

# Mandatory Severance Pay: Its Coverage and Effects in Peru

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## **Abstract**

In Peru, like in many other developing countries, employers have the legal obligation to compensate workers who are dismissed due to no fault of their own. Is this an efficient mechanism to provide income support to the unemployed? This paper seeks an answer to this question using individual records from a household survey with a panel structure. Relying on five coverage indicators, the paper shows that roughly one in five private sector workers, and one in three private sector wage earners, is legally entitled to severance pay. Coverage is more prevalent among wealthier workers. Results based on several empirical strategies suggest that workers “pay” for their entitlement to severance pay through lower wages. Finally, consumption among unemployed workers who receive severance pay is 20 to 30 percent higher than among those who do not. Consumption among these workers is actually higher than among those who are employed, implying that mandatory severance pay is excessively generous in Peru.

## **1. Introduction**

In Peru, like in many other developing countries, employers have the legal obligation to compensate workers who are dismissed due to no fault of their own. The amount of compensation to be paid is based on a formula involving seniority in the job and the last monthly salary. The specific formula has been modified several times over the last decade, but it has often involved a minimum seniority, a maximum severance pay, and various discontinuities or kinks. Depending on seniority, the total amount received by the worker represents a few months to one year of salary. Compliance is complaint-driven, as a dismissed worker who is denied severance pay can report the problem to the Ministry of Labor, and possibly get his or her due. Handling these complaints is actually one of the main tasks of the Ministry.

Mandatory severance pay of this sort could be seen as a convenient substitute for unemployment insurance in countries with low administrative capacity. Workers do not need to be enrolled with the social security administration to be entitled to severance pay; employers do not need to make contributions; and the social security administration does not need to pay benefits or monitor whether the beneficiaries are indeed unemployed. Because compliance is complaint-driven, rather than based on enrollment, the share of the labor force covered by the scheme is potentially larger than the share of the formal sector. And because there are no taxes or administrative costs, other than those related to ex-post enforcement, the burden on the rest of the economy should not be too heavy.

Mandatory severance pay should not distort the incentives faced by workers either. Those who lose their jobs because of misconduct are not entitled to severance pay. As a result, shirking at work carries an explicit penalty. In efficiency wage models, employers elicit effort by paying a wage premium, which workers lose if they are fired and have to take a “bad” job.

Confronted with mandatory severance pay, employers could reduce this premium, and still provide an incentive for their workers to put effort on their job. Moreover, the amount of severance pay received by workers who do lose their jobs is independent from the time they stay unemployed. Therefore, mandatory severance pay does not create an incentive for workers to remain on the dole.

On the other hand, mandatory severance pay could affect the incentives faced by employers. In this respect, mandatory severance pay is often seen as one of the most important labor market distortions characterizing Latin American countries (see Márquez and Pagés, 1998). The increase in separation costs resulting from this mandate is a potential deterrent to job creation. Moreover, mandatory severance pay could allow covered workers to bargain for higher wages, as replacing them would be more expensive for employers. Formal sector employment would therefore drop both because of higher (anticipated) separation costs and higher (current) labor costs.

Finally, one key issue is whether mandatory severance pay is effective at smoothing the consumption of workers who lose their jobs. The answer crucially depends on the alternative consumption smoothing mechanisms available. If severance pay displaced precautionary savings, or intra-household transfers, its benefits would be minimal. However, mandatory severance pay allows some risk pooling among workers and employers, so that it could effectively operate as insurance. If it did, its consumption smoothing benefits could outweigh the allocation costs in a social welfare function that attaches a strong weight to the well-being of urban, formal sector workers and their families (see Basu *et. al.*, 1996).

Mandatory severance pay is certainly popular in Peru. In November 1996, in the context of its policy dialogue with multilateral organizations, the Peruvian government conveyed the

impression that it wanted to slash mandatory severance pay by half. As a draft regulation containing this change made it to the news, there was public-opinion uproar, forcing an immediate reaction. Shortly after, the government announced that the publicized draft contained an unfortunate typo, its true intention being to raise severance pay by half. This effectively happened (Saavedra, 1999). Since then, there have been no further changes to the (now more generous) severance pay formula.

The aim of this paper is to assess the coverage and effects of mandatory severance pay in Peru. The choice of Peru is justified because this country has a very “generous” severance pay regime. Typically, separated workers are entitled to a couple of weeks of salary per year of service, and severance pay caps at three or six months of salary are not uncommon. The costs and benefits observed in Peru could thus be seen as an upper bound for other developing countries. The popularity of mandatory severance pay among vocal segments of the population could suggest that the benefits outweigh the costs. However, public-opinion uproar is no substitute for a thorough economic assessment. The interest of such an assessment is magnified by the fact that relatively little is known about the effects of mandatory severance pay in developing countries.

Based on the literature on industrial countries, mandatory severance pay could affect resource allocation. While an earlier simulation using a calibrated model found a small impact on employment (Bentolila and Bertola, 1990), a subsequent exercise claimed that the costs to society could be considerable (Hopenhayn and Rogerson, 1993). Analyses using cross-country data from industrial countries showed a lower employment-to-population ratio in countries with higher severance pay (Lazear, 1990; Addison and Grosso, 1996) or higher job protection (Nickell, 1997). And studies using sector-level data from Europe and the US concluded that job

protection leads to more reliance on hours adjustment than on employment adjustment (Abraham and Houseman, 1994), and to smaller flows in and out of unemployment (Blanchard and Portugal, 2000).

The evidence is much more scattered concerning developing countries. Given that only a fraction of the labor force is covered by mandatory severance pay, it would be hazardous to extrapolate the lessons from industrial countries. To the extent that coverage is endogenous, the impact on resource allocation could be much smaller. Admittedly, a study linked high separation costs to low employment-to-population ratios in a mixed sample of industrial and Latin American countries (Heckman and Pagés, 2000). But in this study, separation costs appear to be much higher in Latin America than in Western Europe, which is surprising. In a similar spirit, a study using sector-level data from India and Zimbabwe showed a drop in labor demand after job security regulations were made more stringent (Fallon and Lucas, 1991). But the main policy change in these two countries was an extension in the coverage of job security provisions, not an increase in mandatory severance pay. Finally, a study using employment data for specific age groups in Chile claimed that job security was associated with a decline in wage employment among young workers (Pagés and Montenegro, 1999). But no decline was observed for workers who were 26-50 years old.

In the specific case of Peru, there is some evidence, based on plant-level data, suggesting that mandatory severance pay does reduce formal sector employment. Saavedra and Torero (1999) measure firing costs for a panel of roughly 500 firms in the formal sector, based on the average compensation these firms would have to pay if they fired all their workers. Controlling for average wage costs, firing costs appear to have a negative impact on labor demand in the late 1980s and early 1990s. The results could be questioned on methodological grounds, as a lagged

value of the endogenous variable is included among the explanatory variables, thus making the estimates inconsistent, given the chosen econometric technique. Besides, the impact of firing costs becomes statistically insignificant in the mid-1990s, despite the fact that severance pay regulations were not eased.

This paper uses household-level data to evaluate the coverage and effects of mandatory severance pay. Household-level data offer a more credible basis for the empirical analysis than cross-country data or plant-level data, not to mention simulations based on calibrated models. This is because of the potential endogeneity of coverage, which is one of the main problems when assessing the effects of a program. In the case of Peru, the availability of a high-quality household survey with a panel structure, known as *Encuesta Nacional de Hogares Sobre Medición de Niveles de Vida* (ENNIV, 1995), allows implementing several empirical strategies to deal with potential endogeneity biases.

Evaluating coverage is not a trivial issue in developing countries, as programs that are efficient on paper (such as unemployment insurance) may only reach a small fraction of the labor force, whereas more inefficient mandates (like severance pay) may be easier to implement. In this paper, coverage is assessed at the aggregate level but also by population quintiles, based on the countrywide distribution of per capita consumption. This breakdown is justified because the weight attached to the wellbeing of households in different quintiles varies with the objectives that are being pursued. Depending on whether the government is concerned about the poor, the median voter, or the most vocal interest groups, the focus would be on the bottom, the middle or the top quintiles respectively.

The effects of mandatory severance pay considered in this paper refer to labor costs and consumption. The employment effects of mandatory severance pay could be modest, or even

negligible, if covered workers “paid” for their income support when unemployed through lower wages while employed. They could also be low if the shirking penalty created by severance pay reduced the wage premium needed to entice workers to put effort on their jobs. But mandatory severance pay could also allow covered workers to bargain for higher wages, knowing that they can only be replaced at a higher cost. As regards consumption, severance pay provides separated workers with enough cash to spread the negative impact of job loss over several months or years. The amount received could even be large enough to lead to an increase in consumption. However, severance pay would have no impact on consumption if it were just displacing other consumption-smoothing mechanisms.

## **2. Regulations**

Peru combines a heavy regulation of its labor market with very limited compliance, as reflected by the large size of its informal sector. It is often claimed that these two characteristics are related, as heavy regulation increases the cost of playing by the rules (De Soto, 1989; Loayza, 1996). However, it has also been argued that the implicit, dualistic view of the labor market is not supported by the data (Maloney, 1999; Saavedra and Chong, 2000). And causality could very well go in the opposite direction, as the irrelevance of regulations would allow the government to appear socially “generous” on paper, without having to bear large economic costs in practice. In any event, some of the most stringent labor regulations of Peru were removed, or eased, during the 1990s.

A potentially important change was the abolition of job security for new hires, in 1991. Until then, dismissals had to be approved by the government. If they were deemed “unjustified”, the separated workers could choose between getting severance pay or being reinstated in their

jobs. The 1991 reform removed the need for government approval. In 1995, job security was formally abolished for all workers. However, it must be noted that job security regulations had been in place since 1970, giving employers enough time to find ways to bypass them. Workers were often hired on short-term contracts, dismissed before they reached the minimum seniority needed for job security, and then re-hired. Those on longer contracts were sometimes required to sign an undated resignation letter. The regulatory framework itself contained important loopholes. In 1971, the minimum seniority requirement was raised to three years, substantially reducing the legal coverage of job security provisions. In 1986 minimum seniority was set at three months only, but an emergency employment program was launched allowing employers to hire workers without providing social security benefits, or stability (Saavedra, 1999).

While job security regulations lost much of their teeth during the early 1990s, mandatory severance pay regulations were not substantially eased. The definition of “unjustified” dismissal was made more restrictive in 1991. The formula setting the amount to be paid in the event of unjustified dismissal was modified four times over the decade, but its basic structure remained the same, as shown in table 1. This table also displays the timing of the three rounds of the *Encuesta Nacional de Hogares Sobre Medición de Niveles de Vida*, so as to identify the specific formula that was in force when the interviews took place. These interviews were usually carried out within two or three months of the launching date reported in the table.

Because job security regulations were potentially binding in 1991, the empirical analysis in this paper focuses on the 1994 and 1997 rounds of the survey. Admittedly, mandatory severance pay increases the job security of those who are covered by it, because it is more expensive for an employer to fire them. In the limit, for an amount of severance high enough, it would be in the interest of the employer to retain the worker until retirement age, even if his or

her productivity was zero. However, in the case of Peru this amount cannot exceed one year of salary, so that there is a clear difference between mandatory severance pay and mandatory job security. Using data from 1991 would make it difficult to disentangle the effects of each of these two constraints. The implicit assumption, when focusing on data from the 1994 and 1997 rounds, is that job security regulations were largely irrelevant in those two years.

In addition to severance pay, dismissed workers are entitled to an end-of-service gratuity, known as *Compensación por Tiempo de Servicio*. This entitlement was created in 1963. The gratuity amounts to half a month of salary per year of service and it is payable to all workers at the termination of their contracts, regardless of whether they are dismissed or they just quit or retire. Since 1991, the gratuity has to be deposited twice a year in a Bank account under the worker's name. Because these deposits are not contingent on any event other than employment, the end-of-service gratuity can be seen as a delayed compensation, rather than an additional severance payment. It is safe to assume that workers who are covered (not covered) by severance pay regulations are also entitled (not entitled) to the end-of-service gratuity.

### **3. Data**

The *Encuesta Nacional de Hogares Sobre Medición de Niveles de Vida* is basically a Living Standards Measurement Survey, or LSMS for short (Grosh and Glewwe, 1998). The design and methodology of its three rounds were similar, despite the fact that only the first two were supported and monitored by the World Bank. In particular, all three rounds tried to provide a comprehensive picture of the poorest households, whose active members are seldom concerned by job security regulations. Thus, the 1994 and 1997 surveys had a national coverage, including both urban and rural areas. In 1991, the survey did not reach the whole country. Instead, it

covered urban areas and the rural *sierra* (highlands), allegedly because most of the Peruvian population lives in the former whereas many among the poor live in the latter. In practice, however, some provinces were excluded from the survey because terrorist activities made them dangerous for the interviewers. The partial coverage of the 1991 survey is a second reason for this paper to focus on the 1994 and 1997 rounds only.

The number of households in the sample increased over time, from 2,308 in 1991, to 3,623 in 1994, to 3,843 in 1997. A large number of individuals can be found in two consecutive rounds of the survey. In particular, there were 3,493 individuals surveyed in both 1994 and 1997; hereafter these individuals are identified as the 1994-1997 panel. Whenever possible, the person answering the questionnaire was the household head. While some of the information collected refers to the household as a whole, the survey also inquired about each of its members. In particular, questions related to employment, unemployment, cash earnings and job-related benefits were asked at the individual level. Information on variables such as age, gender or educational attainment was also collected at the individual level. Information on housing or expenditures is reported at the household level.

From the point of view of this paper, the key variables in the survey are those referring to employment status and to coverage by severance pay regulations. Following the standard practice, anyone who worked for a wage or salary, was self-employed, or worked in a family business in the seven days preceding the survey, was considered employed. Those who declared to have a job, even if they were out of work in the week preceding the survey, were also considered employed. On the other hand, anyone who was not employed according to this definition, and was actively searching a job in the week preceding the survey, was considered

unemployed. Those who were not employed, and were not actively seeking a job because they were reportedly sick, or discouraged, were also counted among the unemployed.

Coverage by severance pay regulations is more difficult to assess, because these regulations may not always be enforced. To address this difficulty, this paper does not rely on a single variable but rather on five coverage indicators, summarized in table 2. The “legal” indicator is used to identify the workers who meet the formal requirements for coverage. These requirements combine the salaried status with some minimum seniority, as described in table 1. Consequently, the legal label is not given to the self-employed, to family workers, or to salaried workers who have only been in their jobs for a short period of time (how short depends on the regulations in force at the time of the survey). Each of the other four coverage indicators focuses on a job characteristic that makes compliance with severance pay regulations more likely. The characteristics considered are: having a written contract with the employer (“contract”), being enrolled with the social security system (“pension”), working in a firm where at least one trade union operates (“trade union”), and working in an establishment that employs at least 21 persons in all (“large firm”).

Each of the five coverage indicators can be used to construct a dummy variable, set equal to one if the worker meets the corresponding requirement, and to zero otherwise. For instance, the “legal” dummy is equal to one for all workers who are legally entitled to severance pay, regardless of whether they have a written contract, are enrolled with social security, work in a unionized firm, or in a large firm. It is set equal to zero for workers who are not legally entitled to severance pay. In much of the analysis in this paper, the legal indicator is interacted with each of the other four indicators of coverage to generate another set of dummy variables, called “legal + contract”, “legal + social security”, “legal + trade union” and “legal + large firm”. For

instance, the “legal + contract” dummy is set equal to one for workers who are legally entitled to severance pay and have a written contract, but are not enrolled with social security, do not work in a unionized firm, and do not work in a large firm.

The five coverage indicators can also be combined to generate a job score, from “legal + 1” to “legal + 4”. The score “legal + 1” means that the worker is legally entitled to severance pay and his or her job has one (but only one) of the other four characteristics that make compliance more likely. The score “legal + 2” means that the job has two of those four characteristics, and so on and so forth. It is safe to assume that the probability that a worker will get severance pay in the event of unjustified dismissal increases with his or her job score. For a worker who gets a score of “legal + 4”, this probability gets close to certainty.

Unfortunately, not all the coverage indicators can be computed in all three rounds of the survey. The most detailed questionnaire in this respect is the one administered in 1994, and it is therefore the one used hereafter to assess coverage. The 1991 round is the most parsimonious, as it does not report whether the worker had a contract and does not provide information on the total personnel of the employer. The 1997 round, in turn, does not ask about unionization at the workplace. Because of the limitations of the 1991 and 1997 rounds, the cross-sectional analyses in this paper are all based on 1994 data. As regards the longitudinal analyses, the parsimony of the 1991 questionnaire provides a third reason to discard it, and focus on the 1994-97 panel. The analyses based on this panel do not take unionization into account.

Coverage can also be assessed, ex-post, for the unemployed. In principle, the survey questionnaire allows to construct coverage indicators and scores for the last job held by the respondent over the previous 12 months. This information could be used to infer whether those who were unemployed at the time of the survey did receive severance pay. But there are two

problems with this approach. First, the survey does not report information on whether job loss was voluntary or involuntary and, in the latter case, whether it was “unjustified” or not. (Even if it did, few respondents could be expected to declare that their employer was right to fire them.) Second, answering questions related to the last job involves a substantial recall effort by the respondent. Not surprisingly, information on the previous job is altogether missing for many among the unemployed. As a result, it is not possible to identify whom, among the unemployed, are first-time job seekers.

The approach used in this paper to infer coverage among the unemployed is based on information about other income sources of the household over the previous 12 months. The questionnaire inquires about a large number of income sources, such as interest payments, rent, alimonies, remittances, bequests and lottery prizes. It also asks about payments related to old-age pension or end-of service gratuity (lumped together) and to compensation (without further detail). Some households report receiving income from one of the last two sources once, and only once, over the previous 12 months. The paper assumes that a member of those households did get severance pay.

This severance pay indicator is potentially misleading in the early 1990s, when Peru embarked in a large public sector downsizing program, involving more than a quarter million job separations (Haltiwanger and Singh, 1999). As a result, the end-of-service gratuity or compensation reported by some households could bear no relation with the mandatory severance pay scheme for private sector workers. Since separation packages for public sector workers were based on a different formula, the estimates of the consumption impact of severance pay would be biased. The potential mix up between compensation packages for public sector workers and

severance pay for private sector workers provides a fourth and last reason to base the analysis on data from the 1994 and 1997 rounds of the LSMS survey only.

Other relevant variables for the analysis refer to labor earnings and consumption per capita. Labor earnings were calculated for the main occupation, including payments both in cash and in kind. The survey questionnaire is quite detailed concerning the latter. As regards the former, it refers explicitly to “net” payments, which means that contributions in the name of the worker to the social security administration, or towards the end-of-service gratuity, are in principle not counted as labor earnings. However, the extent to which interviewers actually stuck to the questionnaire, and deducted contributions from “gross” payments, is unclear. Data on consumption per capita were taken from other studies dealing with poverty in Peru. A detailed description of the methodology used in those other studies to calculate consumption based on expenditure data can be found in World Bank (1999). Both the earnings and the consumption figures are measured at June 1994 prices.

#### **4. Coverage and Beneficiaries**

Slightly more than one in five private sector workers is legally entitled to severance pay in Peru, as shown in table 3 (the exact figure in this table is 21.15 percent). This fraction is not as low as it may appear as a first glance, as many private sector workers are self-employed or work in household enterprises. The salaried relationship is not as common in a country at that development level as it is in industrial countries. In Peru, less than two thirds of private sector workers are wage earners. Many among them do not have the minimum seniority to be entitled to severance pay. Overall, roughly one in three private sector wage earners ( $= 21.15/62.60$ ) is legally entitled to severance pay.

Both the salaried relationship and entitlement to severance pay are more prevalent among relatively wealthier workers. Table 3 presents a breakdown of coverage indicators by consumption quintiles, based on the distribution of consumption per capita among all Peruvians (and not just among private sector workers). In this table, the share of private sector workers who are legally entitled to severance pay increases monotonically with the consumption level, from roughly 15 percent in the poorest quintile to almost 30 percent in the richest quintile. However, the share of wage earners who are entitled to severance pay does not depend so much on wealth. It is equal to 34 percent for both the poorest quintile ( $= 15.12/44.74$ ) and the richest quintile ( $= 29.05/76.55$ ). It follows that most of the variation in legal coverage across consumption quintiles is due to variation in the prevalence of the salaried relationship.

Legal coverage does not guarantee that severance will be paid in the event of separation. Table 3 shows that all of the attributes that make compliance more likely are highly correlated with wealth. For instance, the share of private sector workers who are legally entitled to severance and in addition are enrolled with social security varies from half of one percent in the poorest quintile to more than 16 percent in the richest quintile. The picture is similar when job scores are considered instead, as shown in table 4. Very few private sector workers get the job score “legal + 4”, even in the top quintiles. On the other hand, almost 10 percent of them get a score equal to “legal + 1” or higher. This represents slightly less than half of the private sector workers are legally entitled to severance pay. However, this ratio ranges from one tenth in the poorest quintile to almost two thirds in the richest quintile.

The fraction of the unemployed who actually received severance pay appears to be much lower than the fraction of private sector workers who are covered by it. The distribution of the unemployed by consumption quintile is displayed in table 5. The average unemployment rate in

the sample is around 7 percent. The unemployment rate is highest in the third and fourth quintiles and lowest in the poorest quintile. Receiving severance pay is also more common among the middle class and quite uncommon among the poor. Overall, few unemployed persons appear to have received severance pay. This result is partly due to two measurement problems. First, the unemployed include many first-time job seekers, who obviously are not entitled to severance pay. Unfortunately, it is not possible to identify them in the data, as information on previous activity is missing for too many respondents. Second, some (if not most) of the workers who got severance pay over the previous year might have found a job in the meantime, so that they are not considered unemployed at the time of the survey.

In spite of these measurement problems, it also appears that workers who are covered by severance pay are less likely to lose their jobs. Table 6 displays the changes in employment status for the respondents of the 1994 round who were surveyed again in 1997. The figures in this table do not allow to infer job market transitions, as the time span between two consecutive rounds is too long. Still, among the private sector workers who had a score of “legal + 2” or higher in 1994, only 1.4 percent (= 1/74) were unemployed in 1997. The share is substantially higher for all other groups, including those who were in the public sector. It is equal to 7.9 percent for those who were in the private sector but had the job score “legal + 1” or less, and to 10.2 percent for those who were unemployed or out of the labor force.

This crude comparison suggests that those effectively covered by mandatory severance pay enjoy a substantial job security. However, based on this crude comparison it is not possible to identify the direction of causality. Coverage by mandatory severance makes job separation more expensive, so that higher job security could be the result of the regulation. On the other hand, employers know this, and in many cases can decide whether to offer coverage to their

workers. For instance, at the time of the 1994 round, the key decision for employers was whether to extend the work relationship beyond 12 months. It is plausible that this threshold was crossed only for workers who were expected to stay with the enterprise within the foreseeable future, either because of their high productivity or because of their relationship with the employer. Coverage would thus be revealing a characteristic of the worker that is observable to the employer, but not to outsiders.

## **5. Impact on Earnings**

Workers who are covered by mandatory severance pay could earn less than other, similar workers if they “paid” for their higher job security through lower wages. Or they could earn more, if this higher job security allowed them to extract rents from their employers. The main problem in trying to assess whether they actually earn more or less is self-selection. The discussion in the previous section suggests that coverage could be associated with unobservable worker characteristics, such as talent, information or connections, in which case it would be difficult to disentangle the differences in earnings due to coverage from the differences due to these unobservable characteristics. In particular, the coefficients of an ordinary least-squares (OLS) regression linking earnings and coverage indicators would be biased. And it is difficult to find an appropriate instrument to apply Heckman selectivity-corrected estimators.

This paper uses two empirical strategies to address the self-selection problem. One of them is household fixed effects. The implicit identifying assumption in this approach is that the unobservable characteristics that underlie coverage are correlated across household members. The household fixed effects approach implicitly focuses on households that have at least two wage earners, and “removes” any systematic difference between their earnings and those of

individuals in other households. This approach can be implemented with cross-sectional data, without requiring two points in time. In this paper it is applied to data from the 1994 survey. The second empirical strategy relies on individual fixed effects. In this case, the implicit comparison is between the earnings of the same individual between two points in time. Table 6 showed that a large number of individuals moved from covered to uncovered jobs, and vice-versa, between 1994 and 1997. The individual fixed effects approach exploits these changes in coverage to assess whether they bear a systematic relationship with the observed changes in earnings. The approach is implemented using data from the 1994-1997 panel.

The first of these two approaches has some advantages over the second one. In particular, the household fixed effects approach can be applied to data from the 1994 round of the survey, which is the only one containing information on all five coverage indicators. By requiring two points in time, the individual fixed effects approach leads to the loss of the unionization indicator. Unionized workers are more likely to be entitled to severance pay. If trade union membership affected wages, the omission of the unionization indicator would bias the coefficients multiplying the other coverage indicators. Another advantage of the household fixed effects approach is that it allows to use a much larger number of observations, so that the estimates should be more precise. Finally, with the household fixed effects approach it is possible to use a larger set of controls, as members of the same household typically differ in their individual characteristics. Most individual characteristics are the same at two points in time, so that they are dropped in the individual fixed effects approach. Because of these advantages, household fixed effects is the preferred approach in this paper.

Earnings functions estimated using household fixed effects and individual fixed effects are presented in tables 8 and 9 respectively. An attempt was also made to estimate the

regressions in tables 8 and 9 using random effects, rather than fixed effects. Results are not reported, however, as these regressions failed to pass the Hausman test. For comparison purposes only, table 7 presents results obtained applying OLS to data from the 1994 survey. In this case, it is not necessary to restrict the sample to households with at least two income earners. As a result, the number of observations is substantially larger than in table 8. Despite this difference in sample size, the gap between the coefficients multiplying the coverage indicators in tables 7 and 8 can be interpreted as resulting from self-selection bias.

The explained variable in tables 7 to 9 is the log of labor earnings, with earnings measured both on an hourly basis and on a monthly basis. The monthly earnings variable does not take into account the number of hours worked. The latter variable is not included among the regressors, however, as it is clearly endogenous. The first two columns in tables 7 to 9 refer to private sector workers only. The last two columns replicate the analysis for a broader sample, including workers who have a job in the public sector. A dummy variable is included among the explanatory variables in these two columns to account for the possible earnings gap between the public and the private sector. The most relevant explained variable, when trying to assess whether workers “pay” for their severance pay coverage, is hourly earnings in the private sector. Therefore, from the point of view of this paper, the first column in tables 7 to 9 is the preferred one. The results in the other columns are mainly used as a check.

Other explanatory variables in tables 7 and 8 are standard in the analysis of labor earnings. They control for gender, schooling, experience, position in the household and region of residence. The regressions also take the cultural background of the respondent into account. In a country with a large indigenous population, mainly speaking *Quechua*, fluency in the official language (Spanish) cannot be taken for granted. The inclusion of a dummy variable for

Spanish as the mother tongue substantially reduces the estimated returns to schooling, and blurs the comparison with other studies on this issue. However, the focus of this paper is on the impact of severance pay coverage, not on returns to schooling.

These other explanatory variables are not included in the regressions in table 9, as most of them should be invariant over time. For instance, some workers could display more years of education in 1997 than in 1994, but this change is most likely to reflect measurement error than additional schooling. The only characteristic that does change for a large number of individuals is work experience. Unfortunately, work experience is measured in the conventional way, as the number of years of age minus the number of years of education minus six. As a result, most workers have three more years of experience in 1997 than they had in 1994, and it becomes difficult to disentangle the effects of those additional three years from the effects of other changes in the general economic context. The combined effects are captured by the coefficient on the year variable in table 9.

The results obtained when using ordinary least squares, in table 7, suggest that workers who are legally entitled to severance pay and, in addition, have a written contract, earn substantially more than other workers. If these results were not tainted by self-selection, they would imply that coverage allows workers to extract rents from their employers. This is perfectly plausible, as are also other results in this table. For instance, the coefficient on the unionization indicator is large and statistically significant when all the employed are considered, which is consistent with the idea that trade unions raise the earnings of their members, especially in the public sector. And the coefficient on large firms is positive in all specifications, and significantly so in two of them, which is consistent with previous evidence on the effect of firm

size on earnings (see Schaffner, 1998). However, all of these effects vanish when unobservable differences among workers are taken into account.

Based on the household fixed effects approach, which is the one preferred in this paper, private sector workers who are legally entitled to severance pay earn roughly 12 percent less per hour than workers who are not (see the first column in table 8). On the other hand, they also tend to work longer hours, so that their monthly earnings are substantially higher than those of workers who are not legally entitled to severance pay (second column). Coefficients are similar when all the employed are considered, but the downward effect on hourly earnings becomes statistically insignificant. The impact of legal coverage on earnings is not affected by any of the effective coverage indicators, as shown by the lack of significance of all other coefficients in the first panel of this table. The results in the second panel suggest that coverage indicators do not have a separate impact on earnings either.

The picture that emerges from the individual fixed effects estimates in table 9 is not too different, but the fit of the regressions is poor. Taken literally, legal entitlement to severance pay has no significant impact on hourly earnings, but is associated with monthly earnings which are 15 to 20 percent higher. However, the hypothesis that all the coefficients in table 9 are equal to zero cannot be rejected at the 5 percent significance level. This results from the high noise-to-signal ratio characterizing individual fixed effects regressions of this sort.

## **6. Impact on Consumption**

While coverage by mandatory severance pay regulations concerns individual workers, consumption is determined at the household level. Therefore, an empirical analysis of the impact of severance pay on individual consumption has to take into account household characteristics,

and not just individual characteristics. Two of the most relevant household characteristics are its size and age composition. Other things equal, individual consumption can be expected to be lower when there are more mouths to feed and when there are fewer potential income earners. A third relevant characteristic is the households' ability to smooth consumption on its own. In the LSMS survey this ability can be captured through the household's access to credit (as revealed by the possession of at least one credit card), or through its possession of a bank account in the 12 months preceding the survey.

The individual characteristics to consider are also slightly different from those used in the earnings analysis. Severance pay regulations can affect the consumption level of individuals who are at work. This is because coverage has a potential impact on the labor earnings of those individuals, and also on the savings decisions of the households they belong to. But severance pay regulations should also affect the consumption level of individuals who are unemployed, as the consumption smoothing ability of the households they belong to probably depends on whether they actually got severance pay. Consequently, the sample has to include both the employed and the unemployed, and the specification has to consider appropriate severance pay indicators for each group. The regressions presented in this section include among their right-hand-side variables both the coverage indicators used in the earnings analysis and the dummy variable for actual severance pay used in the coverage analysis.

An OLS regression linking individual consumption to these coverage and severance pay indicators would suffer from the same self-selection problems as the earnings functions considered in the previous section. Unfortunately, one of the two approaches used in that section to correct the self-selection problem cannot be applied in this case. The households fixed effects approach focuses on the difference in outcomes between individuals belonging to the same

household. By construction, however, consumption per capita is the same for all adult members of the same household. In the absence of variation in the explained variable, the households fixed effects approach cannot be implemented. Results are thus presented only for OLS regressions (in table 10) and the individual fixed effects approach (in table 11).

The number of observations in these two tables is larger than in the corresponding earnings tables (7 and 9 respectively). This is because information on earnings is missing for a considerable number of workers. The regressions in table 10 refer to all labor force participants in 1994. Those in table 11 refer to all individuals who participated in the labor force in both 1994 and 1997. In both tables, the label “job characteristics” is used to identify the set of dummy variables indicating whether the individual has a contract, is enrolled with social security, is a member of a trade union or works for a large firm. The specifications used in table 11 omit all the individual and household characteristics that should be relatively stable over time, so as to reduce the noise in the data.

The impact of household characteristics on consumption, reported in the third panel of table 10, is relatively uncontroversial. At the sample mean, adding a person of working age to the household reduces consumption per capita by roughly 6 percent. The drop in consumption is more than twice as large if this person is less than 15 or more than 64 years old. On the other hand, having access to credit or previous savings substantially boosts consumption. In this case, the estimates in table 10, when using ordinary least squares, are almost four times larger than those in table 11, when using individual fixed effects. This is an indication of self-selection. It suggests that households which are relatively more productive (in unobservable ways) also tend to have better possibilities to smooth their consumption.

From the point of view of this paper, the most interesting results are those concerning the impact of severance pay on consumption. Based on those results, it is difficult to claim that coverage by severance pay has a significant impact on consumption among those who have a job. When using fixed effects, all the coefficients multiplying the coverage indicators are statistically insignificant. At a first glance, the absence of any impact on consumption seems to contradict the negative impact on hourly earnings, revealed by table 9. These two outcomes are not inconsistent, however, if the additional income security resulting from coverage entices households to reduce their precautionary savings.

Conversely, actually receiving severance pay has a substantial impact on consumption. Those who are unemployed and received no severance pay consume, on average, 15 percent less than those who are employed. The actual impact could actually be larger, as the estimated coefficient refers to all the unemployed, including both workers who lost their jobs and first-time job seekers, and the latter should not experience any drop in consumption. An actual impact in excess of 15 percent is in line with results obtained elsewhere. For the US, Gruber (1997) estimates that job loss would entail a 20 percent drop in food consumption if the replacement rate of the unemployment insurance system were set equal to zero

On the other hand, those who are unemployed and received severance pay consume substantially more percent more than those who did not. In table 10, the coefficient multiplying the severance pay variable is very similar to the coefficient multiplying the unemployment variable, suggesting that the consumption impact of job loss is totally offset. However, the results in this table may suffer from self-selection bias, as revealed by the gap with the coefficients obtained using the individual fixed effects approach. According to table 11, those

who received severance pay consume roughly 25 percent more than those who did not. It follows that they consume about 10 percent more than those who are employed.

Finally, the regressions in tables 10 and 11 interact the unemployment and the severance pay variables with access to credit. The impact of unemployment on consumption could be expected to be smaller for households who can smooth consumption on their own, and the impact of severance pay should be smaller as well. However, the coefficients multiplying the interactive terms in tables 10 and 12 do not conform to the expected pattern. Access to credit does not appear to mitigate the consumption loss of the unemployed. And it seems to boost the consumption level of those who get severance pay, as if the latter was perceived as a windfall by its beneficiaries.

## **7. Conclusion**

Mandatory severance pay can be found in many (if not most) developing countries. If the case of Peru serves as an indication, it is highly popular among vocal segments of the urban population. Of course, popularity should not be confused with soundness. But there are some a priori reasons to believe that the benefits of mandatory severance pay could exceed its costs, especially in countries with low institutional capacity. The only direct burden on the government's budget is related to the handling of non-compliance complaints; there are no incentives for separated workers to stay out of a job; and those who are at work could "pay" for their coverage through lower wages. On the other hand, mandatory severance pay could make firing more costly to employers, and therefore discourage hiring.

This paper is, to our knowledge, the first to provide a thorough assessment of mandatory severance pay in a developing country. The paper shows that one in five private sector workers,

and one in three private sector wage earners, is legally entitled to severance pay in the event of unjustified dismissal. This figure is higher than it may appear at a first glance, as wage earners include rural laborers and informal sector workers. However, the coverage rate is twice as large in the richest population quintile than in the poorest one. This gap is amplified when effective coverage indicators are considered. The jobs of only half of the workers who are legally covered have any of the features that make compliance more likely. These features include: having a written contract, being enrolled with social security, working in a unionized enterprise, and working for a large enterprise. Less than five percent of private sector workers in the poorest two quintiles of the population have jobs with any of these features, compared to twenty percent among the richest quintile. Finally, the results of the paper also suggest that workers who are entitled to severance pay are less likely to become unemployed, but it is unfortunately not possible to disentangle cause and effect.

The paper also provides some evidence that workers do “pay” for their coverage through lower hourly wages, but the evidence is not conclusive in this respect. The main problem is the endogeneity of coverage: those who are entitled to severance pay may also be “better” workers hence have the potential to earn more. The paper uses two different empirical strategies to “remove” the unobservable worker heterogeneity. One of them fails, as it introduces too much noise in the data, thus leading to unreliable estimates. The results obtained with the other one imply that workers who are legally entitled to severance pay earn roughly 10 percent less per hour than those who are not, but the statistical significance of the estimated coefficients is not too high. On the other hand, covered workers also work more hours, so that their monthly earnings are higher.

Finally, some of the most interesting results in this paper refer to the ability of mandatory severance pay to smooth consumption. If uncovered workers saved more, or borrowed from

friends and family, to confront the effects of job loss, mandatory severance pay would only be displacing these other self-insurance mechanisms. However, it appears that individuals who are unemployed and did not receive severance pay consume roughly 15 percent less than those who are at work. Those who are unemployed and did receive severance pay, on the other hand, consume about 10 percent more. The first of these two results imply that mandatory severance pay is an effective consumption smoothing instrument. The second result reveals that the severance pay formula used in Peru is unnecessarily generous.

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Table 1

## Severance Pay in the Event of “Unjustified” Dismissal

Date	Regulation	Severance pay formula (W = monthly wage and T = years of service)	Household survey
June 1986	L. 24514	$0$ if $T < 0.4$ $3 \times W$ if $0.4 = T < 1$ $6 \times W$ if $1 = T < 3$ $12 \times W$ if $3 = T$	
October 1991			Yes
November 1991	D.L. 728	$0$ if $T < 1$ $3 \times W$ if $1 = T < 3$ $T \times W$ if $3 = T < 12$ $12 \times W$ if $12 = T$	
April 1994			Yes
July 1995	L. 26513	$T \times W$ if $T < 12$ $12$ if $12 = T$	
November 1996	D.L. 855	$1.5 \times T \times W$ if $T < 8$ $12$ if $8 = T$	
September 1997			Yes

Notes: Since 1963, all workers are entitled to an end-of-service gratuity, regardless of the reasons for job termination. The gratuity, which is paid in addition to severance pay, amounts to half a month of salary per year of service. The date reported for the household surveys corresponds to the first month of the field work. Interviews usually took place within a quarter. L. stands for ley and D.L. for decreto legislativo.

Source: Constructed by the authors based on Saavedra (1999).

Table 2

**Indicators of Severance Pay Coverage  
In each of the Household Surveys**

Indicator	Household survey		
	1991	1994	1997
Legal	Wage earner in private sector Tenure in job $\geq$ 3 months	Wage earner in private sector Tenure in job $\geq$ 12 months	Wage earner in private sector Any tenure
Contract	Not available	Has a signed contract	Has an open-ended contract
Social security	Worker is enrolled with IPSS or military pension plan	Worker is enrolled with IPSS, police or military pension plan	Worker is enrolled with IPSS, police or military pension plan
Trade union	Firm is unionized	Firm is unionized	Not available
Large firm	Not available	Firm's employment $\geq$ 21	Firm's employment $\geq$ 21
Maximum score	Legal + 2	Legal + 4	Legal + 3

Notes: These indicators are set equal to one when the corresponding criterion is met, and equal to zero otherwise. They are necessarily equal to zero for self-employed and unpaid family workers, as well as for public sector workers. The maximum score is the highest possible figure for the sum of effective coverage indicators in each of the surveys. IPSS stands for Instituto Peruano de Seguridad Social.

Source: Constructed by the authors based on table 1 and LSMS questionnaires.

Table 3

**Coverage Indicators among Private Sector Workers  
By Consumption Quintile, 1994 Survey**

Coverage indicator in current job		Consumption quintile (based on entire population)					
		Poorest	2 <sup>nd</sup>	3 <sup>rd</sup>	4 <sup>th</sup>	Richest	All
Legal	No	84.88	83.99	80.24	73.49	70.95	78.85
	Yes	15.12	16.01	19.76	26.51	29.05	21.15
Legal + Contract	No	99.50	99.39	96.98	94.08	89.86	96.04
	Yes	0.50	0.61	3.02	5.92	10.14	3.96
Legal + Social security	No	99.44	97.41	94.59	89.42	83.85	93.08
	Yes	0.56	2.59	5.41	10.58	16.15	6.92
Legal + Trade union	No	99.75	99.11	98.71	97.43	95.61	98.16
	Yes	0.25	0.89	1.29	2.57	4.39	1.84
Legal + Large firm	No	99.25	98.43	95.43	92.21	88.58	94.87
	Yes	0.75	1.57	4.57	7.79	11.42	5.13
Private sector workers		21.30	19.45	20.59	19.04	19.61	100.00
Wage earners	No	55.26	42.85	36.23	27.49	23.45	37.40
	Yes	44.74	57.15	63.77	72.51	76.55	62.60

Notes: Refers to the LSMS sample, without applying factors of expansion. Private sector workers include wages earners, but also the self-employed and unpaid family workers. For a definition of the effective coverage indicators, see table 2.

Source: Authors' calculations based on LSMS data.

Table 4

Coverage Score among Private Sector Workers  
By Consumption Quintile, 1994 Survey

Job score	Severance pay coverage based on current job's score	Consumption quintile (based on entire population)					
		Poorest	2 <sup>nd</sup>	3 <sup>rd</sup>	4 <sup>th</sup>	Richest	All
Legal	No	84.88	83.99	80.24	73.49	70.95	78.85
	Yes	15.12	16.01	19.76	26.51	29.05	21.15
Legal + 1	Below score	98.44	95.84	92.02	85.80	80.07	90.66
	Equal or higher	1.56	4.16	7.98	14.20	19.93	9.34
Legal + 2	Below score	99.56	98.84	95.95	92.00	86.89	94.78
	Equal or higher	0.44	1.16	4.05	8.00	13.11	5.22
Legal + 3	Below score	99.94	99.66	98.20	96.31	93.18	97.52
	Equal or higher	0.06	0.34	1.80	3.69	6.82	2.48
Legal + 4	Below score	100.00	100.00	99.55	99.03	97.77	99.28
	Equal or higher	0.00	0.00	0.45	0.97	2.23	0.72

Notes: Refers to the LSMS sample, without applying factors of expansion. Private sector workers include wages earners, but also the self-employed and unpaid family workers. The score is the sum of the severance pay coverage indicators. For a definition of these indicators, see table 2.

Source: Authors' calculations, based on LSMS data.

Table 5

**Unemployed and Severance Pay Beneficiaries  
By Consumption Quintile, 1994 Survey**

Severance pay indicators (refer to last job)		Consumption quintile (based on entire population)					
		Poorest	2 <sup>nd</sup>	3 <sup>rd</sup>	4 <sup>th</sup>	Richest	All
Got severance (unemployed only)	No	98.67	97.89	96.18	95.04	95.45	96.38
	Yes	1.33	2.11	3.82	4.96	4.55	3.62
All the unemployed		13.59	17.21	23.73	25.54	19.93	100.0
All labor force participants		20.90	19.28	20.76	19.42	19.64	100.0
Unemployment rate		5.61	6.87	8.25	8.82	6.34	7.22

Notes: Refers to the LSMS sample, without applying factors of expansion. To be counted as unemployed a person had to be actively searching a job in the 7 days preceding the survey, or be a discouraged job seeker. First-time job seekers are included among the unemployed. When the household of the unemployed person received one, and only one, indemnity or social payment in the 12 months preceding the survey, it is assumed that the unemployed member got severance pay.

Source: Authors' calculations based on LSMS data.

Table 6

## Changes in Employment Status, 1994-1997 Panel

Employment status in 1994		Employment status in 1997						All in 1994	
		Public sector job	Private sector job				Unemployed		Out of the labor force
			Higher Score	Legal + 1	Legal Only	Not Covered			
Public sector job		76	5	6	8	18	3	7	123
Private sector job	Higher score	9	33	7	10	10	1	4	74
	Legal + 1	3	14	9	11	15	5	6	63
	Legal only	3	4	4	54	69	5	6	145
	Not covered	17	21	23	101	558	38	84	842
Unemployed		3	5	7	26	28	11	21	101
Out of the labor force		26	10	13	61	169	64	291	634
All in 1997		137	77	69	271	867	127	419	1982

Notes: Refers to the LSMS sample, without applying factors of expansion. Private sector workers include wages earners, but also the self-employed and unpaid family workers. Excludes persons below 15 or above 64 years of age.

Source: Authors' calculations based on LSMS data.

Table 7

**Labor Earnings by Coverage Indicator**  
**OLS Estimates, 1994 Survey**

Explanatory variables	Dependent variable: Log of earnings, in nuevos soles at June 1994 prices			
	Private sector jobs		All the employed	
	Hourly earnings	Monthly earnings	Hourly earnings	Monthly earnings
Legal (yes = 1)	-0.0482 (-1.439)	0.0266 (0.789)	-0.0420 (-1.274)	0.0303 (0.915)
Legal + Contract (yes = 1)	0.2142 ** (2.100)	0.0897 (0.918)	0.1297 ** (2.037)	0.0109 (0.181)
Legal + Social security (yes = 1)	-0.0172 (-0.229)	-0.0197 (-0.249)	-0.0213 (-0.359)	-0.0420 (-0.692)
Legal + Trade union (yes = 1)	0.1437 (0.913)	0.2035 (1.225)	0.0242 (0.333)	0.1340 * (1.804)
Legal + Large firm (yes = 1)	0.0980 (1.105)	0.0305 (0.364)	0.1429 ** (2.370)	0.1237 ** (2.095)
Contract (yes = 1)	-0.0786 (-0.781)	-0.0064 (-0.074)	-0.0003 (-0.007)	0.0681 * (1.693)
Social security (yes = 1)	0.1815 *** (2.818)	0.2504 *** (3.600)	0.1907 *** (4.259)	0.2826 *** (5.957)
Trade union (yes = 1)	0.0123 (0.084)	-0.0395 (-0.256)	0.1347 *** (3.126)	0.0343 (0.811)
Large firm (yes = 1)	0.0661 (0.861)	0.1601 ** (2.263)	0.0309 (0.754)	0.0779 ** (1.997)
Public sector job (yes = 1)			0.0251 (0.541)	-0.1214 ** (-2.495)
Individual characteristics	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.214	0.292	0.237	0.299
F test	81.62	117.60	105.62	126.14
Number of observations	4493	4495	5158	5160

Notes: Estimated by ordinary least squares, using White heteroskedasticity-corrected errors. Figures in parentheses are t-statistics. Significant coefficients at the 10, 5 and 1 percent level are indicated by one, two and three asterisks respectively. Excludes those who work less than 35 hours per week.

Source: Authors' calculations based on LSMS data.

Table 8

**Labor Earnings by Coverage Indicator**  
**Household Fixed Effects, 1994 Survey**

Explanatory variables	Dependent variable: Log of earnings, in nuevos soles at June 1994 prices			
	Private sector jobs		All the employed	
	Hourly earnings	Monthly earnings	Hourly earnings	Monthly earnings
Legal (yes = 1)	-0.1150 *	0.1202 **	-0.0730	0.1401 ***
	(-1.815)	(2.168)	(-1.244)	(2.703)
Legal + Contract (yes = 1)	-0.0030	-0.0128	-0.1195	-0.0967
	(-0.017)	(-0.070)	(-0.991)	(-0.774)
Legal + Social security (yes = 1)	-0.0603	-0.0038	0.0230	-0.0897
	(-0.439)	(-0.027)	(0.217)	(-0.835)
Legal + Trade union (yes = 1)	-0.0975	-0.1082	0.1295	0.0877
	(-0.323)	(-0.363)	(0.837)	(0.539)
Legal + Large firm (yes = 1)	0.1025	-0.1101	0.1850	0.0999
	(0.661)	(-0.733)	(1.643)	(0.879)
Contract (yes = 1)	-0.0869	0.0667	0.0031	0.1099
	(-0.553)	(0.425)	(0.037)	(1.255)
Social security (yes = 1)	0.1225	0.1047	0.0805	0.2303 ***
	(1.080)	(0.915)	(1.074)	(2.951)
Trade union (yes = 1)	0.3397	0.3485	0.1364	0.1685 *
	(1.243)	(1.300)	(1.503)	(1.784)
Large firm (yes = 1)	-0.0243	0.1728	-0.1287 *	-0.0182
	(-0.194)	(1.465)	(-1.745)	(-0.249)
Public sector job (yes = 1)			0.0149	0.0264
			(0.166)	(0.303)
Individual characteristics	Yes	Yes	Yes	Yes
Regional dummies	No	No	No	No
Overall R <sup>2</sup>	0.098	0.150	0.114	0.182
F test	5.07	27.29	5.68	32.26
Number of observations	1545	2773	2165	3464

Notes: Fixed-effect estimates, with t-statistics reported in parentheses. Only households with two wage earners or more are included. Significant coefficients at the 10, 5 and 1 percent level are indicated by one, two and three asterisks respectively. Excludes those who work less than 35 hours per week.

Source: Authors' calculations based on LSMS data.

Table 9

**Labor Earnings by Coverage Indicator**  
**Individual Fixed Effects, 1994-1997 Panel**

Explanatory variables	Dependent variable: Log of earnings, in nuevos soles at June 1994 prices			
	Private sector jobs		All the employed	
	Hourly earnings	Monthly earnings	Hourly earnings	Monthly earnings
Legal (yes = 1)	0.0631 (0.619)	0.1639* (1.698)	0.0806 (0.855)	0.1929 ** (2.183)
Legal + Contract (yes = 1)	0.1278 (0.269)	0.1141 (0.253)	-0.0141 (-0.068)	-0.0255 (-0.130)
Legal + Social security (yes = 1)	-0.2730 (-0.930)	-0.1681 (-0.605)	-0.1574 (-0.702)	-0.2125 (-1.011)
Legal + Large firm (yes = 1)	0.7015 ** (2.216)	0.4420 (1.474)	0.3502 (1.613)	0.2224 (1.093)
Contract (yes = 1)	0.0350 (0.078)	-0.0292 (-0.069)	0.1447 (0.972)	0.0674 (0.483)
Social security (yes = 1)	0.2310 (0.970)	0.2108 (0.934)	0.1197 (0.698)	0.2223 (1.384)
Large firm (yes = 1)	-0.3909 (-1.408)	-0.1780 (-0.677)	-0.0860 (-0.523)	0.0311 (0.202)
Public sector job (yes = 1)			0.1377 (0.707)	0.0242 (0.133)
Individual characteristics	No	No	No	No
Regional dummies	No	No	No	No
Year 1997 (yes = 1)	-0.1124 ** (-2.330)	-0.0833 * (-1.822)	-0.0647 (-1.526)	-0.0261* (-0.656)
Overall R <sup>2</sup>	0.059	0.076	0.087	0.097
F test	1.89	1.84	1.12	1.47
Number of observations	1364	1364	1628	1628

Notes: Fixed-effect estimates, with t-statistics reported in parentheses. There are two observations per person. Significant coefficients at the 10, 5 and 1 percent level are indicated by one, two and three asterisks respectively. Excludes those who work less than 35 hours per week, and those whose change in log earnings between 1994 and 1997 was lower than -0.5 or higher than 0.5.

Source: Authors' calculations based on LSMS data.

Table 10

Consumption per Capita by Employment Status  
OLS Estimates, 1994 Survey

Explanatory variables	Dependent variable: Log of consumption, in nuevos soles at June 1994 prices			
	Unemployed (yes = 1)	-0.1343 *** (-5.613)	-0.1469 *** (-5.525)	-0.1352 *** (-5.660)
Unemployed x Savings or access to credit		0.0517 (0.927)		0.0432 (0.776)
Got severance (yes = 1)	0.1407 *** (3.268)	0.1390 *** (3.236)	0.0574 (1.172)	0.0581 (1.186)
Got severance x Savings or access to credit			0.2239 ** (2.463)	0.2183 ** (2.402)
Legal (yes = 1)	-0.0958 *** (-5.125)	-0.0962 *** (-5.142)	-0.0958 *** (-5.121)	-0.0960 *** (-5.135)
Legal + Contract (yes = 1)	0.0849 * (1.808)	0.0853 * (1.817)	0.0843 * (1.795)	0.0847 * (1.803)
Legal + Social security (yes = 1)	0.0634 (1.531)	0.0633 (1.528)	0.0624 (1.506)	0.0624 (1.504)
Legal + Trade union (yes = 1)	0.0954 (1.563)	0.0954 (1.563)	0.0951 (1.557)	0.0951 (1.557)
Legal + Large firm (yes = 1)	0.0408 (0.966)	0.0406 (0.962)	0.0419 (0.992)	0.0417 (0.988)
Household size (all persons)	-0.0574 *** (-15.936)	-0.0574 *** (-15.930)	-0.0574 *** (-15.916)	-0.0574 *** (-15.912)
Dependents (below 15 and over 64)	-0.0833 *** (-16.795)	-0.0834 *** (-16.809)	-0.0833 *** (-16.790)	-0.0833 *** (-16.802)
Savings in bank or access to credit (yes = 1)	0.2581 *** (17.824)	0.2546 *** (17.009)	0.2538 *** (17.351)	0.2510 *** (16.634)
Job characteristics	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes
R <sup>2</sup>	0.497	0.497	0.497	0.497
F test	311.92	298.73	299.77	287.62
Number of observations	7796	7796	7796	7796

Notes: Estimated by ordinary least squares, using White heteroskedasticity-corrected errors. Figures in parentheses are t-statistics. Significant coefficients at the 10, 5 and 1 percent level are indicated by one, two and three asterisks respectively.

Source: Authors' calculations based on LSMS data.

Table 11

**Consumption per Capita by Employment Status**  
**Individual Fixed Effects, 1994-1997 Panel**

Explanatory variables	Dependent variable: Log of consumption, in nuevos soles at June 1994 prices			
	Unemployed (yes = 1)	-0.1566 *** (-2.803)	-0.1545 ** (-2.285)	-0.1549 *** (-2.778)
Unemployed x Savings or access to credit		-0.0060 (-0.056)		-0.0273 (-0.255)
Got severance (yes = 1)	0.2448 *** (3.215)	0.2452 *** (3.205)	0.0821 (0.844)	0.0828 (0.850)
Got severance x Savings or access to credit			0.4245 *** (2.677)	0.4275 *** (2.687)
Legal (yes = 1)	-0.0130 (-0.332)	-0.0130 (-0.334)	-0.0210 (-0.538)	-0.0214 (-0.547)
Legal + Contract (yes = 1)	0.0686 (0.678)	0.0685 (0.677)	0.0818 (0.809)	0.0814 (0.805)
Legal + Social security (yes = 1)	-0.0499 (-0.485)	-0.0499 (-0.485)	-0.0559 (-0.544)	-0.0558 (-0.543)
Legal + Large firm (yes = 1)	0.1384 (1.440)	0.1385 (1.440)	0.1327 (1.384)	0.1330 (1.387)
Savings in bank or access to credit (yes = 1)	0.0771 *** (2.940)	0.0775 *** (2.861)	0.0650 ** (2.447)	0.0666 ** (2.438)
Year 1997 (yes = 1)	0.0682 *** (3.892)	0.0682 *** (3.891)	0.0686 *** (3.923)	0.0686 *** (3.922)
Job characteristics	Yes	Yes	Yes	Yes
Individual characteristics	No	No	No	No
Regional dummies	No	No	No	No
Overall R <sup>2</sup>	0.039	0.039	0.037	0.037
F test	5.16	4.76	5.33	4.95
Number of observations	2726	2726	2726	2726

Notes: Fixed-effect estimates, with t-statistics reported in parentheses. There are two observations per person. Significant coefficients at the 10, 5 and 1 percent level are indicated by one, two and three asterisks respectively.

Source: Authors' calculations based on LSMS data.