

Labor Market “Rigidity” and the Success of Economic Reforms

Across more than One Hundred Countries

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Abstract

This paper shows that labor market policies and institutions have an impact on the effectiveness of economic reform programs. The analysis compares annual growth rates across 119 countries, using data from 449 adjustment credits and loans given by the World Bank between 1980 and 1996. The results indicate that countries with relatively “rigid” labor markets experienced deeper recessions before adjustment and slower recoveries afterwards. The paper also disentangles the mechanisms through which labor market rigidity operates. It finds that minimum wages and mandatory benefits are not detrimental to growth. But the relative size of organized labor, in government and overall, appears to be crucial. Labor market rigidity thus seems to be relevant for political reasons, more than for economic reasons. The paper shows that the results are robust to changes in measurement, controls and sample, and argues that they do not suffer from self-selection bias. These findings suggest that insufficient attention has been paid to vocal groups who stand to lose from economic reforms.

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1. Introduction

What determines the success or failure of economic reforms? Confronted with adverse external conditions and self-inflicted imbalances, many developing countries have embarked in ambitious reform programs. Depending on the countries, the programs have aimed at removing trade barriers, revamping the tax system, restructuring government spending, spurring financial liberalization, privatizing state-owned enterprises, or some combination of these. Over the last two decades, reform programs have more often than not been supported by adjustment credits and loans from the World Bank. In fact, support by the World Bank (and the International Monetary Fund) is interpreted by the international community as a signal that the country is committed to reform. But not all these programs have worked. Earlier assessments compared the change in the economic performance of countries with and without programs supported by the World Bank or the International Monetary Fund, and concluded that these programs did increase economic growth (Goldstein and Montiel, 1986, Khan, 1990, and Corbo and Rojas, 1992). However, a study using a similar methodology found no impact on the economic growth of the poorest countries (Elbadawi, 1992). And based on a case-by-case evaluation of 182 adjustment programs, the World Bank's own Operations Evaluation Department concluded that 36 percent of them had not met their objectives (Dollar and Svensson, 1998). Given these mixed results, the conditions under which the programs were adopted are receiving increasing attention.

This paper evaluates whether the "rigidity" of the labor market matters for the success of economic reforms. If labor costs cannot vary freely in response to changes in labor demand, economic reforms could lead to a decline in output, at least for some time. Consider trade liberalization for instance. With a flexible labor market, real wages in the import-competing

sectors of the economy should decline, pushing labor costs down across the economy and thus making the export sectors more competitive. But if wages cannot be cut, the import-competing sectors could be forced to shed more labor than is warranted whereas the export sectors would remain uncompetitive. Overall, the reallocation process would take much longer than with a flexible labor market, and it could be associated with high unemployment or under-employment. Note that this argument is economic in nature. Although it has been made in more elaborate ways before (see Edwards, 1988, and Rama, 1997, among others), the basic idea stems from the second-best principle: economic reforms require labor reallocation, so that they could be counter-productive in countries where this reallocation cannot take place.

Labor market conditions may also affect the adoption and success of economic reforms through other, unrelated mechanisms. Most economic reforms create winners and losers. A vast political economy literature has emphasized resistance by potential losers as one of the main obstacles to adjustment (Alesina and Drazen, 1991, and Fernández and Rodrik, 1991, are two of the best-known examples). Without stretching the argument too far, resistance by losers could also lead to a half-hearted adoption of reforms, thus diluting their economic impact. Workers in protected industries, in the public sector, or in banks are amongst the most obvious losers, at least in the short run. In countries where these workers are a large and well-organized group, resistance to reform could be fierce. In fact, the mere threat of prolonged strikes or massive street demonstrations could make a government delay the adoption of economic reforms, or water down their substance. Clearly, this argument is political in nature.

The economic and the political argument have different implications for the design of reforms. Based on the economic argument, labor market deregulation should be one of the components of the reform package. Policy measures such as the suppression or freeze of

minimum wages, and the abolition of mandatory benefits, would have to be considered. This conclusion has been reached by many in the international community. A report prepared by the World Bank for Latin America and the Caribbean concluded that “labor market reform is the area of structural reform where the least progress has been made in the region” (Burki and Perry, 1997, p. 57). This report proposed to “remove the distortions, many of them induced by *government regulations*, that make labor costly and risky in relation to its relative abundance in the economy” (p. 38, emphasis added). In a similar vein, the Inter-American Development Bank (1997) concluded: “*labor code* reforms have been few and not very deep”, adding that “current *labor legislation* may have hindered the reabsorption of workers who were displaced during the reform process” (p. 79, emphases added).

The political argument leads, arguably, to an almost opposite conclusion. In the political economy literature, the more equal the distribution of adjustment costs the shorter and weaker the resistance to economic reforms. From this perspective, the appropriate complement to adjustment programs would not be labor market deregulation, but rather the introduction of mechanisms that compensate the workers affected by the reforms, at least partially. Examples include job separation packages, early retirement programs and unemployment benefits. These mechanisms have actually been used in many adjustment programs entailing substantial public sector downsizing, often with the financial support of the World Bank. Some of the separation packages offered to redundant workers in state-owned enterprises to be privatized would easily qualify as “golden handshakes” (Kikeri, 1997). Over a sample of 41 downsizing programs financially supported by the World Bank, Haltiwanger and Singh (1999) estimated the average spending per job separation at \$2,400. The figure was as high as \$13,000 in the civil service of

Senegal, \$16,000 in the mining sector of Bolivia, and \$17,000 in the public enterprises of India, although all three countries had a per capita income of less than \$1,000 per year.

The fact that the economic and the political argument have different implications is somewhat blurred by the emphasis, by the proponents of the former, on enhanced safety nets. The aim of these safety nets is to prevent households from falling into poverty as a result of the reforms. Examples include public works programs and means-tested income transfers. However, the workers who are affected by liberalization and downsizing are better seen as part of the urban middle class. Adjustment might dramatically reduce their living standards, but relatively few of them become poor, even several years after losing their jobs (see Rama, 1999). As a result, these workers may not be eligible for, or not interested in, most of the safety net initiatives. By combining labor market deregulation and enhanced safety nets, the economic argument would then lead to an income transfer from the urban middle class to the poor. By recommending compensation, on the other hand, the political argument would aim at mitigating (or even offsetting) the income loss of the urban middle class.

The results in this paper show that labor market “rigidity” is indeed a key determinant of the success or failure of economic reforms. They also show that the political mechanism is more plausible than the economic mechanism. The empirical analysis compares the annual growth rates of 119 countries, over period 1970-1986. These countries differ considerably in their labor market policies and institutions. Starting in 1980, many among them undertook substantial economic reforms with the support of the World Bank. The analysis thus combines information on economic growth, on labor market policies, and on adjustment lending. The labor market information is from a database of labor market policies and institutions across countries which is currently under construction at the World Bank (Rama and Artecona, 2000). This database

allows computing a variety of rigidity indicators whose definition will be discussed in detail below. The results show that countries with more rigid labor markets experienced declines in growth rates before they adopted adjustment programs, and weaker recoveries afterwards.

This pattern is summarized in figure 1, which compares the estimated growth rates of two adjusting countries, corresponding to the 25th and the 75th percentiles of an aggregate labor market rigidity indicator. These countries are identified hereafter as “flexible” and “rigid” respectively. The growth rates reported in the figure are annual averages, based on an econometric analysis that will be presented in the next section of the paper. The figure includes four periods or phases, corresponding to the 10th to 4th year before the launching of the first serious adjustment effort (D1, or “long before”), the three years up to the adoption of the program (D2, or “right before”), the three years immediately following the program (D3, or “right after”), and the 4th to 10th years after the beginning of economic reforms (D4, or “long after”). The figure shows that long before adjustment the growth rate is similar in both countries. But it subsequently declines more sharply, and recovers less rapidly, in the rigid country. Adjustment can be deemed successful in both countries, to the extent that the growth rate increases right after it. However, long after adjustment the flexible country grows faster than it did long before it, whereas the opposite is true for the rigid country.

The paper then replicates the econometric analysis using four pairs of more narrowly defined indicators of labor market rigidity. These pairs measure the level of minimum wages, the cost of mandated benefits, the strength of the labor movement, and the size of government employment respectively. Given that a vast majority of government employees are unionized in developing countries, the last two pairs of indicators arguably capture the ability of potential losers from reform to convey their grievances. The first two pairs, on the other hand, reflect the

extent to which the government directly interferes with the adjustment of labor costs. The results reported in figure 1 hold for the last two pairs of indicators, but not for the first two. Put differently, countries where organized labor is potentially influential experience recessions right before adjustment, and slower recoveries afterwards, but growth performance is not affected by the level of minimum wages and non-wage costs. These results provide support to the political argument, while questioning the validity of the economic argument.

2. The Empirical Strategy

Adjustment programs are comprehensive policy packages, usually affecting several areas of the economy at once. Changes in taxation, government spending, trade barriers, financial regulations and enterprise ownership are among their most frequent components, but they are not the only ones. Given the multiplicity of measures, it is not possible to spell out all the channels through which the programs could affect economic performance. Hence the need of a reduced-form approach to evaluation. Moreover, adjustment programs were launched at different points in time in different countries. This continuity of the reform process makes it difficult to identify a common program period, as earlier studies did (see, for instance, Corbo and Rojas, 1992, and Elbadawi, 1992). Hence the need for an empirical strategy explicitly accounting for economic reforms being adopted in different years depending on the countries.

The strategy chosen in this paper aims at evaluating the impact of labor market rigidity on growth rates over the decade preceding the adoption of a serious reform effort and the decade immediately after. These two decades are decomposed in four periods, as described in the Introduction. This strategy thus leads to the definition of four dummy variables, labeled D1

(long before), D2 (right before), D3 (right after) and D4 (long after). For all industrial countries and some (non-adjusting) developing countries, the dummies verify $D1 = D2 = D3 = D4 = 0$ in all years. But for most developing countries, some of the dummy variables take positive values in specific years. More specifically, in each of the 10th to 4th years before the first serious adjustment effort, $D1 = 1$, whereas the other dummies are set equal to zero. Subsequently, in the 3rd year before adjustment D2 becomes equal to one while D1 switches back to zero. The same logic applies to D3 in the 1st year following the launching of economic reforms and to D4 in the 4th year. Even in an adjusting country, all four dummies are equal to zero in all the years more than one decade before or after the beginning of the adjustment process.

The comparison between growth rates in these four periods is carried out for countries with different degrees of labor market rigidity. Let L be a rigidity indicator. Because labor market policies and institutions evolve gradually, and have seldom been the target of adjustment programs, the rigidity indicator L can be assumed to be relatively stable over time within each country. Under this hypothesis, the empirical approach adopted in this paper can be summarized by the following equation:

$$\begin{aligned}
 y_{it} = & \beta_0 + \beta_1 D1_{it} + \beta_2 D2_{it} + \beta_3 D3_{it} + \beta_4 D4_{it} + \\
 & + \lambda_1 D1_{it} L_i + \lambda_2 D2_{it} L_i + \lambda_3 D3_{it} L_i + \lambda_4 D4_{it} L_i + \lambda_5 L_i \\
 & + \mu_1 y_{it-1} + \mu_2 W_t + \mu_3 X_{it} + u_i + \varepsilon_{it}
 \end{aligned} \tag{1}$$

where the subscript ‘i’ is used for countries and the subscript ‘t’ for years. Equation (1) links the growth rate of output in a specific year (y_{it}) to its level in the previous year (y_{it-1}), the phase of

the country's adjustment process ($D1_{it}$ to $D4_{it}$), its labor rigidity indicator (L_i), the external conditions (W_t) and other variables (X_{it}). Depending on the specification, the latter include year dummies, interaction terms between the external conditions and the phase of the adjustment process, and other controls. The assumptions made regarding the country-specific term u_i and the stochastic disturbance ε_{it} determine the appropriate way to estimate the coefficients in this equation, as will be discussed below.

The coefficients in equation (1) can be used to assess the impact of labor market rigidity on economic growth under a variety of circumstances. Consider two countries that differ in their labor market rigidity indicator by ΔL , but are otherwise identical. In a “normal” year, distant from an adjustment effort by more than one decade, the difference in growth rates between these two countries, Δy , verifies:

$$\Delta y = \lambda_5 \Delta L \quad (2)$$

Over the adjustment phase DK (with $K = 1, 2, 3$ or 4) this difference becomes:

$$\Delta y = (\lambda_K + \lambda_5) \Delta L \quad \text{with } K = 1, \dots, 4 \quad (3)$$

If all the λ_K coefficients were equal to zero, economic performance around the reform period would not depend on the degree of labor market rigidity. But the latter could still affect long-run performance if coefficient λ_5 were different from zero. The empirical strategy of this paper thus focuses on the sign and significance of coefficients λ_1 to λ_5 .

Figure 1 can be interpreted as a graphical representation of this strategy. The two lines in the figure report the predicted levels of the growth rate y_i , based on equation (1). These levels are calculated under the assumption that the values of all exogenous variables, except the labor market indicator L , are the same in the two countries. In drawing the figure, the values of the exogenous variables are replaced by their sample means. The upper line corresponds to a country with a relatively small L , whereas the lower line is for a country with a relatively large L . The vertical distance between the two lines, in turn, represents the predicted value of Δy_i , based on equation (3). This distance varies across the four phases of the adjustment process. For given values of coefficients λ_1 to λ_5 , the difference increases with the assumed difference in the level of indicator L between the two countries.

Because it focuses on the determinants of growth rates before and after a policy change, equation (1) differs from “standard” growth regressions. The latter try to account for long-run performance, and therefore deal with average growth rates over several decades. Within this literature there is a trend towards using higher frequency data, such as five-year averages (see, for instance, Islam, 1995). This trend has been criticized, as it may confuse growth effects and business-cycle effects (Pritchett, 1998). However, relying on high-frequency data is not uncommon when assessing the effects of adjustment policies. For example, annual growth rates have been used to evaluate the effects of stabilization programs (Easterly, 1996; Calvo and Végh, 1999; Echenique and Forteza, 2000) and monetary policies (Karras, 1999), whereas average growth rates over two and three years were used to assess the consequences of programs supported by the International Monetary Fund (Khan, 1990) and the World Bank (Corbo and Rojas, 1992; Easterly *et. al.*, 1997).

3. Defining Rigidity and Reforms

The empirical strategy just outlined crucially depends on the availability of at least one indicator of labor market rigidity. Ten of them are used in what follows. Two are intended to capture the aggregate rigidity level, whereas the other eight focus on specific distortions. In dealing with aggregate rigidity, it is important to keep in mind the limited enforcement capabilities of many developing countries. Their labor codes may include an impressive array of clauses aimed at protecting workers, but their labor inspection agencies are often too weak or corrupt to force employers to comply. This distinction suggests that the regulations that are most distortive on paper may well be the least enforced in practice (see Squire and Suthiwart-Narueput, 1997).

The number of ILO conventions ratified by a country is a reasonable proxy for the “thickness” of its labor code, hence for the degree of labor rigidity as stated on paper. This number will be identified as L0 in what follows. The conventions issued by the International Labour Organisation reflect the ideal regulatory framework from an ‘institutionalist’ perspective (see Freeman, 1993). These conventions cover a variety of labor market issues, from child labor to placement agencies. Their ratification by a country gives them legal status, thus superseding domestic regulations on those issues. Because the institutionalist perspective sees employees as weaker than employers, ILO conventions usually restrict the ability of the latter to decide on the terms and conditions of work. Not surprisingly, these conventions are seen as a source of labor market distortion from a neoclassical perspective.

In practice, however, the degree of labor market rigidity depends on how labor market regulations are implemented and enforced. From this point of view, it is the outcome of the

regulations that matters, rather than their number. Different observers emphasize different outcomes though. High minimum wages are a favorite candidate, as they mimic the standard textbook distortion of market equilibrium. Mandated benefits, such as old-age pension, health insurance or maternity leave, feature high in the list too. If workers do not “pay” for these benefits through lower wages, their burden falls on employers. Mandated job security and high firing costs are yet another typical example of a labor market distortion. Finally, the labor market can also be distorted when trade unions are large and powerful, or when governments employ a substantial share of the labor force. Note that distortions of this latter sort do not necessarily stem from a “thick” labor code, which re-emphasizes the distinction between rigidity on paper and in practice.

The available data can be used to construct indicators of labor market rigidity dealing with minimum wages, mandated benefits, trade unions and government employment. Unfortunately, there are not enough data to construct an indicator of job separation costs covering a large number of countries. This particular dimension of labor market rigidity is thus ignored in the empirical analysis, although it is potentially important. For instance, using data from 22 countries in Latin America and the Caribbean, Heckman and Pagés (2000) have argued that job separation costs have a substantial impact on the level of employment. A similar claim had been made by Fallon and Lucas (1991), using data from India and Zimbabwe. In principle, job separation costs could affect the effectiveness of economic reforms in the sample of countries considered in this paper. If they did, the conclusion that labor rigidity matters for political reasons, more than for economic reasons, would need to be qualified.

The variables measuring minimum wages, mandated benefits, trade unions and government employment will be identified in what follows as MW, BF, TU and GT respectively.

Given that labor issues tend to be sensitive and controversial, two indicators are used for each of these four dimensions of labor market rigidity. The preferred indicator carries the number one (for example, MW1). The aim of the second indicator (MW2 in the example) is to verify the robustness of the empirical results. All eight indicators, as well as L0, are normalized, so that the country with the highest level gets a one, the country with the lowest level gets a zero, and the rest falls in between. Moving from zero to one can thus be interpreted as moving from maximum flexibility to maximum rigidity.

The four dimensions of labor market rigidity considered so far can be combined into a single indicator, identified as L1 in what follows. This indicator, which captures aggregate labor market rigidity in practice, is defined as:

$$L1_i = \frac{MW1_i + BF1_i + TU1_i + GT1_i}{4} \quad (4)$$

The aggregate labor rigidity indicator is also calculated for countries where information on one of the four indicators in the numerator is missing, in which case the denominator is adjusted accordingly. All the indicators in the numerator being normalized, L1 varies between zero and one, like each of its individual components. But unless all dimensions of labor market rigidity are perfectly correlated, the variance of L1 is smaller than the variance of its components.

The potential correlation between the four dimensions of labor market rigidity implies that the corresponding indicators cannot be used in the empirical analysis without taking additional precautions. For the sake of the argument, suppose that minimum wages do not affect economic performance whereas mandated benefits do. Suppose also that minimum wages tend

to be higher in countries with more generous mandated benefits. Under these assumptions, replacing L by MW1 in equation (1) could lead to statistically significant values for coefficients λ_1 to λ_5 , thus suggesting that minimum wages matter. A possible solution to this problem would be to include the other three indicators among the X_{it} variables. But this solution could lead to a different problem. Because of the potential correlation between the different dimensions of labor market rigidity, the precision of the estimates would fall. Now, replacing L by BF1 in equation (1) and using the other three indicators as controls could lead to statistically insignificant values for coefficients λ_1 to λ_5 , thus suggesting that mandated benefits do not matter.

To avoid the omitted-variable bias and mitigate the multicollinearity problem, a set of complementary indicators is defined. For instance, if L is replaced by MW1 in equation (1), the complementary indicator (called L1-MW1) is given by:

$$(L1 - MW)_i = \frac{BF1_i + TU1_i + GT1_i}{3} \quad (5)$$

This indicator is then included among the X_{it} variables in equation (1), jointly with its interaction with the four phases of the adjustment process (dummy variables D1 to D4). When information on one of the dimensions of labor rigidity is missing, the numerator of the complementary indicator is adjusted accordingly.

The definition of the labor market rigidity indicators used in this paper is partly inspired by criteria used in other cross-country studies aimed at evaluating the impact of specific policies or institutions on economic performance. The distinction between rigidity on paper (indicator L0) and in practice (L1) is reminiscent of the distinction between legal and effective central bank

independence, explored by Cukierman (1995). The composite rigidity indicator (L1), which encompasses a variety of labor market distortions, bears some resemblance with the openness indicator constructed by Sachs and Warner (1995), which combined four possible distortions to international trade flows. The attempt to disentangle the role of the individual labor market distortions (MW, BF, TU and GT) is similar to the attempt, by Rodrik and Rodríguez (1999), to identify the role of each of the four possible distortions to trade combined by Sachs and Warner. Finally, the introduction of a complementary indicator for each of the individual labor market distortions (L1-MW to L1-GT) is directly borrowed from a previous study on labor market rigidity and economic performance by Rama (1995).

The empirical strategy adopted by this paper also requires the identification of serious reform efforts. The four phases of the adjustment process (D1 to D4) are defined relative to the beginning of those efforts, making it necessary to ascertain if and when they begin. Because reforms deal with a wide range of economic policies, from trade barriers to government spending to financial regulation, it would be difficult to rely on policy outcomes. As several authors have pointed out, a variety of indicators can be considered for each of these outcomes. Their simultaneous inclusion in a cross-country regression would lead to multicollinearity and dramatically reduce the number of degrees of freedom (see Levine and Renelt, 1992, and Sala-i-Martin, 1999). An alternative would be to combine these indicators in some policy score, but the credibility of any specific combination would be questionable.

Measuring reform efforts through policy outcomes could also obscure one of the mechanisms through which labor market rigidity potentially operates. Resistance to economic reforms may lead to the half-hearted adoption of key policy measures. For instance, a country that has in principle agreed with the international community to liberalize its foreign trade may

end up erecting new barriers under pressure from vocal constituencies. These barriers, in turn, may adversely affect economic performance. If reform efforts were measured through policy outcomes, such as the average tariff or the ratio of foreign trade to output, the country would have to be considered as a non-reformer. Poor performance would then be seen as the consequence of lack of reform, and labor market rigidity would seem irrelevant. On the other hand, if reform efforts were measured through the agreement reached with the international community, the country would have to be considered as a failed reformer. In this case, poor performance could be linked to labor market rigidity.

This paper adopts a direct approach to the measurement of reform efforts, based on the accumulated borrowing from the World Bank for structural and sectoral adjustment programs. Adjustment lending by the World Bank carries stringent policy-related conditions that the borrowing countries have to meet for the disbursements to be authorized. Loans including more than one hundred conditions each are not uncommon. While waivers on specific conditions are often granted, massive adjustment lending is usually associated with the launching of sweeping economic reforms. The diversity of the conditions associated with adjustment lending makes it difficult to use them as the basis for an indicator of economic reform. Instead, this paper assumes that a country begins its reform process in earnest when accumulated adjustment borrowing exceeds a critical threshold.

4. The Data

The implementation of the empirical strategy outlined in the previous section requires information on macroeconomic aggregates, on labor market policies and institutions, and on

reform efforts. Information on macroeconomic aggregates is readily available. The World Development Indicators database of the World Bank (2000) reports annual data on output, measured in real terms, for a large number of countries. These data are used in this paper to account for the dependent variable y_{it} . Moreover, based on these data it is possible to calculate the annual growth rate of industrial countries, taken altogether. Because this growth rate is a good indicator of the external conditions facing individual countries, it can be used as the independent variable W_t . Data on y_{it} and W_t are for period 1970-1996, so as to cover one full decade before the beginning of structural adjustment programs.

Measuring the extent of labor market rigidity, or the seriousness of reform efforts, is not as simple. Indicators of labor market rigidity are constructed based on the cross-country database compiled by Rama and Artecona (2000). In addition to the number of ILO conventions ratified by the country, the following indicators are used: the ratio of minimum wages to average labor costs in large manufacturing firms (MW1), the ratio of minimum wages to income per capita (MW2), the percentage of salaries that employers and employees have to contribute to the social security administration (BF1), the legal number of days of maternity leave with full pay for a first child born without complications (BF2), the membership of the labor movement measured in percentage of the labor force (TU1), the ratification by the country of ILO convention 87 on the right to bargain collectively (TU2), employment in the general government, including local administrations, as a fraction of the labor force (GT1) and employment in the central government as a fraction of the labor force (GT2).

All these indicators are calculated as averages over period 1970-1999. Because comparable data on labor markets in developing countries are scattered, some of these averages actually result from a small number of observations, mainly in the 1980s and early 1990s. For

some developing countries, information on specific indicators is missing altogether. The paucity of the data implies that time-variant indicators of labor market rigidity cannot be used in the empirical analysis. Data on indicators MW1, BF1, TU1 and GT1 are combined to construct the aggregate labor rigidity indicator L1, as defined by equation (4). Because labor market indicators are not available for all countries, this is the information set that determines the size of the sample that can be used in the analysis. The Appendix reports the estimated value of L1 for each of the countries in the sample, and ranks them based on this indicator.

The coverage of the sample can be evaluated based on table 1. The regions in this table are defined according to the geographic boundaries used by the World Bank in its operational work. Almost sixty percent of the countries and territories in the world are included in the sample. Although there are regional disparities, only in East Asia and the Pacific islands does the fraction of countries included in the sample drop considerably. Moreover, the countries included in the sample tend to be large. Overall, the sample accounts for more than 90 percent of the world's population and output. The shares are similar across all regions, with the exception of Sub-Saharan Africa. But even there, the countries in the sample represent more than 70 percent of the region's population, and more than 80 percent of the region's output. Almost three-quarters of the countries in the sample embarked in substantial economic reforms with support from the World Bank. Except for industrial countries, which do not borrow from the World Bank, all regions had a considerable share of reformers.

The regional averages of the ten rigidity indicators considered in the empirical analysis are shown in table 2. By construction, the aggregate rigidity indicator varies between zero and one. The other labor market indicators are also normalized in the regression analysis below. In table 2 they are presented without any transformation, so as to simplify their interpretation.

Based on the aggregate labor rigidity indicator, the countries in East Asia and the Pacific Islands are the most flexible, whereas those in Eastern Europe and Central Asia are the most rigid. But the regional ranking varies across indicators. For instance, social security contributions are lowest in Sub-Saharan Africa, whereas minimum wages are highest in South Asia.

The extent of labor market rigidity varies considerably across countries. However, program and non-program countries are quite similar, as differences in means do not reach one standard deviation for any of the ten indicators considered. Program countries exhibit slightly higher averages than non-program countries for minimum wages, social security contributions, maternity leave and the ratification of ILO convention 87. Non-program countries exhibit slightly higher averages for union membership and government employment. The labor rigidity indicator is larger in program than non-program countries, but the difference is only a quarter of a standard deviation. These figures suggest that the adoption of economic reforms is not directly related to the nature of the labor market policies and institutions in place. The timing of the reforms could be affected, however, as will be discussed below.

The number of ILO conventions ratified and the aggregate rigidity indicator can be used to identify the most flexible, the median and the most rigid country in each region. This is done in table 3. The results suggest that in spite of the relative arbitrariness underlying the definition of labor market rigidity, the resulting country classification is consistent with conventional wisdom. Thus, regardless of the indicator used, the USA appear as the most flexible of industrial countries, and Uruguay as the most rigid of Latin American countries. At the worldwide level, the most flexible countries are in East Asia (Korea or Hong Kong, depending on the indicator), whereas the most rigid are in Western Europe (Italy or Sweden).

The difference between rigidity on paper and rigidity in practice is also highlighted by table 3. For example, based on the number of ILO conventions ratified, Uganda and Chile are the median countries in their regions. However, when actual rigidity is considered instead, they both turn out to be most flexible. The case of India is even more extreme, as its position in the South Asian region switches from most rigid on paper to most flexible in practice. Conversely, Korea is the most flexible country in East Asia based on the number of ILO conventions it has ratified, but it is the median country based on its aggregate labor rigidity indicator.

For reform efforts, this paper relies on a database of disbursements for World Bank adjustment credits and loans over period 1980-1996 (World Bank, 1997). The reform process is assumed to begin when the country-specific ratio of accumulated adjustment borrowing to predicted (or trend) output exceeds the 25th percentile of the distribution of this ratio across all borrowing countries and all years from 1980 to 1996. Predicted output is used instead of the actual output to avoid a situation where countries facing a recession would suddenly qualify as reformers, even if they did not borrow any additional resources to support adjustment program. The Appendix identifies with the label “No program” the countries in the sample that never exceeded the critical threshold. For the countries that did, the Appendix indicates the year in which the threshold was reached for the first time. This year is used to assign values to the dummy variables D1 to D4.

The timing of major reforms depicted by the indicator constructed in this paper is quite consistent with information from other sources. For instance, in their study on global integration, Sachs and Warner (1995) report the year of opening to trade for a large number of countries. This information can be used to compute the number of years each country has been open over period 1980-1996. For developing and transition economies, the correlation between

the number of years open according to Sachs and Warner and the number of years since the beginning of reforms according to this paper is 0.32. In a similar spirit, the Inter-American Development Bank (1997, page 45) reports the initial year of financial liberalization in two dozen countries in the region. The correlation between the number of years liberalized and the number of years since the beginning of reforms is 0.29.

5. Main Results

Equation (1) was estimated replacing L by each of the ten labor market rigidity indicators, and using three different econometric techniques: fixed effects, random effects and the generalized method of the moments (GMM). The use of these three techniques is justified because none of them is clearly preferable in the context of this paper. The random effects technique is more efficient than the fixed effects technique, but the estimated coefficients are biased when the country-specific effects are correlated with the explanatory variables. The fixed effects technique proceeds by differentiation, so that it effectively removes country-specific effects and the bias they may cause. But in the process it also removes the (time-invariant) labor rigidity indicator, implying that coefficient λ_5 cannot be estimated. Moreover, in dynamic models this differentiation introduces a different type of inconsistency, known as Nickell's bias. This other inconsistency could be substantial in data sets covering a limited number of years. Whether it is relevant in a data set like the one used in this paper, which includes annual data over more than a quarter of a century, is unclear. The GMM estimates presented in this paper use suitably lagged values of the endogenous variable, in orthogonal deviations and in levels, as instruments (Arellano and Bover, 1995, Arellano and Bond, 1998). The validity of those

instruments can be assessed using a Sargan test. If the instruments are valid, the GMM technique yields consistent estimates of all the parameters in the model, including λ_5 . However, the asymptotic properties of the GMM technique may not be verified over a sample containing scarcely more than one hundred countries (Blundell and Bond, 1998). Also, the total number of observations that can be used with the GMM technique drops substantially. This is because the data set is an unbalanced panel, and only consecutive observations for each country could be included (Arellano and Bond, 1998). To sum up, the three econometric techniques have strengths and weaknesses. Consequently, rather than choosing one over the others, the paper presents results obtained with all three. The credibility of these results should be higher if they are similar regardless of the technique used.

The main results are reported in tables 4 to 9. In order to interpret them, it is convenient to focus on the central panel of the tables. The independent variables included in this panel are the four phases of the adjustment process, D1 to D4, their interaction with the selected labor rigidity indicator, and the labor rigidity indicator itself. In terms of equation (1), the coefficients multiplying these variables are β_1 to β_4 , λ_1 to λ_4 , and λ_5 , respectively. These coefficients are not dramatically different when the model is estimated in levels (with the random effects or the GMM technique) or in differences (with the fixed effects technique). This relative similarity suggests that the unobservable country-specific disturbances u_i do not play an important role. More formally, it is not possible to reject the hypothesis that country-specific effects are uncorrelated with the explanatory variables, except in the regressions using the second rigidity indicator in tables 8 and 9. This hypothesis was assessed using the Wald test proposed by Arellano (1993). The results reported for the regressions with the second rigidity indicator in

tables 8 and 9 use only orthogonal deviations as instruments, implying that the coefficient λ_5 could not be estimated

One of the main implications of the results in tables 4 to 9 is that economic reforms are effective when labor markets are flexible. GDP growth rates increase after adjustment, as shown by the positive and generally significant values of coefficients β_3 and β_4 in almost all the tables. These coefficients measure the increase in the GDP growth rate that a fully flexible country would experience in the aftermath of economic reforms. Only in Table 4, where labor market rigidity is measured through the number of ILO conventions ratified, does the size and significance of coefficients β_3 and β_4 decline. But this decline might be due to the fact that actual labor market rigidity is not appropriately accounted for in these regressions. When the number of ILO conventions ratified is replaced by indicators of actual labor market rigidity, the values of coefficients β_3 and β_4 become remarkably consistent. Their averages across Tables 5 to 9 are 0.042 and 0.024 respectively. Over a ten-year period, therefore, economic reforms can make an impressive contribution to economic growth.

Labor market rigidity, on the other hand, is associated with dramatic drops in GDP growth rates in the years preceding adjustment, and weak recoveries or continued recessions in subsequent years. This adverse effect is revealed by the negative and statistically significant values of coefficients λ_2 and λ_3 in table 5. Consider the two countries (rigid and flexible) depicted in figure 1. The values of L in these two countries, corresponding to the 75th and 25th percentiles of the worldwide distribution of the aggregate rigidity indicator, are 0.41 and 0.22 respectively. Based on the results in table 5, the GDP growth rate would be almost three percentage points lower in the rigid country than in the flexible country, both right before adjustment and right after it.

Not all labor market rigidity indicators have the same effect, however. Differences across tables are revealing in this respect. The comparison between the central panels of tables 4 and 5 shows that the success of economic reforms depends on how rigid a country is in practice, but is not affected by its rigidity on paper. When rigidity is measured by the number of ILO conventions ratified, the coefficients λ_1 to λ_4 estimated with all three techniques are statistically insignificant. This is additional evidence that the number of ILO conventions ratified is not a good indicator of labor market rigidity. The analysis is replicated in Tables 6 to 9 for all the different dimensions of actual labor market rigidity. The number of columns in these tables is higher, as they include two rigidity indicators each. Because of this multiplicity of columns, the interpretation of the results is somewhat more difficult. However, some distinct patterns emerge.

The strongest effects are associated with trade union membership and government employment. Coefficients λ_2 and λ_3 are negative in all the columns in tables 8 and 9, and significantly so in roughly half of them. These coefficients are especially large when the rigidity indicators considered are the share of the labor force that is unionized (TU1) or employed in the general government (GT1). They are always significant in the latter case. For the share of the labor force employed in the general government, a negative impact on GDP growth rates can be found even long after adjustment, as revealed by the large and statistically significant value of coefficient λ_4 . The pattern is not as clear when the rigidity indicators considered are the ratification of ILO convention 87 (TU2) and the share of the labor force employed by the central government (GT2), but this is not surprising. While trade union membership is an indicator of rigidity in practice, the ratification of an ILO convention is an indicator of rigidity on paper. As regards government employment, a relatively slim central administration is not incompatible with over-staffing at the provincial or the state level.

At the other end, minimum wages and mandated benefits do not appear to reduce GDP growth rates in any of the four phases of the reform process. In tables 6 and 7, coefficients λ_1 to λ_4 are either insignificant or (in a few cases) significantly positive, but they are never significantly negative. Taken literally, the results in table 6 would imply that relatively high minimum wages are associated with a better economic performance before adjustment. The size of coefficients λ_1 and λ_2 is quite consistent across all the estimations in this table, and coefficient λ_2 is statistically significant in half of the cases. But no significant effect is observed on growth rates after adjustment.

Finally, the results in tables 4 to 9 suggest that labor market rigidity may have an adverse effect on growth rates in non-adjustment years. This effect is captured by coefficient λ_5 , which is large (in absolute terms) and significant when L is measured by either the number of ILO conventions ratified or by the aggregate rigidity indicator, in tables 4 and 5. This coefficient is also negative in a few of the regressions using specific rigidity indicators, in tables 6 to 9. However, these negative effects on growth performance in non-adjustment years vanish when industrial countries, or countries with high administrative capacity, are excluded from the sample, as will be discussed in the next section.

6. Robustness

Some of the assumptions underlying the regression results reported in tables 4 to 9 could be challenged. First, some of the explanatory variables considered could be defined, or measured, differently. The timing of economic reforms is a case in point. Second, important

explanatory variables could be missing. If labor market rigidity, or economic reforms, were correlated with those missing variables, the reported regression results could be biased. Third, all the regressions assume that the effects of the explanatory variables are the same everywhere. In practice, labor market rigidity and economic reforms could have different impacts in some countries, or subsets of countries. And fourth, the regression analyses reported in tables 4 to 9 assume that all the explanatory variables are predetermined. While this is a plausible assumption in most cases, it is questionable regarding the timing of economic reforms.

The main results reported in the previous section are considered robust if the size and significance of coefficients β_1 to β_4 , λ_1 to λ_4 , and λ_5 do not change substantially when these four challenges are considered. Changes in the coefficients multiplying other control variables are deemed irrelevant from the perspective of this paper. For the first three challenges, it is actually possible to re-estimate the regressions in tables 4 to 9 under different assumptions. While those assumptions are explained in more detail below, the corresponding regression results are not discussed, or even reported, unless they contradict the findings in the previous section. For the fourth challenge, namely the endogeneity of economic reforms, it is not possible to produce alternative estimates. The discussion thus focuses on the possible effects of endogeneity on the estimated value of the coefficients of interest.

The first challenge concerns the measurement of the explanatory variables in the regressions. As the analysis described in the previous sections had already considered a large number of labor market indicators, the robustness analysis focused on the other explanatory variables. Thus, the GDP growth rate of industrial countries was replaced by a set of annual dummy variables. More importantly, the definition of the phases of adjustment, captured by the dummy variables D1 to D4, was modified. These phases had been determined in relation to the

beginning of a serious reform effort, as identified by a critical threshold of the ratio of accumulated adjustment borrowing to predicted output. To assess the robustness of the results, a different borrowing indicator and two other critical thresholds were considered. The new borrowing indicator was the accumulated credit from the World Bank for all purposes, and not just for adjustment. The two other thresholds were zero and the median of the ratio of accumulated borrowing to predicted output. Using zero as a threshold means that reforms are assumed to begin in earnest when the country receives its first adjustment loan or credit from the World Bank. The median threshold is more demanding and leads to the classification of countries that borrowed small amounts as non-reformers.

Another change in the definition of the dummy variables D1 to D4 was related to transition economies. Countries in Eastern Europe and the former Soviet Union were under central planning before the adoption of economic reforms, which usually took place as they became members of the World Bank. Although their labor market indicators before the beginning of the transition were formally comparable to those of market economies, their actual labor market policies and institutions were quite different. Pooling market and non-market economies in the years preceding the reforms could therefore be misleading. To assess the potential bias resulting from this pooling, the dummies D1 and D2 were not set equal to one if the country was not a member of the World Bank at that time.

The second challenge concerns omitted variables. Labor market rigidity could be correlated (positively or negatively) with the quality of government policies in other areas that matter for economic growth. If this was so, the effects attributed to labor rigidity in the previous section could actually be due to those other policies. The importance of good policies for growth performance has been highlighted by recent research on the effectiveness of foreign aid.

Burnside and Dollar (2000) constructed a policy index combining indicators of trade, monetary and fiscal policy. They found that foreign aid had had a positive impact on growth only in countries where this index revealed good policies. The index constructed by Burnside and Dollar was therefore included in all the regressions in tables 4 and 9, both as a self-standing explanatory variable and interacted with the D1 to D4 dummy variables.

The validity of the third challenge, namely that effects could vary systematically across subsets of countries, can be assessed by splitting the sample, or removing groups of observations. The exclusion of outliers from the sample caused no significant changes in the results. Also, all the regressions in tables 4 to 9 were re-run excluding industrial countries. This change can be justified on the grounds that industrial countries do not borrow for adjustment for the World Bank. As mentioned before, the exclusion of industrial countries made the long-run impact of labor rigidity indicators on growth vanish. On the other hand, the negative impact of unionization and government employment on GDP growth rates right before and right after adjustment became stronger.

Countries differ in their administrative capacity too. The same labor market regulations could have different effects in different countries depending on the extent to which they are enforced. Compliance is in principle much higher in industrial countries than in developing countries. But there is also a substantial heterogeneity among the latter. To address this concern, all countries in the sample were ranked according to three of the components of the political risk index compiled by the International Country Risk Guide (see Knack and Keefer, 1995). These components refer to lack of corruption, law and order, and the quality of bureaucracy. The rankings were based on the average value of each of these components from 1984 to 1997. Countries were split in two sub-samples of equal size based on the simple average

of the three rankings. Again, the main difference with the results in tables 4 to 9 was the weakening of the effect of labor market rigidity on growth in non-adjustment years.

The fourth and final challenge concerns self-selection. An important concern when estimating equation (1) is the possible endogeneity of economic reforms. Variables that affect economic growth rates might also condition the decision to implement a reform program. One potential source of endogeneity is the correlation between the (time-invariant) country-specific disturbances u_i and the dummy variables D1 to D4. This is not likely to be an issue in the context of this paper, however. As discussed before, the hypothesis that the country-specific disturbances are uncorrelated with the explanatory variables cannot be rejected, except in a few regressions. Even if they were correlated, the fixed effects estimates and the GMM estimates obtained using orthogonal deviations would be unbiased, as they both get rid of the country-specific disturbances.

A potentially more problematic source of endogeneity is the correlation between the time-variant disturbances ε_{it} and the dummy variables D1 to D4. When equation (1) is estimated imposing the condition that all the λ coefficients be equal to zero, the coefficient β_2 , which captures growth right before economic reforms, becomes significantly negative. This is an indication of self-selection bias, in the spirit of Heckman's (1989) "pre-program test". If the adoption of reform programs was independent from the macroeconomic context, there should be nothing special about growth performance in the previous years. In fact, coefficient β_2 is statistically insignificant when the pre-program test is run on flexible countries only, suggesting the absence of self-selection in their case.

The possible endogeneity of economic reforms in rigid countries is key to interpret one of the main results in tables 4 to 9. In the previous section, the negative and statistically significant

value of coefficient λ_2 was interpreted as evidence that rigid countries experience deeper recessions before adjustment, but no explanation was offered for this result. The correlation between the time-variant disturbances ε_{it} and the dummy variable D2 makes it more meaningful. The negative value of coefficient λ_2 is an indication that countries with rigid labor markets are reluctant to adopt economic reforms, unless they are forced to do so by the severity of the recessions they face.

The endogeneity of economic reforms in rigid countries should not affect other key results in tables 4 and 9. In particular, it should not affect the value or the interpretation of coefficient λ_3 . The latter would be biased only if the disturbances ε_{it} were correlated over time. Serial correlation of this sort implies that the adverse shocks preceding economic reform in rigid countries may persist. In this case, the negative value of coefficient λ_3 in rigid countries would just be reflecting the continuation of adverse shocks, rather than the ineffectiveness of economic reforms. However, the hypothesis that the residuals of equation (1) are not serially correlated could not be rejected using the tests for first- and second-order serial correlation in the GMM estimation. This is not surprising to the extent that the two main sources of persistence in growth rates are controlled for in the regressions. These two sources are the worldwide business cycle, captured through the growth rate of industrial countries, and the dynamics of the output growth rate, captured through the lagged value of the dependent variable. In any event, lack of serial correlation implies that the estimated coefficients λ_3 and λ_4 are not biased, and do reflect the impact of labor market rigidity on the effectiveness of reforms.

7. Conclusion

This paper has shown that labor market policies and institutions do matter for the success of economic reforms, but they probably do so for political reasons, more than for economic reasons. In particular, high minimum wages or mandated benefits do not appear to hinder economic growth, neither before nor after adjustment. This result is consistent with the evidence available for industrial countries, where labor market policies arguably have modest, hard-to-uncover effects on economic efficiency (see Freeman, 2000). Even the controversial result that relatively high minimum wages are associated with a somewhat better performance before adjustment, and not necessarily with a worse performance afterwards, is to some extent consistent with recent findings for the USA (see Card and Krueger, 1998).

The possible irrelevance of minimum wages and mandated benefits for the success of economic reforms questions the wisdom of efforts to deregulate the labor market. Admittedly, specific labor market regulations which are potentially very distortive, such as mandated job security, have not been considered in this paper. Efficiency gains from removing or bypassing those regulations could be sizeable. But abolishing minimum wages or curtailing social security benefits might not contribute much to economic performance, if at all. Moreover, labor market deregulation might be effective at reducing rigidity on paper, but not necessarily in practice. For instance, based on the analysis above, repealing ILO conventions would be a futile endeavor. Given that the usual ingredients of adjustment programs appear to be highly effective at raising output growth rates, it seems preferable to concentrate reform efforts on issues such as taxation, government spending, trade barriers, financial regulations and enterprise ownership, rather than on re-drafting the labor code.

On the other hand, unionization and government employment are associated with deeper recessions before adjustment, and weaker recoveries (or continued recessions) afterwards. One interpretation of this finding is that organized interest groups that stand to lose from the reforms may succeed in delaying their adoption and diluting their content. This is the natural interpretation from the perspective of the new political economy, which views distributional conflict as one of the main forces shaping economic policy. The contribution of this paper is to provide some empirical evidence in support of this perspective. In the process, the paper identifies organized labor as a key opponent of economic reforms. Many policy makers around the world would consider this finding quite obvious. But its policy implications deserve some attention.

Recent analyses of the effectiveness of development assistance have focused on the need to identify the conditions for its success. The probability of failure has been shown to be higher in countries with poorly defined property rights and high levels of corruption (Burnside and Dollar, 2000). It also has been shown to be higher in countries where governments have not been democratically elected or have been in power for a long time, as well as in countries that are ethnically fragmented (Dollar and Svensson, 1999). The recommendation emerging from these analyses is that development agencies should do a better job at selecting promising candidates for adjustment support. Unfortunately, some of the countries that most desperately need this kind of support may not qualify.

The findings in this paper imply that the focus should not just be on picking winners, but also on defusing the opposition of (vocal) losers. Based on the econometric results, reforms have been successful in countries where trade union membership and government employment are small. Arguably, this conclusion should be valid in spite of country differences in property

rights, corruption, democracy or ethnicity, as the econometric analysis takes unobservable country heterogeneity into account through the use of panel data. The failure of reforms in countries where trade union membership and government employment are large suggests that insufficient attention has been paid to the impact of economic reforms on urban, middle-class groups. This choice is justifiable on economic grounds, as these groups are not poor. But it might have been self-defeating on political grounds.

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Table 1

The Sample and the World

	All	Sub-Saharan Africa	East Asia/ Pacific Islands	Eastern Europe/ Central Asia	Industrial	Latin America/ Caribbean	Middle East/ North Africa	South Asia
The world								
Number of countries	200	47	31	27	28	39	20	8
Program countries	103	36	10	25	0	21	6	5
The sample								
Number of countries	119	23	12	17	23	21	18	5
Annual growth rate (%)	3.5	3.4	6.5	0.2	3.0	3.3	4.4	4.4
Program countries	76	20	8	17	0	20	6	5
Annual growth rate (%)	3.3	3.0	5.9	0.2	...	3.4	4.9	4.4
Sample/ World								
% of countries	59.5	48.9	38.7	63.0	82.1	53.8	90.0	62.5
% of 1995 GDP	99.1	82.4	98.0	93.3	99.9	96.2	98.3	99.9
% of 1995 population	93.4	71.1	95.4	81.5	99.9	94.7	98.9	99.9

Source: Authors' calculations using data from IMF and World Bank. Growth rates are averages over period 1970-1986.

Table 2

Labor Market Policies and Institutions across Regions

Labor market rigidity indicators	All	Sub-Saharan Africa	East Asia/Pacific Islands	Eastern Europe/Central Asia	Industrial	Latin America/Caribbean	Middle East/North Africa	South Asia
Minimum wage/Industrial wage (%)	29.7	18.0	26.8	32.1	32.6	30.2	28.4	44.1
Minimum wage/GDP per capita (%)	73.4	164.1	50.7	33.1	46.7	59.8	88.9	171.0
Social security contributions (%)	19.8	12.7	14.3	34.2	22.2	18.9	18.9	20.0
Maternity leave with full pay (days)	85.7	73.9	74.4	175.5	103.4	71.2	60.1	69.0
Union membership/Labor force (%)	23.8	9.9	14.7	67.3	37.3	18.3	16.6	9.1
Ratification of ILO convention 87 (0 or 1)	0.469	0.444	0.131	0.500	0.687	0.612	0.306	0.304
General government/Labor force (%)	10.8	5.2	6.5	14.6	15.6	10.2	12.5	5.3
Central government/Labor force (%)	4.9	3.2	3.3	3.6	5.7	6.0	7.9	3.0
ILO conventions ratified (number)	33.8	25.5	10.7	41.5	55.9	38.5	24.0	20.7
Aggregate rigidity indicator (0 to 1)	0.316	0.226	0.177	0.477	0.393	0.317	0.318	0.273

Source: Authors' calculations using data from Rama and Artecona (2000). Labor market indicators are averages over period 1970-1999.

Table 3

Labor Market Rigidity within Regions

Classification based on	All	Sub-Saharan Africa	East Asia/ Pacific Islands	Eastern Europe/ Central Asia	Industrial	Latin America/ Caribbean	Middle East/ North Africa	South Asia
ILO conventions ratified								
Minimum	Korea	Botswana	Korea	Croatia	USA	El Salvador	Oman	Nepal
Median	Pakistan	Uganda	Thailand	Romania	Portugal	Chile	Cyprus	Sri Lanka
Maximum	Italy	Guinea	Singapore	Bulgaria	Italy	Uruguay	Iraq	India
Aggregate rigidity indicator								
Minimum	Hong Kong	Uganda	Hong Kong	Turkey	USA	Chile	Jordan	India
Median	Colombia	Ghana	Korea	Poland	Finland	Guatemala	Kuwait	Sri Lanka
Maximum	Sweden	Burkina Faso	Philippines	Belarus	Sweden	Uruguay	Algeria	Bangladesh

Source: Authors' calculations using data from Rama and Artecona (2000). See the appendix for details.

Table 4

ILO Conventions Ratified, Adjustment and Growth

Independent variables	Dependent variable: annual growth rate of GDP (1970-86)		
	Fixed effects	Random effects	GMM
Years long before the program (D1)	0.000 (0.000)	-0.008 (-0.810)	-0.010 (-1.22)
Years right before the program (D2)	-0.011 (-0.686)	-0.020 (-1.319)	-0.02 (-1.159)
Years right after the program (D3)	0.018 (1.318)	0.014 (1.061)	0.011 (0.750)
Years long after the program (D4)	0.020* (1.708)	0.015 (1.307)	0.011 (1.001)
D1 x ILO conventions ratified	0.007 (0.374)	0.007 (0.405)	0.015 (0.991)
D2 x ILO conventions ratified	-0.025 (-0.960)	-0.024 (-1.047)	-0.018 (-0.643)
D3 x ILO conventions ratified	-0.013 (-0.519)	-0.012 (-0.539)	-0.007 (-0.308)
D4 x ILO conventions ratified	-0.007 (-0.329)	-0.004 (-0.225)	0.002 (0.142)
ILO conventions ratified		-0.023*** (-3.353)	-0.027*** (-3.927)
Industrial GDP growth	0.738*** (9.778)	0.672*** (9.179)	0.661*** (5.064)
GDP growth rate in previous year	0.220*** (13.273)	0.262*** (16.261)	0.218*** (4.264)
Industrial GDP growth x D1 to D4	Yes	Yes	Yes
Adjusted R ²	0.196	0.161	
Sargan test (p value)			1.000
Wald, uncorrelated effects (p value)			0.837
1 st order serial correlation (χ^2)			-4.723***
2 nd order serial correlation (χ^2)			-2.396***
Number of observations	3408	3408	2516
Number of countries	112	112	106

Note: Figures in parentheses are “t” statistics. Significant coefficients at the 10, 5 and 1 percent significance levels are indicated by one, two and three asterisks respectively. One-step version of the GMM estimates are reported, due to small sample considerations (Blundell and Bond, 1998). The GMM estimates use the third to sixth lags of the endogenous variable as instruments.

Table 5

Aggregate Rigidity Indicator, Adjustment and Growth

Independent variables	Dependent variable: annual growth rate of GDP (1970-96)		
	Fixed effects	Random effects	GMM
Years long before the program (D1)	-0.017 (-1.541)	-0.019* (-1.872)	-0.022 (-1.778)
Years right before the program (D2)	0.006 (0.417)	0.007 (0.503)	0.003 (0.161)
Years right after the program (D3)	0.047*** (3.142)	0.045*** (3.098)	0.045*** (2.500)
Years long after the program (D4)	0.025** (2.164)	0.020* (1.849)	0.018 (1.331)
D1 x Aggregate rigidity indicator	0.042 (1.436)	0.038 (1.473)	0.048** (1.734)
D2 x Aggregate rigidity indicator	-0.094** (-2.492)	-0.103*** (-2.947)	-0.090 (-1.713)
D3 x Aggregate rigidity indicator	-0.097*** (-2.607)	-0.095*** (-2.770)	-0.092* (-1.840)
D4 x Aggregate rigidity indicator	-0.042 (-1.474)	-0.032 (-1.253)	-0.029 (-0.808)
Aggregate rigidity indicator		-0.055*** (-4.349)	-0.053*** (-3.370)
Industrial GDP growth	0.709*** (10.964)	0.675*** (10.674)	0.693*** (7.243)
GDP growth rate in previous year	0.150*** (8.412)	0.194*** (10.890)	0.195*** (4.664)
Industrial GDP growth x D1 to D4	Yes	Yes	Yes
Adjusted R ²	0.222	0.171	
Sargan test (p value)			1.000
Wald, uncorrelated effects (p value)			0.382
1 st order serial correlation (χ^2)			-5.600***
2 nd order serial correlation (χ^2)			-1.544
Number of observations	2914	2914	2144
Number of countries	92	92	88

Note: Figures in parentheses are “t” statistics. Significant coefficients at the 10, 5 and 1 percent significance levels are indicated by one, two and three asterisks respectively. One-step version of the GMM estimates are reported, due to small sample considerations (Blundell and Bond, 1998). The GMM estimates use the second to ninth lags of the endogenous variable as instruments.

Table 6

Minimum Wages, Adjustment and Growth

Independent variables	Dependent variable: annual growth rate of GDP (1970-96)					
	Minimum wage indicator = MW1			Minimum wage indicator = MW2		
	Fixed effects	Random effects	GMM	Fixed effects	Random effects	GMM
Years long before the program (D1)	-0.007 (-0.575)	-0.009 (-0.815)	-0.012 (-0.908)	-0.003 (-0.232)	-0.008 (-0.644)	-0.011 (-0.820)
Years right before the program (D2)	-0.019 (-1.150)	-0.014 (-0.902)	-0.021 (-0.820)	-0.005 (-0.272)	-0.004 (-0.240)	-0.010 (0.430)
Years right after the program (D3)	0.055*** (3.424)	0.051*** (3.319)	0.050*** (3.069)	0.041** (2.261)	0.036** (2.056)	0.033** (2.228)
Years long after the program (D4)	0.033** (2.599)	0.028** (2.339)	0.024 (1.613)	0.042*** (3.037)	0.035*** (2.654)	0.030** (2.237)
D1 x Minimum wage indicator	0.018 (0.971)	0.016 (0.925)	0.039* (1.795)	0.016 (0.980)	0.020 (1.199)	0.012 (0.672)
D2 x Minimum wage indicator	0.056** (2.259)	0.048** (2.084)	0.055 (1.612)	0.035 (1.491)	0.039* (1.691)	0.032 (1.262)
D3 x Minimum wage indicator	0.026 (1.072)	0.021 (0.932)	0.023 (1.063)	0.010 (0.410)	0.012 (0.508)	0.007 (0.466)
D4 x Minimum wage indicator	-0.016 (-0.863)	-0.015 (-0.889)	-0.009 (-0.432)	-0.017 (-0.916)	-0.013 (-0.722)	-0.017 (-0.878)
Minimum wage indicator		0.002 (0.278)	-0.003 (-0.376)		-0.015 (-1.321)	-0.011 (-1.056)
Industrial GDP growth	0.742*** (11.349)	0.709*** (11.008)	0.744*** (7.535)	0.754*** (9.706)	0.719*** (9.382)	0.726*** (6.406)
GDP growth rate in previous year	0.169*** (8.156)	0.210*** (10.398)	0.207*** (3.605)	0.148*** (6.758)	0.187*** (8.746)	0.187*** (3.573)
Industrial GDP growth x D1 to D4	Yes	Yes	Yes	Yes	Yes	Yes
Complementary rigidity indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R ²	0.298	0.242		0.252	0.194	
Sargan test (p value)			1.000			1.000
Wald, uncorrelated effects (p value)			0.460			0.115
1 st order serial correlation (χ^2)			-5.015***			-4.888***
2 nd order serial correlation (χ^2)			-1.436			-1.293
Number of observations	2165	2165	1593	1994	1994	1530
Number of countries	66	66	64	60	60	59

Note: Figures in parentheses are “t” statistics. Significant coefficients at the 10, 5 and 1 percent significance levels are indicated by one, two and three asterisks respectively. One-step version of the GMM estimates are reported, due to small sample considerations (Blundell and Bond, 1998). The GMM estimates use the second to tenth lags of the endogenous variable as instruments.

Table 7

Mandated Benefits, Adjustment and Growth

Independent variables	Dependent variable: annual growth rate of GDP (1970-96)					
	Benefits indicator = BF1			Benefits indicator = BF2		
	Fixed effects	Random effects	GMM	Fixed effects	Random effects	GMM
Years long before the program (D1)	-0.013 (-1.003)	-0.019* (-1.776)	-0.021 (-1.629)	-0.010 (-0.746)	-0.016 (-1.585)	-0.018 (-1.424)
Years right before the program (D2)	0.004 (0.228)	0.001 (0.092)	-0.003 (-0.128)	0.019 (1.047)	0.016 (1.045)	0.011 (0.551)
Years right after the program (D3)	0.050*** (2.795)	0.046*** (3.059)	0.044** (2.420)	0.035** (2.004)	0.030** (2.054)	0.027** (1.976)
Years long after the program (D4)	0.031** (2.234)	0.025** (2.144)	0.023* (1.743)	0.034** (2.497)	0.026** (2.427)	0.024* (1.923)
D1 x Benefits indicator	0.019 (0.891)	0.022 (1.334)	0.015 (0.889)	0.010 (0.379)	0.015 (0.762)	0.023 (1.351)
D2 x Benefits indicator	0.003 (0.120)	0.009 (0.378)	0.003 (0.122)	-0.007 (-0.205)	-0.002 (-0.069)	0.006 (0.201)
D3 x Benefits indicator	-0.020 (-0.723)	-0.019 (-0.806)	-0.020 (-0.928)	0.004 (-0.124)	0.010 (0.380)	0.017 (0.963)
D4 x Benefits indicator	0.014 (0.680)	0.013 (0.766)	0.010 (0.488)	-0.028 (-1.101)	-0.024 (-1.177)	-0.016 (-0.810)
Benefits indicator		-0.009 (-1.013)	-0.008 (-0.645)		-0.022* (-1.952)	-0.026** (-2.015)
Industrial GDP growth	0.760*** (10.248)	0.670*** (10.140)	0.681*** (6.980)	0.798*** (11.165)	0.690*** (11.083)	0.693*** (7.487)
GDP growth rate in previous year	0.199*** (11.208)	0.186*** (10.332)	0.186*** (4.334)	0.209*** (12.079)	0.198*** (10.889)	0.199*** (4.837)
Industrial GDP growth x D1 to D4	Yes	Yes	Yes	Yes	Yes	Yes
Complementary rigidity indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R ²	0.201	0.168		0.198	0.173	
Sargan test (p value)			1.000			1.000
Wald, uncorrelated effects (p value)			0.896			0.115
1 st order serial correlation (χ^2)			-5.405***			-5.786***
2 nd order serial correlation (χ^2)			-0.877			-1.119
Number of observations	3072	3072	2097	3166	3166	2144
Number of countries	100	100	83	101	101	83

Note: Figures in parentheses are “t” statistics. Significant coefficients at the 10, 5 and 1 percent significance levels are indicated by one, two and three asterisks respectively. One-step version of the GMM estimates are reported, due to small sample considerations (Blundell and Bond, 1998). The GMM estimates use the second to tenth lags of the endogenous variable as instruments.

Table 8

Unionization, Adjustment and Growth

Independent variables	Dependent variable: annual growth rate of GDP (1970-96)					
	Unionization indicator = TU1			Unionization indicator = TU2		
	Fixed effects	Random effects	GMM	Fixed effects	Random effects	GMM
Years long before the program (D1)	-0.010 (-0.756)	-0.019* (-1.757)	-0.018 (-1.361)	-0.019 (-1.431)	-0.024** (-2.397)	-0.012 (-0.969)
Years right before the program (D2)	0.012 (0.640)	0.003 (0.205)	0.002 (0.087)	0.007 (0.375)	0.004 (0.301)	0.016 (0.777)
Years right after the program (D3)	0.035* (1.952)	0.027* (1.763)	0.025* (1.813)	0.048*** (2.622)	0.047*** (3.234)	0.059*** (3.077)
Years long after the program (D4)	0.029** (2.135)	0.021* (1.896)	0.019 (1.535)	0.024* (1.675)	0.019* (1.741)	0.032** (2.157)
D1 x Unionization indicator	-0.017 (-0.609)	-0.020 (-1.078)	0.014 (0.681)	-0.017* (-1.686)	-0.016** (-2.124)	-0.016* (-1.679)
D2 x Unionization indicator	-0.030 (-0.896)	-0.042* (-1.744)	-0.022 (-0.655)	-0.023* (-1.680)	-0.023** (-2.186)	-0.023 (-1.488)
D3 x Unionization indicator	-0.047 (-1.359)	-0.051* (-1.913)	-0.049** (-2.318)	-0.008 (-0.608)	-0.006 (-0.550)	-0.007 (-0.588)
D4 x Unionization indicator	-0.054* (-1.880)	-0.050** (-2.282)	-0.039* (-1.878)	-0.012 (-1.179)	-0.010 (-1.272)	-0.010 (-0.954)
Unionization indicator		-0.014 (-1.442)	-0.017 (-1.640)		-0.010** (-2.357)	
Industrial GDP growth	0.762*** (10.648)	0.684*** (10.690)	0.682*** (7.463)	0.734*** (10.123)	0.678*** (10.738)	0.695*** (7.847)
GDP growth rate in previous year	0.209*** (11.884)	0.178*** (10.112)	0.185*** (4.553)	0.231*** (13.759)	0.184*** (10.558)	0.187*** (5.002)
Industrial GDP growth x D1 to D4	Yes	Yes	Yes	Yes	Yes	Yes
Complementary rigidity indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R ²	0.190	0.163		0.205	0.175	
Sargan test (p value)			1.000			1.000
Wald, uncorrelated effects (p value)			0.249			0.029
1 st order serial correlation (χ^2)			-5.347***			-5.521***
2 nd order serial correlation (χ^2)			-1.102			-1.062
Number of observations	3243	3243	2083	3548	3548	2144
Number of countries	103	103	85	118	118	88

Note: Figures in parentheses are “t” statistics. Significant coefficients at the 10, 5 and 1 percent significance levels are indicated by one, two and three asterisks respectively. One-step version of the GMM estimates are reported, due to small sample considerations (Blundell and Bond, 1998). The results of the second regression (TU2) are estimations in orthogonal deviations, because the Wald test suggests rejection of the hypothesis that the effects are uncorrelated with the regressors in the equation in levels. The GMM estimates use the second to tenth lags of the endogenous variable as instruments.

Table 9

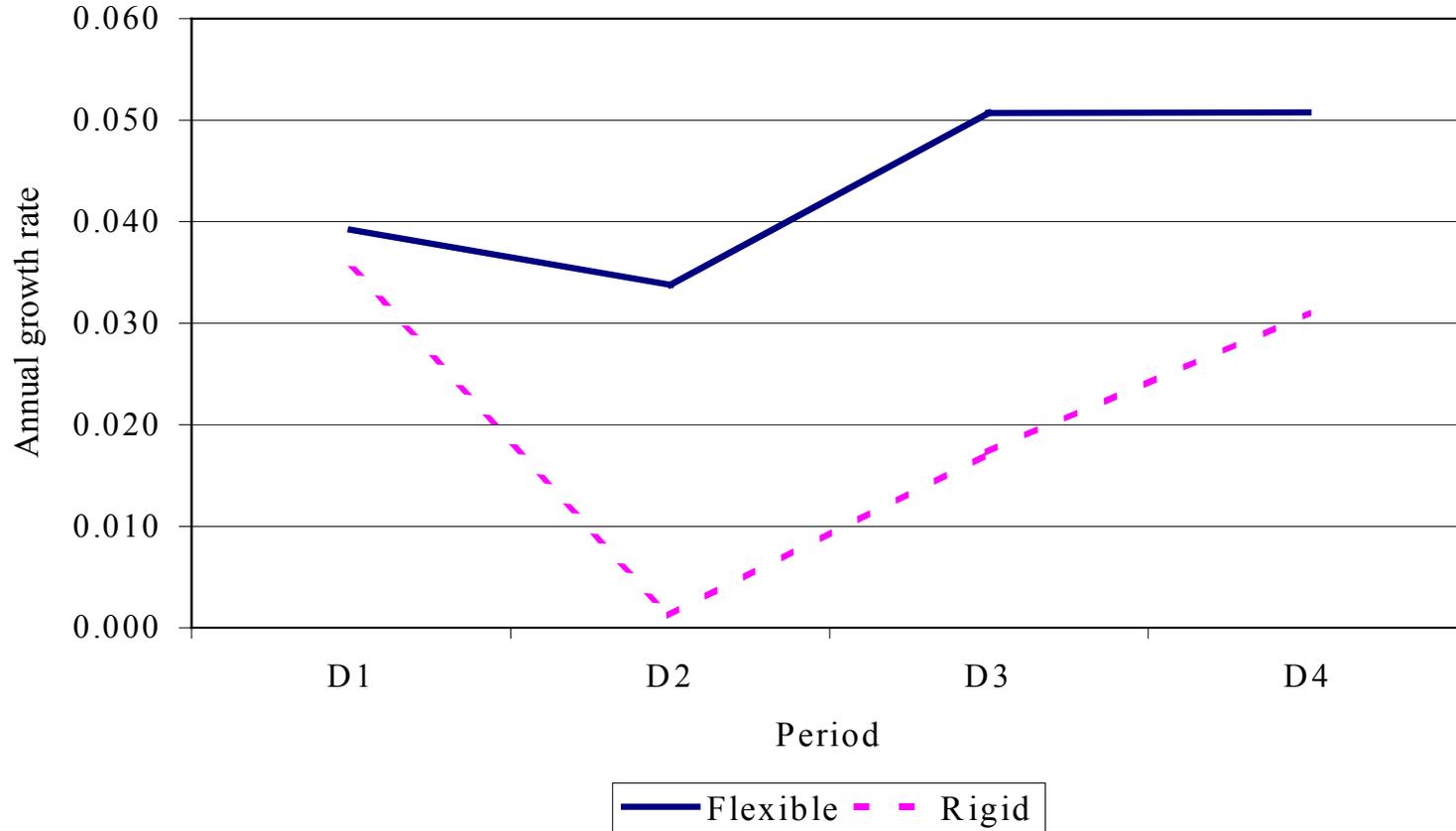
Government Employment, Adjustment and Growth

Independent variables	Dependent variable: annual growth rate of GDP (1970-96)					
	Government indicator = GT1			Government indicator = GT2		
	Fixed effects	Random effects	GMM	Fixed effects	Random effects	GMM
Years long before the program (D1)	-0.013 (-1.047)	-0.013 (-1.206)	-0.015 (-1.180)	-0.021* (-1.776)	-0.022** (-2.079)	-0.014 (-1.083)
Years right before the program (D2)	-0.005 (-0.249)	-0.002 (-0.141)	-0.007 (-0.286)	0.008 (0.458)	0.010 (0.653)	0.017 (0.807)
Years right after the program (D3)	0.039** (2.214)	0.039** (2.545)	0.038** (2.131)	0.036** (2.222)	0.038** (2.624)	0.045** (2.514)
Years long after the program (D4)	0.025* (1.871)	0.024** (2.055)	0.021 (1.502)	0.024* (1.864)	0.023** (2.048)	0.031 (2.085)
D1 x Government indicator	0.025 (1.087)	0.027 (1.433)	0.031 (1.153)	0.033** (2.109)	0.028** (2.160)	0.014 (0.944)
D2 x Government indicator	-0.066** (-2.282)	-0.069*** (-2.790)	-0.068** (-2.306)	-0.020 (-0.988)	-0.026 (-1.475)	-0.037* (-1.939)
D3 x Government indicator	-0.069** (-2.356)	-0.069*** (-2.800)	-0.073** (-2.059)	-0.016 (-0.780)	-0.025 (-1.437)	-0.032 (-1.021)
D4 x Government indicator	-0.003 (-0.123)	0.003 (0.133)	-0.000 (-0.009)	0.014 (0.908)	0.009 (0.675)	-0.004 (-0.231)
Government indicator		-0.003 (-0.307)	0.002 (0.137)		0.003 (0.449)	
Industrial GDP growth	0.646*** (8.658)	0.670*** (9.648)	0.681*** (6.755)	0.596*** (8.077)	0.615*** (9.166)	0.611*** (6.388)
GDP growth rate in previous year	0.212*** (11.049)	0.194*** (10.248)	0.193*** (4.524)	0.225*** (11.898)	0.210*** (11.231)	0.212*** (5.002)
Industrial GDP growth x D1 to D4	Yes	Yes	Yes	Yes	Yes	Yes
Complementary rigidity indicator	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R ²	0.232	0.177		0.233	0.180	
Sargan test (p value)			1.000			1.000
Wald, uncorrelated effects (p value)			0.741			0.061
1 st order serial correlation (χ^2)			-5.094***			-4.903***
2 nd order serial correlation (χ^2)			-0.811			-0.933
Number of observations	2538	2538	1889	2584	2584	1852
Number of countries	83	83	75	83	83	76

Note: Figures in parentheses are “t” statistics. Significant coefficients at the 10, 5 and 1 percent significance levels are indicated by one, two and three asterisks respectively. One-step version of the GMM estimates are reported, due to small sample considerations (Blundell and Bond, 1998). The results of the second regression (GT2) are estimations in orthogonal deviations, because the Wald test suggests rejection of the hypothesis that the effects are uncorrelated with the regressors in the equation in levels. The GMM estimates use the second to tenth lags of the endogenous variable as instruments.

Figure 1

Growth Rates Before and After Adjustment



Note: Constructed based on regression coefficients reported for the random effects method in Table 5, using the labor rigidity indicator for the 25th and 75th percentiles for the flexible and the rigid country respectively.

Appendix

Selected Country Data

Country	Year of first program (until 1996)	Aggregate rigidity indicator	Labor market rigidity ranking
ALGERIA	1990	0.45	84
ARGENTINA	1987	0.38	62
ARMENIA	1995	N/A	N/A
AUSTRALIA	No program	0.43	78
AUSTRIA	No program	0.45	83
BAHAMAS	No program	0.23	24
BAHRAIN	No program	0.27	36
BANGLADESH	1980	0.33	55
BELARUS	No program	0.64	92
BELGIUM	No program	0.54	88
BENIN	1989	N/A	N/A
BOLIVIA	1980	0.29	45
BOTSWANA	No program	0.21	18
BRAZIL	1986	0.28	42
BULGARIA	1992	0.51	87
BURKINA FASO	1991	0.40	68
BURUNDI	1986	N/A	N/A
CAMBODIA	1996	N/A	N/A
CAMEROON	1989	N/A	N/A
CANADA	No program	0.26	32
CENTRAL AFRICA	1987	N/A	N/A
CHAD	1989	N/A	N/A
CHILE	1986	0.15	9
CHINA	No program	0.10	3
COLOMBIA	1985	0.30	47
COMOROS	1991	N/A	N/A
CONGO	1986	N/A	N/A
COSTA RICA	1983	0.41	69
COTE D'IVOIRE	1982	0.27	34
CYPRUS	No program	0.36	60
CZECH REPUBLIC	1991	N/A	N/A
DENMARK	No program	0.51	86
DOMINICAN REPUBLIC	No program	0.42	75
ECUADOR	1986	0.32	53
EGYPT	1991	0.39	63

(Continued)

Appendix (Continued)

Country	Year of first program (until 1996)	Aggregate rigidity indicator	Labor market rigidity ranking
EL SALVADOR	1991	0.17	12
EQUATORIAL GUINEA	1986	N/A	N/A
ESTONIA	1993	N/A	N/A
ETHIOPIA	1993	0.23	26
FINLAND	No program	0.41	71
FRANCE	No program	0.61	91
GABON	1988	N/A.	N/A.
GAMBIA, THE	1987	N/A.	N/A.
GERMANY	No program	0.30	50
GHANA	1983	0.23	25
GREECE	No program	0.32	51
GUATEMALA	1993	0.30	48
GUINEA-BISSAU	1988	N/A	N/A
GUYANA	1981	N/A	N/A
HONDURAS	1989	0.32	54
HONG KONG	No program	0.07	1
HUNGARY	1986	0.57	90
INDIA	1993	0.22	22
INDONESIA	1988	0.13	7
IRELAND	No program	0.36	61
ISRAEL	No program	0.39	66
ITALY	No program	0.41	73
JAMAICA	1981	0.28	39
JAPAN	No program	0.26	30
JORDAN	1990	0.11	4
KENYA	1980	0.15	10
KOREA, REPUBLIC OF	1984	0.17	13
KUWAIT	No program	0.30	49
KYRGYZ REPUBLIC	1993	0.57	89
LAO PDR	1989	N/A	N/A
LATVIA	1993	N/A	N/A.
LITHUANIA	1993	N/A	N/A
LUXEMBOURG	No program	0.27	33
MADAGASCAR	1985	0.28	38
MALAWI	1981	N/A	N/A
MALAYSIA	No program	0.18	14
MALI	1988	0.28	41
MAURITANIA	1986	0.27	35

(Continued)

Appendix (Continued)

Country	Year of first program (until 1996)	Aggregate rigidity indicator	Labor market rigidity ranking
MAURITIUS	1981	0.34	58
MEXICO	No program	0.33	56
MOLDOVA	1995	N/A	N/A.
MONGOLIA	1994	N/A	N/A
MOROCCO	1984	0.24	28
MOZAMBIQUE	1988	N/A	N/A
NEPAL	1987	N/A	N/A
NETHERLANDS	No program	0.44	80
NEW ZEALAND	No program	0.42	74
NICARAGUA	1992	0.25	29
NIGER	1986	N/A	N/A
NIGERIA	1984	0.21	17
NORWAY	No program	0.41	72
PAKISTAN	1982	0.28	43
PANAMA	1984	0.45	81
PAPUA NEW GUINEA	1990	N/A	N/A
PARAGUAY	No program	0.41	70
PERU	1992	0.28	44
PHILIPPINES	1981	0.33	57
POLAND	1991	0.45	82
PORTUGAL	No program	0.29	46
ROMANIA	1992	N/A.	N/A.
RUSSIA	1997	0.43	76
RWANDA	1991	N/A	N/A.
SAO TOME AND PRINCIPE	1987	N/A	N/A
SENEGAL	1981	0.32	52
SIERRA LEONE	1984	N/A	N/A
SINGAPORE	No program	0.22	21
SLOVAK REPUBLIC	1994	N/A	N/A
SLOVENIA	1994	N/A	N/A
SOMALIA	1986	N/A	N/A
SOUTH AFRICA	No program	0.12	5
SPAIN	No program	0.35	59
SRI LANKA	1990	0.26	31
SUDAN	1980	N/A	N/A
SWEDEN	No program	0.69	93
SWITZERLAND	No program	0.27	37
SYRIAN ARAB REPUBLIC	No program	0.39	64

(Continued)

Appendix (Continued)

Country	Year of first program (until 1996)	Aggregate rigidity indicator	Labor market rigidity ranking
TAIWAN, CHINA	No program	0.21	20
TANZANIA	No program	0.13	8
THAILAND	1982	0.15	11
TOGO	1983	N/A	N/A.
TRINIDAD AND TOBAGO	1990	0.39	65
TUNISIA	1987	0.39	67
TURKEY	1981	0.21	16
UGANDA	1982	0.09	2
UKRAINE	1995	N/A	N/A
UNITED KINGDOM	No program	0.43	77
UNITED STATES	No program	0.22	23
URUGUAY	1985	0.47	85
VENEZUELA	1989	0.23	27
VIET NAM	1995	0.21	19
YEMEN, REPUBLIC OF	No program	0.28	40
YUGOSLAVIA, FORMER	No program	0.44	79
ZAMBIA	1984	0.19	15
ZIMBABWE	1983	0.12	6

Source: World Bank and authors' calculations using data from Rama and Artecona (2000). The table only reports countries that have had a program between 1980 and 1996, have enough labor market data to estimate the labor rigidity indicator or both. The labor rigidity indicator is an average over period 1970-1999. The labor rigidity ranking increases with the labor rigidity indicator.