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Wages, layoffs, and privatization: Evidence from Ukraine

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This paper estimates the effects of privatization on worker separations and wages using retrospective data from a national probability sample of Ukrainian households. Detailed worker characteristics are used to control for compositional differences and to assess types of observable "winners" and "losers" from privatization. Pre-privatization worker–firm matches are used to control for unobservables in worker and firm selection. The results imply privatization reduces wages by five percent and cuts the layoff probability in half. Outside investor ownership reduces separations but leaves wages unaffected. Winners from privatization tend to be higher skilled employees of larger firms, but there is no discernible relationship with gender, education, or experience. *Journal of Comparative Economics* **34** (2) (2006) 272–294. School of Management and Languages, Heriot-Watt University, Edinburgh EH14 4AS, UK; Upjohn Institute for Employment Research, Kalamazoo, MI 49007; Central European University (CEU); University of Kentucky; Kyiv-Mohyla School of Economics, Kyiv, Ukraine.

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1. Introduction

The principal argument for privatization around the world is that profit orientation will raise firm efficiency and competitiveness. However, does any increase in efficiency come at the expense of workers? Although privatization has frequently been opposed by workers expecting layoffs and wage cuts, economic reasoning does not imply such effects unambiguously. To be sure, cost-efficiencies may be achieved by reducing excess employment and worker rents (e.g., Boycko et al., 1996). But they may also be achieved through savings in other areas, and they may lead to an expansion of production that increases the need for well-compensated labor. The increase in labor demand after privatization could be still larger if the new private owners act entrepreneurially to extend their markets (e.g., Frydman et al., 1999). Both the need to attract new workers and the enhanced use of incentive pay under private ownership could increase wages. These mechanisms work to offset the negative effects that workers expect, and they may even result in expanded employment and higher wages.

Moreover, the effects of privatization on workers may vary with the type of new private owners, particularly if these include managers or the workers themselves. Indeed, although workers in transition economies were not always successful in stopping privatization, they favored privatization methods leading to majority insider ownership, so they could control the amount of labor restructuring and/or share in any gains (e.g., Earle and Estrin, 1996). According to Aghion and Blanchard's (1998) model of insider privatization, restructuring only takes place with outsider control, and the restructuring hurts workers. However, although a slow restructuring strategy could benefit workers in the short term, it may result in reduced competitiveness and force layoffs and/or wage cuts in the longer run. In contrast, a more vigorous restructuring program could allow the firm to expand its scale, raising employment and wages.

The effects of privatization could also be heterogeneous for different types of workers, creating winners and losers within firms. For example, new private owners could engage in skill-biased restructuring, benefiting workers with higher skills associated with education, occupation, or computer training. Older and longer tenured workers may be losers if they are less flexible or if their productivity has fallen disproportionately more than other types of workers. Previous research has shown that inequality has risen during transition and that differentials associated with schooling, gender, and firm size have changed, but there has been little attention to the role played by privatization.[1]

The average effects of privatization on workers and the effects of different types of privatization on different types of workers are therefore empirical questions. However, while a large number of studies have estimated the effect of privatization on firm efficiency, few have investigated the effects on workers, and the limitations of the existing research are profound.[2] Sample sizes are often very small. The seminal paper in the literature, Haskel and Szymanski (1993), analyzes 14 British public companies, of which just four are privatized, while Kikeri (1998) describes case studies of privatization effects on labor in several developing countries. Moreover,

1 On inequality, see Milanovic (1999). For gender differentials see Brainerd (2000), and for schooling and firm size see Gorodnichenko and Sabirianova Peter (2005), both on Ukraine. Two studies examine the role of privatization in wage inequality: Fleisher et al. (2005) carry out a meta-analysis of studies across countries, finding that increases in schooling premia are positively associated with large-scale privatization, but Münich et al.'s (2005) study of Czech workers finds that the premium increased prior to privatization.

2 On firm performance effects of privatization in transition economies, see Djankov and Murrell (2002) for an overview of the literature and Brown et al. (2006) for evidence on Ukraine.

few papers control fully for selection problems. One selection issue is the potential for systematic differences in firms chosen for privatization versus those remaining state-owned. Nearly all studies of the private–public wage gap use cross section analysis not accounting for this selection.[3] Most studies that do follow firms before and after privatization have very short time series, such as La Porta and Lopez-de-Silanes' (1999) study of 170 privatized firms in Mexico with information on only one year after privatization. Jones and Simon (2005) estimate wage regressions with two-year panels at the very start of privatization in Bulgaria. Only two studies, Lizal and Svejnar (2002) and Brown et al. (2005), use large samples of firms before and after privatization, include state firms as a control group, and employ firm fixed effects; thus they are able to handle selection through regression-adjusted difference-in-differences.

However, even the firm-level studies that analyze large panels such as these suffer from a number of other limitations. Firm-level studies of the effects of privatization on workers capture changes in only net employment and average wages.[4] They are unable to observe individual worker characteristics and behavior, so they cannot measure worker turnover or changes in the composition of employment. Nor can they estimate the effects of privatization on different types of workers, defined with respect to characteristics such as skills, age, tenure, gender, and occupation. Moreover, a second type of potential selection bias may arise if workers choose employment in privatized or state firms and if this self-selection is related to their pay (e.g., because more productive workers prefer to work in privatized firms). Firm-level studies cannot control for this worker self-selection process. Finally, the measure of private ownership in any of these studies is seldom disaggregated more than between domestic and foreign ownership; measurement constraints prevent attention to the heterogeneity of domestic ownership in many transition economies, in particular the presence of large-scale insider ownership.

This study estimates the wage and layoff effects of privatization using data that greatly mitigate these problems. We analyze the Ukrainian Longitudinal Monitoring Survey (ULMS), a database drawn from a nationally representative probability sample of about 4000 Ukrainian households containing more than 8000 individual adult respondents. The survey was carried out in 2003, and the questionnaire contains questions not only on current labor force activities but also retrospectively. We use the retrospective information to construct time series of employment and wages at each employer of each respondent. This longitudinal tracking of firms and workers permits us to observe worker–firm matches before and after privatization, information we use to address potential biases arising from the selection of firms into ownership type and the selection of workers into employers of different types. The sample of privatizations is larger than in most other studies, and the data also contain information on a comparison group of worker–firm matches remaining in state ownership throughout.

The ULMS data contain detailed information on worker characteristics, which we exploit both to control for changing composition of employment at firms as they go through the privatization process and to assess the potentially differential impact of privatization on various types of workers. Using interaction specifications, we estimate the influence of privatization on layoffs and wages by gender, schooling, experience, and firm size, among other variables. The ULMS

[3] Such studies in former Soviet economies include Brainerd (2002) for Russia and Gorodnichenko and Sabirianova Peter (2005) for Russia and Ukraine. Worker-level studies frequently face measurement problems both concerning ownership (as new private and old privatized companies may not be distinguished) and wages (due to the volatility associated with wage arrears).

[4] Firm-level data sets rarely contain information on hiring and separations. The only previous paper examining the impact of privatization on turnover is Brown and Earle's (2003) study of a Russian firm survey.

also clearly distinguishes new private from privatized firms, and we exclude the former from the analysis to focus on the effects of privatization. The data permit us to distinguish three types of controlling owners of privatized firms: worker, manager, and outsider. In some specifications, we exploit this information, disaggregating private ownership into these categories.

The potential outcomes of privatization that we study with these data are layoffs and wages. Our analysis of layoffs, which comprise all involuntary separations, includes information also on voluntary separations, which we classify by their declared motivation into "professional" and "personal" quits, defined below; this distinction is potentially important if some quits are disguised layoffs. The separations analysis therefore relies on multinomial logit regressions, where the dependent variable is categorical: staying with the same employer, layoff, quit for professional reasons, or quit for personal reasons. Using a sample of individuals who were employed at state firms in 1991, we estimate the effect of privatization on separation from that job in each subsequent year from 1992 to 2002, controlling for other variables that may affect separations. The wage variable we use from the ULMS is the contractual wage, which reduces measurement problems associated with wage arrears. The wage equations are estimated for employees initially employed in the state sector, and they include standard controls from a Mincer earnings regression and, in some specifications, more detailed controls for occupation, tenure, computer use, marital status, region, industry, and firm size. In addition to estimating by ordinary least squares (OLS), we also add worker fixed effects and worker–firm fixed effects in alternative specifications to control for selection bias. In both cases, we interact privatization measures with worker and firm characteristics to investigate the privatization effect on wage differentials.

The results of the analysis imply that privatization cuts the layoff probability in half, but reduces wages by five percent. Outside investor ownership reduces separations more than insider ownership, but leaves wages unaffected. Winners from privatization tend to be higher skilled employees of larger firms, but there is no discernible relationship with gender, education, or experience.

The remainder of the paper is structured as follows. Section 2 describes the data and Section 3 explains our estimation methods. Section 4 presents results, and conclusions are summarized in Section 5.

2. Data

The Ukrainian Longitudinal Monitoring Survey (ULMS) was carried out in 2003 on a probability sample of households across Ukraine. The response rate was 66 percent for households and 87 percent for individuals within those households, resulting in a sample of 4005 households with 8671 adult respondents (age 15–76 at the survey date).[5] The survey contains standard questions on characteristics and current labor force participation. For our purposes in this paper, however, a particularly useful feature of the ULMS is that it requested work history data from respondents. In particular, the survey contains data on the main jobs held in December 1986 and December 1991 and on all main jobs between December 1997 and mid-2003. Moreover, the survey contains detailed questions on the characteristics of each of those employers.

The sample for the analysis of layoffs is determined as follows. We restrict attention to main jobs held by respondents in December 1991, because nearly all privatization activity occurred af-

[5] The ULMS was organized by Hartmut Lehmann and carried out by the Kyiv International Institute for Sociology with financing from a consortium of institutions led by the Institute for the Study of Labor (IZA) in Bonn. We are grateful for all their efforts. See Lehmann (2005) for more information on the survey.

ter this date while a fair amount occurred before December 1997.[6] Since privatization had hardly started, workers' selection of their December 1991 jobs was unlikely to have been influenced by the probability their employers would be privatized in the future. Workers were asked if and when their 1991 jobs ended, as well as the reasons for the separation if it occurred. We follow these 1991 jobs annually each December through the separation year or 2003, whichever comes first.[7] The analysis is carried out on workers who were between 16 and 62 years old in 1991.

We also restrict the sample of 1991 jobs to those in state firms located on Ukrainian territory, and we exclude the budgetary sector (culture, education, health, the military, and public administration), since our interest is in firms that could potentially be privatized. The ULMS asks workers about ownership changes occurring during their job tenure, not subsequently, so we cannot observe subsequent privatization and turnover for firms privatized after a worker separates from a state firm. This implies that we are unable to control for anticipatory effects of firm selection: if a firm is classifiable as "later privatized" in the data, that automatically means the worker did not separate from the firm prior to privatization. To be in the sample in year t, the worker must have been employed in the December 1991 job in December of year $t - 1$.

As shown in Table 1, Panel A, the basic regression sample for analyzing worker separations includes 22,203 worker-year observations with non-missing data for all relevant variables, corresponding to 3392 workers. The sample includes 423 worker–firm matches in which privatization takes place during the workers' tenure, with an average of 4.3 pre- and 4.8 post-privatization worker–firm observations for these workers. Because of missing values, the sample is diminished when we add more variables to the equation, as shown in Table 1.

Separations of workers from their employers are classified according to a detailed set of reasons given in the questionnaire, as shown in Table 2. We distinguish different types of quits, because some types are more likely to represent disguised layoffs. Layoffs, in which we include all involuntary separations from the worker's perspective, comprise separations due to shutdown, reorganization, bankruptcy, privatization, employment reduction, expiration of a contract, expiration of probation period, and other dismissals initiated by the employer. We classify as "professional quits" those separations where the motivation was described as wanting a higher salary, better working conditions, more interesting work, or own business; and "personal quits" as those separations where the motivation was retirement (early or regular), end of own business, military conscription, imprisonment, own illness, return to school, marriage, parental leave, change of residence, or need to care for other family members. Respondents to the ULMS were permitted to provide multiple reasons, and we prioritize layoffs over professional quits, and professional over personal quits, except for expiration of contract or probation period, in which case the professional reasons are given priority over personal, and personal quits over layoffs. According to the respondents, most of the dismissals are due to personnel reductions, plant closures, bankruptcies, and reorganizations, while very few are associated with privatization.

Table 3 provides mean separation rates for these constructed categories. The total separation rate averages 11.5 percent per year, and it breaks down into a 3.9 percent dismissal rate, 3.0 per-

[6] Prior to 1993, the only legal process leading to privatization in Ukraine involved leasing and co-op arrangements inherited from the Soviet Union (Frydman et al., 1993). For a time series on privatization in Ukrainian manufacturing, see Brown et al. (2006).

[7] The ULMS questionnaire does not contain information on jobs starting after December 1991 and ending before December 1997, so we cannot systematically follow workers to new jobs after they separate from their December 1991 jobs.

Table 1
Sample construction
Panel A. Separation regression sample

	Individuals	Individual-years
Total ULMS sample	8641	
Employed in December 1991	5207	
December 1991 employer was state, budgetary, municipal firm, or collective farm	5008	
December 1991 employer not in budgetary sector	3747	
Non-missing dependent variable (*separation category*):	3677	24,553
• No separation	875	21,751
• Dismissed	915	915
• Professional quit	706	706
• Personal quit	1181	1181
Non-missing dependent and independent variables:		
• Basic regression (Table 3) sample	3392	22,203
• Additional control variables (Table 4) sample	2791	18,709
• Disaggregated ownership (Table 5) sample	2791	18,006

Panel B. Wage regression sample

	Workers	Worker–firm matches	Worker-years
Total sample (employed)	5512	7083	25,106
Full-time employee	5245	6669	23,487
Employer initially state	3379	3797	14,185
Not in budgetary sector	1901	2051	7457
Non-missing wage	1560	1666	5493
Non-zero wage	1555	1660	5461
Non-missing independent variables:			
• Table 7, Specification 1	1555	1660	5461
• Table 8, Specification 1	1549	1653	5334
• Table 9	1273	1350	4482

cent professional quit rate, and 4.6 percent personal quit rate. Workers are in firms privatized in the year before or earlier in nearly 11 percent of the observations in the sample.

Turning to the wage regression sample, the ULMS contains the contractual wage data for the main job in each December of 1986, 1991, and 1997 to 2002. Our wage regression sample uses only the 1997–2002 observations, so that we have the same regular intervals for all respondents. Observations are included on workers aged 15–72 in full-time jobs at firms that were state-owned either in December 1997 or in the first observation of the job thereafter, and if the firm is not in the budgetary sector (again, because budgetary organizations are not subject to privatization). Only full-time workers are included so as to reduce the problem of wage variation due to differences in hours worked. Ideally we would like to control for hours worked, but unfortunately the ULMS does not contain hours data for full-time workers. The variation in hours worked for full-time employees is likely to be small, however, since worker hours in state and privatized firms are regulated by labor-protection laws. Table 1, Panel B, shows the sample construction and sample sizes by year for the various specifications of the wage equation. The sample contains 159 worker–firm matches where privatization takes place during the workers' tenure, with an average of 3.2 pre- and 2.3 post-privatization worker–firm observations.

The use of the contractual wage measure avoids the volatility problem associated with wage arrears, which has plagued much of the previous research of wage changes in former Soviet

Table 2
Reasons for separations

Reason	Number	Percent
Layoffs		
Closing down of enterprise	210	7.5
Reorganization of enterprise	199	7.1
Bankruptcy of enterprise	126	4.5
Privatization of enterprise	15	0.5
Dismissal initiated by employer	76	2.7
Personnel reduction	342	12.2
Expiring of employment contract	12	0.4
Expiring of probation time	1	0.0
Total layoffs	915	32.7
Professional quits		
Wanted/was proposed higher salary	333	11.9
Wanted/was proposed better working conditions	196	7.0
Wanted/was proposed more interesting work	67	2.4
Wanted to start own business	27	1.0
Total professional quits	706	25.2
Personal quits		
Military service	9	0.3
Imprisonment	4	0.1
Own illness or injury	189	6.7
Studies	4	0.1
Retirement	847	30.2
Early retirement	67	2.4
Marriage	22	0.8
Parental leave	71	2.5
Need to take care of other family members	102	3.6
Change of residence	69	2.5
Main job became second job	1	0.0
End of farming/sole proprietorship	0	0.0
Total personal quits	1181	42.1

Notes. The sum of answers exceeds the number of separations because respondents were permitted to give multiple reasons. In addition to the responses shown, two respondents said the reason was difficult to say, and none refused to answer.

economies (as described by Earle and Sabirianova, 2002). We deflate wages by the Ukrainian State Statistics Committee's December-to-December consumer price index. Region and time dummies are included in most regressions to control for price variations not captured by this index.

Summary statistics for all variables are shown in Table 3, and precise definitions for the variables are provided in Appendix A. The ULMS questions on privatization and ownership are very detailed and carefully worded, and they elicit information on whether the firm was state-owned or privatized, and on which type of owner held the most shares both at the time the job began and when it ended (or as of the interview date), as well as the month and year in which ownership change occurred. About 11 percent of observations in the separation sample pertain to privatized firms, and about 7 percent are privatized in the wage sample. The sample of privatized observations falls when private ownership is disaggregated because workers are unsure which owner-type is dominant. Among those providing this information, however, most privatized firms

Table 3
Descriptive statistics

	Separation sample	Wage sample
Layoffs	0.039	
Professional quits	0.030	
Personal quits	0.046	
Real wage (December 1992 hryvnyas)		311.083 (194.126)
Privatized	0.108	0.068
Worker-owned	0.012	0.004
Manager-owned	0.054	0.035
Outsider-owned	0.007	0.005
Experience (years)	25.126 (10.723)	23.046 (11.787)
Education (years)	10.250 (1.706)	10.392 (1.634)
Female	0.481	0.421
Tenure (years)	17.065 (10.197)	12.623 (10.926)
Unskilled	0.131	0.116
Skilled blue-collar	0.597	0.634
Technician	0.064	0.078
Professional	0.208	0.172
Computer	0.052	0.116
Married	0.852	0.831
Micro firm	0.090	0.118
Small firm	0.356	0.379
Medium firm	0.240	0.191
Large firm	0.315	0.312

Notes. The sample mean is shown for each variable (standard deviation in parentheses for continuous variables). The separation sample is from Table 5 and the wage sample is from Table 10. The disaggregated ownership variables are based on the same samples, minus individual-years where disaggregated ownership is missing. Variable definitions are in Appendix A.

in the sample are controlled by managers, with very few controlled by outsiders.[8] Nevertheless, the samples are large enough for us to be able to estimate the effects of these different types separately.

To the extent that the ULMS is genuinely representative of the Ukrainian adult population, there is little reason to think that these samples are non-representative of the relevant subpopulations. In the separations analysis, the subpopulation is full-time employees of state-owned firms in December 1991. In the wage analysis, the subpopulation is full-time employees of a state-owned firm either in December 1997 (if they were employed at that time) or in December of the years 1998–2002 (if they were not employed in December 1997). In both cases, few observations are lost due to missing values, so there is little discrepancy between the original ULMS sample and our analysis sample for these subpopulations. However, as an additional check, we compared our sample proportions by detailed region and by six industry-sectors with official statistics from the Ukrainian State Statistics Committee. For the separation sample in 1995 (roughly in the middle of our period), the correlations are 94.3 percent for regions and 95.9 percent for sectors. For the wage sample in 2000, the correlation is 84.4 percent for regions and 92.3 percent

[8] The sample sizes with foreign ownership are still much smaller, so we eschew analysis of this interesting dimension with these data. For a firm-level study of employment and wage effects of privatization to foreign investors in Ukraine, see Brown et al. (2005).

for sectors. Thus, our regression samples are very similar to the official statistics, implying that they are indeed representative of the subpopulations we investigate.

A natural concern with the data we are analyzing is the possibility of measurement error. Two types are especially salient for our analysis. The first is imperfect knowledge by workers of the ownership of their firms, which is a common problem in surveys of this type. However, besides the careful wording of the ULMS questions, the nature of the Ukrainian privatization process was such that workers were frequently involved. Concerning the distinction between state and private, fewer than 10 percent of workers are unable to respond to the question, while an additional 10 percent or so are unable to state which owner-type has the most shares.

The second type of measurement error that deserves discussion is recall bias. The separation regressions require workers to report accurately on their 1991 jobs including their reasons for separation, if they did separate. The 1991 date was chosen for the ULMS questionnaire because it was that fall that Ukraine gained independence from the Soviet Union, and therefore workers would be more likely to remember what they were doing around the time of those momentous events. Moreover, very few workers (70, less than 2 percent of those employed at non-budgetary state firms in 1991) are unable to provide information on separation from this job.

Concerning potential recall bias in the wage regressions, the sample requirement in this case is that respondents remember wages for jobs held since December 1997, just over 5 years before their interviews. Just over 20 percent of the sample has a missing value for the wage. If workers' wage reports are accurate when they are reported and if non-reports are random, then there would be no recall bias. If the reports are not accurate, then our standard errors are over-stated. If non-reports are nonrandom there could be an induced bias, although it is likely to be limited in magnitude because of the low level of non-reporting. Moreover, there is only a slight difference in the incidence of privatization for workers reporting wages compared to those failing to report (6.6 percent of those with non-missing wages are in privatized firms, compared with 5.3 percent of those with missing wages). As a check on the plausibility of the data, we estimate standard Mincer earnings regressions and report the results below. We also estimate using median (least absolute deviations) to reduce the influence of outliers. Finally, our inclusion of worker fixed effects in some specifications would control for any fixed differences in wages and ownership across workers. Our identification strategy of difference-in-differences removes all such time-invariant biases.

3. Estimation framework

The two outcome variables we investigate in this paper are job separations and wages. As described in the previous section, the separation analysis uses a sample of workers who are employed at non-budgetary state firms in December 1991 and investigates whether or not a job separation takes place from these employers subsequently. Separations are then categorized according to the dominant reason, implying four categories for the dependent variable: remained employed in the firm, layoff, professional quit, and personal quit. We use multinomial logit models to estimate the probability of these events as a function of firm ownership and control variables.[9] In our analysis of the ULMS data, we find that Wald and Likelihood Ratio tests reject pooling any of these categories at the one percent level. The Hausman test for independence of

[9] Though not reported here, we have also estimated multinomial probit models, which produce virtually identical results.

irrelevant alternatives (IIA) does not reject independence, while the Small-Hsiao test rejects it, which is a quite common result.

The basic separations regression equation can be written as follows:

$$Pr(s_{it} = j) = f(\delta_j Private_{it-1} + X_{it}\beta_j + \alpha_{tj} + u_{itj}) \tag{1}$$

where $Pr(s_{it} = j)$ is the probability of separation type j by individual i in year t, f is the logistic function, $Private_{it-1}$ is an indicator of whether the employer was private in the previous year, δ_j is the coefficient to be estimated, X_{it} is a vector of controls and β_j the associated vector of coefficients, the α_{tj} are year effects, and u_{itj} is the regression's disturbance for alternative j. Identification of δ in this model is based on the change in $Private_{it-1}$ for workers in firms becoming privatized compared to workers in firms remaining state-owned.[10]

We estimate several alternative versions of this model. The year effects are included in all specifications to account for aggregate shocks that could affect separation probabilities, but the alternative versions differ in how the X_{it} and ownership variables are specified. In a first, parsimonious specification, the X_{it} vector includes only years of experience, years of experience squared, years of education, and gender. A second specification adds years of tenure, four occupational skill categories, computer use, marital status, four firm-size categories, six regional categories, and eight industry-sector dummies. A third specification disaggregates $Private_{it-1}$ into types of new private owners, based on which type has the largest shareholding. We refer to these types as worker-owned, manager-owned, and outside-owned. Finally, we assess the possibly differential impact of privatization on various types of workers by including an interaction between ownership and worker characteristics:

$$Pr(s_{it} = j) = f(\delta_j Private_{it-1} + X_{it}\beta_j + Private_{it-1}X_{it}\delta_{Xj} + \alpha_{tj} + u_{itj}) \tag{2}$$

where the coefficient vector δ_{Xj} permits heterogeneity in the average effect δ_j with respect to characteristics X_{it}. We consider each characteristic in turn, estimating separate equations with interactions for each.

Turning to the effects of privatization on wages, we analyze workers who are employed at state-owned firms in the first observation of the worker–firm match in the regression sample. Again the basic empirical strategy is to compare wage changes of workers whose firms are privatized with those that are not, adjusting for individual characteristics. In this case, however, we can also control for a variety of types of correlated effects. The basic earnings equation can be written as follows:

$$ln(w_{it}) = \delta Private_{it-1} + \gamma EverPrivate_i + X_{it}\beta + \rho_i + \alpha_t + u_{it} \tag{3}$$

where $EverPrivate_i$ indicates whether the employer is ever privatized (during our observation period) and γ is the associated coefficient. The ρ_i are region effects, defined over the 26 Ukrainian

[10] Since the privatization information comes from respondents' reports on whether and when their employers were privatized rather than from an independent source, we are unable to estimate a difference-in-differences model to better control for ownership selection bias. Group effects for firms that are privatized during the period of observation are collinear with staying for the observations prior to the privatization year. For the same reason, we are unable to estimate whether firms to be privatized in the following year engage in larger than usual personnel reductions. The fact that almost no respondents who experienced a separation report that this was due to privatization suggests that firms did not engage in large personnel reductions near the time of privatization, however. We are able to estimate regressions with $Private_{it}$ (whether the employer was private in the current year); the results from these are very similar to those with the lagged variable.

oblasts plus Kiev, to permit geographic variation in average wages and costs of living. We start with a simple difference-in-difference estimator that sets $X_{it} \equiv 0$. Next, as a check on the data, we estimate a basic Mincer earnings regression, where $Private_{it-1}$ and $EverPrivate_i$ are excluded and the X_{it} vector includes only years of experience, years of experience squared, years of education, and gender. Then we combine these two specifications into a regression-adjusted difference-in-difference estimator, where the controls are those from the Mincer specification.

The difference-in-difference estimator could still be subject to selection bias, for instance, if workers have some unobservable skill that is correlated with their propensity to work in privatized companies. In other specifications, therefore, we include worker fixed effects. Concern that worker–firm matches may also reflect specific match quality correlated with ownership leads us to include worker–firm fixed effects. We investigate the within-worker–firm-matches specification controlling also for sector–year and region–year interactions, in separate specifications.

Analogously to our approach with the separations regressions, we estimate versions of Eq. (3) in which ownership is disaggregated into managerial, worker, and outsider types. We also add a larger set of control variables, and we investigate whether the estimated privatization effects are heterogeneous for the observable characteristics represented by those variables.

4. Results

Figure 1 plots the unconditional mean layoff and professional quit rates using the December 1991 job sample for state and privatized firms separately. Layoff rates in state firms display a strong upward trend throughout the period, reaching over 9 percent in 2002. Except in 1998, the state layoff rates are always higher than in privatized firms, where they are usually about 3 percent. The professional quit rate is always higher in state firms also, although the difference is much less striking than for layoffs. Though not displayed, the personal quit rates are higher in state firms in every year except 1995. These numbers imply that separations tend to be lower after privatization, but of course they do not control for worker characteristics that could be correlated with both ownership and turnover behavior.

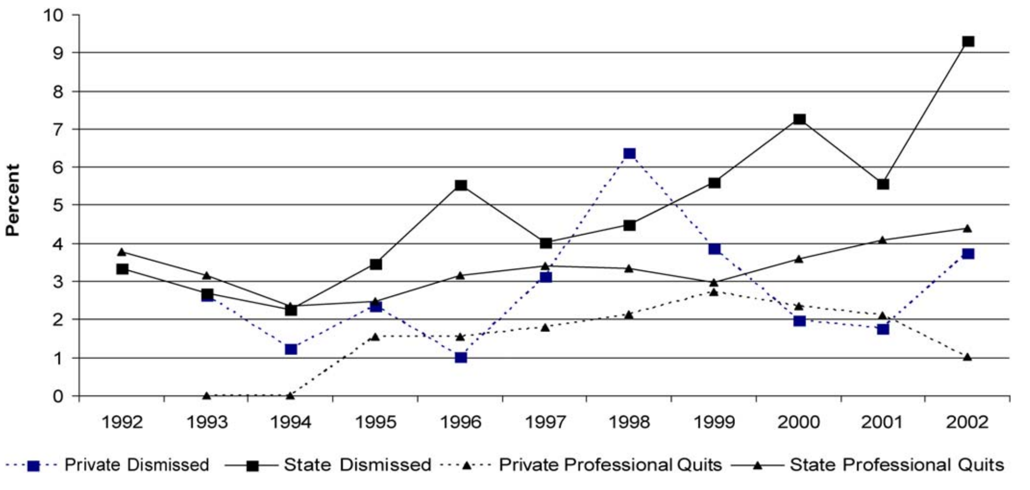

Fig. 1. State and privatized separation rates. *Note*: These are annual dismissal and professional quit rates from the December 1991 job, as of December of each subsequent year.

Table 4
Effect of privatization on separations

	Dismissal	Professional quit	Personal quit
Privatized	−0.019***	−0.014***	−0.016***
	(0.003)	(0.003)	(0.004)
Experience/100	−0.022	−0.128***	−0.313***
	(0.051)	(0.037)	(0.059)
$(\text{Experience}/100)^2$	−0.038	0.051	0.965***
	(0.101)	(0.077)	(0.112)
Education/100	0.118	−0.012	−0.360***
	(0.074)	(0.068)	(0.078)
Female	0.009***	−0.013***	0.025***
	(0.002)	(0.002)	(0.003)

Notes. The marginal effect is shown for each variable, with the standard error (corrected for firm clustering) in parentheses. Year dummies are also included. $N = 22{,}203$. Pseudo $R^2 = 0.058$.
*** Significant at the 1-percent level.

For the purpose of taking such controls into account, we report multinomial logit regressions for separation from the December 1991 job, as in Eq. (1). The basic multinomial logit marginal effect results, including controls for experience, education, gender, and time effects, are displayed in Table 4. Consistent with the differences-of-means analysis in Fig. 1, privatization is negatively and statistically significantly associated with all three types of separations. Given that the unconditional mean for layoffs is 0.039, the marginal privatization effect of −0.019 implies that layoffs are cut approximately in half. Privatization also approximately halves the professional quit rate and lowers the personal quit rate by a third. Experience is unrelated to dismissals, but is negatively related to professional quits. At low levels of experience, the impact on personal quits is also negative, but the shape is strongly concave, becoming strongly positive at higher levels, presumably with retirement. More highly educated workers are estimated to face higher dismissal rates, but the marginal effect is statistically insignificant at conventional levels. Females have a higher probability of job loss and of quitting for personal reasons, but males engage more often in professional quits.

The specification in Table 5 includes additional worker and firm characteristics, as well as regional and sector dummies. Their inclusion has little effect on the privatization coefficients, which are still negative and significant. Tenure has a strong negative association with dismissals and professional quits. Skilled blue-collar workers and technicians are more likely to be dismissed than unskilled workers, and single persons are more likely to be laid off. Surprisingly, both quit rates are higher in larger firms.

We next distinguish between privatizations resulting in worker, manager, and outsider control. Table 6 shows that all three ownership types have negative effects on all types of separations. We can reject the hypothesis that workers are better able to protect themselves from dismissals if they control the firm post-privatization than if outsiders control it, as the outsider ownership coefficient is even more negative than the worker ownership coefficient. The managerial ownership coefficient is statistically significantly less negative than worker and outside ownership for professional quits and is significantly less negative than worker ownership for personal quits. These results diverge somewhat from Brown and Earle's (2003) from Russia. They find that outside blockholders are associated with less worker churning, similar to the findings here, but they also find that managerial ownership is associated with increased worker churning.

Table 5
Effect of privatization on separations with full controls

	Dismissal	Professional quit	Personal quit
Privatized	−0.019***	−0.010***	−0.012***
	(0.003)	(0.003)	(0.003)
Experience/100	0.069	−0.053	−0.331***
	(0.056)	(0.039)	(0.057)
$(\text{Experience}/100)^2$	−0.136	0.004	0.973***
	(0.109)	(0.080)	(0.105)
Education/100	0.025	0.013	−0.170**
	(0.091)	(0.081)	(0.086)
Female	0.005*	−0.013***	0.027***
	(0.003)	(0.002)	(0.003)
Tenure/100	−0.047***	−0.089***	0.007
	(0.017)	(0.015)	(0.014)
Skilled blue-collar	0.008*	−0.001	0.009***
	(0.004)	(0.003)	(0.003)
Technician	0.025***	−0.001	−0.013**
	(0.009)	(0.005)	(0.005)
Professional	0.008	−0.004	−0.000
	(0.006)	(0.004)	(0.005)
Computer use	−0.007	0.001	−0.006
	(0.005)	(0.005)	(0.006)
Married	−0.007**	−0.002	−0.004
	(0.004)	(0.003)	(0.003)
Small firm	−0.002	0.002	0.004
	(0.005)	(0.004)	(0.005)
Medium firm	0.003	0.010*	0.015***
	(0.005)	(0.005)	(0.006)
Large firm	−0.001	0.012**	0.010*
	(0.005)	(0.005)	(0.006)

Notes. The marginal effect is shown for each variable, with the standard error (corrected for firm clustering) in parentheses. Region, sector, and year dummies are also included. $N = 18{,}709$. Pseudo $R^2 = 0.072$.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

Table 6
Effects of ownership types on separations

	Dismissal	Professional quit	Personal quit
Worker-owned	−0.022***	−0.020***	−0.022***
	(0.006)	(0.001)	(0.005)
Manager-owned	−0.019***	−0.007***	−0.008*
	(0.003)	(0.002)	(0.005)
Outsider-owned	−0.030***	−0.017***	−0.023*
	(0.005)	(0.001)	(0.010)

Notes. The marginal effect is shown for each variable, with the standard error (corrected for firm clustering) in parentheses. All non-ownership variables from Table 5 are also included. $N = 18{,}006$.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

To see whether privatization creates winners and losers in firms, in the sense that it influences the relative propensity of different types of workers to be separated, we interact privatization with each of the worker characteristics, along with firm size. We do not interact privatization with all the variables at the same time, but rather estimate separate regressions for each of the characteristics or groups of characteristics where interactions are included, because of the limited number of privatizations in the sample. The specifications are otherwise identical to that in Table 5. As displayed in Table 7, we find that privatized firms lay off women with a similar propensity to state-owned firms. Privatization has an insignificant influence on the human capital coefficients, with the exception of skills. It lowers the dismissal and quit rates of professional employees, the quit rates of skilled blue collar workers and technicians, and the dismissal rate of workers who have used computers. No relationship is found with firm size. Finally, we interact privatization with the residual from a 1991 wage regression, so as to test whether privatized owners would have a greater propensity to lay off workers who had enjoyed rents during the Soviet period. No such evidence is found here.

Next we turn to the wage results. Figure 2 shows that mean real wage levels in state and privatized firms fell at similar rates through the year 2000, but state wages rebounded more strongly and a year earlier than those in privatized firms as the economy recovered from the aftershocks of Russia's 1998 fiscal crisis.

In a difference-in-difference specification controlling only for region and year effects (specification 1 in Table 8), privatization has an insignificant effect on the wage level. The insignificance of the ever private coefficient suggests no systematic difference in wage levels between to-be-privatized firms and always state-owned firms. We explore the plausibility of the data with a Mincerian wage regression in specification 2. The coefficients are highly significant and are of similar magnitudes to those found in other transition economies.[11] Specification 3 combines the variables from the first two specifications, and again the privatization coefficient is insignificant. Worker fixed effects are included in specification 4, which results in a lower and statistically sig-

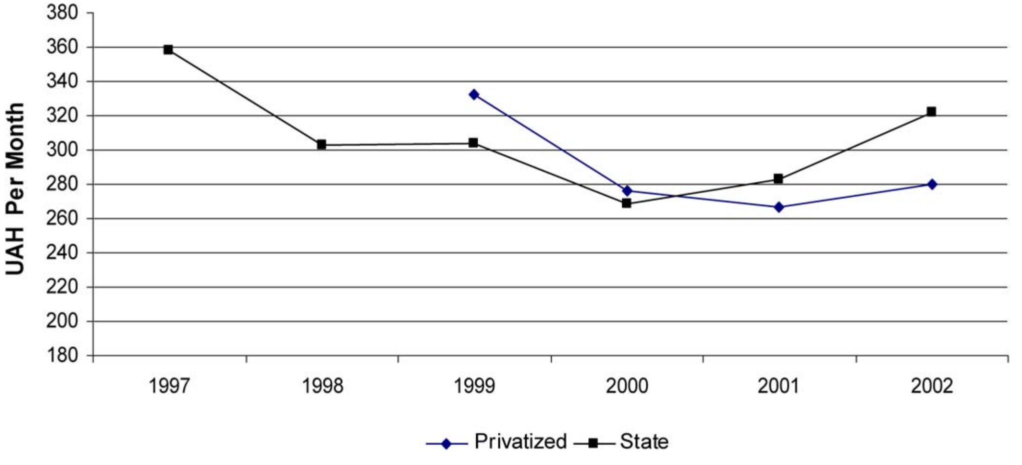

Fig. 2. State and privatized wages. *Note*: These are mean monthly wages in constant December 2002 UAH. Only privatizations occurring between 1998 and 2002 are included here.

[11] See, for example, Münich et al. (2005).

Table 7
Separation interactions regressions

	Dismissal	Professional quit	Personal quit
Privatized	−0.032***	−0.027***	−0.027
	(0.012)	(0.008)	(0.018)
Experience/100	0.052	−0.060	−0.348***
	(0.058)	(0.039)	(0.060)
$(\text{Experience}/100)^2$	−0.097	0.019	1.005***
	(0.113)	(0.081)	(0.112)
Privatized ∗ Experience/100	0.269	0.289	0.212
	(0.268)	(0.244)	(0.291)
Privatized ∗ $(\text{Experience}/100)^2$	−0.549	−0.504	−0.344
	(0.475)	(0.468)	(0.429)
Privatized	−0.016	−0.002	−0.002
	(0.017)	(0.027)	(0.023)
Education/100	0.028	0.020	−0.016*
	(0.095)	(0.082)	(0.090)
Privatized ∗ Education/100	−0.039	−0.102	−0.113
	(0.247)	(0.271)	(0.224)
Privatized	−0.023***	−0.008**	−0.017***
	(0.004)	(0.004)	(0.005)
Female	0.004	−0.012***	0.026***
	(0.003)	(0.002)	(0.003)
Privatized ∗ Female	0.016	−0.007	0.014
	(0.014)	(0.007)	(0.014)
Privatized	−0.016***	−0.018***	−0.005
	(0.007)	(0.005)	(0.009)
Tenure/100	−0.045***	−0.094***	0.010
	(0.017)	(0.015)	(0.014)
Privatized ∗ Tenure/100	−0.022	0.072	−0.035
	(0.048)	(0.045)	(0.042)
Privatized	−0.014**	0.001	0.008
	(0.006)	(0.006)	(0.009)
Skilled blue-collar	0.009**	0.001	0.012***
	(0.004)	(0.003)	(0.004)
Privatized ∗ Skilled blue-collar	−0.010	−0.010***	−0.020***
	(0.007)	(0.004)	(0.004)
Technician	0.025***	0.002	−0.009
	(0.010)	(0.005)	(0.006)
Privatized ∗ Technician	−0.010	−0.025***	−0.024***
	(0.013)	(0.001)	(0.009)
Professional	0.012*	−0.000	0.002
	(0.007)	(0.004)	(0.005)
Privatized ∗ Professional	−0.013*	−0.010**	−0.014**
	(0.008)	(0.004)	(0.006)
Privatized	−0.018***	−0.009***	−0.012***
	(0.003)	(0.003)	(0.004)
Computer	−0.004	0.003	−0.006
	(0.006)	(0.006)	(0.006)
Privatized ∗ Computer	−0.020**	−0.013	0.001
	(0.009)	(0.008)	(0.020)

(*continued on next page*)

Table 7 (*continued*)

	Dismissal	Professional quit	Personal quit
Privatized	−0.019***	−0.010	0.008
	(0.006)	(0.007)	(0.009)
Married	−0.007**	−0.002	−0.001
	(0.004)	(0.003)	(0.003)
Privatized ∗ Married	0.000	−0.001	−0.020***
	(0.012)	(0.011)	(0.005)
Privatized	−0.025***	−0.012	−0.009
	(0.009)	(0.012)	(0.016)
Small firm	−0.002	0.003	0.004
	(0.005)	(0.004)	(0.005)
Medium firm	0.003	0.009*	0.016***
	(0.005)	(0.005)	(0.006)
Large firm	−0.002	0.012**	0.011*
	(0.005)	(0.006)	(0.006)
Privatized ∗ Small firm	0.013	−0.005	−0.002
	(0.030)	(0.017)	(0.021)
Privatized ∗ Medium firm	0.009	0.007	−0.007
	(0.028)	(0.026)	(0.018)
Privatized ∗ Large firm	0.025	0.005	−0.001
	(0.036)	(0.024)	(0.022)
Privatized	−0.021***	−0.005	−0.014***
	(0.003)	(0.004)	(0.004)
1991 rents	0.003	−0.004	0.008**
	(0.003)	(0.003)	(0.003)
Privatized ∗ 1991 rents	−0.011	−0.014	−0.015
	(0.010)	(0.010)	(0.015)

Notes. Different regressions appear as separate blocks. The marginal effect is shown for each variable, with the standard error (corrected for firm clustering) in parentheses. All variables from Table 5 are also included in each regression.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

nificant privatization coefficient.[12] The inclusion of worker–firm fixed effects (specification 5) makes the coefficient slightly less negative, and it is significant only at the 10 percent level. As a robustness check, we add sector–year effects (specification 6) and region–year effects (specification 7), since wages could change differently across time by sector and/or region. Once again, the privatization coefficient is negative, though not significant when including sector–year effects. The fixed effects specifications suggest that privatization lowers wages by about 5 percent. This contrasts with Brown et al. (2005), who find no such privatization-induced wage reduction in Ukraine.

Table 9 shows the estimated effects of the disaggregated ownership variables on wages. The specifications are analogous to specifications 1 and 4–7 in Table 8. None of the ownership categories shows systematic differences from always state firms prior to privatization in the difference-in-difference specification. In the fixed effects specifications, worker and manager ownership have negative and usually significant effects, and outside ownership has a small

[12] We have also tried specifications including random effects, but they fail the Hausman test at the 1 percent level.

Table 8
Wage regressions

	Spec. 1	Spec. 2	Spec. 3	Spec. 4	Spec. 5	Spec. 6	Spec. 7
Privatized	−0.011		−0.002	−0.061**	−0.051*	−0.033	−0.051*
	(0.044)		(0.043)	(0.028)	(0.028)	(0.027)	(0.029)
Ever private	0.009		0.017	0.256**			
	(0.045)		(0.044)	(0.135)			
Experience		0.019***	0.019***				
		(0.004)	(0.004)				
Experience^2/1000		−0.422***	−0.420***				
		(0.080)	(0.079)				
Education		0.058***	0.058***				
		(0.008)	(0.008)				
Female		−0.355***	−0.355***				
		(0.028)	(0.028)				
Controls	R, Y	R, Y	R, Y	Y	Y	S–Y	R–Y
Fixed effects				Worker	Worker–Firm	Worker–Firm	Worker–Firm
R^2	0.124	0.237	0.237	0.232	0.258	0.268	0.289
N	5461	5424	5424	5461	5461	5418	5461

Notes. R = region dummies, Y = year dummies, R–Y = region-year dummies, and S–Y = sector-year dummies. The R^2 in the fixed effects specifications are R^2 within. Standard errors (corrected for firm clustering) are shown in parentheses.

* Significant at the 10% level.

** Idem, 5%.

*** Idem, 1%.

Table 9
Wage regressions with disaggregated ownership

	Spec. 1	Spec. 2	Spec. 3	Spec. 4	Spec. 5
Worker-owned	−0.133	−0.314***	−0.303***	−0.274**	−0.298***
	(0.167)	(0.118)	(0.118)	(0.113)	(0.120)
Ever worker	−0.126	−0.138***			
	(0.146)	(0.013)			
Manager-owned	−0.027	−0.080**	−0.072**	−0.048	−0.072**
	(0.061)	(0.036)	(0.036)	(0.035)	(0.037)
Ever manager	0.015	0.410***			
	(0.060)	(0.132)			
Outsider-owned	0.245	0.013	0.022	0.037	0.014
	(0.185)	(0.094)	(0.094)	(0.098)	(0.095)
Ever outsider	0.091				
	(0.244)				
Controls	R, Y	Y	Y	S–Y	R–Y
Fixed effects		Worker	Worker–Firm	Worker–Firm	Worker–Firm
R^2	0.127	0.238	0.265	0.272	0.298
N	5334	5334	5334	5291	5334

Notes. R = region dummies, Y = year dummies, R–Y = region-year dummies, and S–Y = sector-year dummies. The R^2 in the fixed effects specifications are R^2 within. Specification 1 also includes experience, experience sq., education, and female. Standard errors (corrected for firm clustering) are shown in parentheses.

** Significant at the 5% level.

*** Idem, 1%.

positive, but statistically insignificant effect on wages. The worker effect ranges from −28 to −31 percent, and the manager effect is −5 to −7 percent, and the difference in their coefficients is statistically significant at the 10 percent level. The worker and outsider coefficients are statistically significantly different from one another at the 5 percent level. These results in conjunction with the separations disaggregated ownership results suggest that workers clearly do better under outsider than insider ownership, particularly worker ownership. Aghion and Blanchard's (1998) trade-off between efficiency and worker welfare appears not to exist.

We next explore whether privatization affects wage differentials. To see the main effects of each variable of interest, we include additional worker and firm characteristics in a difference-in-difference specification in the first column of Table 10. There is a positive return to skills, but none to tenure. Married workers and those in larger firms enjoy a premium. In the second and

Table 10
Wage regression with full controls

	Pooled	1997	2002
Privatized	−0.025		
	(0.043)		
Ever private	0.030		
	(0.045)		
Experience	0.017***	0.021***	0.023***
	(0.004)	(0.005)	(0.006)
Experience^2/1000	−0.416***	−0.432***	−0.611***
	(0.085)	(0.114)	(0.117)
Education	0.025***	0.036***	0.011
	(0.010)	(0.014)	(0.014)
Female	−0.325***	−0.348***	−0.305***
	(0.031)	(0.040)	(0.042)
Tenure	0.002	0.000	0.004*
	(0.002)	(0.002)	(0.002)
Skilled blue-collar	0.166***	0.147**	0.194***
	(0.048)	(0.066)	(0.064)
Technician	0.212***	0.180**	0.341***
	(0.063)	(0.085)	(0.078)
Professional	0.299***	0.238***	0.430***
	(0.063)	(0.083)	(0.086)
Computer use	0.136***	0.028	0.142***
	(0.042)	(0.094)	(0.053)
Married	0.070*	0.075	0.086*
	(0.038)	(0.052)	(0.052)
Small firm	0.154***	0.038	0.171***
	(0.047)	(0.062)	(0.063)
Medium firm	0.183***	0.053	0.299***
	(0.057)	(0.073)	(0.073)
Large firm	0.261***	0.151**	0.352***
	(0.060)	(0.075)	(0.076)
R^2	0.346	0.314	0.420
N	4482	868	718

Notes. Sector, region, and year dummies are also included. Standard errors (corrected for firm clustering) are shown in parentheses.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

third columns we report cross section regressions in 1997 and 2002, so as to see differences in returns across time. The coefficient on tenure has become positive, while that on education has fallen by two-thirds. The gender wage gap has been reduced by over four percentage points and the marriage premium has increased by one point. Returns to skills have increased considerably, as has the wage premium for working in larger firms. The statistically significant changes are technician (10-percent level), small firm (10-percent level), medium firm (1-percent level), and large firm (5-percent level).

Has privatization played a role in any of these wage differential changes? To test this, we interact each of the characteristics with privatization, done analogously to what was done in the separations analysis above.[13] Table 11 shows that privatization has had an insignificant effect on the changes in returns to education and the gender wage gap, while it has played an important role in increasing the returns to professionals and computer users and raising the large firm

Table 11
Wage interactions regressions

Privatized	0.070
	(0.108)
Privatized ∗ Experience	−0.010
	(0.010)
Privatized ∗ $Experience^2/1000$	0.181
	(0.189)
Privatized	0.059
	(0.170)
Privatized ∗ Education	−0.011
	(0.016)
Privatized	−0.045
	(0.033)
Privatized ∗ Female	−0.012
	(0.056)
Privatized	−0.039
	(0.043)
Privatized ∗ Tenure	−0.001
	(0.002)
Privatized	−0.081
	(0.055)
Privatized ∗ Skilled blue-collar	−0.012
	(0.066)
Privatized ∗ Technician	0.017
	(0.116)
Privatized ∗ Professional	0.128*
	(0.076)
Privatized	−0.063**
	(0.029)
Computer	0.186***
	(0.040)
Privatized ∗ Computer	0.182***
	(0.068)
Ever private ∗ Computer	−0.217***
	(0.078)
Privatized	−0.139**
	(0.062)
Privatized ∗ Small firm	0.023
	(0.085)
Privatized ∗ Medium firm	0.085
	(0.074)
Privatized ∗ Large firm	0.170**
	(0.074)
Privatized	0.069
	(0.060)
Married	−0.009
	(0.076)
Privatized ∗ Married	−0.146**
	(0.066)
Ever private ∗ Married	−0.080
	(0.158)

Notes. Different regressions appear as separate blocks. Year dummies and worker–firm fixed effects are also included. Standard errors (corrected for firm clustering) are shown in parentheses.

* Significant at the 10% level.
** Idem, 5%.
*** Idem, 1%.

[13] It would be interesting to run interactions with disaggregated ownership, but unfortunately the data contain too few worker and outsider privatizations to produce reliable estimates.

wage premium. In contrast, privatization has significantly attenuated the increase in the marriage premium.

5. Conclusion

Workers frequently express strong concerns about how privatization may affect their job security and wages, and their pessimistic expectations seem to be widely shared by many observers who believe that layoffs and wage cuts are a cost of increased productive efficiency. But whether privatization actually produces these consequences for workers has been the subject of relatively little systematic analysis. Some previous studies in this area have used firm-level data, where selection of firms into ownership type could be controlled for, but worker characteristics, composition, turnover, and self-selection could not. Other studies have used a cross-sectional approach with worker-level data, where neither type of selection process can be handled and the ownership and wage variables are sometimes poorly measured.

In this paper, we control for both worker and firm selection, as worker–firm matches are followed before and after privatization. Motivated by the questions in this paper, the instrument used to collect the ULMS data was designed to contain better ownership questions than past surveys, allowing us to make clearer distinctions between state, privatized, and new private firms, as well as to analyze the effects of worker-, manager-, and outsider-controlled firms separately. The survey paid careful attention to the measurement of wages to avoid the extreme volatility created by wage arrears. The survey sample was carefully selected and it appears to be genuinely representative. However, a disadvantage of the survey data is the small sample size. In particular, the small number of privatizations resulting in worker and outsider control makes it difficult to draw strong conclusions about the differences in their effects relative to managerial control, where there are many observations. A second possible disadvantage is the retrospective nature of the data, which is necessary for our longitudinal estimation methods but which may produce recall errors.

Bearing potential problems in mind, we have constructed our empirical methods to exploit the structure of the data and maximize the possibilities for identifying the effects of privatization. In our analysis of worker separations, we focus on jobs held by workers in 1991, well before privatization really started, and we follow these jobs until a separation occurs or 2002, whichever comes first. The data permit us to distinguish three principal categories of separations, layoffs and professional and personal quits, and they contain a rich set of control variables for the analysis. Some firms are privatized and others remain state-owned, so that identification is based on the comparison of not only before and after privatization but also relative to the always state-owned comparison group. In our analysis of wage effects, we examine a panel of workers initially in state-owned employers with annual observations from 1997 to 2002. We follow these workers' wages even if they move across employers, as long as the new employer is state-owned. This procedure allows us to control not only for detailed worker characteristics but also for worker fixed effects, and in alternative specifications for worker–firm match fixed effects. The latter estimator is identified by changes in ownership during worker–firm matches, using all employment spells when there are no changes as the comparison group.

The results of the separations analysis suggest that privatization reduces worker separations of all types, halving the dismissal and professional quit rates. Wage levels are also reduced by about five percent, however. Workers in worker-controlled firms have suffered large wage losses, while those in outsider-controlled firms may have enjoyed wage gains. A possible explanation for this pattern could be that worker-controlled firms have not enjoyed substantial efficiency gains, necessitating labor cost cuts. Workers have chosen to accept lower wages in exchange for continued employment. In contrast, workers in outsider-controlled firms need not make such an unpleasant trade, as the firms may have expanded their scale, making cuts in labor costs unnecessary.

The biggest winners from privatization appear to be high-skilled workers in large firms. Private owners may have invested more in new technologies, leading to increased demand for skilled workers. This is particularly likely to be true in large firms, which tend to be more capital-intensive and have more capital–skill complementarity.

Workers have frequently opposed privatization, particularly to outside investors. However, the evidence in this paper implies that Ukrainian workers suffered little if at all from privatization, at least in terms of job security and wages. Those in outsider-privatized firms appear to have gained. Of course, workers' fears of privatization could relate to considerations other than employment and wages, but this is a question for future research.

Acknowledgments

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Appendix A. Variable definitions

Computer is equal to 1 if the person has used a computer (on the job or elsewhere) prior to year t.

Education is the adjusted years of schooling in year t. We impute the modal value of years of schooling by five-year age cohort for each level of completed education.

Ever private is equal to 1 if the firm is privatized during the observation period of the regression sample.

Experience is the number of years, as of year t, since the respondent started the first job.

Female is equal to one if the person is female.

Large firm is equal to 1 if the employer has 1000 or more employees, as reported by the worker.

Manager-owned is equal to 1 if the employer was privatized in year $t-1$ or before and the largest proportion of shares was owned by managers of the firm after the ownership change.

Married is equal to 1 if the worker is married in year t.

Medium firm is equal to 1 if the employer has 250–999 employees, as reported by the worker.

Micro firm is equal to 1 if the employer has 19 or fewer employees, as reported by the worker. This is the omitted firm size category in the regressions.

Outsider-owned is equal to 1 if the employer was privatized in year $t-1$ or before and the largest proportion of shares was owned by outside domestic shareholders or foreign investors after the ownership change. We combine these two groups, because there are so few firms where foreign investors hold the largest proportion of shares.

Privatized is equal to 1 if the worker reports an ownership change in the employer from state firm (defined as a budgetary organization, state enterprise, or a local municipal enterprise) to privatized firm while the worker was employed at the firm, the majority of shares was owned by private entities after the change, and the change occurred in year $t-1$ or before.

Professional is equal to 1 if the worker's occupation is in the one-digit ISCO categories of legislators, senior officials, and managers (1) or professionals (2).

Separations are divided into four categories: dismissals (closing down of the enterprise, reorganization of the enterprise, bankruptcy of the enterprise, privatization of the enterprise, dismissal initiated by employer, personnel reduction, expiring of the employment contract, and expiring of probation time), professional quits (wanted/was proposed higher salary, wanted/was proposed better working conditions, wanted/was proposed more interesting work, wanted to start own business, main job became second job, and end of farming/sole proprietorship), and personal quits (military service, imprisonment, own illness or injury, studies, retirement, early retirement, marriage, parental leave, need to take care of other members of family, and change of residence). If workers provide multiple reasons for the separation, we prioritize dismissal reasons over professional reasons over personal reasons, except for expiring of employment contract or probation time, in which case we prioritize professional and then personal reasons over dismissal reasons. The reason for dividing quits into ones for professional reasons and ones for personal reasons is that the former are more likely to be disguised layoffs. The separations variable is measured as of the end of year t for the December 1991 job. It is set to missing if the person did not hold the job in December of year $t-1$.

Small firm is equal to 1 if the employer has 20–249 or more employees, as reported by the worker.

Skilled blue-collar is equal to 1 if the worker's occupation is in the one-digit ISCO categories of clerk (4), service workers and shop and market sales workers (5), skilled agricultural and fishery workers (6), craft and related workers (7), or plant and machine operators and assemblers (8).

Technician is equal to 1 if the worker's occupation is in the one-digit ISCO category of technicians and associate professionals (3).

Tenure is the number of years since the job start year as of year t.

Unskilled is equal to 1 if the worker's occupation is in the one-digit ISCO category of elementary occupations (9). This is the omitted skill category in the regressions.

Wage is the monthly contractual wage after taxes at the primary job in December of year t. It is converted to December 2002 hryvnyas, using the Ukrainian State Statistics Committee December-to-December national consumer price indices. Regional deflators are not available prior to 2001.

Worker-owned is equal to 1 if the employer was privatized in year $t-1$ or before and the largest proportion of shares was owned by the firm's non-managerial workers after the ownership change.

1991 rents is the residual from a 1991 wage regression including experience, experience squared, education, female, tenure, skill dummies, married, firm size dummies, economic sector dummies, and six regional dummies.

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