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Do unemployment benefits promote or hinder job reallocation?☆

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ABSTRACT

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. In this paper, we use a new set of yearly panel data from a large number of countries to evaluate empirically the relationship between unemployment benefits and job reallocation. 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 variable 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. These findings appear to be robust to changes in the countries in the sample, control variables or estimation methods. We discuss to what 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.

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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[2].

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 since 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) and stochastic job matching models (Acemoglu and Shimer, 1999, 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 important still, these models have different implications as to the effects of UBs on job creation and destruction, which could be possibly tested empirically. Because of the critical role that job reallocation plays in fostering productivity growth (Caballero and Hammour, 2000; Krizan, Haltiwanger and Foster, 2002; Bartelsman et al., 2004), understanding whether in fact unemployment benefits help or hinder job reallocation stands out as an important task with far-reaching policy implications notably in countries with high productivity dispersion[3].

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

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[2] Atkinson and Micklewright (1991) and Krueger and Meyer (2002) provide excellent surveys of this rich and insightful literature.

[3] Brown and Earle (2002, 2008) as well as Konings and De Loecker (2006) document that Schumpeterian creative destruction contributed significantly to productivity growth in emerging countries coming from central planning, with a large share of the population initially employed in low-productivity jobs and in the presence of a new dynamic business sector.

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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 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 a 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, 1992; 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, 2003; Algan and Cahuc, 2006) 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 on 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, 2002; Layard et al., 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 and 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 the primary sector (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 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 variable 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 et al. (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 1–2 percentage point increases in the yearly rate of job destruction and a 2.5–3 percentage point increases 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 that introduced UBs also experienced 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 2 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 according to this literature, motivating our empirical strategy. Section 3 describes the data and the outcome variables in detail, presents some preliminary descriptive evidence and outlines our empirical strategy. In Section 4, we present and discuss the results of the empirical analysis. Finally, in Section 5 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 Fig. 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 Fig. 2. At the same time, employees endowed with a higher

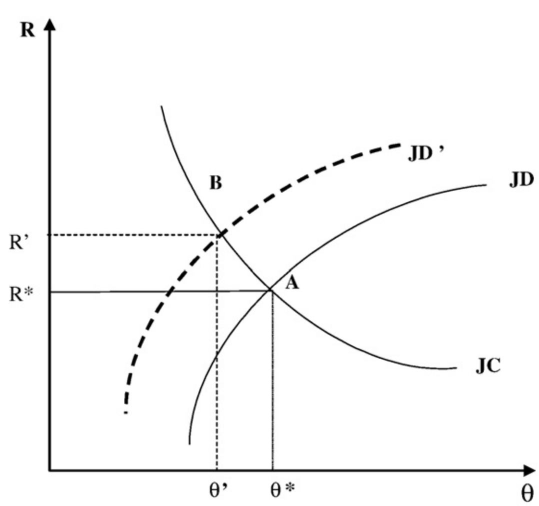


Fig. 1. Job creation and destruction without (continuous line) and with (dotted line) UBs.

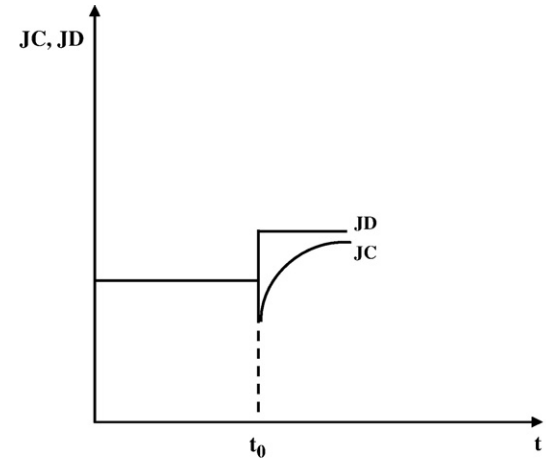


Fig. 2. Adjustment to the long-run equilibrium following the introduction of UBs.

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, fewer vacancies are created, and gross job creation declines. Since job destruction increases and job creation declines, unemployment starts rising. Given that the number of jobseekers increases, *total* outflows from unemployment, hence gross job creation (the left-hand-side of Eq. (1), gradually recovers, approaching job destruction from below at the new steady state equilibrium. The latter is depicted as the point B in Fig. 1. It involves a higher R, hence a higher job destruction *rate* by Eq. (2). The job creation *rate* is also larger at the new steady state equilibrium as the unemployment to employment rate, in the left-hand-side of Eq. (2), increases. 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 shifts upward 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 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 for benefits. The introduction of UBs increases job creation in this group. Since 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 *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. Due to matching frictions, 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.

Finally, *stochastic job matching models* (Marimon and Zilibotti, 1999; Acemoglu and Shimer, 1999, 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 of 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. 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 these models imply higher rates of job creation and destruction after the introduction of UBs.

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 these features into a scalar 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 since 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, 1996; Nickell, 1997; Blanchard and Wolfers, 2002; Layard et al., 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 have short duration and cover only 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 while 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 and 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.[4] 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 around. 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 et al. (2002) documented that neglecting policy endogeneity involves a significant overestimate of 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 evaluate the empirical relevance of the predictions of models treating UBs as a one-dimensional institution, since 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 involving regulatory reforms.

For these reasons, in this paper we compare outcomes of countries *with* and *without* unemployment benefits *before* and *after* the reforms

[4] Earnings-related UBs offered at replacement rates decreasing over time also reduce 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 employers need to compensate more their uninsured workers (Boeri, 2000).

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. Moreover, we concentrate on labor market flows, which, unlike aggregate stocks, are sensitive to institutional reforms, even in the short run. Institutional interactions can be taken into account in our framework, provided that other institutions are not altered at the time in which the UB system is introduced. Finally, access to longitudinal data enables us to deal with reverse causality issues.

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 sectoral growth rates. As explained in the previous section, the effect of UB on job turnover operates through different mechanisms according to different theories. This is why, 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 cross-industry job reallocation. In order to better capture genuine sectoral reallocation effects, we shall also consider as outcome variables the employment shares of the primary sector (proxying low-productivity jobs), industry, and the services sector.[5]

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.[6] For all European countries this information was double checked with entries in the Mutual Information System on Social Protection (MISSOC) database[7]. For all OECD countries another crosscheck was made on the basis of the OECD Tax and Benefits publications. For all remaining countries the key source of primary information or cross-check was the ILO Natlex database. In case of inconsistencies between primary and secondary sources, we interviewed labor economists operating in these countries or officials from the local Ministries of Labor. In this paper we use for the first-time this unique dataset.

Table 1
List of industries.

ISIC Rev. 2 code	Description
1	Agriculture, hunting, forestry, and fishing
2	Mining and quarrying
3	Manufacturing
4	Electricity, gas, and water
5	Construction
6	Wholesale, retail trade and restaurants and hotel
7	Transport, storage, and communication
8	Financing, insurance, real estate, and business services
9	Community, social, and personal services

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

Employment data were taken from the ILO LABORSTA database[8]. Our main sample consists of 48 countries which did not have any unemployment insurance scheme in place as of 1980. Of these, 27 countries introduced UBs for the first time between 1980 and 2002.[9] We construct the job flow rates defined above using 1-digit ILO sector level employment data. As shown in Table 1, these are 9 broad sectors of economic activity.[10] In principle, the ILO data span over a 22-year period, from 1980 to 2002. However, the ILO series are complete only for a subset of countries so that the panel is unbalanced. The actual number of observations per country varies between 4 and 23.[11] 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. Fig. 3 plots the number of countries that introduced UB schemes during the time period of 1980 to 2002 (at yearly frequencies), and Table 2 reports, for each country, the year of UB 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

[5] It should be kept in mind that primary sector jobs are just a rough proxy for low-productivity job. In fact, some middle-income countries, such as Argentina and Uruguay, have strong comparative advantages in highly productive agricultural activities.

[6] 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. See http://www.ssa.gov/policy/docs/progdesc/ssptw/.

[7] See http://www.missoc.org.

[8] See http://laborsta.ilo.org.

[9] 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.

[10] We recognize that job flow rates constructed using firm- or plant-level data would provide a more accurate picture of the actual extent of job turnover (Davis and Haltiwanger 1990). However, our focus in this paper is on low- and middle-income countries, for which detailed sector-level employment data are rarely available. Therefore, in our attempt to include as many countries as possible in our dataset, we were forced to use aggregate data.

[11] 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. We have also dropped observations presenting large, erratic changes from year to year in the outcome variables.

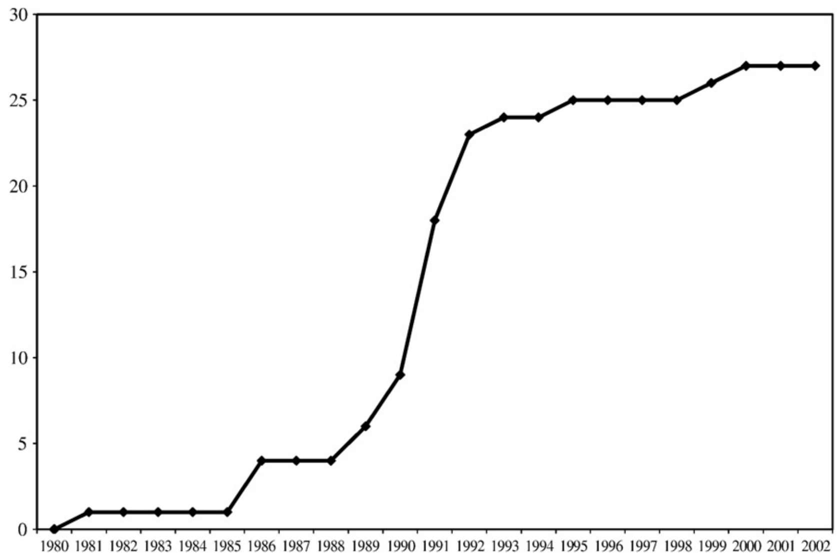


Fig. 3. Count of countries adopting unemployment benefit schemes, 1980–2002.

that never adopted UBs, and (c) countries which had UBs in place throughout the period of analysis. The three panels of Fig. 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 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 Fig. 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 Fig. 4 might be spurious. First, the averages plotted in Fig. 4 were obtained from a variable number of countries each year, due to limitations in the available data (see Table 2 and Appendix Table 1).[12] 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 regardless 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.[13] 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, higher GDP growth rates and a lower degree of trade openness than "untreated" countries. 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

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	1992	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	1990	0	9
Estonia	1990	2002	1991	1	11
Georgia	1999	2002	1991	0	4
Hungary	1992	2001	1986	0	10
Korea, Republic of	1981	2002	1995	14	8
Kyrgyzstan	1987	2002	1991	4	12
Latvia	1997	2002	1992	0	5
Lithuania	1983	2001	1991	8	11
Moldova	2000	2002	1992	0	3
Poland	1982	2002	1990	8	11
Romania	1981	2002	1991	10	12
Russia	1998	2002	1991	0	5
Slovak Republic	1995	2002	1991	0	8
Taiwan	2000	2002	1999	0	3
Turkey	1983	2002	2000	12	2
Ukraine	1988	2000	1991	3	9
Uruguay	1987	2000	1981	0	11
Uzbekistan	1996	1999	1992	0	4
Venezuela	1982	2002	1989	7	14

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. Our main source of information on the date of introduction of UBs is "Social Security Programs throughout the World" (2002–2004).

[12] In our empirical analysis, we follow Heckman and Pages (2004) 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.

[13] 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 1991 and the "after" period years after (and including) 1991. As can be seen in Table 2, the year 1991 is the modal year of adoption of UBs in the group of countries which introduced UBs schemes between 1980 and 2002.

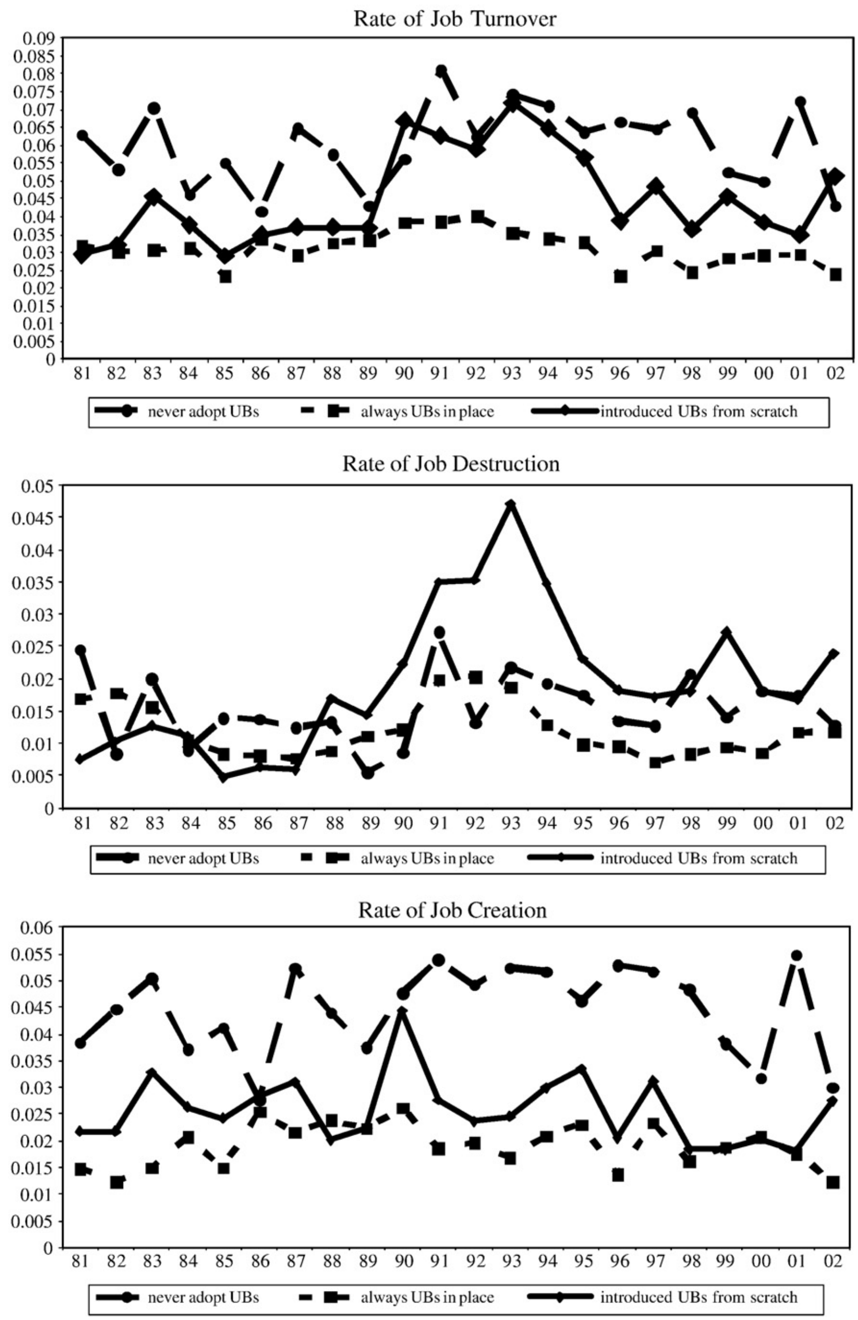


Fig. 4. Evolution of turnover, 1981–2001.

these countries come from central planning.[14] 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 Hotz, 1989 and more recently Brown, Earle and Telegdy, 2006).

[14] 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.

3.4. Methodology

We implement 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:

$$Y_{it} = UB_{it}\theta + \mathbf{X}_{it}\beta + \mu_t + \alpha_i + \tau_i t + u_{it} \quad (3)$$

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

Table 3
Summary statistics of outcome and control variables.

	Before			After		
Variable	Obs	Mean	Std. dev	Obs	Mean	Std. dev
Panel A: Countries which introduced UBs during the period						
Log of per-capita GDP	62	8.50	0.32	218	8.73	0.54
GDP growth	61	2.55	4.76	213	2.21	6.63
Openness to trade	62	32.7	14.2	218	71.8	39.5
EPL strictness	84	0.51	0.13	189	0.53	0.16
Job creation rate	95	0.026	0.020	232	0.025	0.025
Job destruction rate	95	0.013	0.015	232	0.025	0.027
Job turnover rate	95	0.039	0.024	232	0.050	0.033
Agriculture share	95	0.258	0.110	232	0.238	0.178
Industry share	95	0.315	0.084	232	0.254	0.087
Services share	95	0.399	0.118	232	0.460	0.174
Panel B: Countries without UBs throughout the period						
Log of per-capita GDP	79	8.08	0.61	176	8.47	0.59
GDP growth	79	2.01	3.95	176	2.01	3.44
Openness to trade	79	89.2	64.9	176	94.0	70.8
EPL strictness	57	0.39	0.15	125	0.45	0.16
Job creation rate	79	0.042	0.029	176	0.047	0.033
Job destruction rate	79	0.013	0.020	176	0.017	0.017
Job turnover rate	79	0.055	0.031	176	0.064	0.040
Agriculture share	79	0.333	0.220	176	0.291	0.171
Industry share	79	0.200	0.072	176	0.205	0.053
Services share	79	0.417	0.128	176	0.469	0.128
Panel C: Countries with UBs in place throughout the period						
Log of per-capita GDP	163	9.33	0.49	290	9.77	0.52
GDP growth	163	2.40	2.59	290	2.15	2.39
Openness to trade	163	48.7	30.2	290	75.8	49.5
EPL strictness	160	0.46	0.22	290	0.48	0.21
Job creation rate	163	0.020	0.017	290	0.019	0.017
Job destruction rate	163	0.011	0.011	290	0.012	0.013
Job turnover rate	163	0.032	0.017	290	0.031	0.022
Agriculture share	163	0.112	0.100	290	0.090	0.091
Industry share	163	0.294	0.057	290	0.257	0.052
Services share	163	0.589	0.091	290	0.638	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 1991 and the after period years after (and including) 1991. The year 1991 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).

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[15] and the degree of openness to trade.[16] The inclusion of country effects takes care of unobservable heterogeneity possibly correlated with UBs. We will be implementing both random effects and fixed effects specifications. 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 $UB_{i1}, UB_{i2}, \ldots, UB_{iT}$). 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.[17] The inclusion of country-specific time trends 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 et al. (2006). In addition 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. We recognize that, in our setting, positive serial correlation in the error term might cause the standard errors to be understated (Bertrand et al., 2004).[18] Therefore, in all our regressions we adjust the standard errors to allow for arbitrary forms of heteroschedasticity and serial correlation by clustering at the country level.

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):

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

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.[19,20]

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). Columns (1) and (3) in each panel show results of random effects regressions, while the remaining columns report results of fixed effects specifications. The random effects regressions exploit both within- and between-country variation. Using both types of variation allows us to make use of all the available data. In fact, as shown in Table 2, data for both the "before" and "after" periods are available only for 15 of the 27 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. In all cases, year fixed effects and country-specific time trends are included among the regressors.[21] 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. The standard errors, reported in parentheses below the estimates, are robust to arbitrary forms of heteroschedasticity and autocorrelation (clustered by country). It is worth noting that our specification is very demanding of the data, given that we have a limited number of observations per country.

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

[16] 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.

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

[18] In particular, our policy variable (represented by a dummy set to zero up to the time of UB adoption and set to one thereafter) is by construction very much serially correlated.

[19] Due to data limitations, we have defined the dummy $UB_{i,t0+6}$ to be equal to 1 in years t_0+6, t_0+7, etc.

[20] This specification will also reveal whether the effects of introducing UBs kick in with a lag, which could be the case, e.g., in the presence of adjustment costs.

[21] 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 0.001.

Table 4
Effect of the introduction of unemployment benefit schemes on job creation, destruction, and job turnover.

	Random effects all observations (1)	Fixed effects all observations (2)	Random effects with controls (3)	Fixed effects with controls (4)	Fixed effects no missing controls (5)
Dependent variable: job turnover rate					
Unemployment benefits	0.031***	0.035***	0.022**	0.021*	0.025**
	(0.008)	(0.010)	(0.009)	(0.011)	(0.012)
GDP growth			0.000	0.000	
			(0.000)	(0.000)	
Log of per-capita GDP			−0.006	−0.050	
			(0.007)	(0.025)	
Openness to trade			0.0001**	0.0003*	
			(0.0001)	(0.0002)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.054***	0.069***	0.096	0.449**	0.081***
	(0.014)	(0.013)	(0.065)	(0.190)	(0.015)
Observations	582	582	529	529	529
Number of id	48	48	47	47	47
R-squared	0.30	0.21	0.30	0.21	0.19
Dependent variable: job creation rate					
Unemployment benefits	0.014**	0.013	0.011	0.012	0.013
	(0.007)	(0.009)	(0.007)	(0.010)	(0.011)
GDP growth			0.0007***	0.0006*	
			(0.0003)	(0.0003)	
Log of per-capita GDP			−0.003	−0.028	
			(0.007)	(0.022)	
Openness to trade			0.0001*	0.0003	
			(0.0001)	(0.0002)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.034***	0.041***	0.050	0.247	0.044***
	(0.007)	(0.007)	(0.055)	(0.170)	(0.007)
Observations	582	582	529	529	529
Number of id	48	48	47	47	47
R-squared	0.30	0.18	0.31	0.21	0.19
Dependent variable: job destruction rate					
Unemployment benefits	0.017***	0.022***	0.012**	0.009*	0.0124*
	(0.005)	(0.007)	(0.005)	(0.006)	(0.007)
GDP growth			−0.0009***	−0.0008***	
			(0.0002)	(0.0003)	
Log of per-capita GDP			−0.003	−0.023	
			(0.005)	(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	0.020	0.028*	0.046	0.202	0.037**
	(0.013)	(0.014)	(0.039)	(0.140)	(0.015)
Observations	582	582	529	529	529
Number of id	48	48	47	47	47
R-squared	0.30	0.23	0.32	0.26	0.22

Notes: Random and fixed effects regressions, estimated using yearly observations covering the period 1980–2002 (unbalanced panel). 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 two columns 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 (clustered by country) are reported in parenthesis. Levels of statistical significance are indicated by asterisks: *Significant at 10%; **Significant at 5%; ***Significant at 1%.

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 implies that the adoption of UBs is associated with a 3 percentage points higher job turnover rate, and the coefficient is statistically significant at the 1% confidence level. Column (2) reports results of a fixed effects specification, again using all observations in the sample. Once again, the coefficient on the UB dummy is positive and strongly significant. A Hausman test of fixed effects against random effects reveals no statistically significant differences in the estimated coefficients across the two specifications (chi2 = 60.68, p-value = 0.80), suggesting that the random effects estimator is consistent. For completeness, in what follows we will report results of both random effects and fixed effects specifications. The coefficient estimates reported in the first and second column of the second and third panel of Table 4 indicate that the higher job turnover is due to an increase in both the job creation rate and (especially) the job destruction rate. In columns (3) and (4) we include controls for the log of per-capita GDP, the GDP growth rate and the degree of trade openness. Once again, the results are fairly consistent across specifications (a Hausman test comparing the random effects with the fixed effects specification once again failed to detect statistically significant differences in the estimated coefficients). The coefficient on the UB dummy indicates that the introduction of unemployment benefits is associated with a 2 percentage point higher rate of job turnover. The first and second panels 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. To gain more insight on the reason why the estimated coefficients dropped in magnitude compared with the regressions without controls, in column (5) we report the results of a fixed effects specification without controls on a sample that uses only the country-year observations for which the control variables were not missing. The magnitude of the UB coefficient in column (5) is very close to that of the coefficients in columns (3) and (4). This indicates that the drop in magnitude of the UB coefficient is most likely attributable to the missing observations rather than by the inclusion of the controls. In what follows, therefore, we will report results both with and without controls.

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, columns (1) and (3) in each panel show the estimated coefficients of random effects versions of model (3), and columns (2), (4) and (5) those of fixed effects specifications. Although our estimates are often not statistically significant (or only marginally significant), they seem to 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 3–4 percentage points.

Table 5
Effect of the introduction of unemployment benefit schemes on sector shares.

	Random effects all observations (1)	Fixed effects all observations (2)	Random effects with controls (3)	Fixed effects with controls (4)	Fixed effects no missing controls (5)
Dependent variable: agriculture share					
Unemployment benefits	0.026	−0.025	−0.032	−0.040	−0.032
	(0.035)	(0.026)	(0.027)	(0.034)	(0.036)
GDP growth			0.001	0.001	
			(0.001)	(0.001)	
Log of per-capita GDP			−0.253***	−0.074*	
			(0.032)	(0.044)	
Openness to trade			0.0000	−0.0004	
			(0.0003)	(0.0003)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.40***	0.32***	2.36***	0.92***	0.33***
	(0.061)	(0.022)	(0.260)	(0.340)	(0.022)
Observations	582	582	529	529	529
Number of id	48	48	47	47	47
R-squared	0.80	0.55	0.88	0.55	0.54
Dependent variable: industry share					
Unemployment benefits	−0.049**	−0.008	−0.018	0.003	−0.005
	(0.022)	(0.013)	(0.023)	(0.017)	(0.019)
GDP growth			−0.0004	−0.0002	
			(0.0006)	(0.0003)	
Log of per-capita GDP			0.113***	0.084***	
			(0.029)	(0.025)	
Openness to trade			−0.0002	0.0001	
			(0.0003)	(0.0002)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.26***	0.24***	−0.62***	−0.42**	0.23***
	(0.035)	(0.013)	(0.220)	(0.190)	(0.012)
Observations	582	582	529	529	529
Number of id	48	48	47	47	47
R-squared	0.69	0.79	0.84	0.75	0.72
Dependent variable: services share					
Unemployment benefits	0.034	0.028*	0.044*	0.037*	0.038*
	(0.025)	(0.015)	(0.027)	(0.020)	(0.020)
GDP growth			0.0000	−0.0003	
			(0.0008)	(0.0003)	
Log of per-capita GDP			0.139***	−0.019	
			(0.024)	(0.023)	
Openness to trade			0.0003	0.0002	
			(0.0002)	(0.0002)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.35***	0.39***	−0.75***	0.53***	0.39***
	(0.039)	(0.016)	(0.170)	(0.180)	(0.017)
Observations	582	582	530	529	529
Number of id	48	48	47	47	47
R-squared	0.87	0.71	0.91	0.69	0.69

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 two columns 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 (clustered by country) are reported in parenthesis. 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 benefit schemes on job creation, destruction and job turnover (excluding the first years of the transition for CEE and FSU countries).

	Random effects all observations (1)	Fixed effects all observations (2)	Random effects with controls (3)	Fixed effects with controls (4)	Fixed effects no missing controls (5)
Dependent variable: job turnover rate					
Unemployment benefits	0.032***	0.036**	0.024**	0.024*	0.027*
	(0.010)	(0.014)	(0.011)	(0.013)	(0.014)
GDP growth			−0.0002	−0.0001	
			(0.0003)	(0.0004)	
Log of per-capita GDP			−0.006	−0.043	
			(0.008)	(0.026)	
Openness to trade			0.0001**	0.0004*	
			(0.0001)	(0.0002)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.054***	0.072***	0.096	0.389*	0.083***
	(0.014)	(0.013)	(0.067)	(0.200)	(0.015)
Observations	553	553	519	519	519
Number of id	48	48	47	47	47
R-squared	0.30	0.20	0.30	0.21	0.20
Dependent variable: job creation rate					
Unemployment benefits	0.017**	0.018	0.012	0.015	0.016
	(0.008)	(0.011)	(0.008)	(0.012)	(0.013)
GDP growth			0.0007**	0.0007**	
			(0.0003)	(0.0004)	
Log of per-capita GDP			−0.003	−0.038	
			(0.007)	(0.024)	
Openness to trade			0.0001*	0.0004*	
			(0.0001)	(0.0002)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.034***	0.041***	0.052	0.317*	0.044***
	(0.007)	(0.006)	(0.056)	(0.180)	(0.007)
Observations	553	553	519	519	519
Number of id	48	48	47	47	47
R-squared	0.30	0.19	0.31	0.22	0.20
Dependent variable: job destruction rate					
Unemployment benefits	0.015***	0.018**	0.012**	0.009*	0.011*
	(0.005)	(0.007)	(0.005)	(0.005)	(0.006)
GDP growth			−0.0008***	−0.0008***	
			(0.0002)	(0.0003)	
Log of per-capita GDP			−0.003	−0.005	
			(0.005)	(0.014)	
Openness to trade			0.0000	0.0000	
			(0.0000)	(0.0001)	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes
Constant	0.020	0.032**	0.043	0.072	0.040**
	(0.013)	(0.014)	(0.038)	(0.100)	(0.015)
Observations	553	553	519	519	519
Number of id	48	48	47	47	47
R-squared	0.29	0.25	0.30	0.27	0.24

Notes: Random and fixed effects regressions, estimated using yearly observations covering the period 1980–2002 (unbalanced panel). 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. 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 coefficients reported in the first two columns 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 (clustered by country) are reported in parenthesis. Levels of statistical significance are indicated by asterisks: *Significant at 10%; **Significant at 5%; ***Significant at 1%.

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, a UB system before starting the transition. To lessen this concern, we replicated the analyses of Tables 4 and 5 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. As reported in Table 6, the introduction of unemployment benefit schemes is associated with a 2–3 percentage points higher rate of job turnover, especially due to

Table 7
Effect of the introduction of unemployment benefit schemes on job turnover (including among the "control group" countries with UB schemes in place throughout the period).

	Random effects all observations (1)	Fixed effects all observations (2)	Random effects with controls (3)	Fixed effects with controls (4)
Dependent variable: job turnover rate				
Unemployment benefits	0.001	0.038***	0.010	0.024*
	(0.005)	(0.010)	(0.008)	(0.012)
GDP growth			−0.0002	−0.0002
			(0.0002)	(0.0003)
Log of per-capita GDP			−0.020***	−0.037*
			(0.006)	(0.018)
Openness to trade			0.0002***	0.0003**
			(0.0001)	(0.0002)
Year fixed effects	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes
Constant	0.041***	0.038***	0.201***	0.335**
	(0.008)	(0.007)	(0.050)	(0.150)
Observations	1035	1035	982	982
Number of id	77	77	76	76
R-squared	0.36	0.20	0.41	0.20
Dependent variable: job creation rate				
Unemployment benefits	−0.006	0.016*	0.001	0.014
	(0.004)	(0.008)	(0.005)	(0.011)
GDP growth			0.0008***	0.0008***
			(0.0002)	(0.0003)
Log of per-capita GDP			−0.015***	−0.021
			(0.005)	(0.015)
Openness to trade			0.0002**	0.0003
			(0.0001)	(0.0002)
Year fixed effects	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes
Constant	0.027***	0.019***	0.141***	0.188
	(0.005)	(0.005)	(0.041)	(0.130)
Observations	1035	1035	982	982
Number of id	77	77	76	76
R-squared	0.33	0.18	0.38	0.20
Dependent variable: job destruction rate				
Unemployment benefits	0.007***	0.022***	0.009**	0.010*
	(0.002)	(0.007)	(0.004)	(0.006)
GDP growth			−0.001***	−0.0001***
			(0.0002)	(0.0002)
Log of per-capita GDP			−0.005	−0.015
			(0.004)	(0.014)
Openness to trade			0.0000	0.0001
			(0.0000)	(0.0001)
Year fixed effects	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes
Constant	0.015**	0.019**	0.059*	0.148
	(0.007)	(0.008)	(0.031)	(0.110)
Observations	1035	1035	982	982
Number of id	77	77	76	76
R-squared	0.30	0.24	0.33	0.29

Notes: Fixed effects regressions, estimated using yearly observations covering the period 1980–2002 (unbalanced panel). The dependent variables 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. Standard errors (clustered by country) are reported in parenthesis. Levels of statistical significance are indicated by asterisks: *Significant at 10%; **Significant at 5%; ***Significant at 1%.

higher job destruction rates. Our point estimates also detect a higher job creation rate, but the estimates are not statistically significant.[22]

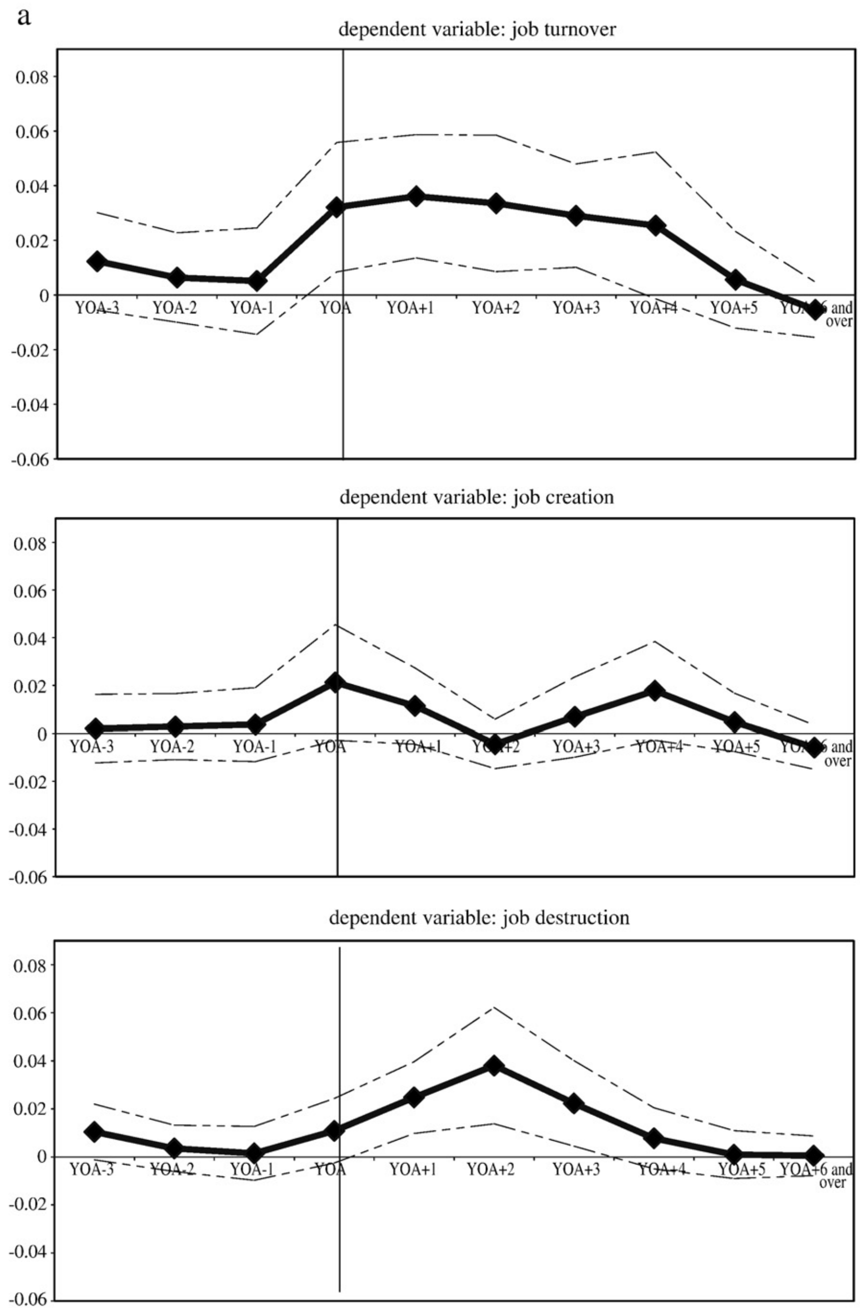
4.2.2. Including in the "control" group countries with UB systems in place throughout the period

In Table 7, 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. Because our premise is that the *introduction* of UBs has the potential to affect the rates of job turnover and sectoral 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 7 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 Fig. 5, we display the estimated coefficients from model (4) for job turnover, job creation and destruction, as well as the associated confidence intervals. Fig. 5a reports results obtained from the full sample, and Fig. 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 clustered by country. The results displayed in Fig. 5a and B show positive effects of UBs on job turnover starting the year of UB introduction, while the coefficients on the years before are

[22] We also confirmed that the introduction of UBs was associated with a reduced agriculture share and an increased services share, although, as before, the coefficients were only marginally statistically significant.



Notes: These figures display coefficients and confidence intervals obtained from estimation of model (3), as explained in the text (the model includes year fixed effects and country-specific time trends). 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 clustered by Country.

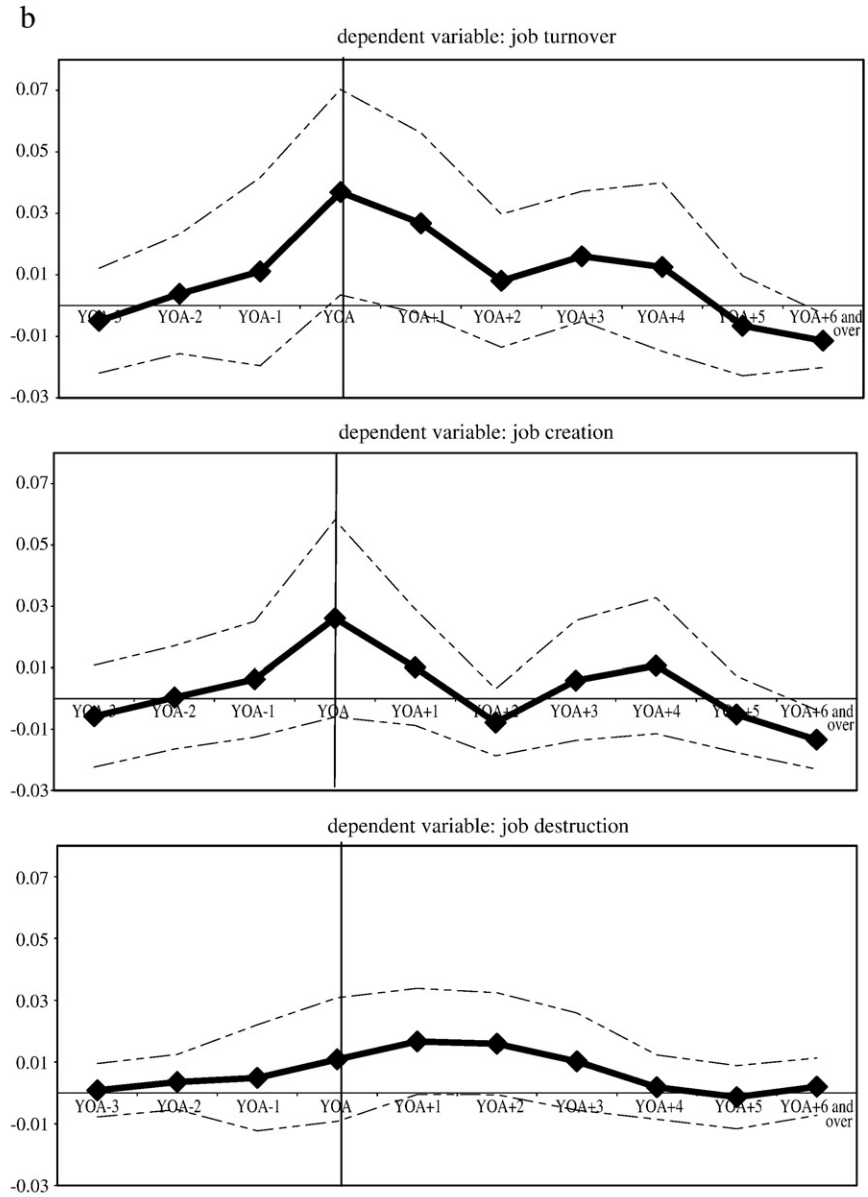
Fig. 5. a) Dynamic effects of UBs on job creation, job destruction, and job turnover — Full sample. b) Dynamic effects of UBs on job creation, job destruction, and job turnover — with control variables.

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 vice versa, which is consistent with a causal interpretation of our results. Overall, the effect of UBs on job turnover appears to be mainly due to effects on the job destruction margin. There is also 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 job reallocation 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 8 use data on EPL taken from Botero et al. (2004).[23] The advantage of this measure is that it is available for a very broad set of countries. Its disadvantage is that of being measured only at a point in time (the end of the 1990s). However,

[23] 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.



Notes: These figures display coefficients and confidence intervals obtained from estimation of model (3), as explained in the text (the model includes year fixed effects and country-specific time trends). 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. Standard errors were clustered by Country.

Fig. 5 (*continued*).

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 only estimate random-effects regressions. The estimates displayed in the first panel of Table 8 confirm a positive and statistically significant effect of UBs on job turnover (job destruction in particular) even when controlling for the degree of EPL strictness. Although somewhat imprecise, the results reported in the bottom panel confirm that UBs are associated with an expansion of the services sector and a contraction of the agriculture sector. The strictness of EPL seems to increases the employment share of agriculture and reduce the industry share. In Table 9, we interact the UB dummy with a dummy that takes a value of one if a country has an EPL strictness index above the median value. Even though the estimated coefficient on the interaction term is often not statistically significant, the results of this empirical test suggest that the impact of UBs on job turnover is strongest in countries with a low degree of EPL strictness. This is consistent with the notion that UBs can better accommodate large-scale restructuring where employment protection is less stringent.

4.2.5. Two-period difference-in-differences

To further probe the robustness of our results, we have performed a difference-in-differences analysis after collapsing the data into two periods: pre-"treatment" and post-"treatment" (Bertrand et al., 2004). For the countries that introduced UBs at some point in the period considered, the definition of the "pre" and "post" periods is straightforward. For the other countries, which never adopted UBs, we defined "pre" as the years before 1991, and "post" as the period after 1991, where 1991 is the modal year of introduction of UBs for countries that did introduce UBs from scratch. The difference-in-differences coefficients

Table 8
Unemployment benefits and job reallocation, controlling for employment protection (EPL).

	Random effects all observations (1)	Random effects with controls (2)	Random effects all observations (3)	Random effects with controls (4)	Random effects all observations (5)	Random effects with controls (6)
	Job turnover		Job creation		Job destruction	
Unemployment benefits	0.0303***	0.0210**	0.0145*	0.011	0.0158***	0.00998**
	(0.009)	(0.009)	(0.007)	(0.007)	(0.006)	(0.004)
Employment protection	0.005	0.030	−0.031	0.013	0.0359**	0.016
	(0.030)	(0.030)	(0.027)	(0.022)	(0.014)	(0.019)
GDP growth		0.000		0.000921***		−0.00105***
		(0.000)		(0.000)		(0.000)
Log of per-capita GDP		−0.007		−0.007		0.000
		(0.007)		(0.006)		(0.004)
Openness to trade		0.000175***		0.000162***		0.0000
		(0.000)		(0.000)		(0.0000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0563***	0.097	0.0511***	0.0790*	0.005	0.018
	(0.020)	(0.062)	(0.013)	(0.048)	(0.015)	(0.039)
Observations	455	420	455	420	455	420
Number of id	34	34	34	34	34	34
R-squared	0.35	0.39	0.35	0.33	0.31	0.36
	Agriculture share		Industry share		Services share	
Unemployment benefits	0.031	−0.035	−0.0452*	−0.017	0.033	0.0518**
	(0.039)	(0.027)	(0.027)	(0.023)	(0.028)	(0.025)
Employment protection	0.051	0.216*	−0.049	−0.199	−0.092	−0.015
	(0.240)	(0.120)	(0.190)	(0.160)	(0.200)	(0.160)
GDP growth		0.002		−0.001		0.000
		(0.001)		(0.001)		(0.001)
Log of per-capita GDP		−0.267***		0.125***		0.142***
		(0.029)		(0.030)		(0.027)
Openness to trade		0.0000		0.000		0.000
		(0.000)		(0.000)		(0.000)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.364**	2.387***	0.290***	−0.611***	0.380***	−0.772***
	(0.140)	(0.130)	(0.094)	(0.087)	(0.100)	(0.093)
Observations	456	421	456	421	456	421
Number of id	34	34	34	34	34	34
R-squared	0.82	0.92	0.64	0.83	0.88	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 Employment Protection strictness is taken from Botero et al., 2004 (variable "index_labor7a" in the dataset available at http://iicd.som.yale.edu). Standard errors (clustered by country) are reported in parentheses. Levels of statistical significance are indicated by asterisks: *Significant at 10%; **Significant at 5%; ***Significant at 1%.

(standard errors) we obtain are as follows: 0.002(0.009) for job creation, 0.018(0.005) for job destruction and 0.021(0.009) for job turnover[24]. In spite of the difficulty in defining "pre" and "post" for "untreated" countries, the results of this robustness test are very similar to those from our main regressions: We confirm a positive, sizable and statistically significant effect of UBs on job turnover, coming mainly from the job destruction margin. When we look at the effects on sector shares, we find a negative and significant reduction in the industry share, and positively but not statistically significant coefficients on the primary and services share.

4.3. Discussion

Overall, the results of our empirical analysis suggest that the introduction of UBs was associated with greater job turnover, mainly as a result of higher job destruction rates. These conclusions are robust to the inclusion of controls for observable characteristics, unobservable country effects, year effects and country-specific time trends, and robust to allowing for heteroskedastic and autocorrelated residuals. The introduction of UBs also seems to determine a shift of jobs from low-productivity sectors (agriculture) to services, although these effects are often only marginally statistically significant, perhaps because in developing countries employment in agriculture is a sort of hidden unemployment notably at times of structural change.[25] 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 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 often downscaled over time as their fiscal costs materialize. This was precisely what happened in the formerly planned economies where the number of beneficiaries of UB systems were heavily underestimated at the outset of transition (Boeri, 2000). Fig. 6 displays the fraction of unemployed individuals receiving

[24] In the regressions, the number of observations per country were used as weights. A table with these regression results is available from the Authors upon request.

[25] In addition, as noted above, some middle-income countries (e.g., Argentina and Uruguay) have strong comparative advantages in highly productive agricultural activities.

Table 9
Unemployment benefits and job reallocation, by level of employment protection (EPL).

	Fixed effects with controls (1) Job turnover	Fixed effects with controls (2) Job creation	Fixed effects with controls (3) Job destruction	Fixed effects with controls (2) Agr. share	Fixed effects with controls (4) Ind. share	Fixed effects with controls (6) Sect. share
Unemployment Benefits (UB)	0.038***	0.028*	0.010	−0.049	0.015	0.040
	(0.011)	(0.014)	(0.007)	(0.049)	(0.025)	(0.029)
UB*(EPL>50th pctile)	−0.035	−0.035**	−0.001	0.023	−0.017	−0.010
	(0.021)	(0.017)	(0.013)	(0.032)	(0.020)	(0.019)
GDP growth	−0.0003	0.0009***	−0.0012***	0.0009	0.0000	−0.0006*
	(0.0004)	(0.0003)	(0.0003)	(0.0006)	(0.0003)	(0.0003)
Log of per-capita GDP	−0.035	−0.031	−0.004	−0.092***	0.107***	0.0004
	(0.027)	(0.024)	(0.016)	(0.030)	(0.026)	(0.018)
Openness to trade	0.0004*	0.0003	0.0001	0.0003	0.0001	0.0000
	(0.0002)	(0.0002)	(0.0001)	(0.0003)	(0.0002)	(0.0002)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country time trends	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.337	0.276	0.061	1.04***	−0.58***	0.39***
	(0.210)	(0.180)	(0.120)	(0.240)	(0.210)	(0.130)
Observations	420	420	420	420	420	420
Number of id	34	34	34	34	34	34
R-squared	0.26	0.25	0.30	0.73	0.78	0.82

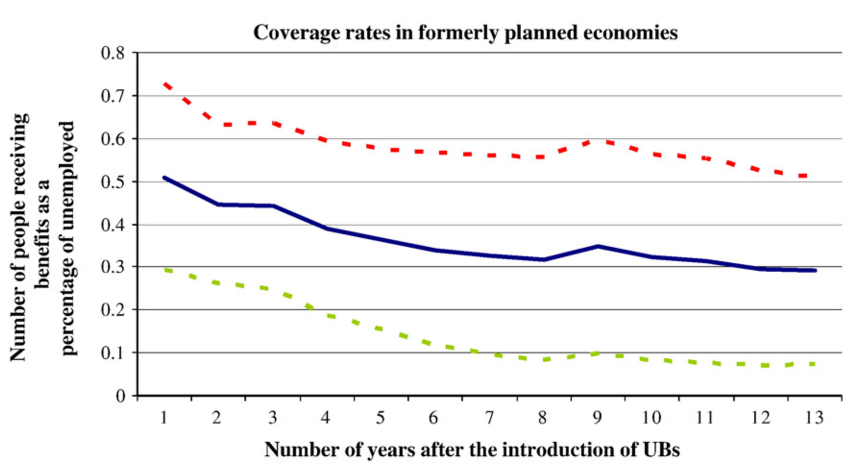
This table reports results of fixed effects regressions. The explanatory variable of interest, "Unemployment Benefits" (UB) is a dummy variable equal to 1 if unemployment benefits schemes were in place in a given country-year and 0 otherwise. The measure of Employment Protection strictness is taken from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

UBs in the 10 formerly planned economies for which this information was available: On average, the coverage of UBs declined from about 50% in the first year after its introduction to less than 30% 13 years down the road. 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 measure 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 attenuate 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 a 1–2 percentage points increase in the yearly rate of job destruction



Notes: This figure displays the fraction of unemployed individuals receiving UBs in the 10 formerly planned economies (Albania, Azerbaijan, Bulgaria, Czech Republic, Estonia, Georgia, Hungary, Latvia, Lithuania, Poland, Romania, Russia, and Ukraine). The dashed lines report the average plus or minus one standard deviation.

Fig. 6. UB coverage rates in formerly planned economies.

and a 2.5–3 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 about the same magnitude; these effects, however, were often not statistically significant (or marginally significant). 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, since 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 smaller increases in job turnover 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 more direct measures of the quality of job reallocation, notably evaluating the effects of UBs on job tenure (a proxy for match quality), represents a potentially fruitful avenue for research, since it permits to directly test the empirical relevance of stochastic job matching models. Results from individual-level data on the US (Centeno, 2004) suggest that there may be indeed important effects on unemployment benefits on match quality. It also should 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, exploiting within country variation (e.g., geographical-time or industry-time variation) in the coverage of UB systems could be another promising avenue of research (Freeman, 2007).

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