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The Cost of Job Security Regulation: Evidence from Latin American Labor Markets

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1. Introduction

Labor market regulations are introduced with the stated objective of improving workers' welfare. Mandated benefits and social security programs improve workers' income security in case of sickness, work accidents and old age. Job security provisions are designed to reduce a worker's odds of losing her job and her means of living. But, as is often true in economics, benefits usually come at a cost: mandated benefits may reduce employment; job security provisions may protect some workers at the expense of others.

This paper gathers evidence from existing and new sources of information on the costs of job security policies. Latin America has experienced a wide range of labor market policies that provide natural experiments with which to evaluate the impact of these polices. Our evidence challenges the prevailing view (e.g., Abraham and Houseman 1994, Blank and Freeman, 1994, Freeman, 2000 and the papers he cites) that labor market regulations do not affect employment and have minimal costs. We establish that job security policies have a substantial impact on the level and the distribution of employment in Latin America. The evidence for their effect on unemployment is much weaker but there are good conceptual reasons why this should be so.

Our focus on the cost side does not imply that we believe the benefits of labor policies for protected workers are small or irrelevant. While the benefits to recipients are well-documented, the costs are often unintended and less well understood. Thus, while the evidence suggests that regulations promoting job security reduce covered workers' exit rates out of employment, it also indicates that demand curves are downward sloping, that regulation reduces aggregate employment and that the greatest adverse impact of regulation is on youth and groups marginal to the workforce. Insiders and entrenched workers gain from regulation, but outsiders suffer. As a consequence, job security regulations reduce employment *and* promote inequality across workers.

The outline of the paper is as follows. Section 2 describes and quantifies job security regulations in Latin America and the Caribbean. In Section 3, we summarize the existing evidence on the impact of job security provisions on employment, unemployment and turnover rates in Latin America. Section 4 presents new evidence. In Section 5 we summarize the paper and present our conclusions.

2. Job Security Regulation in Latin America and the Caribbean

In this paper, we define job security legislation (JS) to include all those provisions that increase the cost of dismissing a worker. In this section, we quantify the costs of abiding by the legislation, in terms of wages, in order to address three questions: (1) How high are the implied costs of JS provisions in Latin America and the Caribbean? (2) Within the region, which countries have costlier termination provisions and which are more deregulated? (3) How do Latin American and Caribbean countries compare with industrial countries in terms of JS legislation?

In Latin American countries, labor codes based on the civil law system regulate the permissible types and durations of labor contracts and the conditions for their termination. In contrast, most Caribbean countries are based on the common law system whereby the law enforces a contract with which both parties privately agree. As a consequence, in some countries there is not a specific body of law regulating employer-employee relationships, while in others some aspects are regulated and others left to the courts.

In Latin America, labor codes favor full-time indefinite employment over part-time, fixed-term or temporary contracts. These types of contracts differ not only in the length of the employment relationship but also in the conditions for termination. While indefinite contracts carry severance pay obligations, temporary contracts can be terminated at no cost provided that the duration of the contract has expired. In contrast, most Caribbean countries do not regulate the range of admissible contracts. Instead, such decisions are left to the parties involved in collective bargaining.

There are important differences as well in the conditions for termination of contracts. In Latin America, the termination of a contract is severely restricted. Thus, labor codes mandate a minimum advance notice period prior to termination, determine which causes are considered "just" or "unjust" causes for dismissal, and establish compensation to be awarded to workers for each possible cause of termination. In some countries, firms must also request permission to dismiss more than a certain fraction of their labor force. Finally, some countries allow the reinstatement of a worker to her post if the dismissal is found to be "unjustified" by the courts, although this provision has been eliminated in many countries. In contrast, in some of the Caribbean countries, advance notice and

severance pay are negotiated as part of collective agreements, so there are no specific laws regulating such provisions.

Termination laws (or collective agreements) require firms to incur four types of costs: advance notification, compensation for dismissal, seniority premiums for dismissed workers and foregone wages during any trial in which the worker contests dismissal. The period of advance notification should be included in the computation of costs because, in general, the various laws typically allow firms to choose between providing advance notice or paying a compensation equivalent to the wage corresponding to that period. Moreover, since productivity can decline substantially after notice, advance notification should be considered as a part of the dismissal cost even when firms choose to notify workers in advance. Advance notification periods vary from country to country, ranging from zero in Nicaragua, Guatemala, Peru and Uruguay to three months in Bolivia, Haiti and Venezuela for workers with more than ten years at a firm (See Table 1.A in the Appendix).

The second component of dismissal cost is compensation for unjustified dismissal. Since in most Latin American countries the economic difficulties of a firm are not considered a just cause for dismissal, any labor force reductions fall in this category. The formula for calculating this compensation is based on multiples of the most recent wage and the years of service. In contrast, under union agreements in the Caribbean, severance pay is only awarded to a worker in the case that a firm needs to reduce the work force for lack of work or technological change. In most other cases, employment at will is still the norm provided that the firm gives reasonable advance notice to a worker. Finally, in Belize, Bolivia, Chile and Nicaragua, the law mandates compensation to the worker in case of a voluntary quit.[1]

In some countries, employers are required to make an additional payment, known as a seniority premium, upon termination of the work relationship regardless of the cause or party initiating the termination. In Ecuador, Colombia, Panama, Peru, and Venezuela, this benefit is available to the worker both in the case of unjustified dismissal and in the case of a voluntary quit. If a worker quits, she obtains this payment, whereas if the worker is dismissed she obtains this payment *plus* the compensation for dismissal. In Brazil, this

[1] In Chile, compensation in case of a quit only occurs after the seventh year of service and if the worker chooses to set up an account.

additional payment is only available in the case of unjust dismissal, and if the worker quits, she receives no pay. In all the above-mentioned countries, firms deposit a certain fraction of workers' monthly wages in an individual trust fund in order to provide for this payment.[2] In Ecuador, Colombia, Brazil and Peru, the worker gains access to the principal plus a yield.[3] In Panama and Venezuela, the seniority premium is fixed in terms of multiples of monthly wages and the amount accrued in the fund (Panama) or the fund plus a certain yield (Venezuela) pays for the seniority premium. However, the firm is responsible for covering the difference between the required seniority premium and the amount accumulated in the seniority premium fund.

Finally, in some countries, firms are also required to pay a worker's foregone wages during the period of any legal process if a worker brings an action against the firm. This provision increases the overall cost of termination by increasing the overall compensation due and/or reducing workers' incentives to settle out of court.[4]

During the 1990s, seven countries (Colombia, Guyana, Guatemala, Nicaragua, Panama, Peru, and Venezuela) reformed their labor codes in order to reduce the cost of dismissing a worker. Not all labor reforms reduced JS, however. In Chile (1991) and in the Dominican Republic (1992), the amount that a firm had to pay upon dismissal of a worker increased considerably during the 1990s.

In an attempt to quantify all of these provisions we construct an index of JS encompassing LAC and industrial countries. There have been previous attempts to construct such types of measures. Bertola (1990), Grubbs and Wells (1993) and the OECD (1993, 1999) constructed ordinal measures of JS for industrial countries whereas Márquez (1998) constructed ordinal measures of job security for a sample of industrial and LAC countries. Also, Lazear (1990) quantified firing costs as the amount (in multiples of monthly wages) owed to a worker if she is dismissed after ten years of service. These measures, however, are unlikely to accurately reflect the magnitude of dismissal costs.

[2] In Brazil, the fund is called FGTS, in Peru, CTS, in Colombia, *Fondo de Cesantía* and in Panama, *Fondo de Antiguedad.*

[3] In Brazil a worker gets access to this fund only if she is dismissed.

[4] Another component of dismissal costs that can be quite important in some countries is given by the specific regulations that govern collective dismissals. Information on those regulations is not available for most countries of LAC and therefore we did not include them in our discussion or measurements.

On the one hand, ordinal measures can only state that one country is more regulated than another, but cannot measure how much more regulated it is. On the other hand, JS tends to increase according to tenure, which implies that measures conditional on a certain level of tenure only measure a given point in the severance-tenure schedule. To address these shortcomings, we construct an alternative cardinal measure of firing costs that summarizes the entire tenure-severance pay profile using a common set of dismissal probabilities across countries. This measure computes the expected future cost, at the time a worker is hired, of dismissing her in the future due to unfavorable economic conditions.[5] The index is constructed to include only firing costs that affect firm's decisions at the margin and therefore it does not include the full cost of regulation on labor demand. It includes the cost of providing statutory advance notice and severance pay conditional on each possible level of tenure that a worker can attain in the future.

The JS index does not include the seniority premium as part of cost because, in most countries, provisions for that payment are regularly deposited in a fund. Thus, because deposits are not directly made conditional on a dismissal they are not likely to alter firing decisions. Rather they should be treated as other labor costs incurred by the firm that do not affect firing decisions and are not included in our index. However, they clearly affect the cost of labor to the firm. The index also does not include the cost derived from foregone wages during trial. Although this component may be a substantial share of the total of cost of dismissal, we do not include it in our index because the information on this cost is not available. Thus we cannot estimate the full cost of resolution of legal costs arising from challenges to dismissals through the courts.

Our measure of JS thus reflects the marginal costs of dismissing full-time indefinite workers. However, this measure does not capture the effects of recent reforms that have made temporary and fixed-term contracts widely available in countries like Argentina and Peru. To the extent that fixed-term and indefinite contracts are not perfect substitutes–since temporary workers may be less productive (see the evidence in Aguirragabiria and Borganso, 2000), our index still captures the marginal cost of firing a tenured worker. However, firms may be at the margin of firing temporary workers and so our index overstates the true marginal cost. Additional information regarding the construction of this

[5] This measure is based on the index developed in Pagés and Montenegro (1999)

index can be found in the Appendix. This measure will be used in Section 3 to quantify the impact of JS on different employment and unemployment measures in a sample of OECD and LAC countries.

Graph 1 displays the costs of advance notice and compulsory severance pay in Latin American and the Caribbean for 1990 and 1999 as summarized by our index. This graph reveals that even after many countries have reduced dismissal costs during the nineties, the average cost of dismissing a worker is still higher in Latin America than in our sample of industrial countries. In comparison, the countries of the Caribbean basin exhibit much lower dismissal costs.

Table 1: Job Security Index across Latin America, the Caribbean and OECD countries. End of the 1990s

Country	*Index Job Security (Monthly wages)*	*% Annual wage*	*Ranking*
United States	0.000	0.000	1
New Zealand	0.221	1.844	2
Australia	0.443	3.696	3
Canada	0.553	4.610	4
Norway	0.912	7.599	5
Germany	1.140	9.498	6
France	1.143	9.526	7
Poland	1.219	10.160	8
Switzerland	1.247	10.395	9
United Kingdom	1.457	12.144	10
Belgium	1.729	14.407	11
Austria	1.784	14.864	12
Brazil	1.785	14.871	13
Greece	1.804	15.034	14
Guyana	1.890	15.750	15
Jamaica	1.920	16.003	16
Paraguay	2.168	18.068	17
Uruguay	2.232	18.599	18
Trinidad & Tobago	2.548	21.230	19
Nicaragua	2.563	21.358	20
Panama	2.718	22.652	21
Dominican Republic	2.814	23.454	22
Venezuela	2.955	24.625	23
Argentina	2.977	24.808	24
Costa Rica	3.121	26.005	25
Mexico	3.126	26.050	26
El Salvador	3.134	26.116	27
Spain	3.156	26.300	28
Chile	3.380	28.164	29
Colombia	3.493	29.108	30
Honduras	3.530	29.418	31
Peru	3.796	31.632	32
Turkey	3.973	33.110	33
Ecuador	4.035	33.621	34
Portugal	4.166	34.720	35
Bolivia	4.756	39.637	36

Source: Authors' computations (See Appendix)

Looking at the individual countries, it may be surprising that countries like Argentina or Mexico exhibit lower JS than Chile, a country traditionally considered as having a more flexible labor market. This divergence is caused by the fact that our index only measures one component of labor market rigidities. So while Argentina and Mexico have stronger unions than Chile, and therefore are likely to have higher wage rigidity, Chile has higher individual job security provisions. Our index also discounts penalties that arise far in the future, and so the fact that labor codes in Chile and other countries establish an upper limit on payments is discounted in our measure.

Graph 1 shows that four countries in Latin America (Nicaragua, Venezuela, Panama and Peru) undertook substantive reforms in their labor codes. Nicaragua and Venezuela reduced the expected dismissal cost by more than three monthly wages, while Panama and Peru reduced it between one and one and half monthly wages. However, Table 1 also makes clear that even after a decade of substantial deregulation, Latin American countries remain at the top of the JS list, with levels of regulation similar to or higher than those existing in the highly regulated South of Europe. We next consider quantitative estimates of the impact of job security regulations.

3. The Impact of Job Security Regulations

The goal of this section is to quantify the impact of job security regulations on employment and turnover rates. The importance of dismissal costs in Latin America is clear in Graph 1. It is thus important to assess the impact, if any, that such policies have on the labor market.

3.1 Theoretical Discussion

Analyzing the impact of job security provisions requires a complex framework that encompasses the dynamic decisions of firms. Bertola (1990) develops a dynamic partial-equilibrium model to assess how a firm's firing and hiring decisions are affected by dismissal costs. In the face of a given shock, the optimal employment policy of a firm involves one of three state-contingent responses: (i) dismissing workers, (ii) hiring workers and (iii) doing nothing, in which case employment in that firm does not change. How are these decisions altered by firing costs? In the face of a negative shock and declining

marginal value of labor, a firm may want to dismiss some workers, but it has to pay a mandatory dismissal cost. This cost has the effect of discouraging firms from adjusting their labor force, resulting in fewer dismissals than in the absence of such costs. Conversely, in the face of a positive shock firms may want to hire additional workers but will take into account that some workers may have to be fired in the future if demand turns down, and this is costly. This prospective cost acts as a hiring cost, effectively reducing creation of new jobs in good states. The net result is lower employment rates in expansions, higher employment rates in recessions and lower turnover rates as firms hire and fire fewer workers than they would in the absence of these costs.

Bertola's model predicts a decline in employment variability associated with firing costs, but the implication of his model for average employment is ambiguous. In particular, whether average employment rates increase or decline as a result of firing costs depends on whether the decline in hiring rates more than compensates the reduction in firings. Indeed, simulations reported in Bertola (1990) and Bentolila and Bertola (1990) suggest that average employment (in a given firm) is likely to increase when firing costs increase. These results, however, are quite sensitive to different assumptions about the persistence of shocks, the elasticity of the labor demand, the magnitude of the discount rate, and the functional form of the production function. Thus, less persistent shocks and lower discount rates are associated with larger negative effects of JS on employment because both factors reduce hiring relative to firing (Bentolila and Saint-Paul, 1994). Furthermore, a higher elasticity of the demand for goods implies a larger negative effect of job security on employment rates (Risager and Sorensen, 1997). In addition, when investment decisions are also considered, firing costs lower profits and discourage investment, increasing the likelihood that firing costs reduce the demand for labor (Bertola, 1991).

The results just reported analyze employment rates in one firm without considering the impact of firing costs on the extensive margin, that is, on how firing costs affect the creation and destruction of firms. Hopenhayn and Rogerson (1993) develop a general equilibrium model based on the U.S. economy that accounts for entry and exit of firms. In their model, the partial equilibrium framework of Bertola (1990) is embedded in a general equilibrium framework in which jobs and firms are created and destroyed in every period in response to firm-specific shocks. In the context of their model, they find that increasing

firing costs in the U.S. would lead to an increase in the average employment of existing firms as a consequence of the reduction in firings. However, they also find that such a policy would result in lower firm entry and lower job creation in newly created firms. For the parameter values they consider, these two last effects offset the increase in employment in existing firms resulting in a reduction of overall employment rates.

Job security may also affect employment through its effect on wages. The insider/outsider literature emphasizes that job security provisions increase the insider power of incumbent workers. This effect results in higher wages for insiders and lower overall employment rates (Lindbeck and Snower, 1987). Caballero and Hammour (1997) consider a model in which job security provisions increase the *appropriability* of capital by labor by increasing capital specifity. That is, a larger part of the capital invested becomes relationship-specific and becomes lost if capital separates from labor. While in the short run, higher firing costs allow labor to extract higher rents from capital, in the long run firms invest in less labor-intensive technologies, reducing employment demand.

Some recent literature has also emphasized the possible impact of job security regulations on the composition of employment. Kugler (2000) proposes a model in which job security regulations provide incentives for high turnover firms to operate in the informal sector. This decision entails producing at a small, less efficient scale in order to remain inconspicuous to tax and labor authorities. In this framework, high job security is likely to increase informality rates. Pagés and Montenegro (1999) develop a model in which JS related to tenure biases employment against young workers and in favor of older workers. As severance pay increases with tenure, and tenure tends to increase with age, older workers become more costly to dismiss than younger ones. If wages do not adjust appropriately, negative shocks result in a disproportionate share of layoffs among young workers. Therefore, job security based on tenure results in lower employment rates for the young, relative to older workers, because it reduces hiring and *increases* firings for young workers.

We conclude that higher JS provisions reduce turnover rates and bias the composition of employment against young workers and against employment in the formal sector. The implications for average employment in the economy at large are, however, somewhat less conclusive since they can depend on specific configurations of parameters

for the economy. To complicate matters further, by the Coase theorem the impact of job security could be completely "undone" with a properly designed labor contract provided that there are no restrictions on transactions between workers and firms (Lazear, 1990). Thus, in a world without transactions costs, wages adjust to offset the possible negative impact highlighted in the previous discussion. Given the ambiguity of theoretical models, the magnitude and direction of the impact of job security on employment has to be resolved empirically. In the following two subsections, we discuss existing evidence relating JS to labor market outcomes and present some new evidence of our own.

3.2. Empirical Evidence for Latin America and the Caribbean

Despite the existence of strict job security regulation in most of the countries of the region, research assessing its impact has been extremely scarce. Fortunately, a recent series of empirical studies assess the impact of job security regulation on employment and turnover rates in Latin America and the Caribbean, providing the first systematic evidence of its impact on the labor market.[6] Several studies assess the impact of job security on turnover rates in the labor market. Changes in turnover are measured using changes in the duration of jobs (tenure), the duration of unemployment and the exit rates out of employment and unemployment.[7] Higher employment exit rates indicate more layoffs (or more quits), while higher exit rates out of unemployment and into formal jobs indicate higher job creation in the formal sector. Other studies examine the impact of job security on employment rates. The definition of employment changes depending on the data considered. In general, most studies focus on employment in large firms, although some also examine more aggregated measures of employment. In addition, a small group of studies also examines the impact of job security on the composition of employment (See Table 2 for an overview of the empirical evidence for Latin America and the Caribbean).

[6] Most of these projects were developed under the IDB Research Network project "Labor Market Legislation and Employment in Latin America" coordinated by J. Heckman and C. Pagés.

[7] These studies estimate hazard rates. The hazard rate is defined as the probability that a given spell of employment or unemployment ends in a given period conditional on having lasted a given period of time (e.g., one month, one year).

A. Turnover Rates

The strongest evidence is on the impact of job security on turnover. As predicted by most theoretical models, the empirical evidence confirms that less stringent job security is associated with higher turnover in the labor market. Kugler (2000) analyzes the impact of the 1990 labor market reforms in Colombia. She finds that a reduction in job security is associated with a decline in average tenure and an increase in employment exit rates.[8] This decline is significantly larger in the formal sector that is covered by the regulations than in the uncovered or informal sector. In addition, the increase is larger in large firms and imprecisely determined in the smallest ones. Her results shows similar patterns within tradable and non-tradable sectors, providing a clear indication that the decline in tenure cannot be attributed to contemporary trade reforms. The increasing use of temporary contracts explains only part of the increase in formal sector turnover rates since job stability also declined for workers employed at permanent jobs.[9] Her results also indicate that the increase in turnover is larger for those workers who are more protected by high levels of job security (i.e., middle-aged and older men employed in large firms).

Kugler also finds a decline in the average duration of unemployment after the reforms. In addition, exit rates out of unemployment increase more for workers who exit to the formal sector than they do for those who exit to informal jobs. Her results show quite similar patterns across sectors and a higher exit rate towards larger firms. Finally, only two-thirds of the increase in the rate of entry into employment can be attributed to higher use of temporary contracts: the rest is explained by increased exit rates into permanent jobs in the formal sector. Her results for different workers suggest that the young and women benefit more from higher exit rates out of unemployment and into the formal sector.

The magnitudes of the estimated effects are not negligible. Kugler estimates that after the reform, the increase in probability of exiting employment was 6.4% larger for covered workers than for uncovered workers, while the exit rates out of unemployment and into formal jobs increased by 5.9% with respect to exit rates to the informal sector.

[8] In this study tenure is measured by the duration of incomplete spells.

[9] In her study, Kugler performs two types of analysis. First, she uses a difference-in-difference estimator to analyze whether changes in average duration of employment (unemployment) are statistically significantly different in the formal than in the informal sector. Second, she estimates an exponential duration model to control for changes in demographic covariates, pooling data from before and after the reform and using interaction terms to assess the differential impact in the formal and in the informal sector.

Saavaedra and Torero (2000) conduct a similar study, evaluating the impact of the 1991 reform in Peru. Like the reform in Colombia, the 1991 reform considerably reduced the cost of dismissing workers. Their analysis shows a consistent decline in average job tenure from 1991 onwards, suggesting higher employment exit rates. As in Colombia, the decline is significantly more pronounced in the formal than in the informal sector, but the magnitude of the fall is larger in Peru. Finally, tenure patterns are also quite similar across economic sectors, suggesting that these findings cannot be explained by the far-reaching trade reforms that took place in that country in the early 1990s.

Finally, Paes de Barros and Corseuil (2000) provide further evidence from Brazil. Their study estimates the impact of the 1988 Brazilian constitutional reform on employment exit rates. In that year, the cost of dismissing workers was raised and therefore a reduction in exit rates would be expected. Their results confirm that aggregate employment exit rates decline in the formal sector relative to the informal sector for long employment spells (two years or more).

The credibility of these studies hinges on the validity of the informal sector as a control group unaffected by the reforms. Kugler (2000) shows that while estimates based on formal-informal sector comparisons are likely to be biased, under plausible conditions such comparisons are still valid, at least as tests of the null hypothesis of no effect of the reform.[10] When taken together, these studies provide consistent evidence that dismissal costs and other employment protection mechanisms reduce worker reallocation in the labor market. Unfortunately, these studies do not identify whether increased worker reallocation is due to increased layoffs, higher quits or a mix of both.

Hopenhayn (2000) provides further evidence of the link between JS and worker turnover rates in Argentina. In 1991, the government of Argentina deregulated the use of temporary and fixed-term short-duration contracts. In 1995, additional contractual forms were allowed, including a three-month trial period. Such contracts reduced or eliminated the cost of terminating an employment relationship. Hopenhayn (2000) finds that after

[10] Kugler shows that lower severance pay may induce high-turnover informal firms to move to the formal sector. Under the assumption of no overlap in the distribution of turnover between covered and uncovered firms, or that entry to the covered sector comes from the high end–or at least from the end that is higher than the formal sector–this shift results in higher turnover in both the formal *and* the informal sector. Fortunately, higher turnover in the informal sector biases the difference-in-difference estimator downwards. Therefore, a positive estimate still provides substantial evidence of increased turnover in the formal sector.

1995, employment exit rates increase substantially for short employment duration while they remain constant for long durations. This increase in separations is due to a rise in both quits and layoffs, although the increase in layoffs is higher.

Summarizing, the evidence provided in this section indicates that JS regulations protect workers against the risk of losing a job. From this point of view, the recent reforms have reduced the income security of formerly protected workers. However, the evidence also suggests that stringent JS provisions reduce exit rates out of unemployment and into formal jobs, thus prolonging the duration of unemployment. Thus, recent labor market reforms have increased the probability of an unemployed worker finding a job in the formal sector.

B. Average Employment

The available evidence for LAC countries shows a consistent, although not always statistically significant, negative impact of JS provisions on average employment rates. Saavedra and Torero (2000) and Mondino and Montoya (2000) use firm-level panel data to estimate the impact of job security on employment in Peru and Argentina, respectively. Both studies estimate labor demand equations in which an explicit measure of job security appears on the right hand side of the equation, and both find evidence that higher job security levels are associated with lower employment rates.[11] In the case of Peru, Saavedra and Torero find that the size of the impact of regulations is correlated with the magnitude of the regulations themselves. Thus, the impact is very high at the beginning of their sample (1987-1990) coinciding with a period of very high dismissal costs (see Table 1.A). Afterwards, and coinciding with a period of deregulation, the magnitude of the coefficient declines, only to increase again from 1995 onwards, after a new increase in dismissal costs. Their estimates for the long-run elasticities of severance pay are very large (in absolute value): between 1987 and 1990 a 10% increase in dismissal costs, keeping wages constant, is estimated to reduce long-run employment rates by 11%. In subsequent periods, the size of the effect becomes smaller but is still quite large in magnitude (between 3% and 6%). In

[11] The data for the Peruvian study covers firms with more than ten employees in all sectors of the economy. The Argentinean study only covers manufacturing firms. Given the nature of these surveys, they are better proxies for formal employment than for employment as a whole. The data used in these two studies does not

Argentina, the estimated long-run elasticity of a 10% increase in dismissal costs is also between 3% and 6%.[12]

capture job creation by new firms, since both panels are based on a given census of firms, without replacement.

[12] While the estimated job-security elasticity in Argentina is much lower (in absolute value) than the wage elasticity reported in Table 2, this elasticity is larger in the Peruvian case. This is somewhat surprising since job security reduces job creation and *also* slows down employment destruction. Therefore, it might be expected that the JS elasticity would be smaller than the wage elasticity in absolute value. One explanation for the seemingly high elasticity found in the Peruvian study is that this measure is upwardly biased by a simultaneity problem arising from the job security measure. Thus, both the Peruvian and the Argentinean studies construct explicit measures of job security based on:

$$JS_{jt} = \delta_j T_{jt} P_{jt} SP_{jt}$$

Where δ_j is the layoff rate in sector *j* in sector t, T_{jt} is average tenure in sector *j*, time period *t*, P_{jt} is the share of firms in sector *j*, time period *t*, that are covered by regulations and SP_{jt} is the mandatory severance pay in sector *j*, given average tenure T_{jt}. This measure provides variability across sectors and periods, and therefore it affords a more precise estimation of the impact of job security than before-after types of comparisons. Yet, such a measure may also be correlated with the error term in a labor demand equation since the tenure structure of a firm might be correlated with its employment level. The fact that average layoff rates vary by sector may also lead to simultaneity if sectors with higher layoffs have lower employment. Thus, periods or sectors with low employment may be associated with less job creation, high average tenure and, consequently, high measures of job security. The Argentinean study shows that fixing tenure to the period average reduces the estimated elasticity of JS. Thus, a JS elasticity between 1/3 and 2/3 of wage elasticity seems a more realistic estimate of its impact.

Table 2: Summary of existing evidence on the impact of job security (JS) in Latin America

A. Studies that analyze exit rates into and out of employment			
Study	*Country*	*Data*	*Results*
Kugler (2000)	Colombia	Household data	Decline in JS leads to reduction in employment and unemployment duration. Also hazard rates out of employment and out of unemployment increase. Some effect due to temporary contracts but not all
Saavaedra and Torero (2000)	Peru	Household data	Lower JS is associated with lower average tenure. Higher decline in formal sector. Hazard rates increase just at the end of probation period.
P. de Barros and Corseuil (2000)	Brazil	Employment Surveys, Administrative data and Household surveys	Higher JS associated with a decline in employment exit rates in formal in relation to informal sector.
Hopenhayn (2000)	Argentina	Household data	Deregulation of temporary contracts leads to increase in hazard rates in short but not in long spells
B. Studies that analyze average employment and unemployment			
Study	*Country*	*Data*	*Results*
Downes et al. (2000)	Barbados	Aggregated employment. Annual. It covers large firms (>10 emp)	Negative effect of JS on labor demand (LD). Coeff. Significant at 10%
Saavedra and Torero (2000)	Peru	Firm and sector-level data. Bimonthy 1986-96. Quarterly 1997-98. Formal firms with more than 10 employees. Balanced panel (it does not account for firm creation or destruction)	Negative effect of JS on LD when using sector level-data for whole period. By subperiods, JS has a negative effect from 1987 to 1994, and no effect since then.
Mondino and Montoya (2000)	Argentina	Panel of manufacturing firms. It does not account for firm creation.	Negative effect of JS on LD. The coefficient in unbalanced panels is slighly more negative than in balanced ones.
Kugler (2000)	Colombia	Household data on employment.	Decline in JS in 1990 brings a decline in unemployment rates. Based on computing the net effect of changes in hazard rates, in and out of U induced by the reduction in JS.
P. de Barros and Corseuil (2000)	Brazil	Monthly establishment-level data. 1985-1998 Manufacturing. Firms employing 5 or more workers	Two step procedure. First, find parameters for labor demand (LD) function for every month. Then see whether those parameters change with labor reforms and other development. They find no effect of JS on LD parameters.
Pagés and Montenegro (2000)	Chile	Household data on employment. Annual 1960-1998	Negative but not statistically significant effect of JS on aggregated employment.
Marquéz (1998)	Cross-Country	Cross-section data for Latin America, Caribbean and OECD countries.	Rank indicator of Job Security. JS is not significantly associated with lower employment once GDP per capita is accounted for.

Table 2, continued			
C. Studies that analyze the composition of employment			
Study	*Country*	*Data*	*Results*
Marquéz (1998)	Cross-Country	Cross-section data for Latin America, Caribbean and OECD countries.	Self-employment rates are positively associated with JS even after accounting for differences in GDP per capita.
Pagés and Montenegro (2000)	Chile	Household Survey Data. 1960-1998	JS is associated with lower employment rates for young workers and higher employment rates for older ones. No significant effect on U for young, middle age or older workers.

In a very different type of study, Kugler (2000) computes the net impact of the Colombia 1991 labor reform on unemployment rates. Using unemployment and employment exit rate estimates for periods before and after the reform, she finds that the reforms cause a decline in unemployment between 1.3 and 1.7 percentage points. Thus, as in Mondino and Montoya (2000) and Saavedra and Torero (2000), Kugler's estimates indicate that the positive impact on the hiring margin outweighs the negative impact on the firing margin, resulting in a decline in unemployment rates.

Other studies find negative, but not statistically significant, effects of job security on average employment rates. Pagés and Montenegro (1999) find that JS has a negative but not statistically significant effect on overall wage-employment rates in Chile. Similarly, Marquéz (1998), using a cross-section sample of Latin American and OECD countries, finds a negative but not statistically significant coefficient of job security on aggregate employment rates. Table 3 summarizes the various estimates of job security on employment. (The Heckman and Pagés results are discussed below).

Thus, while the theoretical models exhibit some ambiguity regarding the impact of JS provisions on long-run employment rates, the empirical evidence for LAC is consistent across studies. To complement these analyses, we examine two other sources of evidence. First, we review the existing evidence on the impact of JS on employment in OECD countries. Second, in Section 4, we provide new evidence combining employment, unemployment and job security measures from a panel of LAC and OECD countries.

Table 3: Summary of Long- Run JS Elasticities

Study	*Mean*	*S.E.*	*Employment Rate*
Saavedra & Torero (2000)	-0.406	0.06	Employment in Large firms
Mondino & Montoya (2000)			
High estimate**	-0.684	0.0145	Employment in Large firms
Low estimate***	-0.305	0.0060	Employment in Large firms
Pagés & Montenegro (1999)	-0.1198	0.2440	Wage-Employment/Population
Heckman & Pagés (2000), FE*	-0.0516	0.0318	Total Employment/Population
Heckman & Pagés (2000), RE*	-0.0502	0.0168	Total Employment/population
Heckman & Pagés (2000) OLS*	-0.0502	0.0168	Total Employment/population

Notes: *Estimates for LAC only. **Based on Table 9, Mondino & Montoya (2000) ,
***Based on Table 10, option B. Mondino & Montoya (2000)

The evidence from OECD countries reinforces the results found for LA. Thus, with the exception of Anderson (1993), who finds a positive association between dismissal costs and long-run employment, the rest of the studies found a negative impact of JS on employment. Using panel data from OECD countries, Lazear (1990) shows that more stringent job security measures are associated with lower employment and labor force participation rates. Grubb and Wells (1993) find a negative correlation between JS and wage-employment rates. Addison and Grosso (1996) reexamine Lazear's estimates using new measures of job security across countries and find similarly negative effects on employment rates. Nickell (1997) finds a negative effect of JS provisions on total employment rates and no effect on prime-age male employment rates. Finally, a recent OECD (2000) study finds a negative but not statistically significant effect of JS on total employment rates. In contrast, the evidence regarding the effect of JS on unemployment in OECD countries is ambiguous but there are conceptual reasons for being so. While Blanchard (1998), Esping-Andersen (2000), Jackman *et al*. (1996) and Nickell(1997) among others find no effect of JS on unemployment, Lazear (1990), Elmeskov *et al*. (2000) and Scarpetta (1996) find positive effects. Yet, it should not be a surprise that a negative impact on employment is not necessarily reflected in a positive effect on unemployment. If workers' participation decisions are influenced by JS policies (as shown by Lazear, 1990), a reduction in employment will be associated to a decline in participation rates. This is particularly true for workers with lower attachment to the labor force or with less access to unemployment insurance benefits.

C. The Composition of Employment

Some recent evidence sheds new light on the possible impact of JS on the composition of employment in LAC. Márquez (1998) constructs a JS indicator for LAC and OECD countries and uses it to estimate the effects of JS on the formal/informal distribution of employment. He finds that more stringent JS provisions are associated with a larger percentage of self-employed workers. In a study of Chile, Pagés and Montenegro (2000) find that more stringent job security is associated with a substantial decline in the wage employment-to-population rates of young workers and an increase in the wage-employment rates of older workers. Their results also suggest that this composition effect is driven by the high costs of dismissing older workers relative to younger ones created by job security provisions related to tenure.

4. New Evidence

In this section, we exploit substantial cross-country and time series variability in job security provisions to estimate whether the negative effects of JS encountered in some of the individual-country studies in LAC generalize to a wider sample of countries and reforms.

4.1 The Data

We construct a data set that spans industrial and LAC countries. To do so we proceed in two stages. We first collect employment and unemployment data for industrial countries from the OECD statistics. Second, we use the OECD definitions of these variables, to construct the same indicators out of Latin American Household Surveys. Table 4 provides summary statistics for the overall sample, the OECD sample (excluding Mexico, which is included in the LAC sample) and the LAC sample. Table 5 describes the household surveys used to compute the LAC variables. Finally, to characterize job security, we use the index of job security described in Section 2.

The number of countries and the average number of observations per country in our sample varies between 36 and 43 countries and between 1 and 5 observations per country, respectively. Among the countries represented, around 28 belong to the sample of OECD

countries, while 15 are from the LAC region. Regarding the period spanned in our sample, for most LAC countries, there are one or two observations from the eighties and one or two from the nineties. The OECD sample only covers the 1990s. In relation to the variables used in this exercise, it should be noted that all employment rates are measured as a percentage of working age population and all unemployment rates as a percentage of active economic population (See the Appendix for a definition of the variables used in this study.)

Table 4 shows some remarkable differences between the OECD and the LAC samples. As noted in Section 2, average job security is higher in Latin America and the Caribbean than in OECD countries. In contrast, all employment rates (except for prime-age female employment) are higher and all unemployment rates are lower in the LAC region than in industrial countries. Especially notable are the higher share of self-employment and the much lower share of long-term unemployment (more than six months) in LAC. Finally, union density and female participation are both lower in the LAC region.

4.2 Methodology and Results

By constructing our own data set from individual household-level surveys, we are guaranteed that all the labor market variables are comparable and reliable. One drawback of our data is that we only have a few time series observations per country (usually three or four), and not necessarily from consecutive years. Given the nature of the data, we decided not to average observations from a given period–as is done in most of the OECD studies on job security–and instead controlled for the state of the business cycle in a given year using GDP growth.

We use a reduced form approach to investigate whether countries and periods with more strict job security regulations are associated with lower employment or higher unemployment rates. Thus we estimate an average net effect of JS as it operates through intermediate variables which we do not include in the regression. In this paper, we do not estimate the theoretically more appropriate state-contingent demand functions because we lack the information on the states of demand confronting individual firms. JS costs govern the marginal costs of labor when firms are firing, but they also affect overall labor demand through their effect on expected (across states) labor cost. It is the latter effect that we

attempt to identify. Since most of the variation is cross-sectional, we use different types of variables to control for country-specific factors that may be correlated with job security. First, we use demographic controls such as the share of the population between 15 and 24 and female participation rates. These variables account for the fact that high job security countries in the south of Europe and Latin America tend to have low female participation and a large share of youth population. Since both factors affect overall employment rates, not including them in the specification may lead to substantial biases in the estimates. We protect against common country-specific unobservables that remain constant over time and that may affect both left hand side and right hand side variables by including country-specific fixed effects in a set of regression specifications reported below. Second, we use GDP (measured in 1995 U.S. dollars) to control for differences in development levels across countries. We also include a dummy variable for LAC to control for regional differences not controlled by GDP levels.[13]

Most of the variability in our sample comes from differences across countries and regions, and from some time series variance within the LAC sample. There is very little time-series variability in the OECD sub-sample. Given this variation, fixed effects (FE) estimates are likely to be very imprecise because they only use the time-series variation within the LAC sample. Instead, random effects (RE) or pooled OLS estimates, that use both the cross-section and the time-series variation included in the sample, are likely to produce estimates with smaller standard errors. Yet, the latter estimates will be biased if variables included as controls are correlated with country specific error terms. To protect against the bias that results from using one estimator, we estimate our basic specification by pooled OLS, RE and FE, comparing whether these different methodologies yield similar point-estimates.

The results, presented in Tables 6.a to 6.c, are striking. First, the point-estimates for the JS coefficient in the total employment specifications are very similar across estimation methodologies. The three estimates suggest a large negative effect of JS on employment rates. This effect is strongly statistically significant in the OLS and the RE estimates while it is not statistically significant, at conventional levels, in the FE case. One

[13] These specifications should include a measure of labor costs that include wages and other non-wage labor costs. Unfortunately, a complete and comparable measure of labor costs across countries and time is not available.

obvious advantage of using a cardinal measure of JS is that we can quantify the impact of these provisions on employment. The magnitudes of JS elasticities are quite large: an increase in expected dismissal costs equivalent to one month of pay is associated with a 1.8 percentage point decline in employment rates. Given that in Latin America the average dismissal cost in 2000 was 3.04 months (see Graph 1), the estimated loss in employment– as a percent of total working population–due to JS provisions is about 5.5 percentage points.

In addition, OLS, FE and RE estimates suggest that JS does not affect the employment rates of all workers in the same fashion. Thus, while the impact on prime-age male employment rates is half the impact on total employment, the impact on young workers' employment rates is almost two times larger. The magnitudes are huge. The OLS and the RE estimates suggest that JS reduces LAC youth employment rates by almost 10 percentage points. This effect is even larger in the FE estimates. Moreover, these magnitudes are consistent with those ones obtained in Pagés and Montenegro (1999) for Chile.

Our estimates of the effect of JS on female employment rates, self-employment and unemployment rates are less consistent. The point estimates for female employment rates change from negative to positive across methodologies, but in no case are the estimates statistically significant. These results suggest that women are less negatively affected by JS than men but, as we will show, these results are not robust across regional sub-samples.

The estimates of the effect of JS on self-employment also change signs across OLS, FE and RE estimates. Thus, while the pooled estimates suggest a *positive* and statistically significant association between the strength of JS provisions and self-employment (as found by Márquez, 1998), the FE estimates show a negative and also statistically significant relationship between both variables. It is clear that more empirical work is required to reach a definitive conclusion on the relationship between JS and self-employment.

Finally, the empirical results on unemployment also greatly depend on the methodology used to estimate the parameters. While OLS and RE yield positive (and often statistically significant) coefficients on JS in all the unemployment specifications, FE yields negative and statistically insignificant results. We do not find a significant

relationship between the proportion of workers unemployed for more than six months and the strictness of JS provisions. Since there is no a priori relationship between disemployment and unemployment, these results are not surprising, especially given differences across regions in the levels of social insurance.

Divergence across estimation methods may result from regional differences in the relationship between JS and some of the variables. This is particularly relevant for our exercise since FE estimates discard practically all of the information for OECD countries. We therefore investigate whether our results are driven by any of the two sub-samples by estimating separate coefficients for LAC and OECD countries. The results from this exercise are presented in Table 7. While this approach results in small samples and lower statistical significance, the results are still quite remarkable. First, in all the employment specifications, with the exception of female employment rates, the coefficients on job security are negative across regions and estimation methods. In addition, most of the coefficients are highly statistically significant.

Second, with one exception, all coefficients of the effect of job security on unemployment rates are positive both in OECD and in LAC countries. However, the impact on unemployment rates seems much larger in the industrial country sub-sample, in particular for women and youth. It should not come as a surprise that the effect of JS on unemployment rates is smaller in developing countries. In the absence of unemployment insurance or other income support programs, workers either quickly find other (less attractive jobs) or drop out of the labor force.[14] The positive and statistically coefficient of GDP level in the unemployment regressions reported in Tables 6a-6c confirms this effect.

Third, the ranking of effects between total, male and young workers' employment rates is preserved. The point estimates tend to be larger (in absolute value) in the LAC sample. It is very likely that the higher level and variability of JS in this region contributes to these larger (in absolute value) point estimates. It is quite puzzling, however, that the estimates for female employment (and unemployment) rates are so different across regions. Thus, while, JS is negatively associated with female employment rates in the OECD sub-sample, this relationship is actually positive in the LAC sample. The added worker effect is

[14] In the case of Chile, Montenegro and Pagés (1999) found that the large effects of JS on youth employment rates were compensated with a large decline in participation rates with no significant effects on unemployment.

more evident in LAC, where adult female attachment to the labor force is still weak. Understanding gender differences in the impact of JS remains one important issue for further research.

Finally, the evidence of the impact of job security on the formal/informal composition of employment is not conclusive. A comparison of our estimates for LAC with the elasticities obtained from the individual-country studies (see Table 3), suggest that the decline in employment associated with JS is greater in the covered (formal) sectors–such as the manufacturing sector or sectors with large-firms–than in the aggregate.[15] This would imply that an increase in job security is associated with a decline in formal employment and an increase, although not enough to compensate the decline in formal jobs, in informal employment. However, the estimates for self-employment–(usually considered part of informal employment) in Table 7 Panel A, indicate an unstable effect of JS on self-employment. While the coefficient resulting from OLS estimation is positive and significant, the coefficient resulting from fixed effect estimation is negative and statistically significant. More research is necessary to understand the relationship between uncovered employment and job security in Latin America.

5. Conclusions

In a recent article, Freeman (2000) writes "the institutional organization of the labour market has identifiable large effects on distribution, but modest hard-to-uncover effects on efficiency." This view is shared by many economists (see Abraham and Houseman, 1994 and Blank and Freeman, 1994). However, the results summarized in this paper suggest that job security regulations have a substantial impact on employment and turnover rates both in Latin America and in OECD countries and thus substantially affect the efficiency of the labor market.

The assertion that job security does not have any impact on employment rates is based on evidence on unemployment, not on employment. However, employment and unemployment are not mirror images of each other. In addition, while there is substantial evidence that unions reduce earnings inequality in industrial countries, there is no evidence that job security provisions reduce income inequality. Indeed, given that job security

reduces the employment prospects (and possibly wages) of younger and less experienced workers, who bear the brunt of regulation, it is likely that regulation widens earnings inequality across age groups. Thus, there is no tradeoff between employment and inequality associated with job security provisions. Such provisions worsen both. The choice of labor market institutions matters.

What policy lessons can be drawn from these results? Our evidence suggests that job security provisions are an extremely inefficient and inequality-increasing mechanism for providing income security to workers. They are inefficient because they reduce the demand for labor; they are inequality-increasing because some workers benefit while many others are hurt. Their impact on inequality is multifaceted: Job security increases inequality because it reduces the employment prospects of young, female and unskilled workers. It also increases inequality because it segregates the labor market between workers with secure jobs and workers with very few prospects of becoming employed. Finally, job security provisions increase inequality if, as predicted by some theoretical studies and most of the available empirical evidence, they increase the size of the informal sector.

In this light, it seems reasonable to advocate the substitution of job security provisions by other mechanisms that provide income security at lower efficiency and inequality costs. However, reducing dismissal costs is a difficult policy to implement in most countries. The persistence of these policies can be explained by a demand for income security for groups with political power (Caballero and Hammour, 2000). A demand for income security arises because job security flows out of unemployment and into employment. Although job security reduces the probability of exiting employment, conditional on having lost a job, the probability of finding a new job is reduced. This produces a sense of insecurity among protected workers, who exert pressure to maintain high levels of job security provisions. A balance of power that favors insider workers helps to sustain job security provisions. Thus, those workers most likely to benefit from such provisions are also more likely to be represented in the political process. Instead, outsider workers are less likely to influence policy. Reform-minded policymakers should

[15] The Heckman and Pagés elasticities, reported in Table 3, are obtained from a model identical to the one reported in Table 6, but where job security provisions enter the specification in logs.

pursue broad coalitions including representatives of outsider workers, such as young, female, unemployed or discouraged workers, to obtain support for labor market reforms.

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Appendix

Construction of the index of job security

The job security index is constructed according to the following formula:

$$Index_{jt} = \sum_{i=1}^{T} b^{i} d^{i-1}(1-d)(b_{jt+i} + aSP_{jt+i}^{jc} + (1-a)SP_{jt+i}^{uc})$$

where *j* denotes country, *d*is the probability of remaining in a job, β is the discount factor, *T* is the maximum tenure that a worker can attain in a firm, $b_{j,t+i}$ is the advance notice to a worker that has been *i* years at a firm, *a* is the probability that the economic difficulties of the firm are considered a justified cause of dismissal, SP_{ij}^{jc} is the mandated severance pay in such event to a worker that has been *i* years at the firm, and finally, SP_{jt+1}^{uc} denotes the payment to be awarded to a worker with tenure *i* in case of unjustified dismissal.[16]

The constructed index measures the expected discounted cost, at the time a worker is hired, of dismissing a worker in the future. The assumption is that firms evaluate future costs based on current labor law. The index only includes statutory provisions, and thus, it does not include provisions negotiated in collective bargaining or included in company policy manuals. It addition, it does not include dismissal costs that are ruled by a judge if a firm is taken to courts. This assumption explains why dismissal costs–according to our index–are zero in the U.S., despite the substantial potential costs associated with legal actions. High values of the index indicate periods or countries of high job security, whereas lower values characterize periods or countries in which dismissal costs are lower. By construction, this index gives equal weight to notice periods and to severance pay since both are added up in the calculation of the dismissal costs. This index however gives a higher weight to dismissal costs that may arise soon after a worker is hired–since they are less discounted at the time of hiring–while it discounts firing costs that may arise further in the future.

In computing the index, we assumed a common discount rate and a common turnover rate of 8% and 12%, respectively. The choice of the discount rate is based on the average return of an internationally diversified portfolio. Finally, the choice of turnover

rate is based on the fact that real turnover rates are unobservable in countries with job security provisions since the turnover rate is itself affected by job security. We therefore choose to input all countries with the observed turnover rates in the U.S., the country in the sample with the lowest job security. The minimum tenure at a firm is considered to be one year, and the maximum is assumed to be twenty years.

We compute SP_ij^{jc} and SP_ij^{uc} based on the two different sources. For LAC countries, we use the legal information summarized in Table 1.A. This information was directly obtained from the Ministries of Labor of the region. In the case of Colombia we consider that severance payment prior to the 1990 reform was one and one-half months per year of work instead of one, as prescribed by law, to include that prior to the 1990 reform, advance withdrawals to the seniority premium fund were accounted in nominal terms. High inflation rates implied that this practice substantially increased overall dismissal costs. For OECD countries, we use the legal information summarized in OECD (1999). In all Latin American countries but Argentina and Chile, economic conditions are not a just cause for dismissal. Consequently, we assumed $a=0$ for those countries. Instead, in Argentina, Chile, economic conditions were a justified cause of dismissal and therefore, $a=1$. For OECD countries, we used the information summarized in Table 2.A.2 OECD (1999) to parameterize severance payments and advance notice. In all cases, but in Spain, $a=1$. In Spain, mandatory severance pay in the case of unjustified cause was substantially larger than severance pay for just cause. Consequently most workers fired for just cause appealed to the courts, and there was a high probability that a judge would declare a dismissal unjustified. Based on Bertola, Boeri and Cazes (2000), we assume that prior to the 1997 reform, a=0.2. After 1997, the scope for ambiguity was reduced and a=0.5. For Canada, we used the information relevant to the federal jurisdiction (although JS provisions may vary across states). Finally, in some European countries statutory dismissal costs vary across blue and white-collar workers. To obtain a single measure per country, we compute a separated index for blue and white-collar workers and performed a simple average among the two. (See OECD, 1999 for a description of dismissal costs in OECD countries and the cost divergences between blue- and white-collar workers.)

Definition of Variables used in Empirical Section

Total Employment. All employed workers between 16 and 65 who declared having a job in the week of reference. It is measured as % of total population 16-65. All measures of aggregate employment include formal and informal workers. They also include unpaid workers. Source: OECD statistics and LAC household Surveys.

Prime Age-Male Employment: % of men 25-50 years old employed in the week of reference. Source: OECD statistics and LAC household Surveys.

Prime Age-Female Employment: % of female 25-50 years old employed in the week of reference. Source: OECD statistics and LAC household Surveys.

Youth Employment: % of people 16-24 years old employed in the week of reference. Source: OECD statistics and LAC household Surveys.

Self-Employment: Share of non-agricultural workers in self-employment or as owners of firms. Source: Maloney (1999)

Total Unemployment: # of people 16-65 that did not work in the week of reference but are actively looking for a job as a % of total active population in that age group. Source: OECD statistics and LAC household Surveys.

Prime-Age Male Unemployment: # of men 25-50 that did not work in the week of reference but are actively looking for a job as a % of male active population in that age group. Source: OECD statistics and LAC household Surveys.

Prime-Age Female Unemployment: # of people 25-50 that did not work in the week of reference but are actively looking for a job as a % of female active population in that age group. Source: OECD statistics and LAC household Surveys.

Youth Unemployment: # of people 16-24 that did not work in the week of reference but are actively looking for a job as a % of active population in that age group. Source: OECD statistics and LAC household Surveys.

Long-term unemployment: # of people 16-65 that have been without a job, and actively looking for one for more than 6 months as a % of total active population in that age group. Source: OECD statistics and LAC household Surveys.

Female Participation: % of total female workers 16-65 that are either employed or actively seeking one. Source: OECD statistics and LAC household Surveys.

GDP: Gross Domestic Product measured in 1995 US dollars. Source: World Bank.

Population 15-24: Proportion of population in this age group. Source: UN Population Statistics

Table 1.A: Legislation Concerning Conditions of Dismissal in 1990 and 1999. X=monthly wages, N=Years of Tenure

	Date of Reform	Advance Notice		Seniority Premium		Compensation if worker quits?		Compensation for dismissal due to economic reasons		To whom the reforms apply?	Upp comp for di
		1990	1999	1990**	1999	1990	1999	1990	1999		1
Argentina	None	1-2 months	1-2month	0	0	0	0	2/3x*N, Min 2 months	No changes		Ma
Bahamas	None	1/2-1 month	No changes	0	0	0	0	Negogtiable	No changes		
Barbados	None	Negotiable in practice 1month ,	No changes	0	0	0	0	0.41*x*N if N>=2	No changes		Max.
Belize	None	1/2 - 1 month	No changes	0	0	1/6x*N if N>10	No changes	1/4x*N If N>5	No changes		Ma
Bolivia	None	3 months	No changes	0	0	1 x*N. if N>=5	No changes	1 x*N.	No changes		
Brazil	1988	1 month	No changes	Fund (8% wage + r)	Fund (8% wage+ r)	0	0	0.4*FUND	No changes		
Chile	**1991**	1 month	No changes	0	0	No	1/2 x*N (2) if N>=7	1 x*N. (3)	No changes	All workers	Ma
Colombia	**1990**	45 days	No changes	x*N Double retroactivity given lack of inflationary adjustment of withdrawals	Fund (8% wage+r)	Fund	No changes	x*4.0 if N=5 x*6.6 if N=10 x*16.5 if N=15 x*21.5 if N=20	x*4.0 if N=5 x*6.6 if N=10 x*21.5 if N=15 x*28.5 if N=20	All workers	
Costa Rica	None	1 month	No changes	0	0	0	0	x*N	No changes		M
Ecuador	None	1 month	No changes	Fund (8% wage+ r)	Fund (8% wage+r)	Seniority Premium	No changes	1/4 x*N plus 3*x if N <=3 plus x*N if N = 3 - 25 plus pension if N>=25	No changes		
El Salvador	**1994**	0-7 days	No changes	0	0	0	0	x*N 0 if bankrupcy	x*N Changes in max. x	All workers	4 min.
Guatemala	None	0	0	0	0	0	0	2 days-4 months if bankrupcy. x*N otherwise	No changes		

Guyana	***1997***	*1/2 month*	*1month* *If N>=1*	*0*	*0*	*0*	*0*	*Negotiable* *In practice,* *2 1/2 weeks per N*	*1/4*x*N if N=1-5* *1/2*x*N if N=5-10*	*All workers*	
Honduras	*None*	*1day-2 months*	*No changes*	*0*	*0*	*0*	*0*	*x*N*	*No changes*		*Max.*
Jamaica	*None*	*2-12 weeks*	*No changes*	*0*	*0*	*0*	*0*	*1/3*x*N if x=2-5* *1/2*X*N if x>5*	*No changes*		
Mexico	*None*	*0 - 1 month*	*No changes*	*0*	*0*	*0*	*0*	*2/3 x*N (Min. 3*x)*	*No changes*		
Nicaragua	***1996***	*1- 2 months*	*0*	*0*	*0*	*0*	*x*N if N=1-3* *3x*N + 2/3x*N if N>3*	*Negotiated* *In practice, 2 x*N.*	*x*N if N=1-3* *3x*N + 2/3x*N if N>3*		
Panama	***1995***	*1 Month*	*No changes*	*1/4*X*N* *if N>=10*	*1/4*X *N*	*1/4*X*N* *if N>=10*	*1/4*X*N*	*X*N if N<=1* *3*x if N=2* *3*x + 3/4*x*N if N>2<10* *9*x+ 1/4*x*N if N>=10*	*3/4X*N if N<10* *7.5*x+1/4*X if N>=10*	*New employees*	
Paraguay	*None*	*1-2 months*	*No changes*	*0*	*0*	*0*	*0*	*1/2 x*N*	*1/2 x*N*		
Peru	***1996***	*0*	*0*	*Determined by*	*Fund (8% wage+r)*	*Fund (8% wage+r)*	*Seniority Premium*	*3 x*N*	*FUND+1.5*x*N*	*1991 New Employees*	*Max.*
	1995			*judge in legal*						*1995 All workers*	
	1991			*Proceedings*						*1996 All workers*	
Rep. Dom.	***1992***	*1/4 -1 month*	*No changes*	*0*	*0*	*0*	*0*	*1/2*x*N*	*.67*x*N if N=1-4* *.74*x*N if N>=5,*	*New employees*	
Suriname	*None*	*1/4.-6 month.*		*0*		*0*	*0*	*Negotiated*	*Negotiated*		
Trin. and Tob.	*None*	*2 months*		*0*	*0*	*0*	*0*	*1/3 x*N if N = 1-4, 1/2 x*N if N>5*	*No changes*		
Uruguay	*None*	*0*	*0*	*x*N*	*No changes*	*0*	*0*	*x*N*	*No changes*		*Max*
Venezuela	***1997***	*1/4 -3 months.*	*No changes*	*x*N*	*2x*N*	*X*N*	*2x*N*	*2/3-2 x*N*	*x*N*	*All workers*	

*Source: Ministries of Labor in the region **In Brazil, the date refers to 1988 (instead of 1990)*

Graph1: Job Security Index
(Expected discounted cost of dismissing a worker, in multiples of monthly wages)

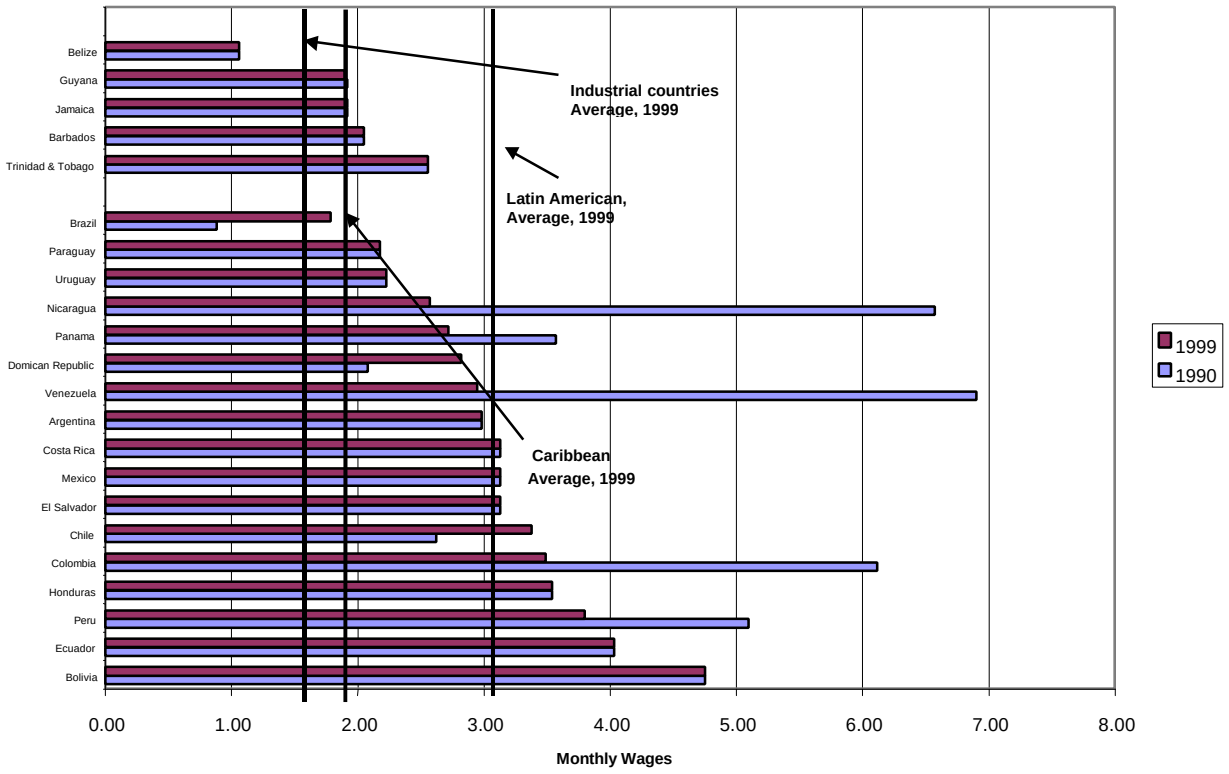

Table 4: Summary Statistics

Average Statistics for the overall sample					
Variable	Observations	# countries	# per country	Mean	Std. Dev.
Total Employment	221	43	5.1	66.09	8.44
Prime-Age Male Employment	139	43	3.2	89.19	4.93
Prime-Age Female Employment	139	43	3.2	56.88	14.85
Youth (15-24) Employment	140	43	3.3	53.05	15.47
Self-employment	84	40	2.1	26.92	11.87
Total Unemployment	221	43	5.1	8.01	4.15
Prime-Age Male Unemployment	221	43	5.1	8.01	4.15
Prime-Age Female Unemployment	139	43	3.2	4.99	3.09
Youth (15-24) Unemployment	139	43	3.2	6.25	4.39
Unemployed > 6months/Total U.	140	40	3.5	13.42	7.71
Job Security	205	36	5.7	2.62	1.74
GDP (US dollars 1995)	212	42	5.0	5.E+11	9.E+11
GDP growth	179	41	4.4	2.90	3.30
Proportion pop 15 to 24	221	43	5.1	0.16	0.03
Female Participation	221	43	5.1	55.64	13.34
Union density	47	39	1.2	26.52	17.79

Average Statistics for Latin America and the Caribbean					
Variable	Observations	# countries	# per country	Mean	Std. Dev.
Total Employment	59	15	3.93	71.950	4.222
Prime-Age Male Employment	59	15	3.93	91.746	3.157
Prime-Age Female Employment	59	15	3.93	47.191	10.699
Youth (15-24) Employment	59	15	3.93	63.662	11.078
Self-employment	59	15	3.93	32.742	8.269
Total Unemployment	59	15	3.93	7.404	3.296
Prime-Age Male Unemployment	59	15	3.93	3.881	2.578
Prime-Age Female Unemployment	59	15	3.93	4.666	3.134
Youth (15-24) Unemployment	59	15	3.93	10.881	4.670
Unemployed > 6months/Total U.	42	15	3.93	14.548	7.262
Job Security	108	16	2.69	3.512	1.567
GDP (US dollars 1995)	66	20	5	1.24E+11	1.99E+11
GDP growth	59	17	3.88	3.312	3.837
Proportion pop 15 to 24	71	17	3.47	0.197	0.016
Female Participation	59	18	3.94	44.255	10.526
Union density	21	17	1.23	18	11.37

Average Statistics for OECD Sample (Excluding Mexico)					
	Observations	# countries	# per country	Mean	Std. Dev.
Total Employment	162	28	5.79	63.96	8.59
Prime-Age Male Employment	80	28	2.86	87.31	5.16
Prime-Age Female Employment	80	28	2.86	64.02	13.39
Youth (15-24) Employment	81	28	2.89	45.33	13.54
Self-employment	25	25	1.00	13.17	6.47
Total Unemployment	162	28	5.79	8.22	4.41
Prime-Age Male Unemployment	162	28	5.79	8.22	4.41
Prime-Age Female Unemployment	80	28	2.86	5.80	3.19
Youth (15-24) Unemployment	80	28	2.86	7.43	4.81
Unemployed > 6months/Total U.	81	24	3.38	15.28	8.90
Job Security	97	16	6.06	1.63	1.36
GDP (US dollars 1995)	146	25	5.84	6.25E+11	1.07E+12
GDP growth	120	24	5.00	2.70	3.00
Proportion pop 15 to 24	150	25	6.00	0.15	0.02
Female Participation	162	28	5.79	59.79	11.77
Union density	26	22	1.18	33.43	19.18

Table 5: Description of Household Surveys

Country	Year	Name of the survey	Sample size		Month when
			Households	Individuals	Survey was Held
Bolivia	96	Encuesta Nacional de Empleo	8,311	35,648	June
	97	Encuesta Nacional de Empleo	8,461	36,752	November
Brazil	81	Pesquisa Nacional por Amostra de Domicilios	103,193	481,480	September
	83	Pesquisa Nacional por Amostra de Domicilios	113,599	511,147	September
	86	Pesquisa Nacional por Amostra de Domicilios	65,277	289,533	September
	88	Pesquisa Nacional por Amostra de Domicilios	68,833	298,031	September
	92	Pesquisa Nacional por Amostra de Domicilios	78,188	317,145	September
	93	Pesquisa Nacional por Amostra de Domicilios	80,054	322,011	September
	95	Pesquisa Nacional por Amostra de Domicilios	85,167	334,106	September
	96	Pesquisa Nacional por Amostra de Domicilios	84,862	331,142	September
Chile	87	Encuesta de Caracterización Socioeconómica Nacional	22,719	97,044	December
	90	Encuesta de Caracterización Socioeconómica Nacional	25,793	105,189	November
	92	Encuesta de Caracterización Socioeconómica Nacional	27,666	110,555	November
	94	Encuesta de Caracterización Socioeconómica Nacional	45,379	178,057	November
	96	Encuesta de Caracterización Socioeconómica Nacional	33,636	134,262	November
Colombia	95	Encuesta Nacional de Hogares - Fuerza de Trabajo	18,255	79,012	September
	97	Encuesta Nacional de Hogares - Fuerza de Trabajo	32,442	143,398	September
Costa Rica	81	Encuesta Nacional de Hogares - Empleo y Desempleo	6,604	22,170	July
	83	Encuesta Nacional de Hogares - Empleo y Desempleo	7,132	23,449	July
	85	Encuesta Nacional de Hogares - Empleo y Desempleo	7,351	23,960	July
	87	Encuesta de Hogares de Propósitos Múltiples	7,510	34,591	July
	89	Encuesta de Hogares de Propósitos Múltiples	7,637	34,368	July
	91	Encuesta de Hogares de Propósitos Múltiples	8,002	35,565	July
	93	Encuesta de Hogares de Propósitos Múltiples	8,696	37,703	July
	95	Encuesta de Hogares de Propósitos Múltiples	9,631	40,613	July
	97	Encuesta de Hogares de Propósitos Múltiples	9,923	41,277	July
Dominican Republic	96	Encuesta Nacional de Fuerza de Trabajo	5,548	24,041	February
Ecuador	95	Encuesta de Condiciones de Vida	5,810	26,941	August to November
El Salvador	95	Encuesta de Hogares de Propósitos Múltiples	8,482	40,004	1995
Honduras	89	Encuesta Permanente de Hogares de Propósitos Múltiples	8,727	46,672	September
	92	Encuesta Permanente de Hogares de Propósitos Múltiples	4,757	24,704	September
	96	Encuesta Permanente de Hogares de Propósitos Múltiples	6,428	33,172	September
	98	Encuesta Permanente de Hogares de Propósitos Múltiples	6,493	32,696	March
Mexico	84	Encuesta Nacional de Ingreso Gasto de los Hogares	4,735	23,985	Third quarter
	89	Encuesta Nacional de Ingreso Gasto de los Hogares	11,531	57,289	Third quarter
	92	Encuesta Nacional de Ingreso Gasto de los Hogares	10,530	50,862	Third quarter
	94	Encuesta Nacional de Ingreso Gasto de los Hogares	12,815	60,365	Third quarter
	96	Encuesta Nacional de Ingreso Gasto de los Hogares	14,042	64,916	Third quarter
Nicaragua	93	Encuesta Nacional de Hogares Sobre Medicion de Niveles de Vida	4,455	24,542	February to June
Panama	79	Encuesta Continua de Hogares - Mano de Obra	8,593	24,284	
	91	Encuesta Continua de Hogares - Mano de Obra	8,867	38,000	August
	95	Encuesta Continua de Hogares	9,875	40,320	August
	97	Encuesta de Hogares	9,897	39,706	August
Paraguay	95	Encuesta de Hogares - Mano de Obra	4,667	21,910	August to November
Peru	85-86	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida	5,108	26,323	July 1985 to July 1986
	91	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida	2,308	11,507	September-November
	94	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida	3,623	18,662	May-August
	96	Encuesta Nacional de Hogares sobre Niveles de Vida y Pobreza	16,744	88,863	
	97	Encuesta Nacional de Hogares sobre Niveles de Vida y Pobreza	3,843	19,575	September-November
Venezuela	81	Encuesta de Hogares por Muestra	45,421	239,649	Second semester
	86	Encuesta de Hogares por Muestra	129,713	682,636	Second semester
	89	Encuesta de Hogares por Muestra	61,385	315,650	Second semester
	93	Encuesta de Hogares por Muestra	61,477	306,629	Second semester
	95	Encuesta de Hogares por Muestra	18,702	92,450	Second semester
	97	Encuesta de Hogares por Muestra	15,948	76,965	Second semester

Table 6.a: OLS Estimation. Full Sample

	Total Emp.	Male Prime-age Emp.	Female Prime-age Emp.	Youth Emp.	Self-Empl.	Total Unemployment	Male Prime-age Unemployment	Female Prime-age Unemployment	Youth Unemployment	Proportion of Unemp. > 6 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LAC	16.04***	4.70***	-11.37	28.47	11.67***	-2.12**	-2.75***	-4.23***	-7.16***	-44.14***
	(1.33)	(.91)	(3.22)	(3.29)	(3.21)	(1.15)	(.70)	(1.11)	(2.57)	(3.76)
Job Security	-1.37***	-0.81***	-1.46	-3.54***	1.37**	0.83***	.87***	.833***	.87*	.86
	(.32)	(.258)	(.90)	(3.97)	(.58)	(.28)	(.19)	(.31)	(.53)	(.89)
GDP growth	-.108	-0.05	-0.124	.008	.50**	0.06	-0.04	.10	0.083	-0.16
	(.133)	(.110)	(.387)	(.36)	(.23)	(.116)	(.08)	(.13)	(.21)	(0.36)
GDP level	-3E-12***	-1.97E-12	2.45E-12	-3.5E-12	-3.01E-12	3.51E-12	2.91E-12***	3.6E-11**	2.55E-12	6.71E-12*
	(1.28e-12)	(1.39e-12)	(4.86e-12)	(4.58e-12)	(3.33e-12)	(1.11e-12)	(1.06e-12)	(1.68e-11)	(2.69e-12)	(3.88e-12)
Female part.	0.399***	-	-	.334***	.240***	-.108***	-	-	-.186	-.65***
	(0.047)			(.12)	(.084)	(.04)			(.078)	(0.14)
Pop 15to24	11.56	-	-	-	115.26**	-34.49	-	-	-69.89	-96.57
	(27.08)				(52.12)	(23.53)			(48.85)	(17.28)
Constant	41.63***	89.95***	62.81***	33.19***	-19.35	17.43	3.24***	5.09	36.21**	104.7***
	(5.21)	(1.21)	(4.27)	(8.32)	(10.59)	(5.07)	(.93)	(1.47)	(10.12)	(17.25)
N. observations	114	77	77	78	65	114	77	77	78	64
R-square	0.73	0.33	0.29	0.53	0.57	0.23	0.32	0.26	0.30	.85

Notes: Standard errors reported within parenthesis. * indicates significant at 10, ** significant at 5% and *** significant at 1%.

Table 6.b: Random–Effects (RE) Estimation. Full Sample

	Total Emp.	Male Prime-age Emp.	Female Prime-age Emp.	Youth Emp.	Self-Empl.	Total Unemployment	Male Prime-age Unemployment	Female Prime-age Unemployment	Youth Unemployment	Proportion of Unemp. > 6 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
LAC	15.26***	4.62**	-11.05**	29.99***	14.56***	-2.24	-2.36*	-3.79	-7.29	-48.61***
	(2.15)	(1.82)	(5.47)	(5.23)	(3.90)	(1.93)	1.26	(1.92)	(3.81)	(6.35)
Job Security	-1.84***	-1.04**	.526	-3.28***	.35	.69	.77**	1.06**	.99	.95
	(.505)	(.48)	(1.33)	(1.38)	(.87)	(.45)	(.34)	(.515)	(.86)	(1.49)
GDP growth	-0.001	.054	.218	0.164	.393***	-.04	.016	.12	-.084	-0.171
	(.073)	(.091)	(.199)	(.278)	(.166)	(.06)	(.07)	(.09)	.135	(.246)
GDP level	-4.14E-12	-2.68E-12	1.31E-11*	-7.18E-12	-5.36E-12	4.23E-11*	3.13E-12*	4.72E-12*	-5.36E-12	9.49E-12
	(2.51e-12)	(2.42e-12)	(7.03e-12)	(6.87e-12)	(4.39e-12)	(2.24e-12)	(1.71e-12)	(2.57e-12)	(4.39e-12)	(6.80e-12)
Female part.	0.33***	-	-	0.63***	.036	.021	-	-	.037	-.304*
	(0.047)			(.13)	(.08)	(.04)			.077	(.161)
Pop 15to24	3.16	-	-	-	40.22	29.98	-	-	41.98	115.79
	(26.84)				(54.40)	(25.22)			(46.25)	(115.28)
Constant	47.77***	90.37***	54.06***	16.80*	6.95	.53	3.36**	4.23**	4.95	50.7***
	(5.74)	(1.89)	(5.34)	(9.43)	(11.13)	(5.38)	(1.36)	(2.01)	(9.81)	(22.22)
N. observations	114	77	77	78	65	114	77	77	78	64
R-square	0.72	.32	.23	0.50	.57	.13	.31	.25	.17	0.82
Hausman Test	5.46 (.36)	3.90 (.27)	2.17 (.57)	9.43 (0.05)	53.56 (0.00)	9.53 (0.08)	4.87 (.18)	3.75 (.28)	8.78 (.11)	8.06 (.15)

Notes: Standard errors reported within parenthesis. * indicates significant at 10, ** significant at 5% and *** significant at 1%.

Table 6.c : Fixed –Effects (FE) Estimation. Full Sample

	Total Emp.	Male Prime-age Emp.	Female Prime-age Emp.	Youth Emp.	Self-Empl.	Total Unemployment	Male Prime-age Unemployment	Female Prime-age Unemployment	Youth Unemployment	Proportion of Unemp. > 6 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Job Security	-1.55	-0.013	3.27	-6.04*	-8.43***	-.187	-1.06	0.021	-1.16	1.51
	(1.07)	(1.183)	(2.29)	(3.55)	(1.73)	(.99)	(.96)	(1.28)	(1.62)	(4.64)
GDP growth	0.049	.143	.145	.278	.111	-0.09	-0.05	0.024	-.25*	-0.17
	(.078)	(.101)	(.19)	(.303)	(.150)	(.07)	(.08)	(.11)	(.13)	(.28)
GDP level	-1.92E-11	-2E-11***	5.5E-11**	-6.7E-11**	-3.01E-12	1.6E-11***	2.1E-11***	2.4E-11**	3.9E-11***	3.90E-11
	(8.84e-12)	(9.97e-12)	(1.93e-11)	(3.25e-11)	(3.74e-12)	(8.1e-12)	(8.15e-12)	(1.08e-11)	(1.48e-12)	(4.55e-11)
Female part.	0.34***	-	-	1.00***	.240	.07	-	-	.08	-.07
	(0.05)			(.19)	(.104)	(.05)			(.09)	(.23)
Pop 15to24	-5.93	-	-	-	115.26	56.03*	-	-	60.71	529.05**
	(31.20)				(51.13)	(28.63)			(49.10)	(218.91)
Constant	59.67***	95.94***	27.14***	42.15***	-19.35	-9.05	3.00	-.008	-7.12**	-63.79***
	(7.21)	(3.37)	(6.54)	(11.35)	(10.37)	(6.62)	(2.76)	(3.66)	(11.63)	(45.53)
N. observations	114	77	77	78	65	114	77	77	78	64
N. countries	28	28	28	28	27	28	28	28	28	25
R-square	0.09	0.05	0.05	0.03	0.30	0.03	0.03	0.08	0.01	0.04

Notes: Standard errors reported within parenthesis. * indicates significant at 10, ** significant at 5% and *** significant at 1%.

Table 7: The impact of job security in the regional sub-samples

A. Latin America and the Caribbean

Dependent Variable	*# Obs.*	*OLS Coefficient*	*OLS S.E.*	*RE Coefficient*	*RE S.E*	*FE Coefficient*	*FE S.E.*
Total Employment	53	-1.29***	(0.36)	-1.62***	(0.59)	-1.83	(1.34)
Male prime-age Employment	53	-1.03***	(0.30)	-1.44**	(0.58)	-0.48	(1.24)
Female prime-age Employment	53	0.78	(1.11)	3.15**	(1.52)	3.10	(2.59)
Youth Employment	53	-4.21***	(0.94)	-4.33***	(1.30)	-7.50*	(3.70)
Self-employment	53	1.09*	(0.63)	-0.58	(0.98)	-8.34***	(1.73)
Total Unemployment	53	0.34	(0.35)	.06	(0.04)	0.13	(1.26)
Male prime-age Unemp.	53	0.94***	(0.24)	0.91***	(0.43)	-0.74	(1.02)
Female Prime-age Unemp.	53	0.27	(0.33)	0.51	(0.52)	0.06	(1.42)
Youth Unemployment	53	0.35	(0.47)	-0.22	(1.60)	-0.22	(1.60)
% Long-term Unemp.	30	0.13	(0.98)	-0.11	(1.36)	0.42	(5.31)

B. OECD Countries (Excluding Mexico)

Dependent Variable	*# Obs..*	*OLS Coefficient*	*OLS S.E.*	*RE Coefficient*	*RE S.E.*	*FE Coefficient*	*FE S.E.*
Total Employment	61	-0.82	(0.57)	-3.30***	(1.16)	-	-
Male prime-age Employment	24	-0.06	(0.66)	-0.07	(1.13)	-	-
Female prime-age Employment	24	-5.80***	(1.69)	-6.16***	(2.38)	-	-
Youth Employment	25	1.32	(2.81)	-4.41	(4.58)	-	-
Self-employment	Not enough observations						
Total Unemployment	61	1.14**	(.56)	2.27**	(1.10)	-	-
Male prime-age Unemp.	24	0.50	(0.49)	0.48	(0.77)	-	-
Female Prime-age Unemp.	24	2.23***	(0.85)	2.04*	(1.19)	-	-
Youth Unemployment	25	.586	(1.98)	4.70*	(2.93)	-	-
% Long-term Unemp.	35	2.003	(1.85)	3.31	(3.62)	-	-

Note: standard errors between parenthesis. The specifications for the two sub-samples include the same repressors than in the overall sample.

* indicates significant at 10, ** significant at 5% and *** significant at 1%.