The political economy of labor market deregulation during IMF interventions

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Abstract: This study examines the relationship between policy interventions by the International Monetary Fund (IMF) and de jure labor rights. Combining two novel datasets with unprecedented country-year coverage—leximetric data on labor laws and disaggregated data on IMF conditionality—our analysis of up to 70 developing countries from 1980 to 2014 demonstrates that IMF-mandated labor market policy measures significantly reduce both individual and collective labor rights. Once we control for the effect of labor market policy measures, however, we find that collective labor rights increase in the wake of IMF programs. We argue that this result is explained by the impact of union pressure on governments which, in such a context, are imbued with the policy space to respond to domestic interest groups. The study has broader theoretical implications as to when international organizations are effective in constraining governments' choices.

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Introduction

Social scientists have long considered how free market policies have diffused around the world (Chorev 2005; Dobbin, Simmons, and Garrett 2007; Fourcade-Gourinchas and Babb 2002; Meyer 2000). These accounts have examined the global trend towards ever-greater economic liberalization, and elaborated on the underlying processes of learning, imitation or coercion that yield high degrees of policy homogeneity (Elkins, Guzman, and Simmons 2006; Lee and Strang 2006; Simmons and Elkins 2004; Swank 2006). Among the key global forces promoting market liberalization, international financial institutions (IFIs)—like the International Monetary Fund (IMF) and the development banks—have been singled out as the world's most powerful agents of economic reform (Kentikelenis and Seabrooke 2017; Stone 2011; Stone and Steinwand 2008). The lending activities of these organizations to developing countries have been linked to poor economic performance (Barro and Lee 2005; Dreher 2006), "hollowed-out" state capacity (Reinsberg et al. 2018), and the dismantling of social policies (Nooruddin and Simmons 2006; Stubbs et al. 2018; Stubbs and Kentikelenis 2018b).

Even though this free market revolution in the developing world has its roots in the 1980s (Babb and Kentikelenis 2018), the full range of its socio-economic consequences is only now beginning to become apparent. In this context, a growing strand of literature has explored the impact of IFIs on labor (Blanton, Blanton, and Peksen 2015; Martin and Brady 2007; Nooruddin and Vreeland 2010; Vreeland 2002). These organizations have notoriously sought to increase the "flexibility" of labor markets—that is, increasing employers' ease in hiring, firing or regulating working hours (Burgess 2010, 202)—in borrowing countries with the promise of future economic growth.

In this article, we elaborate on how IFI policies directly affect labor, with a focus on the lending activities of the IMF. We scrutinize the impact of IMF-mandated policies on labor rights. Previous scholarship has shown that labor rights are the most important labor issues, given their relationship with economic performance and human development (Aidt and Tzannatos 2002). Only few studies have focused specifically on the link between IMF programs and labor rights. Abouharb and Cingranelli (2007) measure collective worker rights for developing countries from 1981 to 2003 and find that countries with more years under IMF programs have lower protection of worker rights. Blanton, Blanton, and Peksen (2015) use a panel of 123 countries from 1985 to 2002 to show that the cumulative years a country is under an IMF program has a negative impact on labor rights laws and practices. Finally, Gunaydin (2018) establishes that left-wing governments can use the Fund as a scapegoat to promote labor market reform.

While these studies offer important insights on the links between IMF programs and labor rights, they contain some limitations. First, to date there are are no large-*N* cross-national statistical studies that cover different types of labor rights; for instance, collective labor rights (CLRs) include freedom of association, collective bargaining, and the right to strike, whereas individual labor rights (ILRs) refer to legal protections against insecure employment relationships, overtime working, and unduly flexible hiring-and-firing procedures. Second, despite calls for disaggregated analyses of IMF conditionality (Vreeland 2007), quantitative studies tend to establish aggregate effects of IMF programs, hence treating such programs as homogenous and not distinguishing the mechanisms via which they affect labor rights. Recent works have addressed this lacuna, albeit incompletely. For instance, Rickard and Caraway (2018) created a new dataset on labor conditions, but focused on wages, rather than labor rights, as the outcome of interest. Gunaydin (2018) is the

first to examine labor conditions, but equates labor market reform with the degree to which borrowing countries comply with labor conditions, rather than tracing changes in labor laws. Third, and relatedly, previous studies do not directly attribute labor policy changes to IMF pressures. To some extent, this is because they focus on labor rights practices—rather than laws—which makes attribution more difficult as they are co-determined by an additional set of factors not under the control of the IMF.

Why is understanding labor rights—and the balance between CLRs and ILRs—important? To be sure, there are often discrepancies between "law in books" and law in action (Pound 1910). However, the study of *de jure* rights offers meaningful insights because changes in practice are often preceded by changes in the law. Further, the distinction between CLRs and ILRs is important, as both can evolve in different ways. Lumping different types of labor rights together would thus be inappropriate.

Our study advances these theoretical and empirical debates by examining the relationship between IMF programs and labor rights laws for a panel of developing countries between 1980 and 2014. Using a new dataset of labor rights laws (Adams et al. 2016), we distinguish between ILRs and CLRs. We also use a new dataset on IMF conditionality (Kentikelenis, Stubbs, and King 2016) to capture the impact of the various types of policy reforms borrowing countries need to implement to access IMF credit. In line with expectations from international political economy scholarship (Abouharb and Cingranelli 2007; Blanton, Blanton, and Peksen 2015; Stubbs and Kentikelenis 2018a), we find that IMF labor conditionality exerts a significantly negative effect on both ILRs and CLRs.

Our data also allows us to address a related puzzle in the literature—the co-evolution of declining ILR and increasing CLR during IMF programs *when controlling for the effect of labor conditionality* (Murillo and Schrank 2005). To resolve this puzzle, we develop a theory of labor regulation during IMF intervention, drawing on scholarship in comparative political economy. Our hypothesis is that the increase in CLRs is a result of lobbying efforts by organized labor, as unions seek higher CLRs for acquiescing to lower ILRs. As previous scholarship has emphasized, unions seek to maintain their own organizational relevance in times of economic trouble (Davidsson and Emmenegger 2013; Emmenegger et al. 2011; Schmitter and Streeck 1999). This allows them to prioritize the protection of those rights that benefit their own influence and members—if necessary, at the expense of workers more generally (Murillo and Schrank 2005). The empirical implication of our argument is that in the wake of IMF programs, ILRs decrease while CLRs may improve, especially when unions are more powerful, as under circumstances of high union density, frequent strikes, and high urbanization.

In sum, our study contributes to long-standing scholarship on the links between globalized free markets and socio-economic rights (Blanton, Blanton, and Peksen 2015; Blanton and Peksen 2016; Mosley and Uno 2007). First, while scholars have long examined the effects of globalization on economic policies, particularly domestic labor standards (Huber and Stephens 2001; Mosley and Uno 2007; Neumayer and De Soysa 2006; Rudra 2008), they have only recently become interested in the conditions of policy (non-)convergence (Avelino, Brown, and Hunter 2005; Mosley 2008; Neilson and Stubbs 2016; Rudra and Haggard 2005). While these studies emphasize structural forces in the global economy as drivers of policy convergence, we shift attention to international

organizations—and the IMF in particular—as key agents of policy convergence, and show that IMF policy interventions can account for diverging labor rights.

Second, by disaggregating the mechanisms through which IMF programs affect labor rights laws, we demonstrate how IFI pressures and domestic politics interact to generate labor policy reform outcomes. When IMF programs do not include labor conditions, labor rights are determined by domestic bargaining dynamics, as governments can legislate CLR increases to accommodate union pressure. Conversely, when IMF programs include labor conditions, government discretion is lower. Considering international pressures and domestic bargaining dynamics together, our study can help explain the puzzles that actual labor market reforms often differ from the IMF policy preferences and that some labor rights increase while others decrease during IMF interventions. Previous studies were unable to identify this mechanism as they used a binary indicator for IMF programs, obtaining a negative aggregate effect of such programs on labor rights.

Third, owing to a lack of global data, most comparative political economy research focuses on economic policy-making in rich countries, and the ensuing arguments may not apply to the distinct institutional environments of developing countries. For instance, global market forces are more prominent for developing countries (Rudra 2002; Simmons and Elkins 2004; Wibbels and Arce 2003), and international organizations have greater sway over domestic policy decisions. While our results confirm the important role of such global forces, they also suggest that core comparative political economy insights extend to developing countries, notably regarding the bargaining dynamics between governments and powerful interest groups. Our article hence addresses important knowledge gaps on the role of labor market institutions and domestic politics in developing countries.

The relationship between IMF programs and labor rights

The evolution of labor rights during IMF interventions

There is near-consensus in the literature that the Fund has pushed governments to deregulate labor markets, oftentimes through explicit labor conditionality (Blanton, Blanton, and Peksen 2015; Burgess 2010; Gunaydin 2018). A majority of IMF labor policy conditions target ILRs—the laws governing employment contracts, working conditions, and hiring-and-firing provisions (Burgess 2010, 202). For example, an IMF staff report on Romania states: "[L]abor market rigidities are impediments to a business-friendly environment and Romania stands out compared with other countries, particularly on costs of hiring and firing workers" (IMF 2006, 29). The IMF has advocated similar conditions in other countries, promoting labor laws that legalize temporary work contracts, extend probation periods, and reduce the cost of firing workers (Caraway, Rickard, and Anner 2012, 33). Indeed, most Latin American governments have made changes to their laws governing hiring, firing, and work hours in the past twenty years as a result of IMF pressure (Burgess 2010, 214). When the IMF was called to rescue indebted countries in the midst of the Asian Financial Crisis, it asked for more flexible ILRs, specifically on firing provisions (Caraway 2009, 161–62).

The Fund has also requested governments to reduce CLRs, albeit to a lesser extent than ILRs. CLRs pertain to organizing activity, collective bargaining, and strikes.[1] IMF conditions have mandated the decentralization of collective bargaining, thus undermining the power of dominant unions (Burgess 2010, 213). For example, in its program with Macedonia, the IMF asked the government to "[a]mend the Law on the Chamber of Commerce to allow existing members of employers' associations to terminate their membership prior to the negotiation of the next collective agreements" (IMF 2005, 59)—with the expected result being an increase in firms not bound by collective bargaining arrangements protecting workers' rights. Labor conditions can also modify contractual provisions in the public sector (Nelson 1992, 229). While most of these conditions entail reductions in the public sector wage bill, some also affect pensions and other employee entitlements (Rickard and Caraway 2018).

Do labor conditions in IMF programs affect labor regulations? In many cases of labor market reforms, it is possible to identify specific IMF-mandated labor policy conditions that precede such reforms, thus suggesting a causal impact of IMF intervention. But oftentimes the reforms that governments adopt differ from the IMF-mandated policy measures, leaving a puzzle to explain. Consider the example of Korea. In the midst of the Asian Financial Crisis, the country turned to the Fund for assistance and obtained a USD 21 billion stand-by credit line. Structural reforms of the Korean program included measures to "enlarge the scope for layoffs" alongside labor re-training measures and improved unemployment insurance (IMF 1998). Indeed, the Korean government relaxed the rules of dismissals. In particular, it extended employer discretion in employment adjustments to allow dismissals for managerial reasons and removing the requirement of prior court permission (Adams et al. 2016, 396). However, Korea simultaneously adopted measures implying significant CLR improvements. In particular, while lockouts were considered to be lawful as a retaliative measure to strikes, the revised CLR law states that lockouts must be reported to the relevant authorities in advance. Furthermore, the revised CLR code allows trade unions during a strike to be supported by an outside party, such as a union federation subject to notification to the relevant authorities (Adams et al. 2016, 398).

The Fund did not prevent these legal changes resulting from government–union collusion. In a speech at Sogan University in January 1998, the IMF director for the Asia and Pacific region emphasized the role of the newly established tripartite committee in "produc[ing] a tripartite consensus" as to how to "distribute the burden of adjustment in a fair manner" (IMF 1998). The case of Korea seems to be no outlier. In many cases of IMF interventions, a characteristic combined pattern of declining ILRs *and* improving CLRs emerges, consistent with earlier qualitative studies (Caraway 2010; Cook 2007; Murillo and Schrank 2005).

Theory and hypotheses

How can one explain the co-occurrence of declining ILRs and increasing CLRs during IMF interventions? We argue this pattern is the outcome of the interplay between the government, the Fund, and organized labor. When devising labor policy, governments must consider competing interests, notably pro-deregulation IMF preferences and pro-regulation union interests. First and foremost, governments have incentives to adopt labor market reform in line with IMF policy

[1] CLRs should be distinguished from core labor standards—developed by the International Labor Organization and endorsed by the IMF—which outlaw child labor and all forms of forced labor, discrimination at the workplace, and repression of collective labor organization.

prescriptions because failure to do so may cause suspension of IMF credits and loss of IMF approval, which may lead to dwindling investor confidence, economic disruption, and government crisis. Consequently, the involvement of the Fund as an agent with free-market preferences shifts policy against the interests of labor. This is especially so when IMF programs include specific labor conditionality, in which case governments will have little policy space to forego these and related measures.

Labor deregulation hypothesis: Labor conditionality decreases ILRs and CLRs.

Yet, as Blanton, Blanton, and Peksen (2015, 327) argue, "[e]ven if labor issues are not explicitly addressed in the conditions of a given loan package, participation in IFI programs sends a clear signal to domestic groups, as well as to the global marketplace, that the recipient country is reforming its economy along the lines of the Washington consensus." That is, governments dismantle barriers to global trade and capital, maintain restrictive monetary policy, and provide minimal regulation of businesses. This can have spillover effects on labor rights; and, indeed, some previous research shows a negative relationship between trade integration and labor rights (Mosley and Uno 2007). With regard to ILRs, we can thus formulate the following hypothesis.

Market liberalization hypothesis: If a country is under an IMF program, controlling for the effect of labor conditionality, ILRs deteriorate.

However, for CLRs, this is not where the story ends. Governments must also cater to powerful domestic interests, which generally oppose reductions in labor rights. Organized labor is a key interest group that opposes labor rights reductions and has significant leverage over government policy. For instance, organized labor can credibly threaten to punish the government for removing labor protections by withholding campaign contributions, initiating large-scale strikes, and mobilizing public protests. In many developing countries, the majority of employees work in the public sector, who are usually organized in unions (Haggard and Kaufman 1989, 224). As political economists have pointed out, the state-owned enterprise sector is "the lynchpin of a reputedly powerful coalition of beneficiaries with well-established claims to public resources" (Waterbury 1992, 183). Governments therefore must be careful not to alienate public-sector workers through cuts in public expenditure, hiring freezes, and redundancies.

The announcement of an IMF program signals to domestic constituencies that the government will reform its economy according to Washington Consensus prescriptions, potentially resulting in policy measures aimed at legalizing less secure forms of employment, reducing regulations of work conditions, and hiring-and-firing restrictions. Anticipating a reduction in labor rights as part of IMF programs, unions will thus mobilize against labor market reforms. However, unions—expecting to make at least some concessions in the negotiations over labor regulation—will strategically lobby for the types of labor protections that are most valuable to them.

Unions have incentives to prioritize CLRs when lobbying governments because the gains from CLRs (unlike ILRs) are concentrated on organized workers, while costs of CLRs are relatively diffuse as they are distributed across employers, consumers, and taxpayers (Murillo and Schrank 2005). In a similar vein, Davidsson and Emmenegger (2013) advance the argument that unions protect their organizational interests, notably by codirecting labor market policy reform, inasmuch as they seek to represent their members. In times of economic pressure, unions deemphasize short-term interests, such as employment benefits to their individual members, while salvaging their

long-term interests, such as their ability to codetermine labor policy and work conditions. In other words, they prioritize long-term survival over short-term interests (Schmitter and Streeck 1999, 54).

Therefore, we expect unions lobby their governments for higher CLRs, while acquiescing to lower ILRs. Governments can readily accept this deal. For them, the cheapest way to obtain IMF credit while keeping unions at bay is to reduce ILRs—thereby following IMF requests to deregulate labor markets—and to simultaneously legislate improvements in CLRs—thereby accommodating labor unions and luring them into accepting the labor reform package.

Union mobilization hypothesis: If a country is under an IMF program, controlling for the effect of labor conditionality, CLRs increase as a result of union pressure.

Anecdotal evidence corroborates our theoretical argument. Consider the case of Korea, which witnessed a simultaneous reduction in ILRs and an increase in CLRs in the wake of the Asian Financial Crisis. Following a massive strike against the curtailing of ILRs in 1996, the Korean government revised the labor law again in 1997—primarily by providing guarantees for collective organization of unions and thus improving CLRs. Consistent with our argument, unions lobbied for CLR improvements, supported by organizations such as the OECD, which expressed concern about low CLRs—particularly the prohibition of unions for public servants, the prohibition of strikes for essential public industries, and the malfunctioning of the tripartite commission (Kim and Kim 2003, 357–58). In sum, while the international financial institutions promoted ILR reductions, organized labor pushed for higher CLRs and succeeded in doing so.

The Argentinean case provides further support for our argument (Cook 2007). In 1996, the Menem government agreed with the Fund to implement labor market reforms—including the lowering of severance pay, a prolonged probation period for new work contracts, and decentralization of collective bargaining. Some of these reforms "threatened core organizational interests such as union control over social welfare funds, contract terms in branch-level agreements, and the level at which collective agreements would be negotiated" (Cook 2007, 79). The major union—the Confederación General del Trabajo (CGT)—intensified its strikes and took the decrees to court, which deemed the collective bargaining reforms unconstitutional. After failing to deregulate labor by decree, Menem offered to work with the CGT on a new labor reform package, involving business associations in a consultative role. Negotiations lingered for another year until the government passed a new labor code in 1998—a mix of changes to several laws touching on both ILRs and CLRs. Some measures lowered employer costs by lowering some ILRs, for instance severance pay, but the government also abolished temporary contracts and reduced the probation period for new employees to one month (rather than following the IMF recommendation to set it at six months). In terms of CLRs, the law reaffirmed centralized collective bargaining, thus maintaining the chief role of CGT and protecting its organizational interests. Cook explained these compromises with the need for the government to fulfill its pledge vis-à-vis the Fund to issue a labor reform bill. However, the only way to comply with this deadline was to collaborate with the CGT, even though this produced an outcome that fell short of IMF expectations (Cook 2007, 80).

Our theoretical argument has additional testable implications with respect to the relationship between IMF programs and labor rights. Therefore, we exploit variation in the strength of labor unions in borrowing countries. In particular, the effect of an IMF program on CLRs should be more positive in countries with more effectively organized labor interests. Indeed higher 'potential labor

power' is positively associated with increased social spending in democracies by helping workers better organize collectively (Rudra and Haggard 2005). Similarly, previous research focusing on state capacity has shown that "[i]n countries where the political representation of labor is likely to be strong—as a result of more democratic political systems, powerful left-wing political parties, or higher rates of union membership—[…] increases in state capacity will be associated with better protection of labor rights" (Berliner et al. 2015, 128). By the same logic, union mobilization will have stronger beneficial effects for labor rights—notably CLRs—as labor power increases.

Labor power hypothesis: The CLR increase is stronger when labor is more powerful.

Data and methods

Labor rights data

Our main dependent variable is the Center for Business Research LABOR REGULATION INDEX, which captures the extent of protection of labor rights for a wide range of countries over the period 1970 to 2014. The data are based on the coding of 40 indicators based on primary legal documents in each country (Adams et al. 2016, 8).

The sub-index for ILRs entails three sub-components:

- Laws governing legal **forms of employment** (which types of contracts fall within the scope of regulation and how easy it is for employers to avoid worker-protective rules by adopting forms of work such as fixed-term employment, part-time work, and temporary agency work);
- Laws on **working time** (safeguard rules which labor law inserts into the employment contract, including working time limits and those on overtime; minimum wage laws are not included);
- Laws on **dismissal protection** (need to show good cause prior to dismissal and to observe due process in the termination decision, rules on probation periods, minimum notice periods, severance pay, notification of dismissals to third parties, and redundancy selection and priority in re-employment);

The sub-index for CLRs consists of two sub-components:

- Laws governing **employee representation** (constitutional guarantees of freedom of association and the right to collective bargaining, existence of duty to bargain on the part of the employer, rules governing closed shops, the extension of collective agreements, and codetermination at board level and in the workplace);
- Laws relating to **industrial action** (extent to which collective industrial action is regulated by laws such as those on the constitutionality of strikes, unofficial strikes, political strikes, pre-strike balloting and notice requirements, mandatory arbitration and conciliation, and dismissal and re-engagement of striking workers).

The CBR dataset has several advantages over existing datasets. First, it offers more comprehensive coverage. For instance, the widely employed Mosley and Uno (2007) dataset, which is based on Kucera's (2002) methodology, only includes the period 1985 to 2002; and the Cingranelli and

Richards (2010) dataset on worker rights ranges from 1981 to 2003. The dataset also covers both collective and individual labor rights, while previous global datasets cover the former. Second, by looking at the contents of labor laws across countries using a common coding protocol, the dataset focuses on *de jure* protections of labor rights. Most existing datasets focus on *de facto* labor rights, implying subjectivity in the ratings and a potential risk of mingling *de jure* rights and *de facto* protection that makes the resulting scores opaque. The few datasets that offer *de jure* rights data have less coverage than the CBR dataset. Although our focus on labor rights laws might be seen as a drawback, we argue that changes to them are a good predictor—if not a necessary condition—for significant changes in subsequent practices. Third, factor analysis confirms the presence of the posited five dimensions, showing high item correlation within each dimension and distinctiveness of items across dimensions.[2] Coding decisions also are fully replicable given a comprehensive codebook justifying each coding decision by referencing the primary legal source.

Our main analysis focuses on the two sub-indices, which we rescale to the range from 0 to 100 for ease of interpretation. In additional tests, we also use all five sub-components, each of them again rescaled to range from 0 to 100.

IMF conditionality data

Our key explanatory variables are based on the coding of IMF conditionality from all agreements between the Fund and its borrowers from 1980 to 2014 (Kentikelenis et al. 2016). This dataset offers a detailed account of all conditions over a range of issue areas. We expect that various IMF program components affect labor rights differently. Allowing for such effect heterogeneity, we employ several measures of IMF activity.

First, we use a binary indicator for an IMF PROGRAM being active in a given year. If used jointly with measures of IMF conditionality, this variable captures all residual effects of IMF programs—for instance, relating to Fund technical assistance. Second, we test for the impact of conditionality with a count for the number of LABOR CONDITIONS. We adopt a broad definition of labor conditions, referring to measures governing public-sector employment and private-sector work contracts. Examples of the former include requirements to enact "a law regulating dismissals of tenured public employees for inadequate performance and a constitutional amendment that establishes remuneration limits for public sector employees" (IMF 2000), while the latter may involve obligations to "pass a new labor code that facilitates work[ing] outside regular hours" (IMF 2004). The supplemental appendix includes an extended list of cases. Following established procedure, we include a simple count of conditions while only considering binding conditions, which directly determine scheduled loan disbursements (Copelovitch 2010; Stubbs et al. 2017; Woo 2013).[3]

Moderator variables

To test the labor power hypothesis, we include an interaction term between the IMF variables and an indicator of labor power. Three alternative indicators are available: URBANIZATION, UNION DENSITY, and NUMBER OF STRIKES. In urban areas, workers find it easier to organize collectively

[2] Replication code for a confirmatory factor analysis is available.

[3] The use of simple counts of IMF conditions—tantamount to assuming a linear effect between these conditions and labor rights laws—is consistent with established practice and reasonable because there is no natural order of importance of conditions.

due to higher population density and the prevalence of formal-sector jobs (Jones and Corbridge 2010). We measure urbanization as the percentage of the population living in cities (World Bank 2015). In addition, union density—the percentage of the workforce being union member—directly captures the organizational capacity of labor (Huber and Stephens 2001). Data are from the ILO industrial relations database (ILO 2017). Capturing *de facto* labor power, we compute the average number of strikes in the country over the 1980-2014 period, sourced from Databanks International (Banks and Wilson 2015).

Control variables

Since our preferred estimation includes fixed effects, we focus on time-varying variables. Following standard practice, we lag all control variables by one period to allow for some delay in their associated effects on labor rights laws. The supplemental appendix presents tables for summary statistics and detailed variable definitions and data sources for all variables.

We control for standard macroeconomic factors that may influence the legal protection of worker rights but also IMF treatments. In particular, we include a measure of logged GDP PER CAPITA to capture the general level of development. This variable is highly correlated with variables capturing the structure of the economy, which are therefore omitted from the baseline specification. We also include the natural logarithm of POPULATION.

Following the literature on the link between economic globalization and labor rights, we include two measures: TRADE OPENNESS—the logarithm of total trade flows (imports and exports) as a percentage of GDP—and logged FDI INFLOWS as a percentage of GDP. With respect to trade openness, previous studies expect a 'race-to-the-bottom' by which countries repress labor rights in order to maintain competitiveness in the global economy. In contrast, the 'climb-to-the-top' story suggests that countries improve their labor rights in order to attract foreign investors, assuming that these investors are interested in sustainable revenues (Rudra 2008).

Changes in labor law may also be due to democratization (Caraway 2009; Kim and Kim 2003; Martin and Brady 2007), because democratic institutions facilitate collective mobilization for labor rights (Blanton, Blanton, and Peksen 2015; Mosley and Uno 2007). At the same time, countries undergoing regime transition are more vulnerable to economic turmoil that lets them turn to the Fund (Haggard and Kaufman 1992). We therefore control for the POLITY IV index (Marshall, Jaggers, and Gurr 2010) measuring democratic institutions.

Furthermore, the political ideology of the government may affect protection of labor rights due to different partisan preferences. Previous studies suggest that leftist governments are more responsive to labor movements and therefore are more likely to legally protect labor rights (Berliner et al. 2015; Peksen and Blanton 2016). We hence include a binary measure for LEFT-WING GOVERNMENTS, drawn from the Database of Political Institutions (Beck et al. 2001). Another variable capturing preferences for labor protection is the dichotomous indicator ILO ratification, which measures whether a country had ratified core labor conventions—the Convention on the Freedom of Association and Protection of the Right to Organize (C087) and the Convention on the Right to Organize and Collective Bargaining (C098)—in a given year. While a positive relationship with labor rights laws might be expected, recent work argues that governments may

use the signing of these conventions strategically to conceal actual deterioration in labor rights (Peksen and Blanton 2016).[4]

Finally, in the extreme event of civil war, states arguably are unable to adopt relevant legislation. We therefore include a binary measure of CIVIL WAR (Abouharb and Cingranelli 2007; Blanton et al. 2015), based on the UCDP/PRIO definition of at least 25 battle deaths in a given year.

In addition to these control variables, we include country fixed effects to account for time-invariant country-level characteristics, and year fixed effects to control for common external shocks across all countries. Past literature has varied with regard to the former. At least three papers include country fixed effects (Berliner et al. 2015; Cole 2013; Davies and Vadlamannati 2013), while many other studies (partially) pool observations and base inference on panel-corrected standard errors (Greenhill, Mosley, and Prakash 2009; Mosley and Uno 2007; Peksen and Blanton 2016). Since we are interested in explaining within-country variation in the protection of labor rights, we use fixed-effects estimation.[5] Due to missing values in the control variables, the panel dataset is unbalanced and includes up to 70 developing countries, with more observations being available in later years of the sample period.[6] We also compute robust country-clustered standard errors to adjust for heterogeneity and serial correlation (Wooldridge 2002, 283).

Econometric methods

A key methodological challenge is that IMF programs and policy conditions may not be randomly assigned. To mitigate concerns about endogeneity, we estimate a system of equations including instrumental variables and allowing for correlated errors across equations (Roodman 2012). The simplest model (with only an IMF program dummy) involves two equations: one labor rights equation, and one equation on the determinants of IMF programs, with errors being connected through a cross-equation covariance parameter. This setup yields the same results as the conventionally used treatment effects model and hence serves to alleviate potential bias due to non-random selection into IMF programs. When we test for the impact of conditionality, we estimate a system of three equations, including these two equations and another equation to account for the endogeneity of IMF conditions. This setup is advantageous for our purpose because it allows us to capture the effect of IMF programs without conditionality and the additional impact of IMF conditionality in a single model. Previous research—by limiting the sample to program years—only identified the differential effect of IMF conditionality (Rickard and Caraway 2018), thereby being unable to estimate the overall effect of IMF policy interventions and potential dynamics beyond conditionality. In the following, we discuss the specifications of the auxiliary equations.

First, we specify a selection model for IMF programs using a set of covariates used by previous literature. As past involvement of a country in IMF programs reliably predicts current participation (Bird, Hussain, and Joyce 2004; Conway 1994; Easterly 2005), we include PAST PROGRAMS—a count variable indicating the number of years during the past five years in which a country had an IMF program. In addition, program participation is also affected by the extent to which the Fund

[4] Results are unaffected by the inclusion of this variable.

[5] A Hausman test also advises against using random-effects estimation. We also conducted augmented Dicky-Fuller tests for stationarity, as well as the Wooldridge test for serial correlation, which both indicated no problems (1%-level of significance).

[6] Without control variables, our analysis would be based on 114 countries.

has resources available, which depends on the current number of program countries (Vreeland 2003, 88). Hence, we include the contemporaneous count variable COUNTRIES UNDER PROGRAMS. The literature also has repeatedly shown that allies of big powers receive favorable treatment by international financial institutions (Dreher, Sturm, and Vreeland 2009; Thacker 1999). We thus measure the alignment of voting patterns between the borrowing country and the G7 countries in the United Nations General Assembly (UNGA VOTE ALIGNMENT). We also include LEFT-WING GOVERNMENT, given that political ideology affects the propensity of a government to turn to the Fund. Legal origin—as it correlates with contemporaneous institutions—can also affect the likelihood of IMF programs; therefore, we include an indicator of COMMON LAW, as well as regional dummies, and year dummies.[7] In robustness checks, we extend this list of covariates to include macroeconomic indicators—GDP per capita, economic growth, reserves in months of imports, current account balance—as well as the Polity IV index and an indicator for executive elections in a given year. These variables, which are all lagged one period further than the IMF dummy, are defined in the appendix. Note that at least one variable in this selection model should serve as exclusion restriction to reduce model dependence of our results. We argue that UNGA VOTE ALIGNMENT fulfills this criterion, because we do not see how geopolitics could directly affect labor conditions (conditional on other covariates included) other than through the propensity of obtaining IMF credit.[8]

Second, we specify a model for IMF conditions to address their potential endogeneity. For instance, democratic governments with powerful domestic groups may have a better bargaining position with the Fund and hence obtain fewer labor conditions, especially if they face upcoming elections (Rickard and Caraway 2014). To mitigate potential endogeneity, we adopt an instrumental-variable design. An instrument is a variable that partially correlates with IMF conditionality but whose impact on labor rights only operates through conditionality. Finding such an excludable instrument presents a formidable challenge. We address potential endogeneity of conditionality by using an instrumentation strategy that has been popularized mainly in aid effectiveness research (Lang 2016; Nunn and Qian 2014; Werker, Ahmed, and Cohen 2009). For each type of condition, we construct a compound instrument based on the interaction of the within-country average of these conditions and the annual number of countries under programs.

We argue this instrumentation is valid. First, the instrument fulfils the relevance criterion because when the Fund assists more countries in any period, its resources are more stretched so that it assigns more conditions to any given country as a safeguard measure (Dreher and Vaubel 2004; Lang 2016; Vreeland 2003). Second, the instrument likely fulfills the exclusion restriction because country-specific changes in conditionality that deviate from its long-run average are brought about only by an IMF decision that does not pertain to the given country—notably to issue more conditions to all its borrowers when its resources are more demanded (Stubbs et al. 2018). Conditional on all other macroeconomic covariates included as control variables in our conditionality equation, we cannot think of any direct pathway from the IMF budget constraint to labor legislation other than through conditionality. While our solution is not perfect, we note that there are currently no satisfactory solutions to address potential endogeneity of conditions in

[7] Inclusion of country-fixed effects would create an incidental parameter problem.

[8] A remaining possibility is that UNGA voting affects IMF conditionality. While this may be possible for the total number of conditions, it is not true for the number of labor conditions, which we verified in auxiliary regressions. Hence, this instrument fulfills the exclusion restriction.

general. We would be more confident in our findings though if we could show robustness to alternative choices on the time-varying component of the compound instrument. To that end, we use the IMF liquidity ratio in a robustness check (Lang 2016).

Results

Illustrative evidence

We first explore the relationship between IMF programs and labor rights graphically. To that end, we split the sample into countries that were never exposed to IMF programs and countries with at least one program over the past 30 years. We then calculate the difference in labor rights for these two groups of countries. Figure 1 shows that countries with IMF programs experienced a decline in ILRs relative to non-program countries over the past 30 years (particularly in the early 1990s though). In contrast, no clear pattern emerges when comparing the evolution of CLRs across these two groups of countries.

[Figure 1]

Next, we graphically explore the impact of labor conditionality on labor rights. Here, we only consider countries with IMF programs but divide them according to whether or not they ever had a labor condition. Again, we calculate the difference between the two groups. Figure 2 illustrates that countries with exposure to labor conditions suffered from a gradual decline in their ILRs, while at the same time having increased their CLRs. Interestingly, the countries with labor conditions have slightly higher levels of labor rights (both ILRs and CLRs) to begin with than the reference group, but their trajectories diverge over time. Both graphs—while unable to establish causality—suggest a negative effect of IMF interventions (particularly labor conditions) on ILRs.

[Figure 2]

Multivariate analysis

Table 1 allows us to untangle impacts of IMF labor conditions from other aspects of IMF programs on labor rights. In support of our hypothesis, we find that labor conditions reduce labor rights. In substantive terms, one additional labor condition reduces ILRs by more than seven points ($p<0.05$)—equivalent to half a standard deviation—and tends to reduce CLRs by 2.28 points ($p<0.1$). These effects need to be interpreted relative to the baseline scenario of the same country being under an IMF program but without labor conditions. For example, Bulgaria was under an IMF program in 1994 without a labor condition, implying predicted labor rights of *ILR*=63.0 and *CLR*=67.4. However, if it had been under a labor condition (as in 1997), predicted labor rights would have been *ILR*=55.5 and *CLR*=65.1 respectively.

[Table 1]

Table 1 also lets us consider the effect of IMF programs on labor rights in the absence of labor conditions. Then the respective term LABOR CONDITIONS drops out of the equation and the effect of interest is captured in the IMF PROGRAM variable. The results reveal a characteristic pattern whereby IMF programs are related to a significant reduction of ILRs but a significant increase in CLRs. Substantively, being under an IMF PROGRAM *without* labor conditions on average lowers

ILRs by 5.46 points ($p<0.01$), increases CLRs by 3.69 points ($p<0.01$), and has no significant impact on aggregate labor rights—compared to not being under an IMF program. Overall, the results lend strong support for our first two hypotheses, and we interpret these patterns as evidence of labor unions to prioritize collective bargaining rights over individual worker protection under heightened economic pressure.

To test the labor power hypothesis, we conduct split-sample analyses using two proxies for the strength of organized labor. Table 2 shows the results using urbanization as proxy variable. Table 3 shows the results using strikes incidence as proxy variable. In both cases, we find that the effect on CLRs of IMF programs with no labor conditions tends to be positive only in contexts in which labor is powerful. These findings provide evidence to suggest that governments must concede to labor to a larger extent when unions are more powerful and hence able to credibly threaten to disrupt the policy-making process. Where labor is more powerful, it can prevent CLR deterioration, thus corroborating our labor power hypothesis.

[Table 2]

[Table 3]

Before probing the robustness of our results, we briefly discuss the adequacy of our model specifications. In the outcome equations, coefficients of control variables (if significant) have their expected signs. For instance, left-wing governments are associated with better protection of CLRs, while ratification of ILO conventions is negatively related with ILRs, broadly consistent with studies on 'radical decoupling' (Cole 2013; Cole and Ramirez 2013; Peksen and Blanton 2016). Furthermore, civil war negatively affects labor rights laws, consistent with prior expectations. As is common in fixed-effect models, slow-moving covariates remain statistically insignificant.

In the selection model, we find evidence for recidivism given the statistically significant coefficient of past programs. Furthermore, countries allied to the major powers are more likely to receive IMF loans. Common law countries are less likely to turn to the IMF. Diagnostics for the above models indicate that our models are well-specified and fit the data reasonably well.

Turning to the IMF labor conditions equation, we find the compound instrument to correlate strongly with the number of labor conditions. This shows that our instrument is relevant. The Kleibergen-Paap F-statistics imply a similar conclusion. In most cases, the size of the weak-instrument bias is at most 10 percent of the endogeneity bias (Stock and Staiger 1997; Stock and Yogo 2005). To the extent that our compound instrument is excludable, our results also have a causal interpretation. Any potential remaining endogeneity bias would work against our posited effects because the IMF arguably would target countries with high levels of labor regulations, which—if uncorrected—would induce a positive correlation between IMF variables and labor rights. And yet, we find significantly negative effects, suggesting that labor conditions causally reduce *de jure* labor protection.

Robustness checks

We present additional robustness checks in the supplemental appendix and report on the results verbally due to space constraints. First, we choose a different set of control variables in the outcome equation—GDP per capita, GDP growth, trade openness, industry share in national output, labor force participation, Freedom House index, left-wing government, and ILO ratification

(Davies and Vadlamannati 2013)—which does not affect the main conclusions from the previous analysis but rather strengthens them.

Next, we extend the set of control variables in the selection equation, including macroeconomic variables and political covariates. While we find the aggregate effects of IMF programs to be less significant, we corroborate that labor conditions reduce labor rights. In the selection model, indicators of economic weakness and democratic governance make IMF program participation more likely.

In addition, we adopt a different instrumentation strategy and replace the time-varying component of the compound instrument. Lang (2016) uses as an instrument for IMF programs the interaction of the long-run propensity of a country to be under an IMF program with the IMF liquidity ratio—a measure of the availability of reserves to be loaned out to countries in need. We follow his approach and replace the number of countries under programs by the IMF liquidity ratio and the above interaction term in the selection equation. We also replace the instrument for IMF conditions by the long-run average number of conditions multiplied with the IMF liquidity ratio, arguing that the IMF will impose more conditions when its resources are stretched. The results lend even stronger support to our previous conclusions: IMF programs reduce ILRs but lead to an increase in CLRs, while labor conditions negatively affect labor rights indiscriminately.

We also assess to what extent our results could be driven by reverse causality (which is only relevant if instruments are not valid). Theoretically, endogeneity may reflect a scenario in which IMF-mandated labor conditionality is sought after by governments rather than imposed by the organization. This kind of potential endogeneity does not affect any of our predictions. Governments with pro-labor preferences will have labor conditions imposed on them by the IMF, yet this will not preclude them from having to negotiate with unions. Reform-minded governments may not need labor conditions imposed, because their goals align with IMF preferences, but will still need to placate organized labor—although labor conditions may still be sought so that the Fund can be used as a 'scapegoat' to implement unpopular policy and weaken the bargaining power of unions (Gunaydin 2018). To test for reverse causality, we regress labor conditions on lagged labor rights, using two-way fixed-effects panel models with standard control variables. We also conduct (non-parametric) t-tests comparing labor rights across groups with varying exposure to IMF interventions. None of these tests indicates reverse causality.

Finally, in the supplemental appendix, we also present additional analyses that lend further credibility to our results in at least three ways. First, we show that the negative effect on ILRs is due to more flexible firing and working time rules, while within CLRs, labor conditions affect employee representation. Second, we also find that de facto CLRs increase during IMF interventions, indicating that concessions to labor are not purely rhetorical. Third, by showing that labor force participation and the shadow economy are unaffected by IMF labor conditions, we dismiss the potential argument that IMF labor conditionality—while reducing labor rights for 'labor market insiders'—might help 'labor market outsiders' to get jobs in the formal economy.

Conclusion

This article examined the potential impact of IMF policy reform programs on the legal protection of labor in developing countries. Extending earlier related work, our article offers new insights on this relationship by combining two new datasets with hitherto-unavailable level of detail, which jointly cover 117 countries and 35 years of observations. The labor rights data allow us to distinguish between individual labor rights—covering regulations on forms of employment, working time, and hiring and firing—and collective labor rights, such as the right to form unions, collective representation, and the right to strike.

Theoretically, we expected a weakening of labor rights in the wake of IMF programs *with* specific labor policy conditionality because borrowing countries lack the policy space to circumvent such measures. In addition, we also expected that IMF interventions catalyze major resistance from organized labor, leading to labor rights improvements. We argued that unions—anticipating a deterioration of their privileges as a result of market-liberalizing policy reforms—lobby governments to increase CLRs for acquiescing to lower ILRs. Unions will prioritize CLRs because these rights benefit only organized labor, but costs associated with higher CLRs are distributed widely across society (Schrank and Murillo 2005). An empirical implication of this argument is that concessions to unions in the form of better CLRs should be strongest when organized labor is relatively powerful.

Multivariate regression analysis for over 70 countries from 1980 to 2014—accounting for non-random selection into IMF programs and potential endogeneity of labor conditions—corroborates our hypotheses. While the results show a positively significant residual impact of IMF programs (beyond labor conditions) on CLRs, we find this positive impact to be particularly pronounced in countries with high levels of urbanization and union density—two proxies for the organizational strength of labor—which further corroborates our argument. Our main result is robust against different measures, lag structures, and a variant of the instrumental variable. Yet, a limitation of our study is that quantitative data does not allow us to provide definitive evidence on the posited union mobilization hypothesis; follow-up case studies could bolster the argument by illustrating the mechanism in action. Another limitation is that our labor conditionality measure prevents us from assessing the extent of policy space reduction implied by such conditions; further disaggregation of the dataset would allow for such an assessment.

These results have important implications for several key debates in social-scientific research. First, while previous research established that free-market policies undermine socio-economic rights (Abouharb and Cingranelli 2007; Blanton, Blanton, and Peksen 2015; Blanton and Peksen 2016), our study demonstrates that IMF conditionality frequently includes measures requiring countries to reform rigid labor markets, and that these policy conditions effectively reduce labor rights. Second, our findings offer important lessons for compliance studies (Holzinger, Knill, and Sommerer 2008; Peksen and Blanton 2016; Simmons 2000). Policy convergence occurs only when external pressure eliminates policy discretion for borrowing governments, notably by explicitly including policy conditions to deregulate labor markets as a precondition for IMF loans. The explanation is straightforward: As labor market reforms are unpopular with governments for domestic reasons, the IMF must deploy labor conditionality to make countries adopt such reforms. Consequently, policy convergence is less likely when IMF programs do not explicitly require labor market deregulation. The effects of IMF programs *without* labor conditions are determined by

domestic politics rather than foreign pressures. Third, our political economy perspective also helps illuminate the puzzling co-occurrence of declining ILRs and growing CLRs during IMF programs (Murillo and Schrank 2005).

Our findings also have ramifications for key domestic constituents. In particular, non-unionized labor has the most to lose when their government borrows from the IMF. Regardless of whether or not an IMF program includes labor conditionality, they can expect a blanket dismantling of their rights. For unionized labor, the waters are more muddied. When an IMF program includes labor conditionality, they can anticipate an improvement in their rights; however, given that around 70% of programs *do not* include labor conditionality, they too should be weary of their government entering an arrangement with the IMF, in which under most circumstances they can expect the same treatment as non-unionized labor. Businesses come out as clear winners in virtually all scenarios, as diminishing labor rights imply greater bargaining power for employers and—consequently—the ability to pay workers even less. Furthermore, to the extent that unionized labor is concentrated in the public sector, employers may also be relatively unscathed by the domestic wrangles between organized labor and the government in IMF programs that do include labor conditionality. Nevertheless, this optimistic outlook for business must be tempered by evidence elsewhere showing the negative impact of IMF programs on economic growth (Dreher 2006).

Our study has shown that IMF policy pressures over the past 30 years have reduced domestic labor rights, begging the question of whether the IMF has drawn the lessons from the past—for example by adopting a more labor-friendly policy stance in recent years. Indeed, the IMF now seems to recognize the importance of "designing labor market institutions so that they enhance flexibility while protecting workers" (Blanchard, Jaumotte, and Loungani 2013, 20); key economists within the IMF note that "greater flexibility can pose challenges for workers, especially those with low skills, and hence play an important role in explaining inequality developments" (Dabla-Norris et al. 2015, 21). Further, the IMF has recently advocated social measures to remedy the impacts of deregulation. However, "the main focus of [its] policy recommendations regarding labor in Latin America has been the deregulation of labor markets, especially the rules governing hiring, firing, and work hours" (Burgess 2010, 215). Our data indicates no significant change in policy—albeit a change in rhetoric—which makes us skeptical about the scope for policy reform inside the IMF (Kentikelenis, Stubbs, and King 2016).

Future research could further scrutinize other policy areas of IMF conditionality and their labor rights impact. Plausible arguments suggests that free-market conditionality beyond labor—for example relating to trade liberalization, privatization of state-owned enterprises, and financial sector liberalization—can adversely affect ILRs. However, as these policy conditions only indirectly affect labor rights, more research is necessary to identify conditions under which they unfold these effects. Research should also address the conditions under which IMF interventions are more or less effective. Our tests have produced results in line with the comparative politics literature showing that the strength of organized labor is a significant moderator of IMF pressure, but the insignificant findings on other domestic politics variables warrant further investigation.

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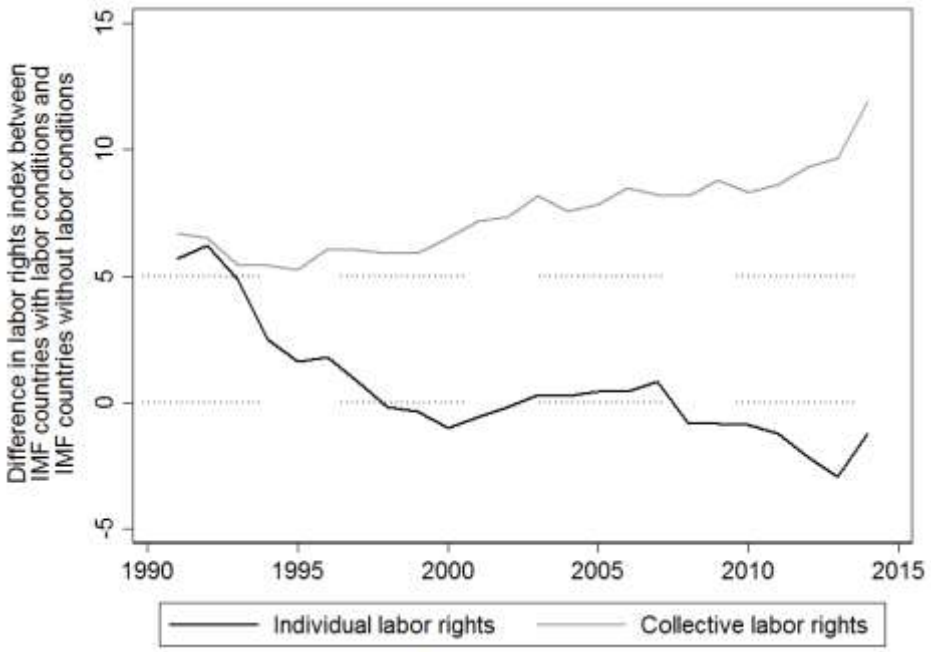
Figures and tables

Figure 1: The differential impact of IMF program exposure on labor rights in the developing world.

Notes: The lines show the difference in the labor rights sub-index between countries that were under an IMF program and countries with no IMF program in the 1980-2014 period.

Sources: CBR data (Adams et al. 2016) IMF conditionality database (Kentikelenis et al. 2016).

Figure 2: The differential impact of IMF labor conditions exposure on labor rights.



Notes: The lines show the difference in labor rights between IMF program countries with at least one labor condition and IMF countries without such condition over the 1980-2014 period.

Sources: CBR data (Adams et al. 2016) and IMF conditionality database (Kentikelenis et al. 2016)

Table 1: The impact of labor conditions on labor regulation.

Labor rights	Labor rights index	Individual labor rights	Collective labor rights
Labor conditions	-4.46*	-7.54**	-2.28*
	(2.29)	(3.75)	(1.36)
IMF program	1.56	-5.46***	3.69***
	(2.37)	(1.70)	(1.20)
Log(GDP per capita)	-0.86	-1.14	-2.46
	(2.95)	(3.57)	(2.16)
Log(Population)	-1.15	-3.09	0.45
	(5.50)	(7.10)	(5.56)
Log(Trade openness)	0.69	0.78	0.70
	(0.85)	(1.16)	(0.91)
Log(FDI inflows)	0.02	0.07	-0.00
	(0.14)	(0.17)	(0.13)
Polity IV index	0.07	0.06	0.16
	(0.09)	(0.11)	(0.10)
Left-wing government	1.42	1.22	1.55*
	(0.93)	(1.30)	(0.91)
ILO ratification	-1.90	-3.55*	-0.53
	(1.63)	(1.93)	(2.28)
Civil war	-3.13***	-3.32***	-2.92***
	(0.75)	(0.95)	(1.11)
Country-fixed effects	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes
Labor conditions (auxiliary equation)			
Compound instrument	0.05***	0.04***	0.05***
	(0.01)	(0.01)	(0.01)
Log(GDP per capita)	-0.08	-0.09	-0.10
	(0.07)	(0.08)	(0.07)
Log(Population)	-0.55**	-0.52**	-0.55**
	(0.26)	(0.26)	(0.26)
Log(Trade openness)	-0.05	-0.04	-0.04
	(0.06)	(0.06)	(0.06)
Log(FDI inflows)	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)
Polity IV index	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
Left-wing government	0.09	0.08	0.09
	(0.10)	(0.10)	(0.10)
ILO ratification	-0.14*	-0.16*	-0.12
	(0.08)	(0.09)	(0.08)
Civil war	-0.08	-0.08	-0.08
	(0.07)	(0.07)	(0.07)
Country-fixed effects	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes
IMF program (auxiliary equation)			
Past programs	0.10***	0.10***	0.10***
	(0.01)	(0.01)	(0.01)
Countries under programs	0.03***	0.03***	0.03***
	(0.01)	(0.01)	(0.01)
UNGA vote alignment	2.83***	3.09***	2.69***
	(0.72)	(0.70)	(0.69)
Left-wing government	-0.06	-0.09	-0.08

	(0.08)	(0.07)	(0.08)
Common law	-0.19*	-0.22**	-0.17*
	(0.10)	(0.10)	(0.09)
Region-fixed effects	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes
Within-R2 (Labor rights)	0.39	0.33	0.25
Within-R2 (Labor conditions)	0.07	0.07	0.07
F-statistic (Labor conditions)	14.73	13.04	15.37
Pseudo-R2 (IMF program)	0.21	0.21	0.21
Observations	1857	1857	1857

Column headers show the labor rights index and its individual sub-indices. All covariates lagged by one period (covariates in the selection equation are twice-lagged). Each model contains three equations as indicated by row headers. Cross-equation correlation is explicitly taken into account. Clustered standard errors in parentheses.

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

Table 2: Split-sample analysis of IMF effectiveness by urbanization.

	Low urbanization			High urbanization		
Labor rights	Labor rights index	Individual labor rights	Collective labor rights	Labor rights index	Individual labor rights	Collective labor rights
Labor conditions	1.36	-0.04	3.96	-5.17***	-6.90**	-1.65
	(4.90)	(6.75)	(4.87)	(1.96)	(2.83)	(1.45)
IMF program	4.67	0.17	3.54	0.82	-1.13	14.42***
	(4.67)	(.)	(3.48)	(1.85)	(2.34)	(5.53)
Log(GDP per capita)	8.32**	8.00**	7.63**	-5.78**	-6.33*	-2.95
	(3.27)	(3.71)	(3.60)	(2.60)	(3.34)	(2.67)
Log(Population)	14.28***	12.34**	19.14**	-9.89	-10.75	-4.54
	(4.85)	(6.01)	(7.97)	(8.74)	(11.53)	(7.28)
Log(Trade openness)	0.24	0.12	0.07	-1.45	-1.36	-1.36
	(1.23)	(1.86)	(1.12)	(1.57)	(2.16)	(1.56)
Log(FDI inflows)	-0.20	-0.23	-0.15	0.11	0.25	-0.13
	(0.14)	(0.17)	(0.15)	(0.24)	(0.29)	(0.21)
Polity IV index	-0.01	0.05	-0.07	0.05	-0.13	0.21
	(0.09)	(0.11)	(0.11)	(0.16)	(0.21)	(0.18)
Left-wing government	-0.35	-0.84	1.04	2.12	1.94	1.94*
	(0.85)	(1.31)	(0.94)	(1.63)	(2.30)	(1.17)
ILO ratification	1.45	1.26	0.88	-4.85*	-6.64*	-3.68
	(1.87)	(2.43)	(1.64)	(2.93)	(3.62)	(3.03)
Civil war	-0.77	-1.12	-0.33	-4.34***	-4.79***	-3.07**
	(0.72)	(0.87)	(0.78)	(0.91)	(1.44)	(1.26)
Country-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Labor conditions (auxiliary equation)	Yes	Yes	Yes	Yes	Yes	Yes
IMF program (auxiliary equation)	Yes	Yes	Yes	Yes	Yes	Yes
Within-R2 (Labor rights)	0.47	0.40	0.35	0.39	0.30	0.28
Within-R2 (Labor conditions)	0.07	0.07	0.07	0.07	0.07	0.07
F-statistic (Labor conditions)	5.04	5.02	5.02	8.25	8.16	8.14
Pseudo-R2 (IMF program)	0.19	0.19	0.19	0.19	0.19	0.19
Observations	635	635	635	1222	1222	1222

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

Table 3: Split-sample analysis of IMF effectiveness by strikes intensity.

	Low strike intensity			High strike intensity		
Labor rights	Labor rights index	Individual labor rights	Collective labor rights	Labor rights index	Individual labor rights	Collective labor rights
Labor conditions	0.45	-1.02	2.66	-4.19***	-5.90***	-1.41
	(3.06)	(4.43)	(3.38)	(1.32)	(1.88)	(1.08)
IMF program	1.03	-1.40	4.35	-0.52	-4.06	9.14**
	(3.67)	(4.16)	(3.90)	(1.81)	(3.54)	(3.81)
Log(GDP per capita)	2.63	3.48	1.40	-8.03**	-8.53*	-5.88
	(2.86)	(4.08)	(1.97)	(3.48)	(4.81)	(4.40)
Log(Population)	12.06***	9.23	16.22***	-19.84**	-21.26	-14.98**
	(4.46)	(6.34)	(4.34)	(9.83)	(13.91)	(7.09)
Log(Trade openness)	1.78**	1.76	1.78**	0.06	-0.08	0.19
	(0.87)	(1.37)	(0.86)	(1.50)	(2.37)	(1.26)
Log(FDI inflows)	-0.08	-0.12	-0.02	0.07	0.22	-0.13
	(0.18)	(0.23)	(0.20)	(0.21)	(0.26)	(0.20)
Polity IV index	0.01	0.10	-0.13	0.07	-0.06	0.22
	(0.08)	(0.13)	(0.09)	(0.12)	(0.13)	(0.16)
Left-wing government	2.90**	3.51**	1.77	0.31	-0.13	1.09
	(1.18)	(1.55)	(1.08)	(1.34)	(1.96)	(0.94)
ILO ratification	-2.41*	-2.80	-2.01*	2.18	-2.87	9.25***
	(1.41)	(1.94)	(1.17)	(2.20)	(3.15)	(2.04)
Civil war	-3.42***	-5.39***	-0.52	-2.26***	-2.16***	-2.55***
	(1.03)	(2.04)	(1.08)	(0.68)	(0.73)	(0.98)
Country-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Labor conditions (auxiliary equation)	Yes	Yes	Yes	Yes	Yes	Yes
IMF program (auxiliary equation)	Yes	Yes	Yes	Yes	Yes	Yes
Within-R2 (Labor rights)	0.50	0.41	0.42	0.46	0.37	0.35
Within-R2 (Labor conditions)	0.07	0.07	0.07	0.07	0.07	0.07
F-statistic (Labor conditions)	3.38	3.39	3.61	13.48	13.48	13.43
Pseudo-R2 (IMF program)	0.19	0.19	0.19	0.19	0.19	0.19
Observations	860	860	860	997	997	997

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