC × *Sum*			0.74 (0.32)‡					1.81 (0.78)‡
Sum × *debt*			−0.27 (0.05)‡					−0.54 (0.09)‡
Number of countries/ observations	23/322	23/322	23/322	7/98	23/322	23/322	23/322	23/322
Joint significance at 5 percent	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.85	0.86	0.88	0.87	0.83	0.56	0.85	0.83

Source: Authors' calculations.

Note: This table shows whether the mean of sovereign credit spread is different before and after the implementation of transparency reforms. Dependent variable: quarterly average of log daily sovereign credit spread, except for column (6), which uses the level of credit spread in basis points. All the measures of transparency (publication of Article IV report, Report on Observance of Standards and Codes, and Special Data Dissemination Standard—*Pub*, *ROSC*, *SDDS,* respectively) are 0–1 dummies. They start out and stay at 0 until the respective reform is first implemented. At this point they turn into 1 and remain unchanged until the end of the sample period. *Sum*, a measure of opacity, becomes 1 following a stand-alone summary publication, which indicates that a country chose not to publish its Article IV report; debt is the log of outstanding sovereign debt stock in U.S. dollars. Negative coefficients on *Pub*, *ROSC*, and *SDDS* imply that the credit spreads fall with transparency reforms. Positive coefficients on their interaction terms point to decreasing marginal benefits from transparency. Heteroscedastic and autocorrelation-consistent standard errors are given in parentheses. † and ‡ denote significance at 10 and 5 percent, respectively. Estimation (4) looks only at nonprogram countries. Estimations (5)–(8) report the second stage results from two-stage least squares (2SLS) estimations. The row "Joint significance at 5 percent" indicates whether all three transparency variables and their interactions are jointly significant at 5 percent. All the estimations allow for country fixed effects and quarter dummies.

transparency. We find the same result when using other indicators of existing transparency, including Transparency International's Corruption Perceptions Index (CPI) and Kaufmann, Kraay, and Mastruzzi's (2003) rule-of-law indicators (not reported here). This inverse relation between the impact of increased transparency and the initial level of transparency echoes the evidence from studies on corporate-level transparency (Leuz and Verrecchia, 2000; and Greenstone, Oyer, and Vissing-Jorgensen, 2006).

An extension of Equation (1) estimates the interaction between the transparency indicators and the size of the debt market. The size of the debt market is our proxy for the extent to which market participants are informed about an economy. It is likely that, in a smaller and less liquid market, a trader's attempt to profit from information advantage would have a bigger impact on prices for a given size of trade—thus transmitting more of the information to other participants. As a consequence, the private sector will have less incentive to conduct private research on a smaller market, and the equilibrium level of knowledge about this economy prior to the reforms will be lower (and the information asymmetry between the country and the market participants more severe). In this context, this interaction term will show whether the impact of transparency reforms depends on the existing level of information available to market participants.

The result for this extended model is as predicted. Countries with larger debt markets see less benefit from adopting the reforms. The impact of SDDS observance on credit spreads is inversely related to the size of the sovereign debt market (Table 3, Estimations 2, 3, and 8). This is consistent with the conjecture that the private sector has less incentive to do its own research in countries with smaller and less liquid debt markets, and hence these markets have less information about country policies and prospects prior to the reforms. SDDS compliance would cut the credit spread by a third for Venezuela, whose level of sovereign debt is close to the overall sample mean. Argentina and Brazil, with the largest debt markets in our sample, would see practically no decline from SDDS compliance.

Another specification includes $Sum_{i,t}$ as regressor, which is a 0–1 dummy that is 1 for the quarters following a stand-alone summary after December 1999. Before December 1999, no country was permitted to publish an Article IV report, and hence a stand-alone summary had not reflected a country's decision to pass up an opportunity to publish such a report. $Sum_{i,t}$ is an indicator for lack of transparency after December 1999 because it signals to the market that an Article IV report was produced but the country failed to publish it.

Credit spreads rise when a country fails to publish an Article IV staff report and issues a stand-alone summary instead (Table 3, Estimations 3 and 8). Faced with the country's decision not to take the opportunity to release more information, market participants reassess the country's sovereign credit risk accordingly. The magnitude of the increase ranges from 10 to 16 percent depending on the specification.

IV. Robustness Checks

Behavior of Credit Spreads Around Reform Dates

Figure 1 is similar to a typical event-study plot, showing how the spreads behave before and after a reform adoption. For this figure, we use monthly credit spreads rather than quarterly, to give a better sense of how the spreads react to the reforms in the short term. It is important to understand that the event dates are different across countries. On the horizontal axis, 0 denotes the month in which a country's reform takes place. To the left of 0 are the months preceding the reform, and to the right are those following the reform.

We first run an unweighted panel regression of log monthly credit spreads only on country fixed effects. We line up the residuals around their respective reform dates, for three months preceding the reform (-3, -2, -1) and three months following the reform (0, 1, 2), and plot their averages across countries. The left panel plots such averages around Article IV report publications (12 countries/events). The center and right panels are, respectively, for ROSC publications (11 countries) and for SDDS compliances (14 countries).

From the figure, Article IV publication and SDDS compliance give an impression of a clean break in both levels and trends. For ROSC, credit spreads decline the month before the reform (between -2 and -1), but not the month before that (between -3 and -2). Thus, Figure 1 suggests that our results do not appear to be driven by a preexisting monotonic trend around the reform dates.[14]

Estimation with a Simpler Measure of Transparency

Our baseline specification (Equation (1)) allows us to capture the effect of individual transparency reforms, but the presence of interaction terms may hinder a straightforward interpretation of the estimates. To come up with a simpler measure of the effect of transparency, we consider an alternative specification, where at any given point a country is either transparent—meaning it has adopted at least one of the three reforms—or not. The estimation equation is

$$\ln spread_{i,t} = \alpha_i + \beta_t Q_t + \gamma TP_{i,t} + \varepsilon_{i,t}, \qquad (2)$$

where $TP_{i,t}$ is a 0–1 dummy that takes the value of 1 as soon as any of the three reforms is adopted. That is, $TP_{i,t} \equiv \max\{Pub_{i,t}, ROSC_{i,t}, SDDS_{i,t}\}$.

The estimation results of Equation (2) are in Table 4. We see that adopting at least one of the three transparency reforms lowers credit spreads.

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[14]Even if such a trend existed, it would not be a simple time trend, because the reform dates (0) are different for different countries.

Figure 1. Average Credit Spread in Months Around Transparency Reform
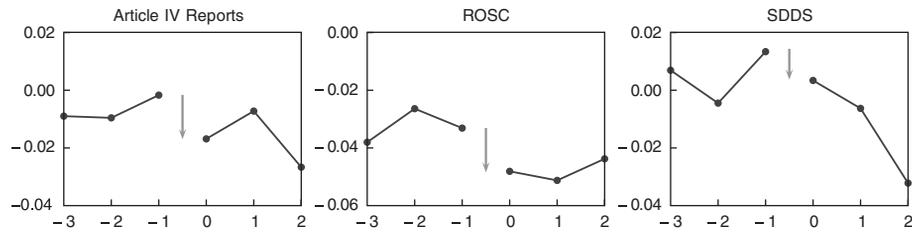
Source: Authors' calculations using EMBI credit spreads.
Note: ROSC = Report on Observance of Standards and Codes; SDDS = Special Data Dissemination Standard.

Table 4. Robustness Check: Combined Transparency Index

Regressor	Panel Regression				2SLS			
	(1) ln *spread*	(2) ln *spread*	(3) ln *spread*	(4) ln *spread*	(5) ln *spread*	(6) *spread*	(7) ln *spread*	(8) ln *spread*
TP	−0.26 $(0.08)^{\ddagger}$	−0.41 (0.52)	−1.21 $(0.70)^{\dagger}$	−0.24 $(0.12)^{\dagger}$	−0.55 $(0.18)^{\ddagger}$	−867 $(308)^{\ddagger}$	−0.18 (0.61)	−3.06 $(1.11)^{\ddagger}$
TP × *debt*		0.01 (0.05)	0.09 (0.07)				−0.04 (0.06)	0.21 $(0.11)^{\dagger}$
Sum			1.6 $(0.71)^{\ddagger}$					3.58 $(1.10)^{\ddagger}$
TP × *Sum*			0.18 (0.12)					0.73 $(0.33)^{\ddagger}$
Sum × *debt*			−0.16 $(0.07)^{\ddagger}$					−0.38 $(0.10)^{\ddagger}$
Number of countries/ observations	23/322	23/322	23/322	7/98	23/322	23/322	23/322	23/322
R^2	0.85	0.85	0.86	0.86	0.84	0.56	0.84	0.82

Source: Authors' calculations.
Note: This table shows whether the mean of sovereign credit spread is different before and after the implementation of a country's *first* transparency reform. Dependent variable: quarterly average of log daily sovereign credit spread, except for column (6), which uses the level of credit spread in basis points. The combined transparency index, *TP*, is a 0–1 dummy. It starts out and stays at 0 until one of the three transparency reforms is first implemented. At this point it becomes 1 and remains unchanged, regardless of further transparency reforms, until the end of the sample period. In other words, *TP* = max{*Pub*, *ROSC*, *SDDS*}. *Sum*, a measure of opacity, turns into 1 following a stand-alone summary publication, which indicates that a country chose not to publish its Article IV report; *debt* is the log of outstanding sovereign debt stock in U.S. dollars. A negative coefficient on *TP* implies that the credit spreads fall with the first transparency reform, regardless of any subsequent reform. Heteroscedastic and autocorrelation-consistent standard errors are given in parentheses. † and ‡ denote significance at 10 and 5 percent, respectively. Estimation (4) looks only at nonprogram countries. Estimations (5)–(8) report the second-stage results from two-stage least squares (2SLS) estimations. All the estimations allow for country fixed effects and quarter dummies.

Macroeconomic Variables

If our identification strategy is correct, the timing of reform events is orthogonal to countries' concurrent macroeconomic developments, and we need not control for macroeconomic factors. To the extent that transparency has its impact through changes in macroeconomic policies (as discussed in the introduction), to add macro variables as controls would be to over-control and lead to biased coefficients on the transparency variables. Nevertheless, in a series of robustness checks, we include as regressors the following macroeconomic variables to Equation (1): inflation, current account balance (as percent of GDP), and fiscal balance (as percent of GDP). Data on other possible determinants of credit spreads are either unavailable at quarterly frequency (for example, debt level and debt service), or are likely to respond within the quarter to the decision to become transparent.[15] The fact that we are not able to include other macro variables as controls is an indication of just how few data were available on emerging market countries at quarterly frequency before the introduction of the SDDS.

Note that we lose many observations as we drop countries that do not have data on inflation, current account, or fiscal balance. The inclusion of these variables slightly alters the estimated effects of transparency, and the standard errors increase. However, macroeconomic variables do not negate the significance of transparency variables completely (Table 5).

Autocorrelated Dependent Variable

In the estimation, we could not reject the presence of an AR(1) component in the residuals, which reflects omitted variables that are persistent.[16] However, given the potential for bias when dynamics are included in a panel with a relatively short length, we do not include lags in the baseline specification. Given the exogenous nature of the timing of our variables of interest, there is no reason to think that omitting a lag term will bias our coefficients of interest. We do, however, check whether the results are robust to the inclusion of a lagged dependent variable along the lines of Arellano and Bond (1991).

Although the inclusion of a lagged dependent variable dilutes the estimated effects somewhat, the results are not overturned (Table 5, Estimation 8). The more the regressors (for example, macro variables and other country characteristics) we put into the regression, the lower the

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[15]For example, one consequence of coming into compliance with the SDDS is to change the definition of official reserves. It would therefore be inappropriate to include reserve figures as a control.

[16]The average (across the 23 countries in our sample) AR(1) coefficient of the dependent variable (credit spread) is 0.70. The average AR(1) coefficient for the residuals from Equation (1) is 0.41. As mentioned in the third part of Section II, we use heteroscedastic and autocorrelation-consistent standard errors for statistical inference.

Table 5. Robustness Check: Macroeconomic Variables

Regressor	(1) ln *spread*	(2) ln *spread*	(3) ln *spread*	(4) ln *spread*	(5) ln *spread*	(6) ln *spread*	(7) ln *spread*	(8) ln *spread*
Pub	−0.15	−0.15	−0.31	−0.24	−0.34	−0.28	−0.22	−0.14
	(0.11)	(0.10)	$(0.19)^\dagger$	(0.16)	$(0.19)^\dagger$	$(0.16)^\dagger$	$(0.14)^\dagger$	$(0.07)^\dagger$
ROSC	−0.17	−0.11	−0.04	−0.04	−0.06	−0.07	0.07	0.71
	(0.20)	(0.20)	(0.22)	(0.22)	(0.25)	(0.24)	(0.74)	(0.49)
SDDS	−0.15	−0.17	−0.06	−0.08	−0.09	−0.13	−5.10	−2.19
	$(0.08)^\dagger$	$(0.07)^\ddagger$	(0.08)	(0.07)	(0.10)	(0.08)	$(1.07)^\ddagger$	$(0.57)^\ddagger$
Pub × *SDDS*	0.26	0.27	0.40	0.31	0.42	0.33	0.38	0.24
	$(0.13)^\ddagger$	$(0.12)^\ddagger$	$(0.18)^\ddagger$	$(0.16)^\dagger$	$(0.19)^\ddagger$	$(0.16)^\ddagger$	$(0.15)^\ddagger$	$(0.10)^\ddagger$
Pub × *ROSC*	0.09	0.04	0.16	0.10	0.17	0.11	0.25	0.00
	(0.21)	(0.20)	(0.23)	(0.20)	(0.26)	(0.23)	(0.25)	(0.12)
SDDS × *ROSC*	0.17	0.09	0.06	0.00	0.04	−0.06	−0.32	−0.24
	(0.16)	(0.17)	(0.18)	(0.18)	(0.19)	(0.19)	$(0.14)^\ddagger$	$(0.10)^\ddagger$
Inflation		1.27		1.09		1.06	0.92	0.00
		$(0.23)^\ddagger$		$(0.25)^\ddagger$		$(0.24)^\ddagger$	$(0.21)^\ddagger$	(0.12)
Current account				0.43		0.48	0.33	−0.05
				(0.28)		$(0.29)^\dagger$	(0.28)	(0.12)
Fiscal balance						−1.04	−0.78	−0.15
						$(0.41)^\ddagger$	$(0.28)^\ddagger$	(0.21)
Sum							4.72	0.49
							$(1.31)^\ddagger$	(0.55)
SDDS × *Sum*							−0.43	−0.14
							$(0.17)^\ddagger$	(0.10)
ROSC × *Sum*							0.59	0.04
							$(0.23)^\ddagger$	(0.15)
Pub × *Law*							0.57	−0.14
							$(0.23)^\ddagger$	(0.12)
SDDS × *Law*							−0.54	0.06
							$(0.21)^\ddagger$	(0.11)
Sum × *Law*							0.68	0.07
							$(0.29)^\ddagger$	(0.16)
SDDS × *debt*							0.49	0.21
							$(0.10)^\ddagger$	$(0.05)^\ddagger$
ROSC × *debt*							−0.03	−0.05
							(0.07)	(0.05)
Sum × *debt*							−0.43	−0.03
							$(0.12)^\ddagger$	(0.05)
ln *spread*(−1)								0.78
								$(0.07)^\ddagger$
Observations	308	308	252	252	210	210	221	221

Source: Authors' calculations.

Note: To check for the robustness of the baseline estimation results (Table 3), we estimate other specifications that include macroeconomic variables and/or lagged dependent variables. Because not all countries' data on inflation, current account, and fiscal balance are available, each estimation has a different number of observations. The estimation result with inflation (Regression 2) is reported alongside the baseline specification result for the same set of observations (Regression 1). The same applies to Regressions (3)–(4) and (5)–(6). Current account and fiscal balance are given as a percentage of GDP. Inflation, current account, and fiscal balance figures are then scaled by 1,000, with corresponding impact on the coefficients. Regression (8) is the one with lagged dependent variables along the lines of Arellano and Bond (1991). Heteroscedastic and autocorrelation-consistent standard errors are given in parentheses, except for Regression (8). † and ‡ denote significance at 10 and 5 percent, respectively. All the estimations allow for country fixed effects and quarter dummies.

coefficient on the lag becomes, suggesting that the lag is at least partly proxying for other slow-moving determinants of credit spreads. The coefficients on the lag in these different specifications range from 0.75 to 0.85.[17]

Falsification Test with Randomized Reform Dates

As a further robustness check, we examine whether our estimation strategy is a valid test for the significance of the transparency reforms. In particular, we randomly assign reform dates to countries and generate a fictitious data set of reform dates. We create 1,000 such fictitious data sets. For each of these randomly generated data sets, we estimate our statistical model (Equation (1)) using 2SLS, the specification of Estimation (5) (Table 3). Note that, for all other variables than the reform indicators, we use the actual data. If our estimation strategy is valid for detecting the effect of transparency reforms, then, roughly speaking, any particular γ in Equation (1) should be significant at the 5 percent level about 50 times out of 1,000.[18]

From these 1,000 estimations of the fictitious data sets with randomized transparency reform dates, $Pub_{i,t}$, $ROSC_{i,t}$, or $SDDS_{i,t}$ was individually significant at the 5 percent level about 17 percent of the time. In 81 out of 1,000 times, any two of the three were significantly different from 0 at the 5 percent level. Only in eight out of 1,000 times were all three significantly different from 0 at the 5 percent level. Our estimation result with the actual data had two of the three, $Pub_{i,t}$ and $SDDS_{i,t}$, significant at the 5 percent level. Thus, these trials with random reform dates suggest that there is less than a 9 percent chance that our result is an outcome of pure coincidence, and that our estimation strategy performs reasonably well in detecting the effect of the transparency reforms.

In a similar type of falsification test, we use a variation of Equation (2) and the actual data, but we redefine $TP_{i,t}$ to capture a neighboring country's transparency reform. That is, now $TP_{i,t}$ goes from 0 to 1 as soon as any country in country i's region adopts any of the three reforms. If a country's credit spread responds only to its own reforms, the coefficient on this redefined $TP_{i,t}$ should be statistically insignificant. We run OLS (with country fixed effect and quarter dummies) and the coefficient is indeed insignificant—an estimate of 0.03 and a standard error of 0.09. This

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[17]Only one of the various specifications involving a lagged dependent variable is reported in Table 5.

[18]Bertrand, Duflo, and Mullainathan (2004) document that differences-in-differences studies often yield too many rejections of the null. For the models that they surveyed, the rejection rate (at the 5 percent significance level) from such random trials is almost 50 percent, casting serious doubts on the validity of the estimation strategies that they surveyed. They show that the high rejection rates are due to the use of inconsistent standard error estimates. Note that we use standard errors that are heteroscedastic and autocorrelation-consistent to address this concern.

falsification test suggests that our result is unlikely to be an outcome of some regional trends.

V. Concluding Remarks

This paper studies whether increased transparency lowers the cost of borrowing in sovereign bond markets. We exploit a unique data set generated when the IMF instituted a series of reforms to promote macroeconomic transparency. The actual implementation procedure provided variation in the timing of the reforms that was uncorrelated with contemporaneous country events. We find that those countries that adopted reforms experienced a structural downward shift in their credit spreads. We further find that there is a diminishing marginal benefit of transparency; that is, countries that start out the least transparent gain the most through the reforms. In addition, there is evidence that increased transparency is particularly beneficial for countries with smaller and less liquid debt markets where, we conjecture, market participants have less information initially.

Our results suggest that countries were able to influence their borrowing costs by changing the accuracy and quantity of information available to market participants. Although we focus on countries' compliance with the IMF transparency initiatives, the adoption of the reforms boils down to governments changing their own disclosure policies, and thus our results may have wider applicability.

Given the magnitude of the reduction in borrowing costs we observe, it is natural to ask why all governments do not adopt these reforms. One answer is that a country weighs the benefit of lower borrowing costs against the loss of discretion on the part of policymakers that comes with more transparency. Alesina and others (1999) make this point with respect to lack of transparency in fiscal policy, but it may well equally apply to other policies. Another answer is that some forms of transparency (such as compliance with the SDDS) require collecting additional data and incur extra administrative expenses that will vary by country.

It should be noted that, by now, most countries that borrow from the international capital markets have adopted these reforms. As of February 2007, all advanced countries and 90 percent of all IMF member countries had published Article IV reports, and most of those eligible had complied with the SDDS.

APPENDIX I

IMF Documents and Their Publication Procedure

The standard timetable for the discussion and publication of IMF country documents is, in most cases, unaffected by events in the country. Section II explains the circumstances under which exceptions are made. The timetable also governs the intermediate steps in the production of the reports. Upon returning from a visit to the country, the staff members draft a report and circulate it to the Executive Board at least three weeks prior

to the Board discussion. Following the Board discussion of the report, minor modifications may be made at the request of the country concerned. In most cases, the request is about factual corrections and the deletion of highly market-sensitive items. Any deletion must clear an internal review procedure.

Contingent on the publication decision of the country, the report is made public usually within 10 days of the Board discussion. In most cases, a bundle of documents, such as an Article IV report, a summary, a press release, and a statement by the Executive Director, are posted together on the IMF's website.

Article IV Staff Reports and Background Documents

An Article IV report is about 20–30 pages long and contains a description of recent economic developments, a short-term projection, and policy suggestions. The report does not assign an explicit grade or rating on the country's performances or policies. If the country opts to publish the report, it can have a rejoinder published alongside the report. This "right of reply" takes the form of a short statement by the Executive Director representing the country.

Background documents to an Article IV report include statistical appendixes, detailed descriptions of the country's institutional changes, and analytical studies such as the estimation of the potential growth rate or the equilibrium exchange rate.

Program Documents

Program documents are produced at a half-year interval for the countries that have an arrangement (i.e., conditional loans) with the IMF. Some simply state the timing, amount, and conditions of the arrangement (Letters of Intent, Memoranda on Economic and Fiscal Policy, and Technical Memoranda of Understanding). Others evaluate whether the country has met its targets under the arrangement and then recommend whether it should receive the next tranche of the loan. However, the key points of these assessments reach the public prior to the actual publication of the documents. There are cases in which program review documents are combined with an Article IV staff report.

Public Information Notices and Press Releases

Public information notices and press releases are short summaries of the Executive Board discussions of an Article IV report and program review documents, respectively. They are based on the staff reports discussed and modified to reflect the comments from the Executive Directors.

Staff Concluding Statements

At the end of an Article IV consultation or a program review, the staff members prepare a concluding statement that can be made public at the country's discretion. The staff may also hold a press conference at the end of the mission. Concluding statements are a few pages long and provide a summary assessment of the economy. For program reviews, the statement will indicate whether an agreement on an arrangement has been reached in principle. It will state the amount and timing of the loan from the IMF and the main conditions of the program. These tentative agreements are rarely overturned by senior management or the Executive Board of the IMF.

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