Bad governance: How privatization increases corruption in the developing world

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Abstract

International organizations have become key actors in the fight against corruption. Among these organizations, the International Monetary Fund (IMF) maintains a powerful position over borrowing countries in its ability to mandate far-ranging policy reforms—so-called 'conditionalities'—in exchange for access to financial assistance. While IMF pressure can force the implementation of anti-corruption policies, potentially reducing corruption, other IMF policy measures such as the privatization of state-owned enterprises can create rent-extraction opportunities and limit the capacity of state institutions to limit corrupt behavior. To test these mechanisms, we conduct instrumental-variable regression analysis using an original dataset on IMF conditionality for up to 141 developing countries from 1982 to 2014. We find that conditions to privatize state-owned enterprises exert large detrimental effects on corruption control. Conversely, other areas of IMF intervention are not consistently related to corruption abatement. These findings offer policy lessons regarding the design of conditionality, which should avoid large-scale privatization, especially under conditions of weak accountability.

1. Introduction

Corruption—the misuse of public office for private gain (World Bank 1997, 8)—is widely considered an obstacle to development, as it dampens economic growth and lowers the quality of public service provision, with follow-on implications for education, health, and the environment (Mauro 1995; Persson and Rothstein 2015; Welsch 2004). Consequently, it is no surprise that corruption abatement has moved center-stage in international policy debates. Among the international organizations promoting anti-corruption reforms, the International Monetary Fund (IMF)—an international financial institution (IFI) mandated to uphold global financial stability— holds an influential position. In its capacity as a global lender of last resort, the IMF provides financial assistance to countries in exchange for the introduction of far-ranging policy reforms (so-called 'conditionalities'). These reforms require governments to cut public expenditure, privatize state-owned assets, liberalize markets, and deregulate prices (Babb and Kentikelenis 2018; Copelovitch 2010; Stone 2002).

While the primary rationale of such reforms is to foster economic growth, a secondary objective is to promote, broadly, 'good governance' and, more narrowly, to abate corruption by eliminating rent-seeking opportunities (Abed and Davoodi 2000; Abed and Gupta 2002; Gaspar and Hagan 2016; IMF 2017; Tanzi 1998). IMF country staff have argued that their "policies to promote deregulation, liberalization, and privatization have aimed at creating an environment less conducive to corruption" (Wolf and Gürgen 2000). Furthermore, IMF researchers have argued that "mediocre growth" in the Middle East and Central Asia was attributable in part to corruption, which, in turn, was rooted in large "patronage-ridden state-owned enterprises, complex business regulations and tax codes, and bureaucratic red tape" (Mitra et al. 2016, 22). In a recent IMF policy report on 'The Role of the Fund in Governance Issues', the Fund reaffirms its critical view on state-owned enterprises with "governance structures" prone to "political interference" (IMF 2017, 18).

While the view that market-liberalizing reforms can abate corruption has gained currency among IMF policymakers, the underlying evidence base is thin. At best, the track-record of market-liberalizing reforms in abating corruption is mixed. Some studies find that free-market policies

like trade openness and market deregulation are associated with lower corruption (Gerring and Thacker 2005; Hopkin and Rodríguez-Pose 2007; Sandholtz and Gray 2003). However, a significant body of research also demonstrates the corruption-inducing impact of another common market-liberalizing reform: the privatization of state-owned enterprises. In post-Soviet transition countries, IMF programs mandated mass privatizations which increased corruption, as managers with good connections to the authorities appropriated many of the former public enterprises (Hamm, King, and Stuckler 2012; Stone 2002; Wedel 2001). Similar cases of increased corruption in the wake of privatization were documented in Latin America, Sub-Saharan Africa, and East Asia (Manzetti and Blake 1996; Mwenda and Tangri 2005; Painter 2005).

In light of this evidence, we scrutinize the corruption-abating effect of privatization, using new data and advanced statistical methods to complement existing research based on case studies. We argue that privatization generates new opportunities for corruption, specifically in an environment of asymmetric information in which insiders can exploit their information advantage to acquire newly-privatized state assets, to the detriment of bidders who lack informal contacts to privatization decision-makers (Martimort and Straub 2009). As this quid-pro-quo—insiders obtaining favorable treatment in the bidding process in exchange for bribing officials with decision-making authority—is illegal, the actors involved have incentives to cover the trails of corrupt activities. In particular, they have incentives to weaken institutions which threaten to uncover their corrupt activities, unwind corrupt deals, and impose additional punishments. Yet, if enough individuals have been corrupt, they make others more easily corruptible because the returns to honest behavior in an environment of weak institutions have decreased. The result is a vicious spiral of increased corruption and weakening institutions (Hoff and Stiglitz 2005).

We test our argument using panel data for up to 141 countries in the 1982-2014 period. To count the number of privatization conditions in IMF programs, we use a new dataset on IMF conditionality from 1980 to 2014 based on original coding of the Letters of Intent and associated Memoranda of Economic and Financial Policies between the Fund and its borrowers (Kentikelenis, Stubbs, and King 2016). In addition, this dataset includes information on all major areas of market-liberalizing reform, which enables testing of more hypotheses compared to narrower datasets (Rickard and Caraway 2018; Wei and Zhang 2010; Woo 2013).

Methodologically, we rely on an instrumental-variable research design to account for potential endogeneity of IMF conditionality, while at the same time correcting for potential bias arising from non-random selection into IMF programs (Stubbs et al. 2018). This methodological strategy allows for a causal interpretation of our main results. We find that privatization conditions have a large corruption-inducing effect. This effect holds for alternative indicators of corruption, such as the International Country Risk Guide (ICRG) corruption control index and the Varieties of Democracy (VDem) corruption index. Conversely, the evidence on conditions aimed explicitly at reducing corruption is less conclusive. While market-liberalizing reforms may reduce corruption in the short term, they tend to be detrimental in the long term. Overall, these results challenge the IMF's 'house view' that structural conditions are uniformly beneficial for corruption abatement.

Our article contributes to several key debates in social-scientific research. In particular, it contributes to cross-country large-N studies examining determinants of corruption. Most studies focus on slow-moving factors (Fréchette 2006; Jetter, Agudelo, and Hassan 2015; Treisman 2000), while others emphasize market-liberalizing policies (Gerring and Thacker 2005; Hopkin and Rodríguez-Pose 2007; Sandholtz and Gray 2003). We extend this line of inquiry by adding the role of IFIs as agents of market-liberalizing policy reform. By focusing on privatization, we further disaggregate market-liberalizing policies. Our study also holds promise for stronger inference by focusing on those policy reforms mandated by powerful IFIs rather than chosen by governments for unobserved reasons.

Our article also relates to studies on corruption in particular geographical regions, including Central and Eastern Europe (Batory 2012; Dimitrova 2010; Falkner and Treib 2008; Fazekas and King 2018) as well as Latin America (Manzetti and Blake 1996; Martimort and Straub 2009; Martinez-Gallardo and Murillo 2011). Notwithstanding distinct regional variation in patterns of corruption control, our results suggest a universal mechanism by which IFI-mandated privatization shocks let individuals reconsider the benefits and costs of corruption and their preferences for weak institutions. The results also suggest that IMF practice did little to change prevailing norms with a view to delegitimize corruption. To the contrary, the Fund long pushed for privatization even when existing institutions were weak, and despite such reforms requiring

regulatory constraints to be successful (Dubash and Morgan 2012; Prakash and Potoski 2016; Stiglitz 2003).

Further, our work contributes to the more general issue of how international organizations affect the regulatory capacity of states in an era of 'hyper-globalization' (Rodrik 2011). High-income countries embraced market liberalism but—to some extent—weathered the pressures of globalization by establishing a form of 'regulatory capitalism' (Braithwaite and Dahos 2000; Levi-Faur 2005; Phillips 2006). In developing countries, however, powerful IFIs have successfully pushed for deregulation because states lacked the capacity to resist such pressures and re-regulate markets (Babb and Kentikelenis 2018; Reinsberg et al. 2018). What is more, IFIs often transplanted 'regulatory innovations' from the developed world into developing countries, with insufficient embedding in local contexts (Dubash and Morgan 2012; Dunning 2004; Prakash and Potoski 2016). Confirming these arguments, our results caution against an overly optimistic view on the role of international organizations as facilitators of 'responsive regulators' (Abbott and Snidal 2013)—arguably because IFIs are not well-equipped to perform the role of orchestrators. Finally, our study also complements research on the socio-economic consequences of IMF programs (Dreher 2006; Kentikelenis 2017; Nooruddin and Simmons 2006; Reinsberg et al. 2018; Vreeland 2003). To date, no study has investigated the link between IMF conditionalities and corruption. And while past research assumed homogeneous effects of IMF programs, we allow for heterogeneity of IMF programs by scrutinizing specifically privatization conditions in such programs and addressing the methodological challenges related to non-random assignment of IMF conditions (Stubbs et al. 2018).

2. The corruption-inducing impact of privatization

The privatization-corruption link

We hypothesize that privatization can induce corruption by creating new rents and unleashing a political-economic dynamic that weakens the institutions of corruption control. Our argument unfolds in three steps. First, we posit that privatization processes create rent opportunities that can

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be exploited, especially under conditions of asymmetric information. Second, we show how the (illegal) pursuit of such rents generates incentives for weak institutions among rent-seeking elites. Third, we derive the dynamic implication that institutions deteriorate further as a result of a collective-action dilemma in post-privatization societies.

While there are different modes to privatize state-owned assets (Irdam, Scheiring, and King 2015), they can all lead to creating economic rents by putting public assets up for sale, and to generating large amounts of concentrated revenue which is appropriable by individual actors (Gray and Kaufman 1998). Corruption opportunities exist along the entire privatization process, from inception, to tender, and sale of public assets, and they likely are larger when the administrators of the former system manage the privatization schemes (Martimort and Straub 2009; Rose-Ackerman 2002; Sandholtz and Taagepera 2005).

Information asymmetries facilitate rent appropriation and leave ample space for corruption. In particular, outsiders have less information than insiders such as managers and public officials, who can exploit this informational advantage to enrich themselves in this process (Tanzi 1998, 7). For example, managers and firm insiders with vested interests make deliberate attempts to falsify records and undervalue firms in order to purchase them at bargain prices (Hamm, King, and Stuckler 2012, 317). Bidders can offer decision-makers side-payments to increase their chances of winning or preserving monopoly rents of former state-owned enterprises (Rose-Ackerman 2002).

Once corrupt individuals have acquired assets in the process, they seek to protect these assets and forego punishment by the state. Within the existing institutional framework, rent-seekers could bribe a public official in exchange for favorable treatment (Martimort and Straub 2009). From the perspective of corrupt elites, a more effective strategy would be to sustain—if not weaken—the institutions that could threaten to punish their corrupt behavior—if it was detected at all. Political institutions are not given but evolve in accordance with collective decisions within societies to alter them. In the short term, privatization can reduce corruption control because individuals who want to enrich themselves and to reduce the probability of detection and subsequent punishment of corruption are better able to do so under weak institutions, which lowers their instantaneous demand for strong institution (Ganev 2007). Even if most individuals are not corrupt, attempts to

weaken institutions under incomplete information can lead to a situation in which weak institutions and high corruption become entrenched. To be sure, all individuals with control over productive assets are better off investing into these assets in a rule-of-law state rather than stripping assets in a lawless environment. However, the more individuals have already stripped assets, the smaller the expected constituency for a rule-of-law state, which makes investment into productive assets and voting for strong institutions less beneficial, even for non-corrupt individuals. Hence, individuals collectively choose inefficient institutions because they fail to internalize the effect of their economic choices on the institutional environment (Hoff and Stiglitz 2005). This game-theoretic logic resembles recent accounts of corruption as a collective action dilemma (Persson, Rothstein, and Teorell 2013).

In sum, our model provides a mechanism for understanding how privatization reduces corruption control in a society. It also generates the dynamic expectation that corruption control declines further within the process of privatization (unless exogenous factors break the vicious circle of rent-seeking and weakening institutions). This implies our main hypothesis: *Privatization is negatively associated with corruption control.*

Case-study evidence on the privatization-corruption link

Qualitative evidence on the corruption-inducing effects of privatization is abundant. In the former Soviet Union countries, managers with good connections to the authorities purchased many former public enterprises (Hellmann and Kaufmann 2001; Hoff and Stiglitz 2005; Stone 2002). During Soviet times, "patronage was rampant but blatant forms of corruption were kept in check by party discipline, anti-bribery laws, and the rigidity of the overall system;" but post-Soviet era, the sheer size of potential rents and the demise of the Communist party removed key constraints to rent-seeking behavior by state enterprise managers and other government officials (Kaufmann and Siegelbaum 1997, 423). Some privatization projects were even setup to be corrupt. In the so-called "loans-for-shares program," Russian oligarchs bought up government debt in return for state assets shares; as state assets were transferred below their actual value, oligarchs in return financed the political campaign of the Kremlin (King and Treskow 2006).

In sub-Saharan Africa—a region where ethnic allegiances dominate national identities—high-level state personnel have worked to manage privatization to the benefit of the well-connected few (Tangri and Mwenda 2001, 132). Public bureaucrats had wide discretion to ensure that their loyal supporters benefited most, for example in Kenya (Bennell 1997), Ghana (Appiah-Kubi 2001), and Uganda (Mwenda and Tangri 2005). The lack of transparency in the privatization programs of Côte d'Ivoire, Madagascar, and Mozambique also enabled officials to accrue private benefits (Bennell 1997). In most cases, perpetrators of corruption crimes were never prosecuted (Tangri and Mwenda 2001).

In Latin America, Argentina, Brazil, Mexico, and Venezuela have had privatization episodes that led to increasing corruption (Manzetti and Blake 1996). Corruption was particularly rampant where executives had discretionary power to implement economic reform. Corruption scandals flourished in the wake of little parliamentary control and weak legal frameworks. Moreover, privatization did not remove the potential for corrupt behavior but merely changed its modalities. Under public ownership, enterprises had access to the general state budget, and corruption would impact public contracts and hiring decisions. Under private ownership, however, these enterprises lacked access to state funds but sought continued protection by bribing officials to bend rules and devise arbitrary regulations for competing firms. This created popular dissatisfaction with the privatization process, despite higher levels of investment after privatization (Martimort and Straub 2009).

In East Asia, the corruption-inducing effect of privatization is comparatively less well-established. For example, Vietnam was urged to reform the state-owned enterprise sector and to adopt various measures to speed up equitization—a form of privatization that entails removing state control over productive establishments and placing those assets into publicly traded stock (Painter 2005, 270–71). While this process faced several obstacles—including technical and legal difficulties—the main impediment was that most of the potential purchasers were existing managers and employees who, in most cases, already enjoyed informal ownership rights (Gainsborough 2002, 358–60). Manager-owners of these semi-privatized entities were not prepared to surrender the advantages of state-owned enterprises, such as entitlement to land ownership, easier access to

loans, benefits for employees, and employment security, and thus preserved their privileges through corruption (Fforde 2002).

There is also anecdotal evidence of a link between IMF privatization programs and increased corruption. Consider the case of Albania. Between 1993 and 2001, the country faced 27 privatization conditions, many of which were binding and seven required full implementation even before Albania was able to access any IMF loans (so-called 'prior actions'). In 1993, the Fund mandated the Albanian government to "privatize (fully or as joint ventures) or liquidate all state farms" (IMF 1993). In 1994, it requested the "liquidation of three firms identified by the Enterprise Restructuring Agency (ERA)" and related action plans for at least five other firms. In 1998, under a new IMF agreement, the government needed to obtain parliamentary approval of its privatization strategy and "liquidate all former ERA enterprises" (IMF 1998). In 1999, the Fund required the "sale of at least 50 SMEs" and the "holding of auctions for at least 90 SMEs" (IMF 1999). As privatization progressed, corruption control deteriorated according to widely used related measures. For instance, the ICRG corruption control index deteriorated from 4.00 in 1993, to 3.83 in 1995, and 2.00 in 2000, and further to 1.83 in 2005. Similarly, the VDem Public Sector Corruption Index—measuring the extent to which "public sector employees grant favors in exchange for bribes, kickbacks, or other material inducements […] and steal, embezzle, or misappropriate public funds" (Coppedge et al. 2016, 67)—increased from 0.46 in 1995 to 0.63 in 2000. In other words, corruption perceptions increased precisely when privatization was at its peak (Figure 1). Although this correlation is merely suggestive, it shows that our hypothesis is entirely plausible.

[Figure 1 here]

The case of Albania is not unique. By the late-1980s, the IMF—and other international financial institutions—had made privatization conditions a requirement for most adjustment loans (Brune, Garrett, and Kogut 2004), with such conditions becoming ubiquitous over the 1990s and beyond (Kentikelenis, Stubbs and King 2016). In many cases, IMF conditionality has played a pivotal role in jump-starting privatization (Haggard and Kaufman 1992; Henisz, Zelner, and Guillén 2005; Hilson 2004).

A key weakness of the IMF's role in privatization is the insufficient attention to the broader institutional framework required to prevent corruption increases (Manzetti and Blake 1996; Mwenda and Tangri 2005). The timing of privatization reforms was often problematic. For example, in the former Soviet countries, privatization was mandated at a time when "fiduciary controls that ordinarily operate to ensure that government transactions are fair and transparent have been largely crippled" (Kaufmann and Siegelbaum 1997, 421). To ensure quick implementation of its conditions, the IMF often turned a blind eye on irregularities as long as authorities implemented other parts of its market-liberalizing agenda (Tangri and Mwenda 2001, 130). Last, the Fund did not give adequate advice on how to privatize well, ignoring evidence that different ways of privatization have different effects on corruption (Irdam, Scheiring, and King 2015); for example, privatization involving international tenders and firm liquidation might be less prone to corruption than other modes of privatization (Kaufmann and Siegelbaum 1997).

In sum, case studies provide evidence of the corruption-inducing effects of privatization, and demonstrate that IMF conditionality has often been crucial for the initiation of privatization programs. Linking these two bases of evidence, we hypothesize that IMF-promoted privatization increases corruption. Going beyond case studies, we present the first systematic cross-national quantitative analysis on the privatizations–corruption relationship.

3. Data and methods

We analyze the relationship between IMF conditions and corruption control using time-series cross-section analysis at the country level. Our sample includes 141 developing countries observed from 1982 to 2014. Developing countries hereby refer to all non-high income countries according to the World Bank definition.[1] Due to missing values in the control variables, panels are unbalanced and include up to 99 countries.

Dependent variables

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[1] Developing countries have per-capita income below US$ 12,736.

Our key dependent variable is CONTROL OF CORRUPTION from ICRG. This perception-based indicator is widely used among scholars studying corruption (e.g., Adserà et al. 2003; Sandholtz and Gray 2003; Larraín and Tavares 2004; Dreher and Siemers 2009) and coincides with our definition of corruption presented earlier. The measure assigns low values when there is widespread "financial corruption in the form of demands for special payments and bribes connected with import and export licenses, exchange controls, tax assessments, police protection, or loans" (The PRS Group 2015, 4).

We use alternative measures of corruption for robustness tests: the Transparency International's (TI) corruption perception index,[2] the VDem project corruption index (Coppedge et al. 2016, 66), and the HRV government transparency index (Hollyer, Rosendorff, and Vreeland 2014). While the former are at the heart of our argument about rent creation in the government sector, the latter captures what is possibly a key facilitator for corruption to thrive—lack of transparency (Gaspar and Hagan 2016, 17).

Among the indicators suitable for time-series cross-section analysis, the TI index is widely known among practitioners, while the VDem measure has greater coverage.[3] The main drawback of the ICRG, TI, and VDem measures are that they are based on subjective ratings and may suffer from perception biases. For example, while ICRG variables seek to capture risks for investors and hence reflect biases in the business community, VDem measures are created by country experts with social sciences training. Consequently, in using different subjective measures, we hedge against specific biases. In addition, government transparency—capturing the extent to which countries report data to the World Bank—promises to be a relatively bias-free indicator as it is based on observational data. While perception-based indicators have recently been criticized (Donchev and Ujhelyi 2014; Stubbs, King, and Stuckler 2014), scholars have attempted to directly measure corruption, for example by examining irregularities in public procurement (Fazekas and

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[2] https://www.transparency.org/research/cpi/overview (accessed June 10, 2018).

[3] The World Bank Governance Indicators measure on corruption control was not designed for use in panel data analysis, as its values cannot be compared across time. By choosing indicators with broad cross-country coverage, we also minimize (to the extent possible) the risk of bias due to non-random missing data.

King 2018; Fazekas and Kocsis 2017; Olken and Pande 2012). We cannot use procurement-based measures here because the relevant data are not available for a wide range of countries.

Key predictors

Our analysis hinges on the ability to measure the number of IMF conditions in specific policy areas. For that purpose, we rely on information from a newly constructed database on IMF conditionality based on our coding of individual conditions from all IMF agreements with borrowers from 1980 to 2014 (Kentikelenis, Stubbs, and King 2016). Our key predictor is the number of binding conditions in an IMF program requiring privatization of state-owned enterprises. Consistent with previous scholarship, we use binding conditions as they determine actual disbursements of scheduled loans (Copelovitch 2010; Rickard and Caraway 2018; Stubbs et al. 2017). Privatization conditions became integrated into IMF programs after the end of the Cold War, reflecting the creation of newly independent states in Eastern Europe, while reaching their peak at the millennium turn and declining thereafter.

In addition, we include a binary indicator for an IMF PROGRAM being active in a given year. If jointly included with a condition count, the coefficient estimate on the program dummy will capture all remaining aspects of IMF programs, including financial resources, technical assistance, and signaling effects. In the robustness checks, we also juxtapose privatization conditions against other policy areas available from the IMF conditionality database.

Control variables

A significant body of scholarship focuses on the long-run macro-historical determinants of corruption, including colonial history, legal origin, latitude, religion, ethno-linguistic fractionalization (La Porta et al. 1999; Treisman 2000), and political-system features such as federalism, presidentialism, and proportional representation (Adserà, Boix, and Payne 2003). Yet others argue that mass education over a century ago was critical to building capable states and to giving citizens more opportunities for opposing corruption (Uslaner and Rothstein 2016).

Since these determinants are time-invariant in our sample period, we control for these using a series of country dummies and focus on time-varying correlates of corruption control. An initial

control variable for corruption is GDP PER CAPITA, given that level of development closely relates to efficient institutions.[4] We take the logarithm of this variable to remove skewness.

Control of corruption is also affected by political factors. Democratic governance can increase the likelihood of exposing corrupt practices and hence reduce incentives for state bureaucrats to ask for bribes (Ades and Di Tella 1999; Larraín and Tavares 2004; Montinola and Jackman 2002). To capture constitutional guarantees for political rights, we use the POLITY IV INDEX. We also consider REGIME DURABILITY from the Polity IV dataset—the number of years that the current political order has survived since the last transformation—to account for deep legacies of political systems (Dreher, Kotsogiannis, and McCorriston 2009; Jetter, Agudelo, and Hassan 2015).

In addition, we consider a series of variables that capture opportunities for rent-seeking. The degree of URBANIZATION may affect corruption because the higher the concentration of the population in urban areas increases opportunities for interaction between bribe takers and bribe givers (Billger and Goel 2009). We also use MINERAL RENTS (as a percentage of GDP), as well as the (natural logarithm) of OIL PRODUCTION PER CAPITA (Treisman 2000; Adserà et al. 2003; Fréchette 2006). According to proponents of the 'resources curse', countries relying on natural resources are particularly vulnerable to corruption given the geographic concentration of such resources and the adverse effects of natural resource endowments on government accountability (Djankov, Montalvo, and Reynal-Querol 2008; Mahdavy 1970; Ross 2001). Detailed definitions and data sources for all variables can be found in appendix B. Descriptive statistics are presented in Table 1.

[Table 1 here]

In addition to country-fixed effects and control variables, we include year dummies to net out the impact of factors that may affect corruption equally across countries. Employing fixed-effect

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[4] GDP per capita might not be fully exogenous with respect to corruption (Treisman 2000), but there is no satisfactory solution to address the problem (Fréchette 2006). Following Jetter and colleagues (2015), we replaced GDP per capita with life expectancy but obtained very similar results. Therefore, we simply report regressions with GDP per capita here.

estimation can mitigate potential bias due to unobserved time-invariant variables.[5] We also report

robust standard errors clustered by country, given that errors of within-country time series might

be correlated.[6]

Methods

A well-known inferential challenge is non-random assignment of countries to IMF programs. For

instance, countries with corruption problems may be more likely to require IMF assistance,

thereby implying a problem of reverse causality. We tackle this challenge by estimating a probit

selection model for IMF programs using a well-established set of variables recommended by the

literature. We include PAST PROGRAMS, a count variable for the prior years of IMF exposure over

a five-year horizon. Previous exposure is a reliable predictor of current participation (Bird,

Hussain, and Joyce 2004; Copelovitch 2010). Program participation is also affected by the extent

to which the Fund has resources available, which depends on the current number of borrowing

countries (Vreeland 2003). Hence, we include the contemporaneous count variable COUNTRIES

UNDER PROGRAMS. In addition, as allies of big powers receive favorable treatment by IFIs

(Dreher, Sturm, and Vreeland 2015; Thacker 1999), we measure the alignment of voting patterns

between the borrowing country and the G7 countries in the United Nations General Assembly—

UNGA VOTE ALIGNMENT (Strezhnev and Voeten 2013).[7] Additional variables capture

macroeconomic conditions—logged GDP PER CAPITA, GDP GROWTH, RESERVES in months of

imports, and DEBT SERVICE (as percentage of GNI)—as well as political characteristics—

democracy, measured by the POLITY IV INDEX, and EXECUTIVE ELECTIONS—that have been

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[5] We also conducted a Hausman test, which rejected the use of random-effect estimation (p<0.001).

[6] We reject the null hypothesis of 'no autocorrelation' using the Wooldridge test (p<0.001).

[7] Selection models are more robust when they have an exclusion restriction. In our case, COUNTRIES UNDER PROGRAMS is plausibly excludable, whereas UNGA voting alignment—the variable conventionally used as instrument in the IMF program literature—is not because countries in which corruption is endemic will also be more likely to accept bribes by powerful donors.

previously found to affect program participation.[8] We also include regional dummies and year dummies (since country-fixed effects cannot be included in a probit-type model).

Another concern is endogeneity of conditionality, for example due to reverse causality. To address endogeneity, we need instruments for IMF conditions—variables that partially correlate with IMF conditionality but whose impact on corruption control only operates through conditionality. Finding excludable instruments presents a formidable challenge, especially if we need generally valid instruments for IMF conditions across different policy areas.

Our instrumentation strategy borrows from recent practice in development effectiveness research (Lang 2016; Nunn and Qian 2014). For each type of condition, we construct a compound instrument based on the interaction of the within-country average number of these conditions and the period-specific budget constraint of the Fund as measured by the number of COUNTRIES UNDER PROGRAMS. The instrument fulfils the relevance criterion because when the IMF assists more countries in any period, resource scarcity prompts the organization to assign more conditions to any given country as a safeguard measure (Dreher and Vaubel 2004; Lang 2016; Vreeland 2003).[9] The instrument likely fulfills the exclusion restriction because deviations of a country's number of conditions from its long-run average are not due to a country's institutional features but the IMF's decision to require more safeguards when its global budget constraint becomes more binding. Conditional on controls and country-fixed effects, we cannot think of any direct pathway from the IMF's budget constraint to corruption control other than through conditionality.

The compound instrumentation strategy offers the advantage that we do not need to find specific instruments for different types of conditions. The drawback is that not all areas of conditionality are equally strongly subjected to the budget-constraint logic described here, which may result in

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[8] While IMF programs are less likely in democracies because leaders must fear electoral punishment, they are more likely after elections—when electoral pressures have waned. Both economic fundamentals and political variables are lagged one period further than the lag of IMF PROGRAM.

[9] Dreher and Vaubel (2004) show that bigger loans come with more conditions attached, since the Fund has more of its assets at stake and thus requires more safeguards. We generalize this argument to the number of loan programs, which should positively relate to the size of loans under management.

weak-instrument bias (though we confirm through F-tests that this is not an issue). We acknowledge that endogeneity of conditions remains a challenge,[10] and we address this issue again in the robustness tests by probing robustness of our findings to a different instrument specification.

As our statistical model includes three equations—corruption control (the ultimate outcome), IMF PROGRAM (a binary outcome), and the number of privatization conditions—we estimate Seemingly Unrelated Regressions using maximum likelihood and allow for correlated standard errors across equations and clustering on countries (Roodman 2012). The latter choice also mitigates temporal autocorrelation in the standard errors. In the context of research on the effectiveness of IMF interventions, this method yields consistent estimates for a broad range of scenarios (Stubbs et al. 2018).

4. Findings

Main results

Table 2 examines the impact of privatization conditions and other aspects of IMF programs on the control of corruption, showing the full results of all stages in the system of equations. Each column corresponds to a different lag of the variables of interest, which enables us to study the impact of conditionality under different time horizons and thus to better test the observable implications of our theoretical argument. Since all our estimations include fixed effects for individual countries, results must be interpreted as within-country effects.

[Table 2 here]

We find that privatization adversely affects corruption control from the second year of an IMF program onwards. One additional privatization condition is predicted to reduce corruption control

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[10] Previous studies suggest some excludable instruments for IMF program participation (Dreher, Sturm, and Vreeland 2015; Nooruddin and Simmons 2006; Vreeland 2003), loan size (Copelovitch 2010), and the number of conditions (Dreher 2006); but there is only limited research on the determinants of specific types of conditions that might yield generally valid instruments (Rickard and Caraway 2018).

by at least 0.31 points (p<0.1); the index ranges from 0 to 6 and its standard deviation is 0.99. For purposes of comparison, it would require full-blown autocratization—a fifteen-point decline in the Polity IV index—to exert a similarly adverse effect on corruption control.[11] While the effect becomes substantively larger for deeper lags, it remains only weakly statistically significant. We find no significant effects for the remaining aspects of IMF programs.

Diagnostic statistics suggest that our model specifications are appropriate. While some of the control variables lack statistical significance, their estimated effects all have the expected direction. The only exception is GDP per capita, which is insignificant.[12] Democratization positively relates to corruption control. Higher regime durability also tends to relate to better control of corruption, though the effect is mostly not statistically significant. The negative coefficient of urbanization is never significant. Consistent with studies on the resource curse, higher non-tax income from mineral rents reduces the control of corruption, as does an increase in oil production.

For completeness, we also discuss the results of the auxiliary equations. In the selection equation for IMF programs, coefficient estimates have the expected direction (Moser and Sturm 2011; Vreeland 2003; Woo 2013), and are highly stable for all variables. For example, program participation is explained by the number of past programs, the level of socioeconomic development, the number of countries under program, and the strategic alignment of a country with the G7. Moreover, indicators of economic crises such as low growth, few reserves, and high debt service can also explain program participation. While democracy is not statistically significant, we find some evidence that countries are more likely to seek programs after an executive election.

In the conditionality equation, we find a strongly significant effect of the compound instrument, which underscores its relevance with respect to the number of privatization conditions. In

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[11] The appendix also presents our findings on a standardized dataset, which can facilitate direct comparisons between effect sizes (and across different outcomes) but raises difficulties of interpretation in terms of the substantive effects for some variables.

[12] As discussed in footnote 4, this most likely is due to reverse causality; moreover, the use of fixed effects absorbs the level effect that one would find when using a pooled estimator.

addition, privatization conditions are more likely to be extended to countries experiencing reductions in their per-capita income, increases in the longevity of their regimes, and reductions in oil production. Most of the variation, however, is absorbed by country dummies and year dummies.

Diagnostic statistics suggest that our model specifications are appropriate. The best-fitting models for corruption control explain about 30% of the within-country variation. In addition, there is little indication of weak instruments. According to the conventional rule of thumb, the Kleibergen-Paap F-statistic should pass the value of ten (Stock and Staiger 1997), which would guarantee that the bias due to weak instruments is relatively small compared to the bias due to endogeneity (Stock and Yogo 2005). In our case, the F-statistic on the compound instruments for privatization is beyond this threshold ($F > 86$).

Mechanisms

Having established a relationship between privatization conditions and declining corruption control, we seek further support for our posited mechanism in two ways. First, a necessary condition for our mechanism to be at work is that countries actually implement privatization programs. To account for potential non-compliance with IMF conditionality, we remove conditions from the condition count that are waived by the Executive Board. A condition may be waived by a simple majority vote among member states in the Board following a related proposal by IMF staff (Copelovitch 2010, 60). Taking waivers into account should strengthen coefficient estimates because it removes a subset of conditions which may not have been met. When doing so, we find more consistent evidence for the negative effect of privatization conditions on perceived control of corruption. In the fifth year following a program, the effect is strongest—about 0.37 points less corruption control for every condition ($p < 0.05$). Conversely, other areas of conditionality remain statistically insignificant (Table 3).

[Table 3 here]

Second, we verify that our effects are genuinely driven by privatization conditions, rather than IMF conditionality more generally. To that end, we include the number of conditions in four other

policy areas: revenues, public sector employment, price liberalization, and trade liberalization. IMF research considers these the most beneficial policy areas towards corruption abatement. To account for potential endogeneity of these conditions, we adopt the same compound instrumentation strategy as before, using the interaction of the country-specific average number of conditions and the time-varying global number of IMF countries. As a result, privatization conditions continue to exert a negatively significant effect on corruption control (Table 4). Effect magnitudes remain stable, which lends support to our hypothesized mechanism. Juxtaposing privatization against other policy areas yields additional insights, confirming that not all IMF policies are uniformly detrimental. For instance, price liberalization has a beneficial impact on corruption control ($p<0.05$), as does trade liberalization ($p<0.1$). However, as we show in the supplemental appendix, these effects are not robust to alternative model choices.

[Table 4 here]

Robustness tests

In the supplemental appendix, we conduct robustness tests and report briefly on the results here. In Table A1, we use alternative dependent variables. First, using TI's corruption perception index, we find a marginally significant deterioration of perceived transparency due to privatization conditions after one year ($p<0.10$). Second, using the VDem corruption index (Coppedge et al. 2016, 66), privatization significantly increases corruption, especially in the first two years ($p<0.05$) but also in the third year ($p<0.1$). Hence, among all perception-based indicators, the VDem corruption index yields the most significant findings. This could reflect potential biases in the ICRG measure, which is produced by business experts who tend to ascribe to market-liberalizing measures the ability to abate corruption (Stubbs, King, and Stuckler 2014). The VDem index is less likely to suffer from such bias as it is based on expert surveys beyond the business sector. Third, using the HRV government transparency measure (Hollyer, Rosendorff, and Vreeland 2014), we find that privatization reduces government transparency after two years, while other aspects of IMF intervention improve it.

Table A2 uses an alternative operationalization for the time-varying component of the compound instrument. In particular, we replace the number of countries under programs by the average

number of conditions in a specific policy area in a given year and interact this part with the country-specific average number of conditionality over the entire sample period. This compound instrument is plausibly excludable because changes in the country-specific number of conditions are brought about by changes in the global popularity of certain conditions ('policy fads')—which is out of the control of individual countries and not directly related to their corruption control. This reasoning is plausible, as the example of IFI-promoted tax policy convergence suggests (Swank 2016). We find consistent support for a corruption-inducing effect of privatization conditions ($p<0.05$), while price liberalization tends to improve corruption control ($p<0.1$).

Table A3 alters the estimate of interest by comparing IMF countries with specific conditions to IMF countries without such conditions. By removing all country-year observations without IMF programs, we thus change the baseline for comparison. This simplifies the estimation as it removes one equation. We find that privatization exerts a significantly negative impact on corruption control, especially in later years. Substantively, one condition has an effect of 0.43 after five years ($p<0.05$)—which is in the order of magnitude of the previous analysis. None of the other conditions are significant.

Table A4 probes robustness to dynamic estimation using Error Correction Models (ECMs). In these models, the dependent variable is the change of corruption control, regressed on its lagged level, and levels and changes of all predictors. The benefit of ECMs is to provide a flexible model structure to uncover dynamic relationships, though at the cost of additional complexity.[13] As reflected in the respective coefficients on the differenced variables, none of the conditions exerts an instantaneous effect on corruption control. The same holds for other elements of IMF assistance beyond conditionality. In contrast, we find that most conditions have negative long-term implications for corruption control. This is particularly the case for privatization: one

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[13] Political methodologists continue to debate under which conditions ECMs are appropriate, which is why we use them only for robustness tests (Beck and Katz 2011; Boef and Keele 2008; Grant and Lebo 2016). And yet, ECMs are still preferable to an atheoretical lagged dependent variable model, which would generate biased estimates in our case (Nickell 1981).

privatization condition reduces long-term corruption control by 1.69 points[14] (p<0.01). This result is consistent with our previous findings obtained from a simpler statistical model.[15]

Finally, we examine how total government expenditure—a potential result of revenue-increasing privatization—co-determines corruption (Table A5). Raising government spending can increase corruption because such spending creates new rents to be captured (Dzhumashev 2014a, 2014b). While we excluded government spending earlier to mitigate post-treatment bias, we now include it to test whether spending shocks affect the privatization–corruption link. We find that spending levels are not related to corruption but that aggregate spending shocks are positively related to corruption control (p<0.1). We also tested the effect of military expenditures on corruption (Hudson and Jones 2008), finding faster growth of military spending significantly decreases corruption control (p<0.05), and attenuates the effect of privatization, which becomes statistically insignificant. This could be interpreted as evidence that privatization generates new rents that governments put into the military, which is an indication of which groups stand to gain from privatization due to their connections to government and thus consistent with our argument. These findings also are in line with earlier studies showing that corruption distorts public expenditures, typically to the detriment of social protection and to the detriment of economic performance (Delavallade 2006; Hessami 2014; Hudson and Jones 2008; Del Monte and Pagani 2001; Rajkumar and Swaroop 2008).

In sum, our analysis suggests that IMF-induced privatizations decrease control of corruption in the developing world. As summarized in Table 5, this result is robust against alternative measures of corruption, different operationalization of predictors, model specifications, and estimation methods.

[Table 5 here]

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[14] Taking the long-term coefficient on this condition and divide by the absolute value of the coefficient on the lagged dependent variable (i.e., (-0.25)/0.15=1.69).

[15] We decided to use ECMs for robustness checks only as the discussion on their use cases is ongoing. To some methodologists, ECMs provide a flexible estimation structure even in the absence of co-integration relationships (Beck and Katz 2011; De Boef and Keele 2008), while others contest the use of ECMs as a single-step estimation method (Grant and Lebo 2016).

5. Conclusion

Using new IMF conditionality data, we found that IMF conditions mandating privatization of state-owned enterprises decrease corruption control. This effect is tangible and holds in most estimations examining different outcome measures (both corruption perceptions and objective indicators), an alternative operationalization of conditions, and estimation techniques. While our main analysis focuses on short-term effects up to five years, we obtain even stronger long-term effects in a dynamic model. In addition, we find some corruption-reducing effects of other market-liberalizing policy conditions, but these effects are not robust across different models.

We interpret these results as evidence that privatization creates highly concentrated rents that increase corruption risks, while at the same time creating incentives among rent-seeking elites to weaken state capacity. This leads to a vicious circle of weakening institutions and increasing corruption, which is hard to break because corruption is a collective action dilemma (Persson, Rothstein, and Teorell 2013). Concerning the lack of robust support for other areas of conditionality, we speculate that IMF conditions seeking to abolish rents are not always successful because governments may find other ways to redistribute wealth to influential groups (Coate and Morris 1995), which tends to mitigate the positive effect on corruption control. It may also be harder to control corruption when states are reduced to essential functions (Persson and Rothstein 2015).

Before turning to policy implications and avenues for future research, we discuss three limitations of our study. First, our main results are based on a rather narrow (yet widely used) definition of corruption and subjective measurements of perceived corruption control—which is a concession to data availability. However, we have also considered government transparency as an objective alternative outcome. Second, missing data is a potential problem for all large-N social science research such as ours. Lacking a statistical fix to the issue of missing data in the context of multiple-equation models, we have sought to address this challenge by using corruption variables with broad cross-country coverage, thus minimizing threats to external validity due to missing data series. Third, while we have employed the best-available methods to address potential

endeogeneity of conditionality, a causal interpretation of our results rests on the excludability of our compound instruments. In view of the qualitative evidence, the argument that privatization induces corruption is entirely plausible.

In terms of policy implications, our findings corroborate the view that market-liberalizing policy packages are unlikely to be uniformly beneficial (Hopkin and Rodríguez-Pose 2007). In fact, we find that privatization undermines corruption control, while other types of market-liberalizing conditions do not have the hoped-for effects of reducing corruption. Our study hence questions the role of IFIs in the advancement of corruption control, particularly when the primary goal of such IFIs is to liberalize markets and reduce the role of the state in the economy. While IMF researchers have begun to acknowledge the potential dangers of promoting privatization (Gaspar and Hagen 2016, 22), such statements are lacking in official IMF publications (IMF 2017). Although it is beyond the scope of this article to examine why the IMF is clinging on to its positive view on privatization, we can draw on established scholarship to intuit the IMF's behavior. A number of studies have examined change and stability in the IMF's policy paradigms, specifically after the Global Financial Crisis, finding that change occurred more in rhetoric than in action (Ban 2015; Broome 2015; Gabor 2015). As emphasized by recent scholarship (Baumgartner 2012; Béland and Cox 2013; Blyth 2012), paradigm change is rare, especially when no counter-paradigm has emerged and the organizational leadership has vested interests in maintaining the old paradigm.

We identify at least three areas of further inquiry for future research. First, while our goal here was to theorize on the effect of privatization on corruption and to quantify the associated effects, further work would be welcome for related areas of market-liberalizing policy reform. For instance, public-sector conditions often mandate countries to reduce the attractiveness of public sector jobs (Rickard and Caraway 2018), and capital account liberalization may allow corrupt elites to move corrupt funds offshore (Brown and Cloke 2004). In addition, in view of criticisms that privatization is detrimental in certain policy environments and regulatory contexts, further research may examine how context moderates the effect of privatization on corruption. While researchers concur that IFI pressure often initiates privatization programs (Brune, Garrett, and Kogut 2004), their modalities are often determined by domestic characteristics, for example

political ideology (Martinez-Gallardo and Murillo 2011). Future research could collect detailed data on the regulatory aspects of privatization programs to inform policy practice on how to avoid corruption in such cases.

Second, we did not explicitly test whether IMF anti-corruption conditions effectively reduce corruption. While this inquiry would encounter irresolvable endogeneity issues, our findings suggest that a potentially beneficial effect of such conditions would be insufficient to compensate for the detrimental effects of privatization conditions. The relative ineffectiveness of anti-corruption measures is not surprising because the IMF—in addition to its commitment to the transactional approach to corruption—remains skeptical about states' willingness and ability to reduce corruption (Mungiu-Pippidi 2015). While increased state capacity could help overcome corruption problems, the Fund has promoted corruption-abatement through reducing government expenditure and promoting market-liberalizing reforms (Brown and Cloke 2004, 286). In doing so, the Fund mistakes that reducing the role of the state does not eliminate corruption, but likely shifts the locus of corruption into the private sector (Goldsmith 1999; Kohl 2002; Manzetti and Blake 1996).

Ultimately, the key policy question is why the Fund continues to advocate privatization despite evidence for the harmfulness of such policies in developing countries. We thus encourage researchers to examine the sources of stability in IMF policy advice on market-liberalizing reforms. This entails research on the political economy of international organizations examining who governs these organizations and how power asymmetries affect policy outcomes (Halliday and Block-Lieb 2009; Halliday, Pacewicz, and Block-Lieb 2013; Stone 2013). The issue can also be examined through the lens of policy paradigms (Babb and Chorev 2016; Broome, Homolar, and Kranke 2018; Kentikelenis and Seabrooke 2017). Such paradigms are hard to overturn as long as there is no consistent alternative to replace them and if leadership to actively reconstruct paradigms is lacking. Such conditions are likely to be met in hierarchical organizations with homogenous professional backgrounds, such as the Fund. While scholars have studied IMF paradigm change in fiscal policy (Ban 2015; Ban and Gallagher 2015; Clift and Tomlinson 2011), a similar analysis on 'good governance' is missing. Text-as-data analysis of policy speeches and IMF publications could be used to track potential early signs of changing rhetoric on anti-

corruption. Findings from such an exercise would help us better understand why the IMF's approach to corruption has not substantively changed since the organization first adopted the staff guidance note on 'good governance' in 1997.

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Figures

Figure 1: The privatization–corruption link for Albania in the post-privatization era.
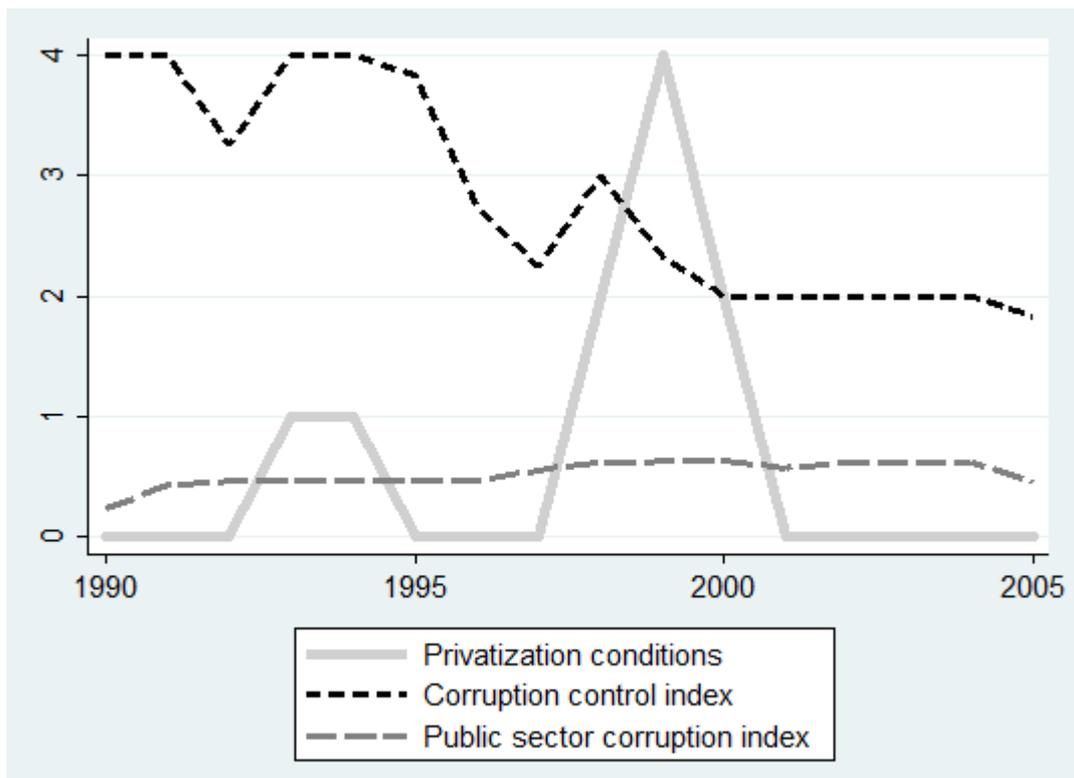
Tables

Table 1: Descriptive statistics

	Observations	Mean	Sd	Min	Max
Corruption indicators					
Control of corruption	2654	2.414	0.990	0.000	6.000
Corruption perception index (TI)	1732	31.028	10.729	4.000	71.000
Corruption index (VDem)	3915	0.631	0.206	0.079	0.969
Government transparency	2852	0.549	1.856	-7.052	9.981
Key predictors					
IMF program	4612	0.396	0.489	0.000	1.000
Privatization	4577	0.079	0.482	0.000	8.000
Revenues	4577	0.324	1.277	0.000	21.000
Public sector	4577	0.154	0.766	0.000	24.000
Price liberalization	4577	0.208	1.004	0.000	28.000
Trade liberalization	4577	0.824	1.819	0.000	28.000
Control variables					
Log(GDP per capita)	4221	7.154	1.045	4.242	9.661
Regime durability	3758	16.174	17.664	0.000	105.000
Polity IV index	3684	0.560	6.620	-10.000	10.000
Urbanization	4810	43.155	19.927	4.339	91.604
Mineral rents (% GDP)	4207	1.289	3.767	0.000	44.644
Log(Oil per capita)	3799	0.244	0.515	0.000	3.391

Table 2: Privatization conditions and corruption control.

	t-1	t-2	t-3	t-4	t-5
Corruption control					
Privatization conditions	-0.268	-0.310*	-0.348*	-0.366*	-0.387*
	(0.172)	(0.187)	(0.199)	(0.206)	(0.203)
IMF program	-0.047	-0.042	-0.076	-0.094	-0.154
	(0.152)	(0.156)	(0.148)	(0.140)	(0.131)
Log(GDP per capita)	-0.164	-0.182	-0.216	-0.259	-0.304
	(0.219)	(0.218)	(0.215)	(0.220)	(0.234)
Regime durability	0.008*	0.007	0.006	0.006	0.006
	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Polity IV index	0.025**	0.022*	0.021*	0.018	0.017
	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)
Urbanization	-0.011	-0.009	-0.006	-0.003	0.002
	(0.018)	(0.017)	(0.017)	(0.016)	(0.016)
Mineral rents	-0.051***	-0.048***	-0.045**	-0.041**	-0.039**
	(0.019)	(0.018)	(0.018)	(0.018)	(0.017)
Log(Oil per capita)	-0.588	-0.633*	-0.657**	-0.663**	-0.584**
	(0.358)	(0.339)	(0.315)	(0.286)	(0.259)
Country-fixed effects	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes
Observations	2252	2243	2231	2219	2153
Within-R2	0.30	0.30	0.29	0.29	0.29
IMF program					
Past programs	0.368***	0.365***	0.363***	0.363***	0.374***
	(0.022)	(0.023)	(0.024)	(0.024)	(0.025)
UNGA vote alignment	3.422***	3.515***	3.514***	3.571***	4.122***
	(0.890)	(0.885)	(0.949)	(0.992)	(1.131)
Countries under programs	-0.028***	-0.028***	-0.029***	-0.029***	-0.031***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Log(GDP per capita)	-0.339***	-0.349***	-0.347***	-0.336***	-0.319***
	(0.075)	(0.078)	(0.078)	(0.077)	(0.076)
Polity IV index	0.002	0.000	-0.001	-0.003	-0.006
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Reserves	-0.067***	-0.070***	-0.071***	-0.071***	-0.067***
	(0.019)	(0.020)	(0.021)	(0.022)	(0.024)
GDP growth	-0.023***	-0.022***	-0.023***	-0.024***	-0.026***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Debt service	0.027***	0.030***	0.030**	0.028**	0.026**
	(0.010)	(0.011)	(0.012)	(0.012)	(0.011)
Executive elections	0.078	0.096	0.139*	0.149*	0.139
	(0.080)	(0.082)	(0.083)	(0.083)	(0.089)
Region-fixed effects	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes
Observations	2252	2243	2231	2219	2153
Pseudo-R2	0.33	0.33	0.33	0.33	0.33
Privatization conditions					
Compound instrument	0.090***	0.091***	0.090***	0.089***	0.095***
	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Log(GDP per capita)	-0.071	-0.079	-0.091*	-0.107*	-0.119**
	(0.047)	(0.050)	(0.053)	(0.057)	(0.059)
Regime durability	0.001*	0.001**	0.001**	0.002**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Polity IV index	0.001	0.001	0.002	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Urbanization	0.002	0.002	0.002	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)

Mineral rents	-0.002	-0.002	-0.001	-0.001	0.002
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Log(Oil per capita)	-0.149***	-0.155***	-0.158***	-0.167***	-0.163***
	(0.053)	(0.056)	(0.056)	(0.057)	(0.053)
Country-fixed effects	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes
F-statistic	114.89	99.45	98.83	99.09	111.71
Observations	2252	2243	2231	2219	2153
Within-R2	0.15	0.15	0.14	0.14	0.15

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. All predictors are included at the lag specified in the column header. Each regression includes an IMF dummy which is instrumented with the selection equation shown. Privatization conditions are instrumented using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table 3: Implementation-corrected conditionality counts and corruption control.

	t-1	t-2	t-3	t-4	t-5
Privatization conditions	-0.287*	-0.300*	-0.327*	-0.341*	-0.380**
	(0.159)	(0.164)	(0.174)	(0.176)	(0.183)
IMF program	-0.010	-0.003	-0.060	-0.092	-0.163
	(0.184)	(0.186)	(0.168)	(1.494)	(0.135)
Control variables	yes	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes
Within-R2	0.30	0.29	0.29	0.28	0.29
Observations	2252	2243	2231	2219	2153
F-statistic (mean)	108.61	108.16	108.37	109.07	111.57

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. The table shows the outcome equation using 'control of corruption'. All predictors are included at the lag specified in the column header. Each regression included an IMF dummy as a control variable, which was further modeled using a selection equation to take non-random selection into account. Privatization conditions only include non-waived conditions and are instrumented using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table 4: Privatization conditions and other policy conditions.

	Revenue conditions	Public-sector employment conditions	Price liberalization conditions	Trade liberalization conditions
Privatization conditions	-0.500***	-0.461*	-0.570***	-0.434**
	(0.178)	(0.248)	(0.208)	(0.202)
Other policy conditions	0.276	0.390	0.510**	0.296*
	(0.195)	(0.374)	(0.207)	(0.160)
IMF program	-0.032	-0.057	-0.053	-0.031
	(0.150)	(0.156)	(0.146)	(0.147)
Control variables	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes
Within-R2	0.30	0.30	0.30	0.30
Observations	2243	2243	2243	2243
F-statistic (line 1)	110.94	106.28	122.36	127.64
F-statistic (line 2)	4.79	11.52	7.43	18.38

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. The table shows the outcome equation using 'corruption control' but suppresses control variables and all auxiliary equations. All predictors are included at their second lag. In each panel, regressions include an IMF dummy, which is instrumented using a standard probit selection equation. For each type of conditionality, the respective number of conditions is instrumented using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table 5. IMF-mandated privatizations decrease control of corruption: summary of methods and findings.

Dependent variable	Key predictor	Method to correct for possible endogeneity	Main finding	Source
1) ICRG control of corruption	a) Number of binding IMF privatization conditions	i) Control function and instrumental-variable approach: IMF participation probit; IMF conditionality instrumented by interaction of mean conditions within country and number of countries under program.	IMF privatizations decrease control of corruption after two, three, four and five years (all $p<0.10$).	Table 2
		ii) As per i), plus controls for other policy area conditions (one per model)	IMF privatizations decrease control of corruption after two years when controlling for revenue ($p<0.01$), public-sector employment ($p<0.10$), price liberalization ($p<0.01$), and trade liberalization ($p<0.05$).	Table 4
		iii) As per i), but IMF conditionality instrumented by interaction of mean conditions within country and mean conditions across years	IMF privatizations decrease control of corruption after two ($p<0.10$), three ($p<0.10$), four ($p<0.10$), and five ($p<0.05$) years.	Table A2
		iv) Instrumental-variable approach for number of IMF conditions, but only among IMF program observations	IMF privatizations decrease control of corruption after four ($p<0.10$) and five ($p<0.05$) years.	Table A3
		v) Error Correction Model with control function and instrumental-variable approach	IMF privatizations decrease control of corruption in the long-term ($p<0.01$).	Table A4
	b) Number of binding IMF privatization conditions, deducting waived conditions	As per i)	IMF privatizations decrease control of corruption after one ($p<0.10$), two ($p<0.10$), three ($p<0.10$), four ($p<0.10$) and five ($p<0.05$) years.	Table 3
2) Transparency International corruption perception index	As per a)	As per i)	IMF privatizations decrease control of corruption after one year ($p<0.10$).	Table A1
3) VDem corruption index			IMF privatizations increase corruption after one ($p<0.05$), two ($p<0.05$), and three ($p<0.10$) years.	
4) HRV government transparency index			IMF privatizations reduce transparency after two and three years (all $p<0.10$).	

Privatization and the fueling of corruption: Evidence from IMF programs in developing countries

Supplemental appendices

Contents

Table A1: Different measures of corruption.

	Corruption perception index			Corruption index			Government transparency		
	t-1	t-2	t-3	t-1	t-2	t-3	t-1	t-2	t-3
Privatization conditions	-2.014*	-1.242	-1.182	0.058**	0.061**	0.055*	-0.739	-0.698*	-0.728*
	(1.152)	(1.107)	(1.038)	(0.029)	(0.031)	(0.028)	(0.642)	(0.372)	(0.376)
IMF program	0.242	0.508	1.056	0.031	0.027	0.025	0.890**	0.825*	0.746*
	(0.994)	(0.925)	(0.808)	(0.019)	(0.019)	(0.018)	(0.401)	(0.428)	(0.407)
Control variables	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1497	1497	1496	3219	3116	3012	1322	1322	1319
Within-R2	0.23	0.22	0.21	0.09	0.08	0.08	0.40	0.40	0.40
F-statistic	115.63	99.36	98.17	116.66	100.53	100.57	25.71	17.12	21.59

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. The table shows the outcome equation using alternative indicators of corruption: TI CPI (higher values indicate better control of corruption); VDem Corruption Index (higher values indicate more corruption in general); HRV transparency (higher values indicate more transparency, restricted to sample in main analysis). All predictors are included at the lag specified in the column header. Lags of privatization deeper than three years not statistically significant with these corruption measures. Each regression included an IMF dummy as a control variable, which was further modeled using a selection equation to take non-random selection into account. Privatization conditions are instrumented using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table A2: Alternative operationalization of compound instruments.

	t-1	t-2	t-3	t-4	t-5
Panel A					
Privatization	-0.134	-0.147*	-0.137**	-0.142**	-0.141**
	(0.101)	(0.083)	(0.06)	(0.063)	(0.067)
Panel B					
Revenues	0.088	0.100	0.118	0.134	0.133
	(0.075)	(0.085)	(0.094)	(0.098)	(0.097)
Panel C					
Public sector	-0.002	-0.062	-0.116	-0.154	-0.145
	(0.123)	(0.134)	(0.153)	(0.174)	(0.178)
Panel D					
Price liberalization	0.143	0.157*	0.182*	0.175	0.145
	(0.089)	(0.094)	(0.106)	(0.112)	(0.108)
Panel E					
Trade liberalization	0.188	0.215	0.209	0.198	0.143
	(0.125)	(0.131)	(0.145)	(0.143)	(0.137)
Control variables	yes	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes
Within-R2	0.26	0.26	0.26	0.26	0.27
Observations	2599	2597	2595	2593	2520
F-statistic (mean)	102.22	99.08	94.23	90.39	89.07

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. The table shows the outcome equation using 'control of corruption'. All predictors are included at the lag specified in the column header. Each regression included an IMF dummy as a control variable, which was further modeled using a selection equation to take non-random selection into account. For each type of conditionality, the respective number of conditions is instrumented using the interaction between the country-specific mean of these conditions and the annual mean of conditions. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table A3: Conditionality effects under different control group.

	t-1	t-2	t-3	t-4	t-5
Panel A					
Privatization	-0.18	-0.29	-0.347	-0.414*	-0.426**
	(0.211)	(0.216)	(0.226)	(0.228)	(0.205)
Panel B					
Revenues	0.127	0.245	0.28	0.279	0.165
	(0.193)	(0.229)	(0.253)	(0.259)	(0.198)
Panel C					
Public sector	0.188	0.205	0.138	-0.017	-0.059
	(0.414)	(0.494)	(0.541)	(0.557)	(0.542)
Panel D					
Price liberalization	0.253	0.265	0.241	0.196	0.137
	(0.184)	(0.212)	(0.221)	(0.206)	(0.181)
Panel E					
Trade liberalization	0.177	0.177	0.135	0.091	0.003
	(0.157)	(0.161)	(0.153)	(0.136)	(0.127)
Control variables	yes	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes
Within-R2	0.27	0.29	0.31	0.30	0.29
Observations	1148	1156	1161	1159	1124
F-statistic (mean)	11.53	11.12	10.73	10.51	11.00

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. The table shows the outcome equation using 'control of corruption'. All predictors are included at the lag specified in the column header. Each regression only includes under-program observations and thus compares country-year observations with conditions to country-year observations without conditions. For each type of conditionality, the respective number of conditions is instrumented using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table A4: Dynamic effects of IMF conditions on corruption control.

	Privatization	Revenues	Public sector	Price liberalization	Trade liberalization
Δ.(Conditions)	-0.014	0.004	0.011	0.010	0.006
	(0.025)	(0.008)	(0.011)	(0.008)	(0.004)
Lagged conditions	-0.257***	-0.073	-0.055	-0.071	-0.057**
	(0.078)	(0.055)	(0.076)	(0.062)	(0.028)
Δ.(IMF program)	-0.004	-0.01	-0.006	-0.009	-0.025
	(0.023)	(0.023)	(0.023)	(0.022)	(0.027)
Lagged IMF program	0.028	0.015	-0.002	0.002	0.001
	(0.043)	(0.045)	(0.05)	(0.043)	(0.05)
Lagged dependent variable	-0.152***	-0.154***	-0.154***	-0.153***	-0.150***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Control variables	yes	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes	yes
Linear trend	yes	yes	yes	yes	yes
F-statistic	138.08	7.01	14.01	11.29	33.71
Within-R2	0.11	0.10	0.10	0.10	0.10
Observations	2097	2097	2097	2097	2097

Notes: Error Correction Model implemented via multiple-equation instrumental-variable maximum-likelihood regression. The dependent variable in the outcome equation is the difference in corruption control, regressed on lags and differences of IMF program, conditions (as indicated in the column head), and control variables (not shown), respectively. The level of the IMF dummy is instrumented using a standard probit selection equation. The level of the specified conditions is instrumented with a compound instrument using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table A5: Taking government spending into account.

	Government expenditure	Change in government expenditure	Change in government expenditure	Military expenditure	Change in military expenditure	Change in military expenditure
Privatization conditions	-0.313*	-0.320*	-0.321*	-0.205	-0.179	-0.178
	(0.186)	(0.190)	(0.189)	(0.190)	(0.190)	(0.190)
Expenditure	0.008	0.011*	0.011*	0.007	-0.015***	-0.015***
	(0.013)	(0.006)	(0.006)	(0.008)	(0.003)	(0.003)
Privatization X Expenditure			0.000			-0.006
			(0.009)			(0.062)
IMF program	-0.099	-0.104	-0.104	-0.087	-0.083	-0.083
	(0.152)	(0.152)	(0.152)	(0.154)	(0.157)	(0.157)
Control variables	yes	yes	yes	yes	yes	yes
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
Observations	2152	2139	2139	1962	1913	1913
Within-R2	0.31	0.31	0.31	0.29	0.30	0.30
F-statistic	98.83	98.89	98.90	98.78	98.82	98.80

Notes: Multiple-equation instrumental-variable maximum-likelihood regression. The table shows the outcome equation using 'control of corruption'. The variable 'expenditure' refers to the variable in the respective column header. All predictors are included at the third lag. The number of conditions is instrumented using the interaction between the country-specific mean of these conditions and the number of countries under IMF programs in a given year. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table A6: Standardized coefficients for estimation using 'control of corruption'.

	(1)	(2)	(3)	(4)	(5)
Privatization conditions	-0.169*	-0.164*	-0.144*	-0.067**	-0.169
	(0.097)	(0.087)	(0.086)	(0.029)	(0.110)
IMF program	-0.076	-0.061	-0.150	-0.095	
	(0.150)	(0.170)	(0.192)	(0.151)	
Trade liberalization conditions			0.026		
			(0.022)		
Log(GDP per capita)	-0.228	-0.342	-0.212	-0.193	-0.302
	(0.227)	(0.250)	(0.229)	(0.228)	(0.367)
Regime durability	0.113	0.118	0.110	0.108	0.056
	(0.094)	(0.106)	(0.094)	(0.095)	(0.105)
Polity IV index	0.140*	0.113	0.140*	0.135*	0.013
	(0.079)	(0.080)	(0.080)	(0.079)	(0.067)
Urbanization	-0.115	-0.127	-0.117	-0.126	0.369
	(0.333)	(0.382)	(0.331)	(0.334)	(0.377)
Mineral rents	-0.172**	-0.173**	-0.171**	-0.173**	-0.129
	(0.069)	(0.071)	(0.069)	(0.070)	(0.102)
Log(Oil per capita)	-0.342**	-0.420**	-0.320**	-0.317*	-0.714**
	(0.164)	(0.186)	(0.162)	(0.164)	(0.301)
Main equation					
... Observations	2231	2231	2231	2231	2231
... Within R-squared	0.293	0.293	0.292	0.292	0.293
IMF program equation					
... Observations	2120	2120	2120	2120	
... Pseudo-R2	0.329	0.329	0.248	0.329	
Conditions equation					
... Observations	3039	2720	3039	3039	3039
... Within R-squared	0.144	0.141	0.147	0.344	0.144
... F-statistic	98.823	108.372	82.621	241.453	32.712

Notes: The table shows a select number of regressions for the main outcome (ICRG index) using a standardized dataset. All predictors (except IMF program) thus have zero mean and unit variance. All estimations apply the thrice-lagged number of privatization conditions, except model 3 which uses the second lag for consistency with the main table. Model 2 uses the implementation-corrected number of privatization conditions. Model 4 uses the alternative compound instrument. Model 5 drops the IMF equation and thus makes comparisons only among IMF programs. Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table A7: Standardized coefficients for additional outcomes.

	Corruption perception index	Corruption index	Government transparency
Privatization conditions $_{t-3}$	-0.053	0.127*	-0.207*
	(0.047)	(0.065)	(0.110)
IMF program $_{t-3}$	0.098	0.122	0.391
	(0.075)	(0.087)	(0.241)
Log(GDP per capita) $_{t-3}$	0.365	-0.032	0.080
	(0.260)	(0.159)	(0.221)
Regime durability $_{t-3}$	0.057*	-0.056	0.065
	(0.031)	(0.039)	(0.073)
Polity IV index $_{t-3}$	0.092	-0.054	0.005
	(0.060)	(0.056)	(0.047)
Urbanization $_{t-3}$	0.142	-0.241	-0.011
	(0.244)	(0.169)	(0.165)
Mineral rents $_{t-3}$	-0.076***	0.053*	0.027
	(0.024)	(0.027)	(0.024)
Log(Oil per capita) $_{t-3}$	0.048	-0.001	-0.109
	(0.098)	(0.067)	(0.216)
Country-fixed effects	yes	yes	yes
Year-fixed effects	yes	yes	yes
IMF program equation	yes	yes	yes
Conditionality equation	yes	yes	yes
Observations	1496	3012	1281
Within R-squared	0.206	0.077	0.398
F-statistic	87.085	88.303	31.127

Notes: The table shows a select number of regressions for additional outcomes using a standardized dataset: TI CPI (higher values indicate better control of corruption); VDem Corruption Index (higher values indicate more corruption in general); HRV transparency (higher values indicate more transparency, restricted to sample in main analysis). All predictors are included at the third lag (but results for other lags are similar to the unstandardized ones). Robust standard errors clustered on countries in parentheses.

Significance levels: * .1 ** .05 *** .01.

Table B1: Variable definition and data sources

Variable	Description	Sources
Dependent variables		
Control of corruption	Expert survey-based measure of bureaucratic quality from the International Country Risk Guide (ICRG). According to the methodology, "high points are given to countries where the bureaucracy has the strength and expertise to govern without drastic changes in policy or interruptions in government services. In these low-risk countries, the bureaucracy tends to be somewhat autonomous from political pressure and to have an established mechanism for recruitment and training"	The PRS Group 2015
Corruption perception index	Corruption perception index from Transparency International (TI-CPI). This index is available from 1995 onwards, ranking countries by their perceived levels of corruption, as determined by expert assessments and opinion surveys. Sourced from the QoG institute	Teorell et al. 2016
Corruption index	Corruption index from the Varieties of Democracies project (VDem). The index combines four sub-indices: public sector corruption index ('petty corruption'), executive corruption index ('grand corruption'), the indicator for legislative corruption, and the indicator for judicial corruption. These four different government spheres are weighted equally in the resulting index. Missing values for countries with no legislature are replaced by only the average of the remaining three indicators. Values for sub-indices are obtained from Bayesian factor analysis	Coppedge et al. 2016, 66
Government transparency	HRV transparency index, measuring the willingness of a country to disclose administrative information (based on an Item Response Model of reporting to the World Development Indicators)	Hollyer, Rosendorff, and Vreeland 2014
Covariates for outcome equation		
IMF program	Binary variable indicating the presence of an IMF program (being active in at least one month in a given year)	Kentikelenis, Stubbs, and King 2016
Privatization	Number of (binding) conditions regarding divestiture of the state from state-owned enterprises (including liquidation and bankruptcy proceedings)	
Price liberalization	Number of (binding) conditions regarding subsidies, price liberalization, marketing boards, and corporatization and rationalization	
Public sector	Number of (binding) conditions on public sector reforms, including employment levels, salaries, wage schedules, pensions, and social security	

Revenues	Number of (binding) conditions regarding government revenue, customs administration, tax policy, tax administration, and audits	
Trade liberalization	Number of (binding) conditions on trade liberalization, exchange rate policy, capital account liberalization, foreign direct investment, and foreign reserves	
Log(GDP per capita)	Natural log of GDP per capita (in constant 2005 USD) [NY.GDP.PCAP.KD]	World Bank 2015
Polity IV index	Combined index of Polity IV, defined as the democracy score net of the autocracy score, from the QoG database (Teorell et al. 2016)	Marshall, Gurr, and Jaggers 2015
Regime durability	Regime durability (total years of existence of current regime), from the QoG database (Teorell et al. 2016)	Marshall, Gurr, and Jaggers 2015
Mineral rents	Mineral rents as of GDP [NY.GDP.MINR.RT.ZS]	World Bank 2015
Log(Oil per capita)	Natural log of one plus oil production in metric tons divided by total population	Ross 2013
Urbanization	Urban population (% of total) [SP.URB.TOTL.IN.ZS]	World Bank 2015

Additional covariates for selection stage

Countries under program	Number of countries under any IMF program (enters the model contemporaneously with respect to IMF program)	Kentikelenis, Stubbs, and King 2016
UNGA Voting Alignment	Average vote alignment index of the country with all G7 countries	Bailey, Strezhnev and Voeten 2015
Reserves (in months of imports)	Total reserves in months of imports [FI.RES.TOTL.MO], from the QoG database (Teorell et al. 2016)	World Bank 2015
Debt service (% GNI)	Total debt service (% of GNI) [DT.TDS.DECT.GN.ZS], from the QoG database (Teorell et al. 2016)	
GDP growth	GDP growth (annual %) [NY.GDP.MKTP.KD.ZG], from the QoG database (Teorell et al. 2016)	
Executive election	Binary variable indicating a executive election, from the QoG database (Teorell et al. 2016)	Beck et al. 2001

Table B2: Correlation matrices

Main analysis

n=2,184		I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
I	Control of corruption	1											
II	IMF program	0.03	1										
III	Privatization	0.01	0.18	1									
IV	Revenues	-0.01	0.29	0.30	1								
V	Public sector	0.01	0.22	0.20	0.25	1							
VI	Price liberalization	-0.002	0.25	0.26	0.39	0.19	1						
VII	Trade liberalization	0.05	0.52	0.17	0.36	0.19	0.26	1					
VIII	Log(GDP per capita)	0.12	-0.28	-0.07	-0.15	-0.03	-0.11	-0.12	1				
IX	Regime durability	0.14	-0.20	-0.07	-0.10	-0.05	-0.10	-0.13	0.21	1			
X	Polity IV index	0.08	0.06	0.02	0.06	0.10	0.02	0.13	0.28	-0.09	1		
XI	Urbanization	0.05	-0.16	-0.02	-0.08	-0.04	-0.06	-0.04	0.80	0.07	0.22	1	
XII	Mineral rents (% GDP)	-0.09	-0.08	-0.04	-0.05	-0.004	-0.04	-0.01	-0.06	0.07	0.08	-0.09	1
XIII	Log(Oil per capita)	-0.09	-0.13	-0.02	-0.10	-0.05	-0.04	-0.12	0.43	0.02	-0.20	0.44	-0.08

Additional variables

n=897	Control of corruption	Corruption perception index	Corruption index	Government transparency
Control of corruption	1			
Corruption perception index	0.53	1		
Corruption index	-0.40	-0.70	1	
Government transparency	0.33	0.43	-0.40	1