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ABSTRACT

Do Unemployment Benefits Promote or Hinder Structural Change?*

According to recent and largely untested theories, unemployment benefits (UBs) could improve the extent and quality of job reallocation even at the cost of increasing unemployment. Using yearly panel data from a large number of countries, we evaluate empirically the relationship between unemployment benefits and structural change. Unlike previous work assessing the effects of UBs on labor market stocks, we focus on flows and rely on policy "experiments", notably the introduction from scratch of unemployment benefits in many countries. We exploit the longitudinal nature of our data to lessen the potentially important selection, endogeneity and omitted variables problems. We find a positive, sizable and significant effect of the introduction of UBs on job reallocation, arising mainly from the job destruction margin although this effect fades away over time. UBs are also found to induce more sectoral shifts from agriculture to services. These findings appear to be robust to changes in the countries in the sample, control variables or estimation methods. We discuss to which extent our results are consistent with equilibrium matching models with or without endogenous sorting of workers into jobs providing entitlement to UBs and stochastic job matching.

JEL Classification: J6, J65, O15

Keywords: unemployment benefits, job reallocation, matching models

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1 Introduction

Empirical studies on the effects of unemployment benefits (UBs) typically concentrate on the microeconomic insurance vs. incentives trade-off. Economic theory predicts that the receipt of unemployment benefits negatively affects job search intensity and increases the reservation wages of jobseekers and a large body of applied studies supports the standard prediction that longer durations of unemployment benefits increase the duration of unemployment. This empirical research also points to the importance of specific design features of unemployment benefits, related to eligibility and entitlement criteria, in addition to the level of the benefits. Kiefer (1988), Atkinson and Micklewright (1991), Krueger and Meyer (2002), as well as Meyer (2005) provide excellent surveys of this rich and insightful literature.

Much less attention has been devoted to date by applied economists to investigating the macroeconomic, reallocation effects, of unemployment benefits. This is a serious shortcoming as a number of recent theoretical contributions point to major effects of UBs on job reallocation and labor productivity. General equilibrium models of the labor market a la Mortensen and Pissarides (Mortensen and Pissarides, 1994), stochastic job matching models (Acemoglu and Shimer, 1999 and 2000; Marimon and Zilibotti, 1999) suggest that UBs act on both job creation and job destruction margins, as well as on the quality of job matches, and hence on average productivity. More importantly still, these models have different implications as to the effects of UBs on job creation and destruction, which could be possibly tested empirically.

Research related to the study of the transition to a market of economies coming from central planning also contributed to highlight other potentially important effects of unemployment insurance, which had been previously overlooked. As pointed out by Aghion and Blanchard (1994), there is a negative fiscal externality on private job creation associated with the financing of UBs, which may counteract the moderating effects of unemployment on wages, reducing job creation in the high productivity sector, hence the speed of job reallocation. Contrary to popular wisdom, formerly planned economies entered the transition with a workforce specialized in very narrowly defined skills because central planning over-invested in vocational schools (Flanagan, 1993; Boeri, 2000). When such "skill specificities" are an important source of rents, UBs improve the quality of job matches by encouraging workers to seek for jobs that are harder to get. Matches are, on average, more productive when unemployment benefits have a longer duration in this setup. Other work on formerly planned economies looked at the interactions of unemployment benefits and wage-setting institutions. Under realistic wage setting mechanisms, more generous UBs strengthen the position of workers at the bargaining table as they improve their outside option. Even in the absence of minimum wages or with low and poorly enforced statutory minimum wages (as in most transitional

economies), flat rate subsidies offered to the unemployed proved to act as a wage floor, "pricing out" of the market the least productive jobs. This role of UBs as wage floors may explain asymmetries in labor market adjustment trajectories of Central and Eastern European countries vis-à-vis former Soviet Republics (Boeri, 2000): more employment adjustment in the former group of countries where UBs were relatively generous and more wage adjustment in Russia, where unemployment remained for long time surprisingly low in spite of dramatic falls in output.

An important macroeconomic reallocation role is assigned to UBs also by political economy models. Those addressing the constraints faced by privatization, for instance, pointed to an additional role of UBs in winning support of workers to outsider privatization and enterprise restructuring (Dewatripont and Roland, 1994; Blanchard, 1997). Models of political-economic institutional interactions in the labor market (Saint-Paul, 2000) suggest that unemployment benefits reduce the demand for employment protection legislation (EPL) (Boeri, Conde-Ruiz and Galasso, 2006; Algan and Cahuc, 2007) as both institutions protect workers against uninsurable labor market risk. "Flexicurity" configurations with more UBs have less EPL, which hinders job reallocations: UBs are more "mobility friendly" (Bertola and Boeri, 2002), and can better accommodate large scale restructuring (Blanchard and Tirole, 2003).

A common thrust of these different strands of literature is that UBs, in addition to influencing the aggregate level of unemployment, significantly affect the scope of job reallocation. Actually, some variants of these models do not yield clear-cut predictions about the effects of UBs of unemployment stocks, while they do have unambiguous predictions as to the effects of UBs on job creation and destruction rates. Moreover, they suggest that there is a slow adjustment of unemployment *stocks* to the introduction of UBs, while the effects on *flows*, notably on job destruction, occur immediately.

To the best of our knowledge, no empirical work has been done to date to test this reallocation role of unemployment benefits on a multicountry and multiperiod basis. Applied macro studies generally estimate the responsiveness of aggregate unemployment to UBs (Scarpetta, 1996; Nickell, 1997; Blanchard and Wolfers, 2000; Layard, Nickell and Jackman, 2005), while neglecting its effects on job reallocation.

The purpose of this paper is to contribute to filling this gap, by using institutional and labor market data from a large number of countries around the world for the period 1980-2002. As pointed out by the microeconometric literature, UBs are multidimensional institutions. This makes it difficult to properly measure UBs generosity in a multicountry-multiperiod setting. In order to cope with this problem, we exploit the fact that several countries introduced unemployment benefits from scratch between the end of the 1980s and the beginning of the 1990s. This empirical strategy isolates reforms that unambiguously made the UB system more generous than in the past. Our

outcome variables are meant to test the predictions of this new body of theory on labor market flows: job creation, job destruction, job turnover and the shares of workers in agriculture (proxying low-productivity jobs), industry and services.

As we focus on dichotomic policy choices and mainly on within-country variation, we can proceed without having to rely on the standard one-dimensional measures of UB generosity, and we do not need to worry about two-tier UB systems. We also concentrate on labor market flows, which, unlike aggregate stocks, are always sensitive to institutional reforms, even in the short-run. Our empirical strategy takes advantage of the longitudinal nature of our dataset to lessen potentially important selection, endogeneity and omitted variables problems. In our regression models, we include country fixed effects as well as country-specific time trends. The inclusion of country fixed effects ensures that we are controlling for omitted time-invariant variables as well as for selection into adopting unemployment benefits based on the level of job turnover. The model including country fixed effects and country-specific time trends is a version of the "random growth" model used in Ashenfelter and Card (1985), Heckman and Hotz (1989) and more recently in Brown, Earle and Telegdy (2006). This specification allows us to control for potentially different trends in rates of job creation and destruction experienced by particular groups of countries (notably formerly planned economies) in the period considered, which could have been affecting a country's propensity to introduce unemployment benefits.

Our analysis indicates that the introduction of UBs is associated with higher rates of job turnover. This effect is economically substantial, statistically significant and robust to changes in countries in the sample, control variables or estimation methods. The introduction of unemployment benefits is associated with about a 1.5 percentage point increase in the yearly rate of job destruction and a 2.7 percentage points increase in job turnover. This implies a positive effect on job creation as well, but this effect was not found to be statistically significant when estimated separately. Countries which introduced UBs also experienced a decrease in the share of employment in agriculture of about 3 percentage points a year, and an increase in the services sector share of 3 percentage points a year. The effects on job destruction are initially larger but fade away rapidly over time. The impact effects are consistent with a wide array of equilibrium matching models, predicting an impact effect of UBs on job destruction and slow adjustment of job creation margins. However, these models also imply permanent effects of UBs on job reallocation rates.

The paper proceeds as follows. Section two surveys the literature on the effects of UBs on job reallocation and characterizes the various dimensions of UBs which are relevant in affecting labor market flows and structural change according to this literature, motivating our empirical strategy. Section three describes the data and the outcome variables in detail, presents some preliminary descriptive evidence and outlines in detail our empirical strategy. In Section four, we present the

results of the empirical analysis. Finally, in Section five we conclude and propose directions for further research.

2 Unemployment Benefits and Job Reallocation

2.1 Theoretical Predictions

Any simple static model of labor supply predicts that non-labor income increases the reservation wage of individuals. Supposing for simplicity that workers have no choice over hours, the introduction of transfers to non-employed individuals involves a shift upward of aggregate labor supply. In presence of a downward sloping labor demand, at the *competitive equilibrium*, employment is lower, while wages and labor productivity of the marginal worker are higher. Assuming that there is an exogenous fraction of jobs destroyed each instant, gross job destruction and creation (replacing the jobs lost to maintain a constant level of employment) *decline* at the new equilibrium. The job destruction *rate* (job destruction over employment) is, by definition, constant throughout, together with the job creation rate. Importantly, at the equilibrium, there is no unemployment, since every person who wishes to work at the ongoing wage can do so. This raises issues as to why UBs exist in the first place and makes the competitive model rather uninteresting in assessing the effects of UBs on job reallocation.

Equilibrium matching models a la Mortensen and Pissarides (1994) with endogenous job destruction provide a much richer framework to analyze the effects of UBs on job reallocation. They endogenously generate an equilibrium with unemployment, vacancies, job creation (unemployment outflows) and job destruction (unemployment inflows). Jobs are destroyed when their instantaneous productivity falls below an endogenously determined reservation productivity level, R. Jobs are created via a matching function that generates unemployment outflows by allocating jobseekers, u to vacancies, v at a rate h which is increasing in market tightness $\theta = \frac{v}{u}$. Wage formation is typically framed as the outcome of an individual bargaining process aimed at sharing the rents induced by the presence of matching frictions. The solution to this (Nash) bargaining process implies that wages are increasing in the outside option of the worker and in market tightness. The labor force is fixed and can be conveniently normalized to one unit, so that employment is simply $(1-u)$. At the long run equilibrium unemployment is constant, hence job creation equals job destruction in absolute levels.

$$h(\theta)\ u = \lambda F(R)(1-u) \tag{1}$$

where λ is the (exogenous) rate at which jobs are hit by productivity shocks, and $F(R)$ denotes the probability that productivity falls below the reservation productivity level. This equilibrium

condition holds also in terms of job creation and job destruction *rates*, the measures used in our panel regressions (see Section 3 below), which divide gross job flows by employment.

$$h(\theta)\frac{u}{(1-u)} = \lambda F(R) \tag{2}$$

Market tightness and the reservation productivity are jointly determined by the intersection of a downward sloping job creation (JC) curve and an upward sloping job destruction (JD) curve in the R , θ space, as in Figure 1.

The impact effect of the introduction of a UB system is equivalent, in this context, to an increase in the reservation productivity threshold, R. The economics is that rent sharing in some low productivity jobs cannot any longer match the value of unemployment, increased by the introduction of UBs. Hence these low productivity jobs are destroyed. The out-of-the-steady state dynamics is as follows. Job Destruction jumps immediately to a higher level, as depicted in Figure 2. At the same time, employees endowed with a higher outside option succeed in extracting a larger share of the surplus, that is, average wages increase. As the value of jobs for a firm declines, less vacancies are created, and gross job creation declines. Since job destruction increases and job creation declines, unemployment start rising. Given that the number of jobseekers increases, *total* outflows from unemployment, hence gross job creation (the left-hand-side of equation 1), gradually increases from its initial fall, approaching job destruction at the new steady state equilibrium. The latter is depicted as the point B in Figure 1. It involves a higher R, hence a higher job destruction *rate* by equation (2). The job creation *rate* is also larger at the new equilibrium as the unemployment to employment rate, in the left-hand-side of equation 2 increased. Thus, in Mortensen and Pissarides model, the impact effect of the introduction of UBs is an *increase in job destruction rates* and a *decrease in job creation rates.* After the initial fall, the job creation rate recovers to equalize job destruction at the new steady state equilibrium, which features, on average, a higher productivity.

In this class of models, unemployment benefits have a direct effect on job destruction margins, and only an indirect effect (via wages) on job creation. Thus, the introduction of a UB system tend to shift upwards the job destruction schedule without affecting the equilibrium job creation condition. Direct effects on job creation can be introduced in these models by allowing effective labor supply to vary. For instance, allowing for endogenous sorting of workers in formal and informal sectors (Boeri and Garibaldi, 2007) – an extension which is well-suited for labor market conditions in many middle-income income countries – the introduction of UBs induces workers to move from the uncovered (informal) sector to the covered (formal) sector, generating equilibria with higher unemployment and higher job creation in the formal sector. The key factor here is related to the presence of *entitlement effects*, that is, the presence of a segment of job applicants who are not currently receiving UBs, but who qualify for benefits only by working in the formal

sector. The introduction of a UB system increases labor supply in the formal sector and this mitigates the effects on wages of a higher outside option for those who already work in the formal sector. Analogous is the case where first-time jobseekers or new entrants in the labor market are not eligible to benefits. The introduction of UBs increases job creation in this group. As there is an additional, participation, margin to be considered, these extensions may fail to deliver unique equilibria and cannot be simply characterized in the R,θ space. Yet, Boeri and Garibaldi (2007) showed that, under some reasonable parameter values, employment in the formal sector increases after the introduction of UBs. This means that, unlike in the Mortensen and Pissarides model, *the impact effect on job creation can be positive*. Job creation, however, unlike job destruction, is not a jump variable in this class of models. Thus, the adjustment of job creation is more gradual than the adjustment of job destruction. If employment in the shadow sector is properly measured by statistics, job creation and destruction rates will be higher at the new long-run equilibrium. If instead available statistics cover only the formal sector, measured *job creation and destruction rates may actually decline* over time, as soon as the entitlement effect induces shifts from the shadow sector to the formal sector.

Stochastic job matching models (Marimon and Zilibotti, 1999; Acemoglu and Shimer, 1999 and 2000) allow for the productivity of any match to be revealed to the worker and the firm only after the match occurs. In this setting, the introduction of UBs increases average labor productivity and wages, by inducing equilibria where only high productivity jobs are created, as workers turn down low productivity jobs from the start. There can be an efficiency enhancing role of UBs in this context as job search continues until a good match is created. The equilibrium with UBs features higher unemployment than without UBs, as well as less job creation and destruction and longer duration unemployment as individuals become more choosy in their job search strategies. However, *job creation and destruction rates are larger*, due to the decline in employment. Importantly, in this case the direct effect is on the job creation margin.

Summarizing, only the (rather uninteresting) competitive model implies that UBs do not affect job creation and destruction rates. Equilibrium matching models with fixed labor supply imply that UBs on the impact increase job destruction rates and decrease job creation rates and that the new long run equilibrium features higher job flows on both margins. Matching models with entitlement effects (endogenous participation in employment allowing for entitlement to UBs) imply a positive effect on both job creation and destruction rates from the start, while stochastic job matching models imply that the effect of the introduction of UBs is on the job creation margin and is negative. In the long-run all of these models imply higher rates of job creation and destruction after the introduction of UBs except in the case where employment in the shadow (no UB entitlement) economy is poorly measured. If this is the case, then shifts of workers from shadow to legal

employment may be counted as increases in aggregate employment, reducing both measures of job reallocation over time.

2.2 Measurement issues

The assessment of the empirical relevance of this literature requires drawing on measures of job reallocation, gross job creation and gross job destruction as well as possibly indicators of the quality of job reallocation, that is, the effects on the distribution of jobs by productivity levels. We discuss our preferred measures, in light of data availability constraints, below. Before turning to that, it is important to address a number of methodological issues related to the measurement of UBs, which motivate our empirical strategy.

Empirical research often treats unemployment benefits as a one-dimensional institution. However, there are several key dimensions which identify an unemployment benefit system: the eligibility conditions, the level of payments, the maximum legal duration and the actual entitlement rules, in light of activation policies conditioning payments to job search requirements. Mapping all of these features into a one-dimensional measure is not an easy task and information on all these dimensions is often not available for all countries and time periods.

Available summary measures of the generosity of UBs can be misleading as they may misreport actual changes occurred in a UB system. Macroeconomic estimates of the effects of UB systems on aggregate employment, unemployment and wage equations (e.g., Scarpetta, 1999; Nickell, 1997, Blanchard and Wolfers, 2002, Layard, Nickell and Jackman, 2005) typically resort to a "summary measure of benefit generosity" tabulated by OECD and defined as the average of the replacement rates (the ratio of the benefit to the previous wage) in the first two years of unemployment for an "average production worker" having sufficiently long seniority to be offered the benefits up to their maximum duration. Sometimes the product of the replacement rate and the coverage rate (the fraction of the unemployed population receiving the benefits) is taken. However, the two features – replacement rates and coverage rates – are not uncorrelated. Coverage is often endogenous to replacement rates via take-up incentives and fiscal constraints. In middle-income countries, UBs offer relatively high nominal replacement rates (e.g., 60% of the best earnings in the last year in Argentina), but are offered for a short period of time and only to a small fraction of the workforce (workers in small business and in rural areas are not covered, as in China). These asymmetries in replacement rates and duration of benefits are somewhat less evident, but still present, in OECD countries. In Southern Europe UBs are relatively generous in terms of replacement rates, but cover less than 50% of the unemployed whilst the UK and, even more so, the US display scarcely generous UBs providing almost universal coverage of job losers and involving – when account is made of means-tested social assistance – unlimited duration.

UB systems typically involve benefits decreasing over time, consistently with predictions of optimal unemployment insurance models (Hopenayn ad Nicolini, 1997): when search effort is unverifiable, the principal (the State) must give to the agent (the unemployed individual) an incentive to make this effort[1]. At longer unemployment durations, as human capital depreciates during the unemployment spell, eliciting search effort may become too costly relative to the social benefits of this activity (Pavoni and Violante, 2004), and hence benefits become flat. Finally, when the maximum duration of UBs is exhausted, individuals become eligible to means-tested social assistance of the last resort. The way in which these various steps in UB payment are integrated is even more important than the level of benefits *per se* in affecting job search incentives.

Moreover, unemployment benefits in practice never act in isolation. They interact with other institutions in "imperfect" labor markets, such as labor taxes, employment protection legislation and unions. These interaction effects are rarely taken into account by theory and empirical work.

Macroeconomic assessments of the effects of unemployment benefits typically include measures of the generosity of the system as right-hand-side and un-instrumented variables. However, recent work suggests that the causality may go the other way round. Governments in countries with a high incidence of long-term unemployment are pressed to increase the duration of benefits: regional diversification in the maximum duration of UBs in the US tends to follow increases in the duration of unemployment in some States (Card and Levine, 2000). Lalive, Van Ours and Zweimueller (2002) documented that this policy endogeneity may lead one to significantly overstate the negative effects of unemployment benefits on the duration of unemployment.

In order to win political opposition to the downscaling of benefits, reforms of unemployment insurance often involve a number of marginal adjustments of the benefit formula and a gradual tightening of entitlement rules. The grandfathering of past entitlements creates two-tier systems in which just a fraction of the workforce is under the new regime. Under these conditions, estimates of the impact of unemployment benefits applying the same rules to everybody, may be misleading. Estimates of the effects of UBs should as much as possible take into account these two-tier regimes.

The high frequency of UB reforms is also an asset for empirical research: there are many "natural experiments" around to be exploited when assessing the macroeconomic effects of UBs. But it is difficult to assess the empirical relevance of the predictions of models treating UBs as a one dimensional institution, as reforms typically manipulate several parameters at once, e.g., they increase benefits, but reduce eligibility. Moreover, changes in entitlement conditions often take place only via changes in law enforcement without observing any change in the legislation.

[1] Earning related UBs offered at replacement rates decreasing over time reduce also incentives to elude or evade payments of payroll contributions. This is particularly important in countries with a large informal sector. If more generous benefits are offered only to workers with some official employment history, then workers' incentives to enter the shadow sector are lower and shadow employer need to compensate more uninsured workers (Boeri, 2000).

For these reasons, in the remainder of the paper we compare outcomes of countries *with* and *without* unemployment benefits *before* and *after* the reforms introducing the UB system. By relying on dichotomic policy choices and mainly on within country variation, we can proceed without having to rely on the standard one-dimensional measures of UB generosity. Given that we deal with regime changes, we also do not need to worry about dual track reform strategies. We also concentrate on labor market flows, which, unlike aggregate stocks, are sensitive to institutional reforms, even in the short-run. Institutional interactions can also be taken into account in this context. However, our identification strategy, detailed in the next section, requires that other institutions are not altered at the time in which the UB system was introduced.

3 Data and Empirical Strategy

3.1 Outcome Variables

We consider a "treatment", the introduction of a UB system, and a series of outcome variables. Motivated by the theoretical considerations outlined in the previous section, our first set of outcome variables are meant to capture the extent of job reallocation. Let us define gross job creation (JC) and gross job destruction (JD) as follows:

$$JC_{it} = \sum_{j \in E_i^+}^{n} \left(\frac{e_{ijt}}{E_{it}} \right) g_{ijt} \qquad \text{and} \qquad JD_{it} = \sum_{j \in E_i^-}^{n} \left(\frac{e_{ijt}}{E_{it}} \right) |g_{ijt}|$$

where i denotes country, j denotes sector, e_{ijt} denotes employment in sector j at time t, E_{it} is total employment in country i at time t , g_{ijt} is the growth rate of employment in sector j at time t relative to time $t-1$ and E_i^+ (E_i^-) is the set of expanding (shrinking) sectors. JC_{it} measures job creation by adding up employment gains in expanding sectors, JD_{it} measures job destruction by adding up employment losses at shrinking sectors. Job turnover is thus defined as

$$JT_{it} = JC_{it} + JD_{it}$$

JT_{it} is therefore the size-weighted mean of the absolute value of sectorial growth rates. As explained in the previous section, the effect of UB on job turnover operates through different mechanisms according to different theories. Therefore, in an effort to discriminate between theories, in addition to considering JT we also study the effects of UBs on JC and JD, separately.

Matching models with endogenous job destruction imply that UBs act *de facto* as a wage floor, cutting off low productivity jobs. Insofar as productivity varies across sectors, UBs are therefore bound to affect also the composition of employment by sector. The above reallocation measures may capture idiosyncratic shocks not necessarily associated with sectorial job reallocation. In order

to better capture genuine sectorial reallocation effects, we shall also consider as outcome variables the employment shares of agriculture, industry and services.

3.2 Data

Our empirical analysis exploits variation in the timing of adoption of a UB system from scratch in a large sample of countries. Information on the date of introduction of unemployment benefits systems was taken primarily from Social Security Programs throughout the World (SSW).[2] Employment data were taken from the ILO LABORSTA database (http://laborsta.ilo.org/). Our main sample consists of 46 countries which did not have any unemployment insurance scheme in place as of 1980 (see Appendix Table 1 for a complete list of the countries in our sample). Of these, 25 countries introduced UBs for the first time between 1980 and 2002.[3] In principle, the ILO data span over a 22 years period, from 1980 to 2002. However, the ILO series are complete only for a subset of countries. The actual number of observations per country varies between 4 and 23.[4] Because calculating job reallocation measures entails using data from consecutive years, this implies that the number of observations per country used in our estimation ranges between 3 and 22, with an average of 12 and a median of 11. Figure 3 plots the number of countries that introduced UB schemes during the time period of 1980 to 2002 (at yearly frequency), and Table 2 reports, for each country, the year of introduction and the number of observations.

3.3 Descriptive Evidence and Identification Issues

We begin our empirical analysis by presenting a visual summary of the raw data on the rates of job creation, destruction and job turnover in three groups of countries in the period 1980-2002: (a) countries that adopted UBs at some point between 1980 and 2002, (b) countries that never adopted UBs and (c) countries which had UBs in place throughout the period of analysis. The three panels of Figure 4 provide initial evidence that the introduction of UBs had an impact on job reallocation in the group of countries that introduced UBs at some point during the period 1980-2002. In this group of countries, the rate of job turnover appears to increase sharply starting in the late 1980s, and then drops so that in the late 1990s it was back to roughly its previous level. This pattern seems to be driven by changes in the rate of job destruction. On the other hand, no discernible trend or pattern is visible in the countries with UBs in place throughout the period or in countries

[2] This is an International Social Security Association (ISSA) publication which comprises four volumes: "Europe", "Asia and the Pacific", "Africa" and "The Americas". The information we use in this paper is taken from the following issues: September 2002 for Europe, March 2003 for Asia and the Pacific, September 2003 for Africa and March 2004 for The Americas.

[3] In our robustness checks, among other things, we extend the sample to also include countries with UBs in place throughout the period, which brings the number of countries to 77.

[4] The ILO data present some breaks in the series due, for instance, to changes in the reference population. We have excluded the years when such breaks occurred.

that never adopted UBs.

Because most of the countries which adopted UBs did so between 1988 and 1992 (see Table 2), the evidence provided in Figure 4 is somewhat suggestive of an effect of UBs on job turnover. In the remainder of the paper, we aim to assess whether such an effect is consistent with a causal interpretation. There are several reasons why the patterns displayed in Figure 4 might be spurious. First of all, the averages plotted in Figure 4 were obtained from a variable number of countries each year, due to limitations in the available data (see Appendix Table 1, where we show the values of our outcome variables for the individual countries).[5] Second, and more important, just looking at differences in outcomes before and after the change and between "treated" and "untreated" countries is not enough to prove the existence of a meaningful empirical association, let alone a causal one. On the one hand, it is possible that our "treatment" countries are a selected group that would have experienced increases in the outcome variables irrespective of the introduction of UBs. In Table 3, we report summary statistics on the outcome and control variables separately for countries in groups (a), (b) and (c) before and after the adoption of UBs.[6] Comparing summary statistics between "treated" and "untreated" groups (Panels A and B), we observe that, before adopting UBs, the "treated" countries have, on average, higher GDP per capita and higher GDP growth rates than "untreated" countries. "Untreated" countries also present higher rates of job turnover, as is also apparent from Figure 4. These observations indicate that it is important to properly take into account differences in observable and unobservable characteristics between UB adopters and non-adopters when assessing the impact of UBs. Moreover, there is the possibility of reverse causality. Quite simply, the introduction of UBs might have occurred as a response to increased job turnover. It is therefore possible that the causality goes from job reallocation to UBs rather than vice versa. This concern also comes from the fact that most of these countries come from central planning.[7] In our empirical analysis, we exploit the longitudinal nature of our data to address the potential problems of unobserved heterogeneity and reverse causality. In particular, we include country fixed effects to control for selection into UBs based on *levels* of the outcome variables, and a full set of country-specific time trends to control for selection into adopting UBs based on the *growth rate* of our outcome variables (see Ashenfelter and Card 1985, Heckman and

[5] In our empirical analysis, we follow Heckman and Pages (2000) and we use yearly data rather than average our outcome and dependent variables over periods of time, as often done in cross-country studies. To control for business cycle conditions, we include GDP growth rates among the control variables.

[6] For the countries in panel A, "before" and "after" refer to the adoption of UBs. For countries in panels B and C, the "before" period includes years before 1992 and the after period years after (and including) 1992. As can be seen in Table 2, the year 1992 is the modal year of adoption of UBs in the group of countries which introduced UBs schemes between 1980 and 2002.

[7] Except for the countries in the sample historically belonging to the former Yugoslavia (Croatia, Serbia and Slovenia, which had some UBs in place since the 1970s) and Hungary (which introduced a seminal unemployment benefit system in 1986-87), the remaining formerly planned economies introduced UB systems at the outset of the transition to a market economy.

Hotz 1989 and more recently Brown, Earle and Telegdy 2006).

3.4 Methodology

We use a reduced form approach to contrast our outcome variables in countries adopting UBs in a given period with countries not adopting UBs. In particular, we estimate the following model:

$$Y_{it} = UB_{it}\theta + \mathbf{X}_{it}\beta + \mu_t + \alpha_i + \tau\gamma_i + u_{it} \tag{3}$$

where i indexes countries and t indexes time periods (years). The outcome variable, Y, is the annual JC, JD or JT as defined in the previous section, or the share of employment in agriculture, industry and services; UB_{it} is a (0,1) variable indicating the presence of unemployment benefits in country i in period t; $\mathbf{X}_{it}$ is a set of time-varying, observable, country-specific characteristics that affect Y_{it}, μ_t is an aggregate time effect, α_i is a country fixed effect, τ_i is a time trend, specific to country i, and u_{it} is a disturbance term. We focus our attention on one single parameter, θ, the coefficient on UBs. An important strength of our empirical strategy is that the interpretation of our empirical "experiment" (and of our coefficient of interest) is very clear: we ask whether the introduction of unemployment benefits is associated with a significant shift in the level of our outcome variables.

The time effects account for evolving aggregate factors that might affect our outcome variables. The vector of controls, $\mathbf{X}_{it}$, includes the growth rate of per capita GDP, to control for the business cycle, the level of per capita GDP (in logs) to account for country "income" effects[8], and the degree of openness to trade[9]. The inclusion of country effects takes care of unobservable heterogeneity possibly correlated with UBs. In particular, our fixed-effects specification allows the country effects to be correlated with current, past and present values of UB (i.e. with any of the $UB_{i1}, UB_{i2}, ..., UB_{iT}$). We will thus be looking at effects of UBs *within* countries over time, while accounting for possible selection based on the *level* of job reallocation. Our coefficient of interest, θ, is the mean within-country-year difference in the outcome variables between countries that adopted UBs and countries without UBs.[10]

The inclusion of country-specific time trends, finally, provides a control for the possibility that the adoption of UBs is correlated with idiosyncratic trends in the rates of job turnover. Our specification is a version of the "random growth model", used in Ashenfelter and Card (1985), Heckman and Hotz (1989) and more recently in Brown, Earle and Telegdy (2006). In addition

[8] Upper-middle income countries tend to have strong administrative capacity, which is required for an effective implementation of UB schemes (Vodopivec, 2004).

[9] Openness to trade is measured as the sum of exports and imports over GDP. GDP and trade openness data were taken from the Penn World Tables version 6.2.

[10] The inclusion of country fixed effects also controls for differences in the coverage and methodology of data collection across country.

to controlling for fixed differences among countries, model (3) accounts for different trends in job reallocation that may affect the propensity of a country to introduce unemployment benefits.

In a further effort to investigate whether the empirical association observed between UBs and job reallocation is consistent with a causal interpretation, we also estimate a dynamic version of model (3):

$$Y_{it} = \sum_{q=-3}^{6} UB_{i,t_0+q}\theta_{t_0+q} + \mathbf{X}_{it}\beta + \mu_t + \alpha_i + \tau\gamma_i + u_{it} \tag{4}$$

where t_0 denotes the year of introduction of UBs, and UB_{i,t_0+q} is a dummy variable equal to 1 in year $t_0 + q$ and 0 otherwise. Each coefficient θ_{t_0+q} measures the mean within-country-year difference in the outcome variables between countries that adopted UBs and countries without UBs in year $t_0 + q$.[11]

4 Results

4.1 Baseline Results

In the three panels of Table 4, we report the estimates of model (3) where the dependent variable is job turnover (first panel), job creation (second panel) and job destruction (third panel). Column (1) in each panel shows results of random effects regressions, while columns (2) through (5) report results of fixed effects regressions. The random effects regressions exploit both within- and between-country variation. Using both types of variation allows us to make use of all of the available data. In fact, as shown in Table 2, data for both the "before" and "after" periods are available only for 14 of the 25 countries which adopted UBs. The fixed effects specification identifies the effect of UBs from within-country variation only, thereby removing any fixed differences in the rates of job turnover, and making sure that we account for the possibility of selection into adopting UBs based on levels of the outcome variables. Controlling for such a possibility is important, especially in the light of Figure 4 and the summary statistics displayed in Table 3, which reveal that the rates of job turnover tend to be higher in absolute terms in countries that never adopted UBs. In all cases, year fixed effects and country-specific time trends are included among the regressors.[12] As we explained above, this is done in an attempt to control for the possibility of selection into introducing UBs based on pre-existing trends in the outcome variables. It is worth noting that our specification is very demanding of the data, given that we have a limited number of observations per country.

The random effects estimate reported in column (1) of the first panel in Table 4 indicates that the introduction of UBs is associated with a higher rate of job turnover. The estimated coefficient

[11] Due to data limitations, we have defined the dummy UB_{i,t_0+6} to be equal to 1 in years $t_0 + 6$, $t_0 + 7$, etc...

[12] We conducted F-tests on the joint probability that all country fixed effects, all year effects and all year time trends are equal to zero. The null hypothesis was in all cases rejected with p-values smaller than 0.0001.

implies that the adoption of UBs is associated with a 3.3 percentage points higher job turnover rate, and the coefficient is statistically significant at the one percent confidence level. The random effects estimates reported in the first column of the second and third panel of Table 4 seem to suggest that the higher job turnover is due to an increase in both the job creation rate and (especially) the job destruction rate.

We now turn to the fixed effects estimates. Column (2) uses all observations in the sample, while column (3) uses only the country-year observations for which the control variables - log of per-capita GDP, GDP growth rate and trade openness - were available. These control variables were included in Columns (4) and (5). In all cases, the standard errors are robust to arbitrary forms of heteroschedasticity. In Column (5), standard errors were also adjusted for first-order autocorrelation.[13] Because the results are fairly consistent across samples and specifications, in describing the fixed-effects results we focus on Column (5). The fixed effects regressions indicate that UBs are associated with a 2.7 percentage points higher rate of job turnover. The coefficient is statistically significant at the five percent confidence level. The first and second panel of Table 4 indicate that the higher job turnover is due to an increase in both the job creation rate and the job destruction rate. However, only the impact on job destruction is statistically significant.

In Table 5, we turn to estimating the effect of the introduction of UBs on the composition of employment. In particular, our dependent variables are the employment shares of agriculture (first panel), industry (second panel) and services (third panel). Once again, column (1) in each panel shows the estimated coefficients of a random effects version of model (3), and columns (2) through (5) those of fixed effects specifications. Our estimates indicate that the countries which introduced UBs experienced a reduction of the share of employment in agriculture of about 3 percentage points a year, and an increase of the services share of about 3.7 percentage points. The estimated standard errors, robust to arbitrary heteroschedasticity and first-order autocorrelation, indicate that the coefficients are statistically significant at the one percent (services) and five percent (agriculture) levels. We find an effect of UBs on the share of employment in the industrial sector only in the random effects specification.

4.2 Further Robustness Checks

4.2.1 Excluding the early years of the transition to markets of formerly planned economies

One possible concern with the above analysis is that most of the countries which introduced UBs in the period considered were formerly planned economies. Thus, we may capture policy endogeneity associated to the transition to a market economy: these countries introduced, mostly from scratch,

[13] Adjusting the standard errors for second-order autocorrelation produced nearly identical results.

a UB system before starting the transition. To lessen this concern, in Tables 7 and 8 we replicate the analyses of Tables 5 and 6 when excluding from the sample, for countries in the former Soviet Union and Central and Eastern Europe, the year they started their transition, the year immediately before and the year immediately after. Remarkably, our analysis is robust to this check, as the estimated coefficients remain similar to those estimated previously, both in magnitude and in terms of statistical significance.

4.2.2 Including countries with UB systems in place throughout the period among the "control" group

In Table 8, we report the results of estimating (3) when we include among the control group the set of countries that had unemployment benefits schemes in place throughout the period of observation. This exercise allows us to increase considerably the number of observations, thus enabling us to obtain more robust estimates of the time effects and the other controls. Because our premise is that the *introduction* of UBs has the potential to affect the rates of job turnover and sectorial reallocation, we expect to find that UB adopters experienced greater job turnover after introducing UBs compared to both countries that never adopted UBs and countries that already had UBs in place. The results from Table 8 are qualitatively and quantitatively very similar to those from the previous tables, indicating that this is indeed the case.

4.2.3 Dynamic Effects

In Figure 5, we display the estimated coefficients from model (4) for job turnover, job creation and destruction, as well as the associated confidence intervals. Figure 5A reports results obtained from the full sample and Figure 5B includes all of our control variables (which implies, as noted above, a reduction in sample size). In all cases, year effects, country fixed effects and country-specific time trends are included, and the standard errors are robust to heteroschedasticity and first-order autocorrelation.

The results displayed in Figures 5A and 5B show positive effects of UBs on job turnover starting the year of UB introduction, while the coefficients on the years before are small (very close to zero) and never statistically significant. This finding indicates that the increase in job turnover followed the introduction of UBs, rather than viceversa, which is consistent with a causal interpretation of our results.

Overall, there appears to be no effect of UBs on job creation, and a tendency for the effects of UBs on job destruction to be stronger initially but fade away over time.

4.2.4 Controlling for Employment Protection (EPL) Strictness

Some of the effects of UBs on structural change may come from substitutability of EPL with UBs. Employment protection is an obstacle to job reallocation and a large body of empirical research points to a negative relationship between gross job flows (notably unemployment inflows) and EPL.

Regressions reported in Table 9 use data on EPL taken from Botero et al. (2004).[14] The advantage of this measure is that it is available for a very broad set of countries. Its disadvantage is that of being measured at a point in time (the end of the 1990s). However, as documented in Boeri et al. (2003), EPL measures, notably those referred to "regular" employment, tend not to vary much over time within countries. The cross-sectional nature of these data forces us to estimate random-effects regressions. The random effects estimates displayed in the first panel of Table 9 confirm a positive and statistically significant effect of UBs on job destruction and job turnover even when controlling for the degree of EPL strictness. The results reported in the bottom panel confirm that UBs are associated with an expansion of the services sector. We also find a marginally statistically significant reduction of the industrial sector and a statistically insignificant reduction of the agriculture sector, while the strictness of EPL increases the employment share of agriculture.

In Table 10, we estimate the effect of UBs for countries in the bottom third, the middle third and the top third of the distribution of EPL strictness. The results of this empirical test suggest that the impact of UBs on job destruction is strongest in countries with a high degree of EPL strictness. As for sectorial reallocation, our results indicate that the effect of UBs on reducing agriculture and expanding the services sector are strongest in countries with moderate levels of EPL. These findings are consistent with the notion that UBs can better accommodate large scale restructuring where employment protection is less stringent.

4.3 Discussion

Overall, the results of our empirical analysis strongly suggest that the introduction of UBs was associated with greater job turnover, as a result of higher job destruction rates. The introduction of UBs seems also to determine a shift of jobs from low-productivity sectors (agriculture) to services. These conclusions are robust to the inclusion of controls for observable and unobservable country effects, year effects and country-specific time trends, and robust to allowing for heteroschedastic and autocorrelated residuals. These results provide corroborating evidence for the theories outlined in Section 2 that highlight the role of unemployment benefits in favoring greater job turnover and reallocation in labor markets departing from perfect competition. Matching models, in particular, imply a slow adjustment along the job creation margin, and a jump in job destruction, which is

[14] This index is the average of four sub-indices: (1) Alternative employment contracts; (2) Cost of increasing hours worked; (3) Cost of firing workers; and (4) Dismissal procedures. It is the variable "index_labor7a" in Botero et al.'s dataset, available at http://iicd.som.yale.edu.

consistent with the observed impact effect of UBs on job turnover via the job destruction margin. However, all the models reviewed in the theoretical section imply a permanent effect on job creation and destruction, that we do not see in the data. This may be due to the fact that, after the initial introduction, UBs are subsequently downscaled as their fiscal costs increase. This was precisely what happened in the formerly planned economies (Boeri, 2000). Another interpretation is related to entitlement effects and problems in the measurement of the shadow sector: as more jobs are created in the sector allowing to gain entitlement to UBs, the denominator of our gross job flow measures increases.

5 Conclusions

The vast empirical literature on the macroeconomic effects of unemployment insurance systems overlooked so far the reallocation effects of UBs, in terms of job turnover and in the inter-industry distribution of employment.

In this paper, we tested the empirical implications of models allowing UBs to play a major role in job creation and destruction as well as interindustry shifts of workers. Our strategy acknowledges the multidimensional nature of UBs and exploits the fact that many countries introduced such systems from scratch at some point during the period 1980-2002. Thanks to the longitudinal nature of our data, we were able to find remedies to potential selection, endogeneity and omitted variables biases in our estimates. In particular, the panel dimension of our data allowed us to control for observable and unobservable country characteristics, as well as for country-specific time trends. The inclusion of country fixed effects ensures that we are controlling for omitted time-invariant variables as well as for selection into adopting unemployment benefits based on the level of job turnover and reallocation, and the inclusion of country-specific time trends helps controlling for selection into introducing unemployment benefits based on the growth rate of job turnover and interindustry job reallocation.

We found economically and statistically significant effects of UBs on gross job turnover, coming primarily from higher rates of job destruction, as well as on inter-industry reallocation, that survive to several robustness checks. The introduction of UBs is associated with about a 1.5 percentage point increase in the yearly rate of job destruction and a 2.7 percentage points increase in job turnover. This implies a positive effect on job creation as well, but this effect was not found to be statistically significant when estimated separately. Countries which introduced UBs also experienced a decrease in the share of employment in agriculture of about 3 percentage points a year, and an increase in the services sector share of 3 percentage points a year. The effects on job destruction are initially larger but fade away over time. While the impact effect on job destruction is consistent with matching models, the dynamic effects are not, as these models imply a permanent

effect of UBs on job creation and destruction rates. We offer two possible interpretations for the time pattern of the effects of UBs on job reallocation. The first interpretation is that, after the initial introduction, UBs are subsequently downscaled as their fiscal costs increase, along with the experience of formerly planned economies. The second interpretation is related to the expansion of the legal sector, allowing workers to gain entitlement to UBs, and statistical under-reporting of the shadow sector. Moreover, our identification assumption requires no additional institutional change at the time in which UBs are introduced and it is plausible that other institutions interfering with labor market were adjusted after the introduction of UBs. One of the institutions interfering with the effects of UBs on job reallocation is employment protection: we find that countries with stricter employment protection experience larger increases in job destruction rates after the introduction of UBs.

Future work would use finer measures of job turnover, based on firm-level information, when these data become available for a sufficiently large set of countries. At the same time, looking at the quality of structural change as well, notably evaluating the effects of UBs on job tenure (a proxy for match quality), represents a potentially fruitful avenue for research, as it permits to directly test the empirical relevance of stochastic job matching models. Results from individual data on the US (Centeno, 2004) suggest that there may be indeed important effects on unemployment benefits on match quality. It should also be noted that our estimates are averages across countries that could mask substantial heterogeneity. We were able to uncover some heterogeneity by allowing the effects of unemployment benefits to vary with the degree of strictness of employment protection. Finally, investment in data gathering on the design of UBs in a large number of countries would pave the way for future work on the dynamic effects of UBs on job reallocation, allowing to improve on our empirical strategy, properly capturing changes in UBs after their introduction. Better measures and micro data sets would also allow researchers to conduct within-country studies, perhaps exploiting geographical-time or industry-time variation (Freeman, 2007).

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Table 1: List of Industries

ISIC code		Description
Rev. 1	**Rev. 2**	
Major 1	A+B	Agriculture, Hunting, Forestry and Fishing
Major 2	C	Mining and Quarrying
Major 3	D	Manufacturing
Major 4	E	Electricity, Gas and Water
Major 5	F	Construction
Major 6	G+H	Wholesale Retail Trade and Restaurants and Hotel
Major 7	I	Transport, Storage and communication
Major 8	J+K	Financing, Insurance, Real Estate and Business Services
Major 9	L+M+N+O	Community, Social and Person Services

Source: ILO LABORSTA Database (http://laborsta.ilo.org/). Note: The "residual" category was computed, case by case, as the difference between total employment and the sum of employment in the available industries.

Table 2: Number of Observations for the Countries which introduced Unemployment Benefits from scratch between 1980 and 2002

Country	Years of Observation		Date UB Introduced	N. Observations	
	first	last		before	after
Albania	1995	2002	1993	0	8
Argentina	1983	2002	1991	2	10
Azerbaijan	1984	2002	1992	7	10
Belarus	1988	1994	1991	3	4
Brazil	1982	1999	1986	4	10
Bulgaria	1981	2001	1989	8	12
China	1988	2002	1986	0	15
Colombia	1986	2002	1990	4	11
Czech Republic	1994	2002	1991	0	9
Estonia	1990	2002	1991	1	11
Georgia	1999	2002	1993	0	4
Hungary	1992	2001	1986	0	10
Korea, Republic of	1981	2002	1993	12	10
Kyrgyzstan	1987	2002	1992	5	11
Latvia	1997	2002	1992	0	5
Lithuania	1983	2001	1992	9	10
Moldova	2000	2002	1992	0	3
Poland	1982	2002	1989	7	12
Romania	1981	2002	1991	10	12
Russia	1998	2002	1992	0	5
Slovak Republic	1995	2002	1991	0	8
Turkey	1983	2002	2000	12	2
Ukraine	1988	2000	1992	4	8
Uruguay	1987	2000	1981	0	11
Uzbekistan	1996	1999	1992	0	4

Notes: The number of observations listed in columns 5 and 6 refer to the years for which we were able to compute job turnover measures. "Before" and "After" refer to years before and after UBs were adopted. The source of information on the date of introduction of Ubs is "Social Security Programs throughout the World" (2002-2004).

Table 3: Summary Statistics of Outcome and Control Variables

Panel A: Countries which Introduced UBs During the period

	Before			After		
Variable:	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Log of per-capita GDP	204	8.712	0.547	52	8.450	0.303
GDP growth	199	2.480	6.651	51	2.923	4.717
Openness to Trade	204	73.174	39.976	52	32.822	14.955
EPL Strictness	175	0.513	0.156	77	0.499	0.130
Job Creation Rate	215	0.013	0.015	88	0.026	0.022
Job Destruction Rate	215	0.026	0.027	88	0.024	0.025
Job Turnover Rate	215	0.050	0.033	88	0.039	0.026
Agriculture share	215	0.247	0.181	88	0.267	0.110
Industry share	215	0.255	0.088	88	0.317	0.089
Services share	215	0.379	0.111	88	0.450	0.173

Panel B: Countries without UBs throughout the period

	Before			After		
Variable:	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Log of per-capita GDP	89	8.069	0.582	167	8.488	0.596
GDP growth	89	1.921	4.169	167	2.089	3.281
Openness to Trade	89	85.846	62.118	167	95.880	72.020
EPL Strictness	63	0.404	0.148	120	0.452	0.158
Job Creation Rate	89	0.044	0.032	167	0.046	0.032
Job Destruction Rate	89	0.014	0.020	167	0.016	0.016
Job Turnover Rate	89	0.058	0.035	167	0.063	0.039
Agriculture share	89	0.334	0.220	167	0.289	0.168
Industry share	89	0.199	0.071	167	0.206	0.053
Services share	89	0.472	0.129	167	0.415	0.126

Panel C: Countries with UBs in place throughout the period

	Before			After		
Variable:	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Log of per-capita GDP	197	9.317	0.513	283	9.753	0.532
GDP growth	197	2.091	3.004	283	2.089	2.518
Openness to Trade	197	49.291	31.087	283	75.830	49.252
EPL Strictness	194	0.469	0.215	280	0.487	0.205
Job Creation Rate	197	0.021	0.019	283	0.019	0.017
Job Destruction Rate	197	0.012	0.012	283	0.012	0.012
Job Turnover Rate	197	0.034	0.020	283	0.031	0.021
Agriculture share	197	0.091	0.090	283	0.113	0.095
Industry share	197	0.254	0.054	283	0.290	0.055
Services share	197	0.638	0.090	283	0.592	0.089

Notes: For the countries in Panel A, before and after are defined based on the date of introduction of UBs. For the countries in panels B and C, the "before" period includes years before 1992 and the after period years after (and including) 1992. The year 1992 is the modal year of adoption of UBs in the group of countries which introduced UBs schemes between 1980 and 2002. Job Creation, Job Destruction and Job Turnover were calculated by the Authors based on ILO data, as described in the text. Data on per capita GDP, GDP growth and Trade Openness are from the Penn Tables version 6.2. The measure of EPL strictness is taken from Botero et al., 2004 (variable "index_labor7a" in the dataset available at http://iicd.som.yale.edu).

Table 4: Effect of the Introduction of Unemployment Benefits Schemes on Job Creation, Destruction and Job Turnover

	random effects robust s.e. (all observations)	fixed effects robust s.e. (all observations)	fixed effects robust s.e. (no missing controls)	fixed effects robust s.e.	fixed effects s.e. adjusted for heteroschedasticity and serial correlation
	(1)	(2)	(3)	(4)	(5)
	dependent variable: **job turnover rate**				
Unemployment Benefits	0.0331***	0.0388***	0.0291**	0.0273**	0.0273**
	(0.008)	(0.010)	(0.014)	(0.014)	(0.012)
GDP growth				0.000	0.000
				(0.000)	(0.000)
Log of per-capita GDP				-0.0464*	-0.0464**
				(0.025)	(0.022)
Openness to Trade				0.000346*	0.000346**
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.0816**	0.032	0.056	0.415**	
	(0.032)	(0.049)	(0.052)	(0.200)	
Observations	559	559	506	506	506
Number of id	46	46	45	45	45
R-squared	0.31	0.22	0.20	0.21	0.21
	dependent variable: **job creation rate**				
Unemployment Benefits	0.0119*	0.008	0.013	0.014	0.014
	(0.007)	(0.009)	(0.011)	(0.012)	(0.011)
GDP growth				0.000570**	0.000570**
				(0.000)	(0.000)
Log of per-capita GDP				-0.022	-0.022
				(0.018)	(0.015)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.0717**	-0.021	-0.010	0.157	
	(0.032)	(0.044)	(0.045)	(0.140)	
Observations	559	559	506	506	506
Number of id	46	46	45	45	45
R-squared	0.3	0.18	0.2	0.21	0.21
	dependent variable: **job destruction rate**				
Unemployment Benefits	0.0212***	0.0307***	0.0159**	0.0134**	0.0134**
	(0.005)	(0.007)	(0.008)	(0.006)	(0.007)
GDP growth				-0.000811***	-0.000811***
				(0.000)	(0.000)
Log of per-capita GDP				-0.025	-0.025
				(0.020)	(0.018)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.010	0.0537**	0.0658**	0.258	
	(0.014)	(0.026)	(0.026)	(0.160)	
Observations	559	559	506	506	506
Number of id	46	46	45	45	45
R-squared	0.31	0.25	0.23	0.26	0.26

Notes: Random and fixed effects regressions, estimated using yearly observations covering the period 1980-2002. The dependent variables (job creation, destruction and turnover) were calculated as explained in the text. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if unemployment benefits schemes were in place in a given country-year and 0 otherwise. The coefficients reported in the first column were obtained from estimating the model using all country-year observations for which we were able to calculate job reallocation measures. The results reported in the other columns only include country-year observations for which data on the control variables (per-capita GDP, GDP growth and openness to trade) were available. Standard errors are reported in parenthesis. In the first three columns, standard errors are robust to arbitrary forms of heteroschedasticity. In the fourth column, they are adjusted for both heteroschedasticity and first-order serial correlation. Levels of statistical significance are indicated by asterisks: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Effect of the Introduction of Unemployment Benefits Schemes on Sector Shares

	random effects robust s.e. (all observations)	fixed effects robust s.e. (all observations)	fixed effects robust s.e. (no missing controls)	fixed effects robust s.e.	fixed effects s.e. adjusted for heteroschedasticity and serial correlation
	(1)	(2)	(3)	(4)	(5)
	dependent variable: **agriculture share**				
Unemployment Benefits	0.0339*	-0.011	-0.022	-0.0318**	-0.0318**
	(0.019)	(0.009)	(0.014)	(0.015)	(0.016)
GDP growth				0.001	0.001
				(0.001)	(0.001)
Log of per-capita GDP				-0.0969***	-0.0969***
				(0.029)	(0.029)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.239***	0.732***	0.712***	1.481***	
	(0.089)	(0.140)	(0.140)	(0.270)	
Observations	559	559	506	506	506
Number of id	46	46	45	45	45
R-squared	0.8	0.58	0.57	0.58	0.58
	dependent variable: **industry share**				
Unemployment Benefits	-0.0484***	-0.010	-0.005	0.004	0.004
	(0.014)	(0.007)	(0.009)	(0.008)	(0.009)
GDP growth				0.000	0.000
				(0.000)	(0.000)
Log of per-capita GDP				0.0979***	0.0979***
				(0.012)	(0.014)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.139***	0.124**	0.125**	-0.648***	
	(0.042)	(0.061)	(0.059)	(0.120)	
Observations	559	559	506	506	506
Number of id	46	46	45	45	45
R-squared	0.7	0.79	0.73	0.77	0.77
	dependent variable: **services share**				
Unemployment Benefits	0.0347**	0.0185***	0.0275***	0.0268***	0.0268***
	(0.014)	(0.007)	(0.009)	(0.010)	(0.010)
GDP growth				0.000	0.000
				(0.000)	(0.000)
Log of per-capita GDP				-0.011	-0.011
				(0.016)	(0.017)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.357***	0.118	0.122	0.211	
	(0.050)	(0.080)	(0.082)	(0.150)	
Observations	559	559	506	506	506
Number of id	46	46	45	45	45
R-squared	0.87	0.72	0.71	0.71	0.71

Notes: Random and fixed effects regressions, estimated using yearly observations covering the period 1980-2002. The dependent variables (share of employment in agriculture, industrial sector and services) were calculated as explained in the text. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if unemployment benefits schemes were in place in a given country-year and 0 otherwise. The coefficients reported in the first column were obtained from estimating the model using all country-year observations for which we were able to calculate sector shares. The results reported in the other columns only include country-year observations for which data on the control variables (per-capita GDP, GDP growth and openness to trade) were available. Standard errors are reported in parenthesis. In the first three columns, standard errors are robust to arbitrary forms of heteroschedasticity. In the fourth column, they are adjusted for both heteroschedasticity and first-order serial correlation. Levels of statistical significance are indicated by asterisks: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Effect of the Introduction of Unemployment Benefits Schemes on Job Creation, Destruction and Job Turnover (Excluding the first years of the transition for CEE and FSU countries)

	random effects robust s.e. (all observations)	fixed effects robust s.e. (all observations)	fixed effects robust s.e. (no missing controls)	fixed effects robust s.e.	fixed effects s.e. adjusted for heteroschedasticity and serial correlation
	(1)	(2)	(3)	(4)	(5)
	dependent variable: **job turnover rate**				
Unemployment Benefits	0.0352***	0.0399***	0.0301**	0.0294*	0.0294**
	(0.009)	(0.013)	(0.015)	(0.015)	(0.014)
GDP growth				0.000	0.000
				(0.000)	(0.000)
Log of per-capita GDP				-0.038	-0.038
				(0.027)	(0.024)
Openness to Trade				0.000378*	0.000378**
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.0829***	0.051	0.068	0.361*	
	(0.032)	(0.048)	(0.050)	(0.210)	
Observations	530	530	496	496	496
Number of id	46	46	45	45	45
R-squared	0.31	0.21	0.20	0.21	0.21
	dependent variable: **job creation rate**				
Unemployment Benefits	0.0199**	0.0191*	0.018	0.018	0.018
	(0.008)	(0.011)	(0.013)	(0.013)	(0.013)
GDP growth				0.000666**	0.000666**
				(0.000)	(0.000)
Log of per-capita GDP				-0.031	-0.0306*
				(0.020)	(0.018)
Openness to Trade				0.000326*	0.000326**
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.0708**	-0.011	-0.006	0.231	
	(0.033)	(0.043)	(0.045)	(0.160)	
Observations	530	530	496	496	496
Number of id	46	46	45	45	45
R-squared	0.28	0.2	0.20	0.22	0.22
	dependent variable: **job destruction rate**				
Unemployment Benefits	0.0153***	0.0208***	0.013	0.0116*	0.0116*
	(0.005)	(0.007)	(0.008)	(0.006)	(0.006)
GDP growth				-0.000807***	-0.000807***
				(0.000)	(0.000)
Log of per-capita GDP				-0.008	-0.008
				(0.019)	(0.017)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.012	0.0618***	0.0742***	0.130	
	(0.013)	(0.024)	(0.025)	(0.150)	
Observations	530	530	496	496	496
Number of id	46	46	45	45	45
R-squared	0.31	0.26	0.24	0.27	0.27

Notes: Random and fixed effects regressions, estimated using yearly observations covering the period 1980-2002. For countries from the former Soviet Union and Central and Eastern Europe, we have dropped the year they begun their transition to market economies, the year immediately before and the year immediately after. The dependent variables (job creation, destruction and turnover) were calculated as explained in the text. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if UB schemes were in place in a given country-year and 0 otherwise. The coefficients reported in the first column were obtained from estimating the model using all country-year observations for which we were able to calculate job reallocation measures. The results reported in the other columns only include country-year observations for which data on the control variables (per-capita GDP, GDP growth and openness to trade) were available. Standard errors are reported in parenthesis. In the first 3 columns, standard errors are robust to arbitrary forms of heteroschedasticity. In the 4th column, they are adjusted for heteroschedasticity and first-order serial correlation. *,**,*** denote significance at 10%, 5%and 1%.

Table 7: Effect of the Introduction of Unemployment Benefits Schemes on Sector Shares (Excluding the first years of the transition for CEE and FSU countries)

	random effects robust s.e. (all observations)	fixed effects robust s.e. (all observations)	fixed effects robust s.e. (no missing controls)	fixed effects robust s.e.	fixed effects s.e. adjusted for heteroschedasticity and serial correlation
	(1)	(2)	(3)	(4)	(5)
	dependent variable: **agriculture share**				
Unemployment Benefits	0.030	-0.0239*	-0.0312*	-0.0426***	-0.0426**
	(0.024)	(0.014)	(0.016)	(0.016)	(0.017)
GDP growth				0.001	0.001
				(0.001)	(0.001)
Log of per-capita GDP				-0.103***	-0.103***
				(0.027)	(0.028)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.232***	0.712***	0.701***	1.515***	
	(0.087)	(0.140)	(0.140)	(0.250)	
Observations	530	530	496	496	496
Number of id	46	46	45	45	45
R-squared	0.8	0.58	0.58	0.59	0.59
	dependent variable: **industry share**				
Unemployment Benefits	-0.0568***	-0.006	-0.001	0.010	0.010
	(0.016)	(0.009)	(0.010)	(0.008)	(0.009)
GDP growth				0.000	0.000
				(0.000)	(0.000)
Log of per-capita GDP				0.0999***	0.0999***
				(0.014)	(0.015)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.148***	0.135**	0.131**	-0.656***	
	(0.042)	(0.061)	(0.059)	(0.130)	
Observations	530	530	496	496	496
Number of id	46	46	45	45	45
R-squared	0.7	0.81	0.73	0.77	0.77
	dependent variable: **services share**				
Unemployment Benefits	0.0490***	0.0280***	0.0335***	0.0335***	0.0335***
	(0.019)	(0.010)	(0.010)	(0.011)	(0.011)
GDP growth				0.000	-0.000355*
				(0.000)	(0.000)
Log of per-capita GDP				0.004	0.004
				(0.016)	(0.016)
Openness to Trade				0.000	0.000
				(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.354***	0.129	0.130	0.101	
	(0.050)	(0.080)	(0.081)	(0.140)	
Observations	530	530	496	496	496
Number of id	46	46	45	45	45
R-squared	0.87	0.72	0.71	0.71	0.71

Notes: Random and fixed effects regressions, estimated using yearly observations covering the period 1980-2002. For countries from the former Soviet Union and Central and Eastern Europe, we have dropped the year they begun their transition to market economies, the year immediately before and the year immediately after. The dependent variables (share of employment in agriculture, industrial sector and services) were calculated as explained in the text. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if UB schemes were in place in a given country-year and 0 otherwise. The coefficients reported in the first column were obtained from estimating the model using all country-year observations for which we were able to calculate job reallocation measures. The results reported in the other columns only include country-year observations for which data on the control variables (per-capita GDP, GDP growth and openness to trade) were available. Standard errors are reported in parenthesis. In the first 3 columns, standard errors are robust to arbitrary forms of heteroschedasticity. In the 4th column, they are adjusted for heteroschedasticity and first-order serial correlation. *,**,*** denote significance at 10%, 5%and 1%.

Table 8: Effect of the Introduction of Unemployment Benefits Schemes on Job Turnover and Sector Shares, including among the control group countries with UB schemes in place throughout the period

	job reallocation		
	job creation	job destruction	job turnover
	(1)	(2)	(3)
Unemployment Benefits (coefficient)	0.018	0.013	0.030
s.e. heteroschedasticity robust	(0.011)	(0.007)*	(0.013)**
s.e. heter. & autocorrel. robust	(0.011)	(0.007)*	(0.013)**
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes
Observations	986	986	986
Number of Countries	76	76	76
R-squared	0.21	0.29	0.20
	employment shares		
	agriculture	industry	services
	(1)	(2)	(3)
Unemployment Benefits (coefficient)	-0.038	0.010	0.0275
s.e. heteroschedasticity robust	(0.017)**	(0.008)	(0.012)**
s.e. heter. & autocorrel. robust	(0.019)**	(0.009)	(0.013)**
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes
Observations	986	986	986
Number of Countries	76	76	76
R-squared	0.58	0.80	0.75

Notes: Fixed effects regressions, estimated using yearly observations covering the period 1980-2002. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if unemployment benefits schemes were in place in a given country-year and 0 otherwise. Standard errors are reported in parenthesis. Levels of statistical significance are indicated by asterisks: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Unemployment Benefits and Structural Change, controlling for Employment Protection (EPL)

	job creation	job destruction	job turnover
	(1)	(2)	(3)
Unemployment Benefits	0.010	0.0127***	0.0225**
	(0.008)	(0.005)	(0.010)
Employment Protection	0.021	0.022	0.044
	(0.025)	(0.020)	(0.034)
GDP growth	0.000952***	-0.00104***	0.000
	(0.000)	(0.000)	(0.000)
log per capita GDP	-0.007	0.001	-0.005
	(0.007)	(0.004)	(0.008)
openness	0.000164***	0.000	0.000175***
	(0.000)	(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes
Constant	0.012	0.024	0.036
	(0.064)	(0.047)	(0.081)
Observations	400	400	400
Number of id	33	33	33
R2	0.39	0.33	0.37

	agriculture	industry	services
	(1)	(2)	(3)
Unemployment Benefits	-0.019	-0.0204*	0.0399**
	(0.017)	(0.011)	(0.018)
Employment Protection	0.231***	-0.147**	-0.0955*
	(0.056)	(0.058)	(0.055)
GDP growth	0.00130**	-0.000747*	0.000
	(0.001)	(0.000)	(0.001)
log per capita GDP	-0.263***	0.135***	0.125***
	(0.015)	(0.013)	(0.012)
openness	0.000	-0.000435***	0.000420***
	(0.000)	(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes
Constant	2.850***	-0.918***	-0.860***
	(0.170)	(0.120)	(0.120)
Observations	400	400	400
Number of id	33	33	33
R2	0.92	0.83	0.92

This table reports results of random effects regressions. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if unemployment benefits schemes were in place in a given country-year and 0 otherwise. The measure of EPL strictness is taken from Botero et al., 2004 (variable "index_labor7a" in the dataset available at http://iicd.som.yale.edu). Robust standard errors are reported in parentheses. Levels of statistical significance are indicated by asterisks: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Unemployment Benefits and Structural Change, by level of Employment Protection (EPL)

	job creation	job destruction	job turnover
Unemployment Benefits	(1)	(2)	(3)
EPL < 33th pctile	0.048*	0.013	0.061**
	(0.028)	(0.012)	(0.030)
33th pctile ≤ EPL ≤ 66th pctile	-0.004	0.004	0.000
	(0.012)	(0.007)	(0.017)
EPL > 66th pctile	0.013	0.052***	0.066***
	(0.013)	(0.016)	(0.018)
GDP growth	0.000843***	-0.00121***	0.000
	(0.000)	(0.000)	(0.000)
log per capita GDP	-0.022	0.001	-0.021
	(0.022)	(0.020)	(0.028)
openness	0.000344*	0.000	0.000440**
	(0.000)	(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes
Constant	0.167	0.057	0.224
	(0.180)	(0.160)	(0.230)
Observations	400	400	400
Number of id	33	33	33
R-squared	0.25	0.33	0.28

	agriculture	industry	services
Unemployment Benefits	(1)	(2)	(3)
EPL < 33th pctile	-0.013	0.015	0.007
	(0.025)	(0.016)	(0.015)
33th pctile ≤ EPL ≤ 66th pctile	-0.049***	0.002	0.044***
	(0.018)	(0.010)	(0.011)
EPL > 66th pctile	0.005	0.002	-0.009
	(0.010)	(0.009)	(0.010)
GDP growth	0.000831**	0.000	-0.000456**
	(0.000)	(0.000)	(0.000)
log per capita GDP	-0.106***	0.122***	-0.001
	(0.024)	(0.015)	(0.017)
openness	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Year fixed effects	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes
Constant	1.576***	-0.823***	0.112
	(0.240)	(0.140)	(0.150)
Observations	400	400	400
Number of id	33	33	33
R-squared	0.80	0.79	0.86

This table reports results of fixed effects regressions. The explanatory variable of interest, "Unemployment Benefits" is a dummy variable equal to 1 if unemployment benefits schemes were in place in a given country-year and 0 otherwise. The measure of EPL strictness is taken from Botero et al., 2004 (variable "index_labor7a" in the dataset available at http://iicd.som.yale.edu). Robust standard errors are reported in parentheses. Levels of statistical significance are indicated by asterisks: * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1: Job creation and destruction without (continuous line) and with (dotted line) UBs

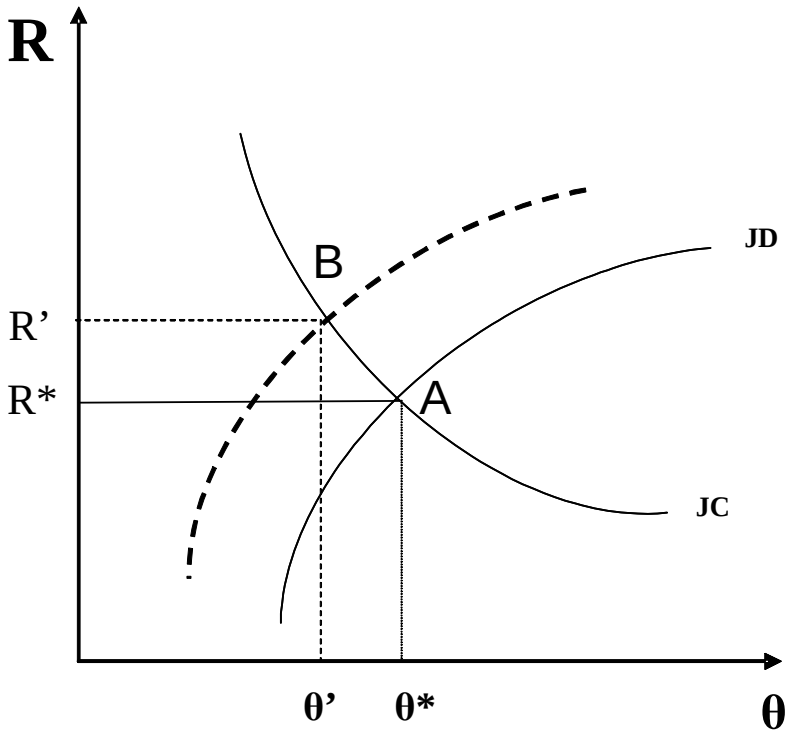


Figure 2: Adjustment to the long-run equilibrium following the introduction of UBs

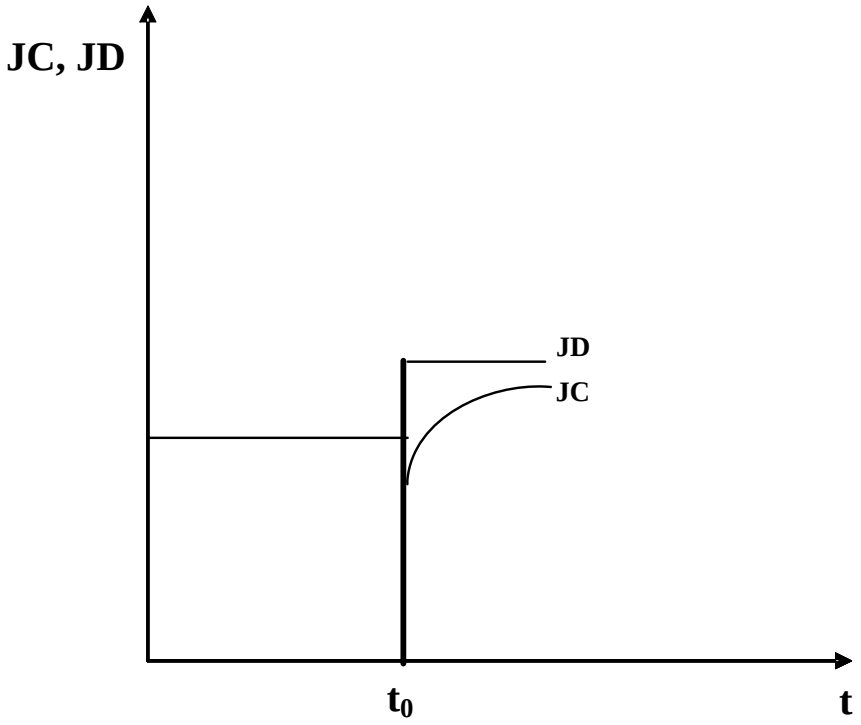


Figure 3: Count of Countries Adopting Unemployment Benefits Schemes, 1980-2000

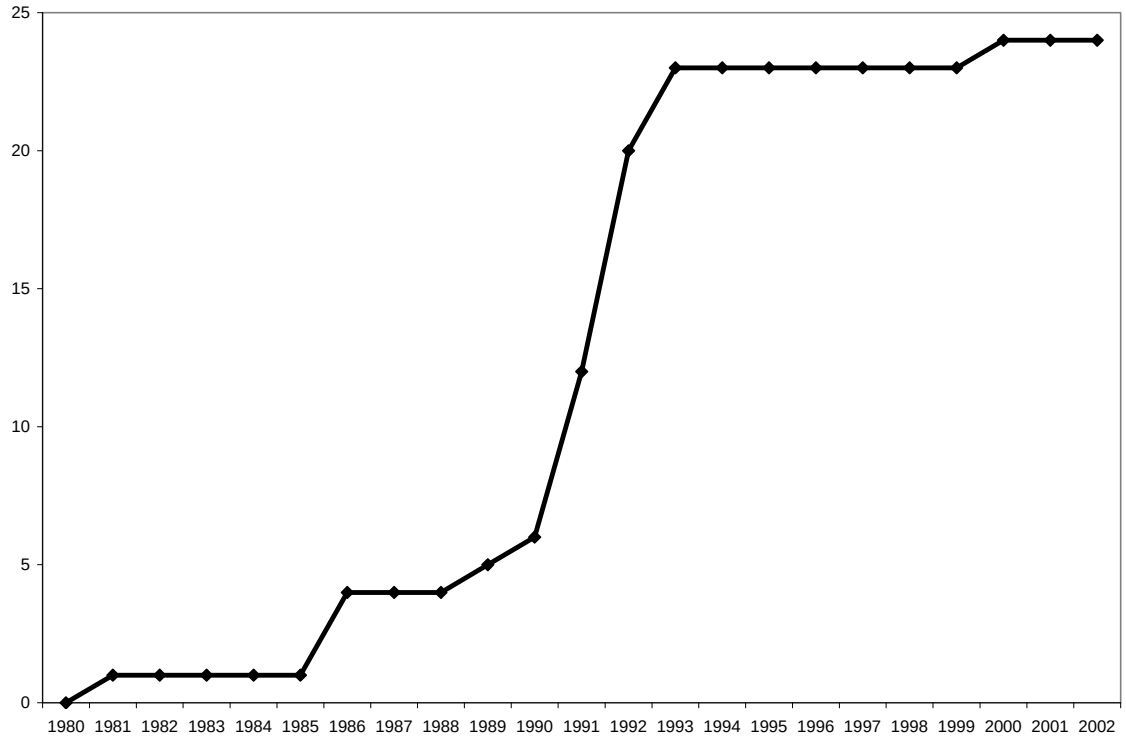
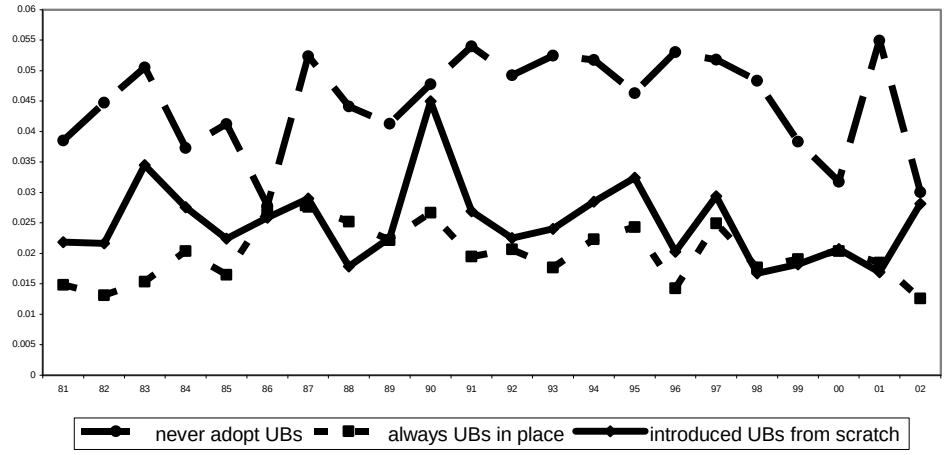
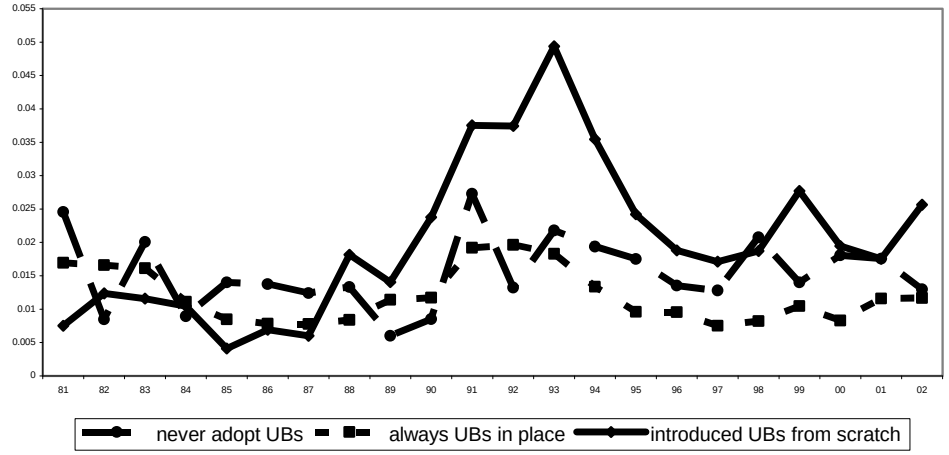


Figure 4: Evolution of Job Creation, Job Destruction & Job Turnover, 1981-2001

Rate of Job Creation



Rate of Job Destruction



Rate of Job Turnover

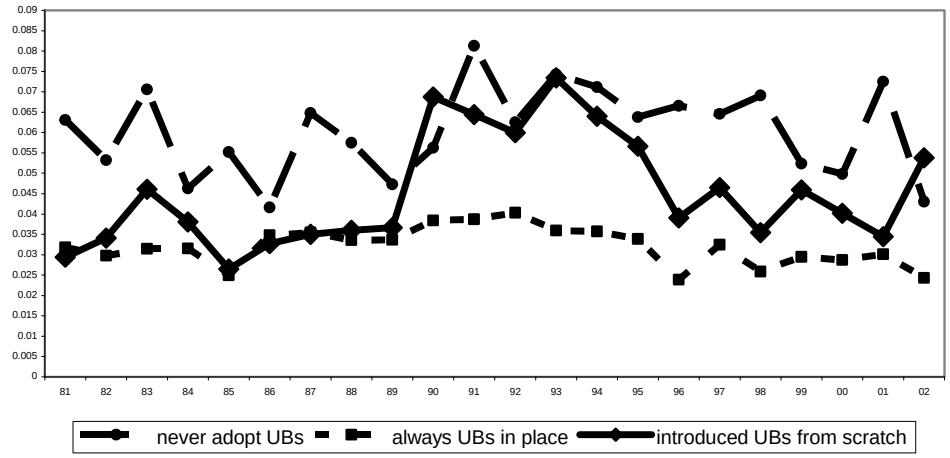
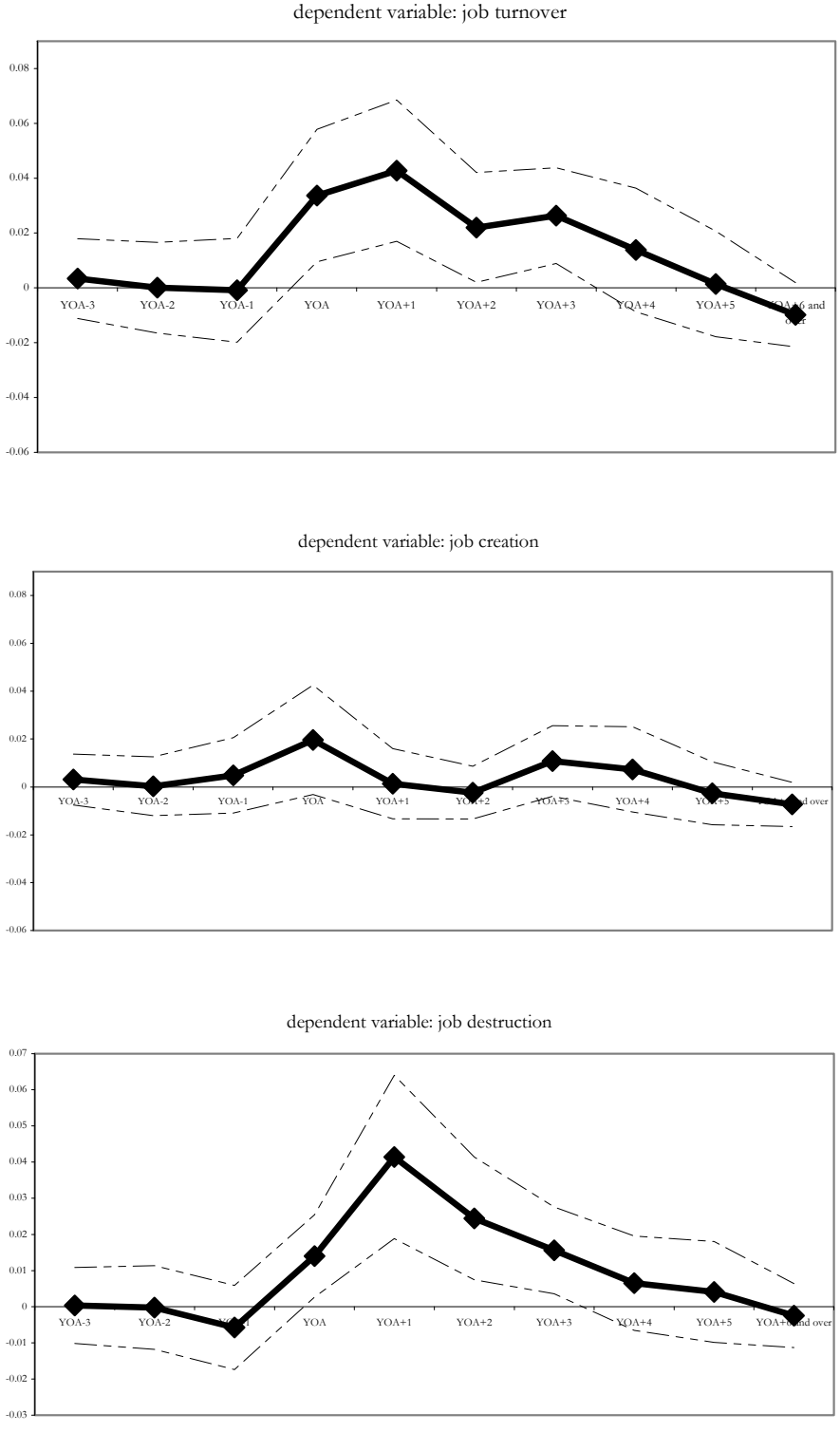
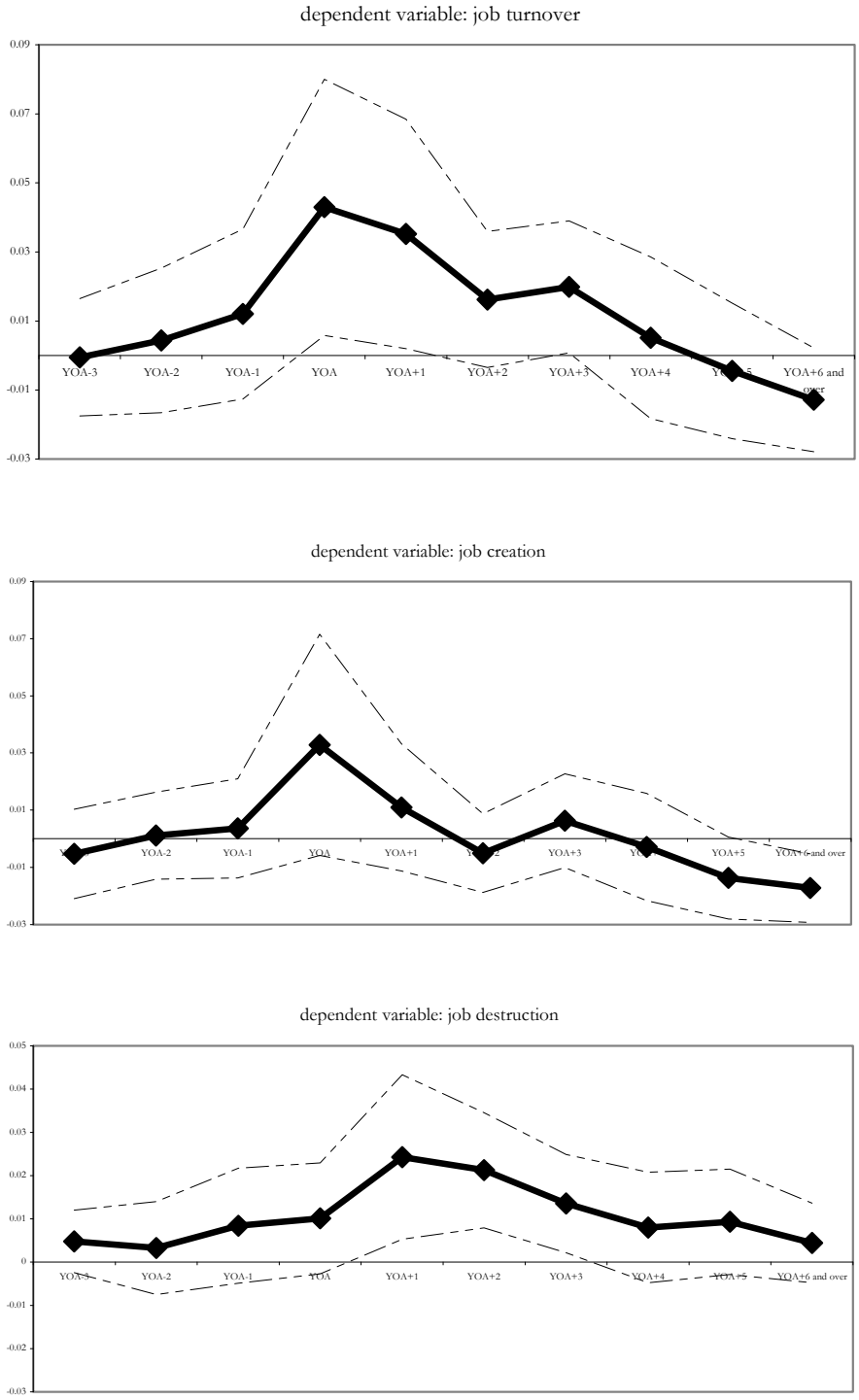


Figure 5A: Dynamic Effects of UBs on Job Creation, Job Destruction & Job Turnover – full sample



Notes: These figures display coefficients and confidence intervals obtained from estimation of model (3), as explained in the text. YOA = Year of Adoption of unemployment benefits schemes. The sample includes all observations for which we were able to compute job reallocation measures. Standard errors were adjusted for heteroschedasticity and first-order autocorrelation.

Figure 5B: Dynamic Effects of UBs on Job Creation, Job Destruction & Job Turnover – with control variables



Notes: These figures display coefficients and confidence intervals obtained from estimation of model (3), as explained in the text. Control variables (log of per capita GDP, GDP growth and openness to trade) were included in the regressions. YOA = Year of Adoption of unemployment benefits schemes. Controls include the log of per-capita GDP, GDP growth, and openness to trade. Standard errors were adjusted for heteroschedasticity and first-order autocorrelation.

Appendix Table A1 - Country-level Summary Statistics

Countries which Introduced Unemployment Benefits Schemes During the Period 1980-2002

	Log of per-capita GDP			GDP growth			Openness to Trade			Job Creation Rate			Job Destruction Rate			Agriculture share			Services share		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Albania	8	8.120	0.200	8	7.782	10.902	8	70.686	11.771	8	0.041	0.050	8	0.026	0.013	8	0.614	0.250	8	0.084	0.019
Argentina	12	9.178	0.195	12	0.603	5.851	12	18.144	4.962	12	0.029	0.020	12	0.023	0.023	12	0.007	0.003	12	0.643	0.046
Azerbaijan	8	7.953	0.273	7	0.426	15.444	8	95.624	14.898	17	0.025	0.016	17	0.019	0.016	17	0.337	0.040	17	0.376	0.060
Belarus	0			0			0			7	0.011	0.006	7	0.031	0.012	7	0.224	0.006	7	0.370	0.016
Brazil	14	8.615	0.192	14	0.828	4.020	14	14.967	4.764	14	0.029	0.016	14	0.010	0.011	14	0.271	0.025	14	0.522	0.033
Bulgaria	10	8.808	0.099	9	-0.503	5.920	10	90.249	24.803	20	0.011	0.009	20	0.028	0.032	20	0.244	0.023	20	0.388	0.048
China	15	7.796	0.463	15	8.298	3.264	15	43.660	6.713	15	0.038	0.038	15	0.008	0.009	15	0.514	0.049	15	0.120	0.016
Colombia	15	8.517	0.191	15	1.550	1.400	15	37.147	7.397	15	0.040	0.042	15	0.009	0.009	15	0.030	0.053	15	0.688	0.035
Czech Republic	9	9.456	0.117	9	2.022	1.581	9	113.564	24.054	9	0.011	0.008	9	0.016	0.009	9	0.073	0.010	9	0.535	0.017
Estonia	12	9.101	0.234	11	0.638	8.865	12	123.602	44.071	12	0.016	0.013	12	0.043	0.028	12	0.136	0.054	12	0.539	0.069
Georgia	4	8.287	0.087	4	3.751	2.267	4	63.079	6.259	4	0.037	0.030	4	0.045	0.054	4	0.510	0.024	4	0.376	0.001
Hungary	10	9.174	0.141	10	2.928	2.433	10	101.083	37.008	10	0.012	0.010	10	0.020	0.021	10	0.083	0.013	10	0.588	0.009
Korea, Republic of	22	9.058	0.573	22	6.353	4.181	22	50.487	14.957	22	0.035	0.013	22	0.014	0.012	22	0.187	0.079	22	0.499	0.075
Kyrgyzstan	10	8.054	0.139	9	0.469	10.759	10	109.251	25.317	16	0.030	0.020	16	0.020	0.019	16	0.440	0.076	16	0.373	0.015
Latvia	5	9.049	0.152	5	5.968	2.146	5	93.591	3.723	5	0.033	0.027	5	0.026	0.014	5	0.175	0.028	5	0.562	0.028
Lithuania	9	8.945	0.156	8	3.058	5.112	9	109.560	27.741	19	0.021	0.023	19	0.034	0.031	19	0.204	0.022	19	0.449	0.058
Moldova	3	7.794	0.097	3	5.893	4.842	3	135.452	8.783	3	0.020	0.021	3	0.015	0.006	3	0.506	0.007	3	0.351	0.005
Poland	19	8.639	0.325	19	2.079	4.160	19	33.644	19.368	19	0.011	0.009	19	0.027	0.026	19	0.269	0.044	19	0.408	0.074
Romania	22	8.433	0.130	22	0.185	5.548	22	49.841	13.224	22	0.022	0.022	22	0.029	0.037	22	0.363	0.052	22	0.285	0.019
Russia	5	9.084	0.181	5	5.510	4.810	5	67.524	3.224	5	0.035	0.025	5	0.013	0.014	5	0.148	0.017	5	0.581	0.011
Slovak Republic	8	9.117	0.111	8	3.695	1.201	8	132.213	17.452	8	0.019	0.011	8	0.020	0.012	8	0.091	0.016	8	0.535	0.020
Turkey	14	8.402	0.253	14	1.821	5.068	14	37.702	10.521	14	0.043	0.031	14	0.019	0.022	14	0.382	0.143	14	0.387	0.105
Ukraine	7	8.509	0.133	6	-5.437	9.963	7	96.488	24.812	12	0.015	0.018	12	0.046	0.047	12	0.231	0.030	12	0.175	0.045
Uruguay	11	8.980	0.212	11	2.176	4.911	11	31.949	5.522	11	0.020	0.018	11	0.015	0.008	11	0.048	0.007	11	0.676	0.020
Uzbekistan	4	8.080	0.062	4	3.093	2.425	4	61.028	4.859	4	0.012	0.004	4	0.003	0.003	4	0.398	0.011	4	0.349	0.003

Countries with No Unemployment Benefits Scheme Throughout the Period

	Log of per-capita GDP			GDP growth			Openness to Trade			Job Creation Rate			Job Destruction Rate			Agriculture share			Services share		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Bolivia	8	7.807	0.100	8	1.295	0.938	8	42.633	2.562	8	0.093	0.042	8	0.025	0.021	8	0.044	0.019	8	0.504	0.063
Costa Rica	21	8.656	0.289	21	0.788	3.723	21	67.330	20.468	21	0.051	0.038	21	0.014	0.012	21	0.239	0.042	21	0.499	0.025
Cuba	7	8.611	0.093	7	2.807	2.353	7	31.899	1.886	7	0.038	0.052	7	0.024	0.031	7	0.250	0.027	7	0.550	0.028
Dominican Republic	11	8.599	0.204	11	5.034	2.111	11	40.687	2.478	11	0.049	0.040	11	0.017	0.014	11	0.167	0.018	11	0.596	0.026
El Salvador	11	8.328	0.189	11	1.642	1.538	11	54.724	13.863	11	0.060	0.039	11	0.019	0.015	11	0.201	0.073	11	0.528	0.064
Honduras	9	7.510	0.214	9	0.392	3.691	9	96.064	6.475	9	0.051	0.041	9	0.023	0.033	9	0.404	0.174	9	0.393	0.148
Indonesia	16	7.998	0.286	16	3.299	3.871	16	76.981	10.288	16	0.044	0.021	16	0.015	0.013	16	0.493	0.055	16	0.356	0.032
Jamaica	9	8.408	0.017	9	-0.024	2.014	9	107.318	12.823	9	0.032	0.027	9	0.019	0.020	9	0.226	0.016	9	0.589	0.026
Malaysia	18	8.718	0.500	18	3.457	3.108	18	155.946	50.339	18	0.043	0.021	18	0.011	0.014	18	0.255	0.070	18	0.468	0.028
Mexico	7	8.920	0.094	7	2.352	2.472	7	55.873	7.762	7	0.044	0.025	7	0.014	0.011	7	0.199	0.024	7	0.509	0.011
Nicaragua	11	8.049	0.063	11	-0.974	4.592	11	59.261	14.531	11	0.040	0.019	11	0.006	0.008	11	0.415	0.020	11	0.392	0.008
Pakistan	22	7.515	0.288	22	2.097	1.801	22	31.475	2.612	22	0.038	0.023	22	0.015	0.018	22	0.493	0.030	22	0.315	0.035
Panama	18	8.671	0.268	18	0.766	3.288	18	145.072	21.223	18	0.045	0.033	18	0.014	0.015	18	0.232	0.050	18	0.542	0.037
Paraguay	11	8.249	0.137	11	0.243	2.279	11	51.453	18.574	11	0.049	0.039	11	0.011	0.009	11	0.016	0.005	11	0.427	0.010
Peru	5	8.330	0.019	5	0.355	2.708	5	34.732	1.123	5	0.055	0.026	5	0.013	0.010	5	0.075	0.014	5	0.693	0.012
Philippines	20	7.880	0.223	20	0.856	4.312	20	86.473	23.535	20	0.028	0.014	20	0.006	0.007	20	0.456	0.049	20	0.394	0.037
Saudi Arabia	3	9.650	0.019	3	-1.856	3.442	3	66.013	2.626	3	0.029	0.005	3	0.015	0.004	3	0.072	0.009	3	0.640	0.003
Singapore	17	9.657	0.456	17	3.601	4.417	17	292.313	59.753	17	0.034	0.026	17	0.012	0.012	17	0.006	0.004	17	0.659	0.039
Sri Lanka	7	8.047	0.145	7	4.586	1.434	7	78.880	4.324	7	0.079	0.037	7	0.045	0.020	7	0.406	0.030	7	0.352	0.019
Thailand	19	8.362	0.474	19	4.420	4.558	19	85.184	25.525	19	0.042	0.025	19	0.023	0.027	19	0.580	0.073	19	0.263	0.043
Vietnam	6	7.668	0.109	6	3.750	1.773	6	100.225	15.779	6	0.048	0.015	6	0.017	0.016	6	0.649	0.012	6	0.151	0.005

Appendix Table A1 - Country-level Summary Statistics (Continued)

Countries with Unemployment Benefits Schemes in place Throughout the Period

	Log of per-capita GDP			GDP growth			Openness to Trade			Job Creation Rate			Job Destruction Rate			Agriculture share			Services share		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Australia	17	9.838	0.302	17	1.903	1.969	17	35.554	7.491	17	0.016	0.016	17	0.008	0.007	17	0.065	0.009	17	0.700	0.038
Austria	19	9.913	0.244	19	2.051	1.298	19	72.744	11.543	19	0.016	0.014	19	0.010	0.007	19	0.077	0.012	19	0.582	0.044
Bangladesh	3	6.979	0.169	3	1.195	1.538	3	16.209	1.402	3	0.051	0.011	3	0.013	0.010	3	0.604	0.052	3	0.229	0.058
Belgium	10	9.933	0.148	10	2.069	1.027	10	144.711	13.200	10	0.016	0.011	10	0.007	0.004	10	0.026	0.005	10	0.660	0.023
Canada	21	9.805	0.265	21	1.670	2.616	21	62.465	14.161	21	0.017	0.014	21	0.008	0.010	21	0.057	0.010	21	0.716	0.024
Chile	19	8.850	0.419	19	3.769	3.514	19	47.978	10.555	19	0.033	0.021	19	0.008	0.008	19	0.186	0.028	19	0.589	0.035
Croatia	6	9.082	0.092	6	3.513	1.661	6	101.421	4.684	6	0.026	0.023	6	0.026	0.015	6	0.166	0.012	6	0.538	0.016
Denmark	17	9.941	0.221	17	1.832	1.840	17	69.658	11.454	17	0.014	0.013	17	0.013	0.007	17	0.049	0.011	17	0.681	0.020
Ecuador	12	8.334	0.045	12	-0.246	1.336	12	66.856	6.678	12	0.046	0.028	12	0.015	0.020	12	0.077	0.008	12	0.678	0.040
Egypt	11	8.214	0.201	11	2.528	1.065	11	45.208	6.073	11	0.040	0.026	11	0.028	0.032	11	0.328	0.041	11	0.455	0.039
Finland	21	9.676	0.265	21	1.648	3.815	21	54.304	12.955	21	0.009	0.007	21	0.016	0.018	21	0.092	0.025	21	0.618	0.048
Germany	11	10.037	0.097	11	1.202	1.247	11	55.841	9.364	11	0.011	0.015	11	0.018	0.013	11	0.036	0.006	11	0.617	0.025
Greece	20	9.227	0.246	20	1.237	2.319	20	39.959	10.077	20	0.014	0.007	20	0.013	0.009	20	0.236	0.048	20	0.507	0.065
Hong Kong	21	9.849	0.398	21	3.477	4.213	21	205.068	69.786	21	0.029	0.017	21	0.014	0.008	21	0.008	0.005	21	0.662	0.106
Ireland	18	9.532	0.427	18	5.082	3.383	18	120.109	37.815	18	0.030	0.025	18	0.014	0.010	18	0.128	0.035	18	0.575	0.027
Israel	18	9.541	0.321	18	2.175	2.749	18	56.843	8.387	18	0.029	0.020	18	0.009	0.007	18	0.039	0.013	18	0.650	0.027
Italy	20	9.713	0.278	20	1.957	1.032	20	42.273	9.025	20	0.011	0.006	20	0.010	0.008	20	0.089	0.028	20	0.582	0.030
Japan	22	9.769	0.310	22	1.976	2.152	22	16.164	2.289	22	0.012	0.007	22	0.010	0.012	22	0.070	0.018	22	0.590	0.029
Netherlands	14	9.969	0.187	14	2.298	1.468	14	112.234	16.277	14	0.019	0.007	14	0.007	0.005	14	0.039	0.007	14	0.709	0.017
New Zealand	16	9.728	0.177	16	1.389	2.140	16	61.297	7.220	16	0.019	0.012	16	0.012	0.012	16	0.100	0.009	16	0.656	0.017
Norway	19	9.935	0.317	19	2.458	1.721	19	68.199	6.660	19	0.011	0.006	19	0.010	0.010	19	0.068	0.015	19	0.693	0.034
Portugal	15	9.425	0.334	15	3.197	2.441	15	58.619	12.059	15	0.029	0.026	15	0.015	0.010	15	0.161	0.047	15	0.493	0.049
Slovenia	9	9.702	0.156	9	4.000	0.930	9	108.131	8.754	9	0.028	0.012	9	0.018	0.010	9	0.438	0.026	9	0.367	0.019
South Africa	2	9.098	0.037	2	2.573	0.159	2	51.319	0.237	2	0.021	0.024	2	0.049	0.036	2	0.155	0.018	2	0.538	0.012
Spain	22	9.446	0.353	22	2.420	1.854	22	38.875	13.825	22	0.025	0.018	22	0.013	0.013	22	0.125	0.048	22	0.548	0.045
Sweden	20	9.807	0.270	20	1.831	2.113	20	62.335	14.127	20	0.007	0.007	20	0.009	0.010	20	0.039	0.012	20	0.689	0.035
Switzerland	11	10.192	0.084	11	0.660	1.312	11	74.302	8.870	11	0.006	0.003	11	0.009	0.006	11	0.046	0.002	11	0.674	0.014
Taiwan	3	9.881	0.028	3	2.051	3.838	3	101.754	5.017	3	0.013	0.004	3	0.012	0.007	3	0.077	0.002	3	0.465	0.008
United Kingdom	22	9.708	0.321	22	2.339	1.795	22	44.674	8.330	22	0.013	0.018	22	0.013	0.013	22	0.029	0.008	22	0.675	0.037
United States	20	10.040	0.324	20	2.156	2.110	20	18.387	4.961	20	0.013	0.008	20	0.004	0.005	20	0.036	0.006	20	0.713	0.027
Venezuela	21	8.735	0.121	21	-1.107	5.073	21	35.620	6.191	21	0.037	0.015	21	0.009	0.008	21	0.138	0.024	21	0.623	0.036

Job Creation, Job Destruction and Job Turnover were calculated by the Authors based on ILO data, as described in the text. Data on per capita GDP, GDP growth and Trade Openness are from the Penn Tables version 6.2.