ABSTRACT

Title of dissertation: ESSAYS ON THE ROLE OF INSTITUTIONS IN PRODUCTIVITY AND REALLOCATION DYNAMICS

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Dissertation directed by: Professor John C. Haltiwanger
Department of Economics

Recent empirical work has shown that the success of an economy depends largely on how successful it is in allocating inputs and outputs across businesses efficiently with minimum disruption and frictions. Reallocation of factors of production plays a major role in productivity growth and it is driven by technological and market forces, coupled with institutional factors. We examine the impact of institutions on allocative efficiency, job flows and wage structure, using longitudinal micro level databases.

First, we estimate the impact of state aid for the rescue and restructuring of firms in difficulty on productivity and allocative efficiency. We use treatment effects estimators allowing for selection on unobservables and exploit variables that affect the chances of getting aid before 2002, but not after, to identify this impact. The empirical analysis indicates that state aid hindered the efficient allocation of resources and prolonged the life span of aid-receiving firms.

Next, we assess the importance of technological factors that characterize different industries in explaining cross-country differences in job flows. We find that

industry/technology and size factors explain a large fraction of the overall variability in job flows, but there remain significant differences in job flows that could reflect differences in business environment conditions. We use a difference-in-difference approach to examine the impact of regulations on worker hiring and firing. The empirical results suggest that stringent hiring and firing costs reduce job turnover and distort the patterns of industry/size flows.

Finally, we study the structure of wages and the importance of firm and person fixed effects in explaining the variance of log real hourly wages in Slovenia, using a longitudinal matched employer-employee database. Most significant changes in employment and wage setting policies occurred in 1991, but incomes policies still suppressed the growth of managerial wages until 1997. We find that this change brought about a change in the wage structure, with an increase in returns to education for the most educated workers. Our results also indicate that person fixed effects account for an overwhelming majority of variation in log real hourly wages, whereas firm fixed effects are not nearly as important.

ESSAYS ON THE ROLE OF INSTITUTIONS IN PRODUCTIVITY AND REALLOCATION DYNAMICS

by

Helena Schweiger

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Advisory Committee:
Professor John C. Haltiwanger, Chair
Professor John Shea
Professor Jeffrey A. Smith
Professor Peter Murrell
Professor Curtis Grimm

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Chapter 1

Introduction

Over the past decade, numerous empirical studies have found evidence suggesting that the reallocation of factors of production plays a major role in driving productivity growth (see, e.g., Olley and Pakes [1996], Griliches and Regev [1995], Foster et al. [2001] and Bartelsman et al. [2004b]). The success of an economy depends largely on how successful it is in allocating inputs and outputs across businesses efficiently with the minimum disruption and frictions (see, e.g., Eslava et al. [2004]). Allocation of inputs and outputs is occurring on a daily basis, via the entry of new firms and exit of unprofitable firms, as well as via the continuing process of adaptation of incumbent firms in response to the development of new products and processes, the growth and decline in markets and changes in competitive forces (Davis and Haltiwanger [1999]).

Since such reallocation is part and parcel of aggregate productivity growth, it is important to understand its nature and what causes the differences in aggregate productivity growth across countries. Part of the explanation are market conditions and technological factors. For example, some industries have higher job flows than others and some industries have to replace the technology they use more often than others. Smaller businesses are more dynamic because they adjust through a learning-by-doing process. Another part of the explanation is institutional factors, which work either in the same or in the opposite direction as the technological and market driven forces. Caballero and Hammour [2000a] argue that a poor institutional environment inhibits reallocation and causes stagnation in the process of creative destruction. For example, if establishing a new firm is costly both in terms of money and time, entry of new firms will be low, and incumbent, yet unprofitable, firms will be more likely to stay in business.

Events in the past two decades offer a host of opportunities to study the intertwinement of technological and market driven factors with institutional factors in explaining why certain countries grow faster than others. Studies of the transition of former socialist and communist economies towards a market economy as well as deregulation and liberalization in other parts of the world (India, Latin America) provide evidence for the increased importance of market driven factors as an unfavorable and prohibitive institutional environment improves. Eslava et al. [2004], for example, find that the net contribution of entry and exit to aggregate productivity rose slightly after market reforms in Colombia. Research is further fueled by the increasing availability of longitudinal micro level databases which are instrumental in applying theoretical advances to empirical work. These data reveal substantial heterogeneity across businesses and workers.

This thesis makes use of such longitudinal micro level databases in examining the role of institutions in productivity and reallocation dynamics. For the most part, it is focused on Slovenia, one of the most successful transition countries, and its experience during the course of transition with respect to the allocation of resources and structure of wages. We augment it with findings from a cross-country study of the impact of institutions on job flows. We do not consider welfare implications in any of the chapters that follow.

The outline of the thesis is as follows. In Chapter 2, we present the impact of state aid for the rescue and restructuring of firms in difficulty on the allocation of resources. Such aid can postpone the exit of unprofitable firms and thus shift the burden of structural adjustment onto more efficient firms who are managing without it. We investigate the impact of this aid on the static and dynamic efficiency of Slovenian manufacturing by combining the aid data with firm-level accounting data. Evaluating the impact of state aid on the allocative efficiency of an economy is difficult due to the lack of a counterfactual and because of selection bias. The latter arises because firms that receive aid may differ from firms that do not receive aid along other dimensions. We use treatment effects estimators that assume selection on observables (linear regression models) and estimators that explicitly allow for selection on unobservables (instrumental variables). Our identification strategy in the latter models involves using variables that affect the chances of getting aid before 2002, but not after. The empirical analysis reveals that state aid hindered the efficient static allocation of resources, as measured by the Olley and Pakes [1996]-inspired micro covariance measure. None of the firms that received aid exited, aid had a positive impact on the growth rate of market shares, but aid did not have a significant impact on the growth of TFP (total factor productivity). These results suggest that aid was distortive.

In Chapter 3, we discuss the role of industry, size and regulations in the magnitude of job flows across a sample of 16 industrial and emerging economies over the

1990s. We exploit a harmonized firm level dataset drawn from business registers and enterprise census data. The chapter assesses the importance of technological factors that characterize different industries in explaining cross country differences in job flows. It shows that industry effects play an important role in shaping job flows at the aggregate level. Even more importantly, differences in the size composition of firms within each industry explain a large fraction of the overall variability in job creation and destruction. However, even after controlling for industry/technology and size factors there remain significant differences in job flows across countries that could reflect differences in business environment conditions. We look at one factor shaping the business environment, namely regulations on the hiring and firing of workers. To minimize possible endogeneity and omitted variable problems associated with cross country regressions, we use a difference-in-difference approach. The empirical results suggest that stringent hiring and firing regulations reduce job turnover, especially in those industries that require more frequent labor adjustment. Regulations also distort the patterns of industry/size flows. Within each industry, medium and large firms are more severely affected by stringent labor regulations, while small firms are less affected, probably because they are partially exempted from such regulations or can more easily circumvent them.

In Chaper 4, we study the structure of wages and the importance of person and firm fixed effects in explaining the variance of log real hourly wages in Slovenia in the 1990s, using a matched employer-employee database covering more than 90 percent of the economy. The late 1980s and 1990s were a period of fundamental political and economic changes in Slovenia, with significant changes in employment and wage policies. Under self-management, wage scales were extremely compressed. The new, three-component wage setting system, provided more flexibility in 1991, allowing firm- and worker-specific deviations from the wage guidelines set in collective bargaining agreements. However, the incomes policies still suppressed the growth of wages, especially of the managerial workforce, until 1997. We exploit this change in the wage setting system to compare the wage structure in the early 1990s to the wage structure in the late 1990s. We estimate a model with observable person characteristics, a model with observable person characteristics and industry or firm fixed effects, and finally a model with firm and person fixed effects and time-variant person effects. Our results indicate that the wage structure changed significantly in the late 1990s compared to the early 1990s, even though the increase in returns to education was not as large as it was in the first few years of the transition. The magnitude, however, depends on the model assumed. We show that it is extremely important to include person fixed effects in the model, as they accounted for more than 90 percent of the variation in log real hourly wages. Firm fixed effects are not nearly as important - hence, it appears that although where you work is not of negligible importance, it matters much more how good you are at what you do.

Chapter 5 provides the conclusion to the thesis.

Chapter 2

The Impact of State Aid for Restructuring on the Allocation of Resources

2.1 Introduction

Aggregate productivity growth has been the subject of numerous studies, and our understanding of it as well as its measurement has improved since Solow [1957]. We now know that the representative firm paradigm, on which the aggregate production function approach is based, does not hold in the real world; on the contrary, there is substantial heterogeneity across businesses. Longitudinal micro business databases are becoming more widely available, allowing one to study the restructuring of economies due to a continuous process of entry and exit of businesses, and expansion and contraction of incumbents.

Recent empirical work has shown that the success of an economy depends largely on how successful it is in allocating inputs and outputs across businesses efficiently with minimum disruption and frictions (see, e.g., Eslava et al. [2004]). The process of allocation consists of two complementary components, cross-sectional (static) allocation and longitudinal (dynamic) allocation of inputs and outputs from less productive to more productive businesses. Olley and Pakes [1996] investigate the first component, whether more productive businesses have a higher market share.

The importance of the second component is explored by, among others, Foster et al. [2001]. They find that most productivity growth (using a five-year window) is explained by growth within firms, but that the net contribution of entry and exit is far from negligible.

Cross-country studies find that there are substantial differences among countries both in the contribution of continuing, entering and exiting firms to aggregate productivity growth as well as in their cross-sectional allocative efficiency. Possible explanations for this heterogeneity lie in different market institutions and market structures. Caballero and Hammour [2000a] argue that the function of institutions is twofold, one of efficiency and one of redistribution, and both are important for macroeconomic outcomes. A poor institutional environment results in technological "sclerosis" since it "permits low-productivity units to survive longer than they would in an efficient equilibrium" (Caballero and Hammour [2000a], p. 20) and thereby causes stagnation in the process of creative destruction. Escribano and Guasch [2004] and Haltiwanger and Schweiger [2005] find that an adverse business climate has a negative impact on static allocative efficiency. Eslava et al. [2004] find that after market reforms in Colombia, the net contribution of entry and exit to aggregate productivity rose slightly and that surviving entrants exhibited more rapid productivity growth than incumbents. Olley and Pakes [1996] reach a similar conclusion for static allocative efficiency in the telecommunications equipment industry after a deregulation. Based on the evidence from a number of developing countries, Tybout [2000] suggests that policies matter: "market share turnover rates are much higher in Korea and Taiwan than in Latin America, where labor markets are relatively regulated" (p. 27).

A problem with the above studies is that they use quite broad measures of institutions, mostly at the country level, and/or survey data with questionable representativeness of the entire economy.[1] This chapter is the first to use census micro level data on a market institution with a potentially very distortive effect, namely state aid for the rescue and restructuring of firms in difficulty.[2] Such aid can postpone the exit of unprofitable firms and thus shift the burden of structural adjustment onto more efficient firms who are managing without it. This problem is relevant not only in transition and developing economies, but also in developed economies. State aid provides soft budget constraints to the firms receiving it, and soft budget constraints have an influence on the life-cycle of firms and thus market selection, which in turn affects aggregate productivity growth.[3] A study by London Economics [2004] is the only existing work to evaluate the impact of rescue and restructuring aid on international competitiveness, but they focus only on a few sectors and case studies.

We investigate the impact of aid for the rescue and restructuring of firms in difficulty on static and dynamic efficiency[4] of Slovenian manufacturing in the period from 1998 to 2003 by combining firm-level data on state aid with firm-level accounting data and information on aid legislation. Slovenia is a transition economy, but major changes in its economy happened prior to 1998, and it is comparable to

[1] Surveys such as the World Bank's Productivity and Investment Climate Survey tend to focus on medium and large businesses, and hence their samples are not necessarily representative of the entire economy.

[2] In this chapter, "state aid" and "aid" refer to the state aid for the rescue and restructuring of firms in difficulty, unless explicitly stated otherwise.

[3] See Kornai et al. [2003] for a definition and survey of the soft budget constraints literature.

[4] From the social viewpoint, such aid might have some positive effects, such as keeping people employed, but we are primarily interested in its impact on allocative efficiency.

other old EU member countries in terms of state aid per capita and percentage of sectoral aid in GDP, which includes restructuring and rescue aid.[5] The majority of non-agricultural state aid has been oriented towards manufacturing in almost all members of the EU, and policy makers in many countries have paid special attention to manufacturing, either because they view it as "the leading edge of modernization and skilled job creation, as well as a fundamental source of various positive spillovers" (Tybout [2000], p.11) or because a lot of restructuring was going on in manufacturing in the 1990s.

Evaluating the impact of aid on static and dynamic efficiency is difficult due to the lack of a counterfactual and the need to handle selection bias. The latter arises from the fact that firms that receive aid usually differ from firms that do not receive aid along other dimensions. We use treatment effects estimators that assume selection on observables and estimators that explicitly allow for selection on unobservables. Our identification strategy in the latter models involves firm-level variables that affect the likelihood of receiving aid prior to 2002, but not after 2002, when Slovenia sharply scaled back its aid in order to comply with EU regulations. None of the firms that received aid ceased to exist during this period and, overall, exit rates were low compared to those in the OECD countries, so most of the "action" happened among the continuing firms and the allocation of resources among them. The empirical analysis reveals that state aid for the rescue and restructuring of firms in difficulty hindered the efficient allocation of resources, and that it had a positive

[5]This can be seen from the data on state aid in Commission of the EC [2004], presented in Appendix A.

impact on the growth rate of market shares of weak firms. This indicates that aid had a distortive effect.

The outline of the chapter is as follows. In Section 2.2, we review related literature on static and dynamic efficiency. We describe the background and data sources in Section 2.3. In Section 2.4, estimation methods are discussed and Section 2.5 presents the results. Section 2.6 concludes.

2.2 Allocative Efficiency

Aid can postpone the exit of unprofitable firms and thus shift the burden of structural adjustment onto more efficient firms who are managing without it. Hence, aid can have an impact not only on the exit decision of firms, but also on the allocation of inputs and outputs across firms. Aggregate productivity growth depends on both, as we explain in Sections 2.2.1 and 2.2.2.

2.2.1 Static Allocative Efficiency

Aggregate productivity and its growth depend not only on how productive businesses are on average, but also on whether more productive businesses have a higher market share. Olley and Pakes [1996] show this formally by decomposing aggregate productivity P in an industry j at time t as follows:

$$P_{t,j} = \bar{p}_{t,j} + \sum_{i=1}^{H_{t,j}} \Delta s_{it,j} \Delta p_{it,j}$$

$$\Delta s_{it,j} = s_{it,j} - \bar{s}_{t,j} \qquad \Delta p_{it,j} = p_{it,j} - \bar{p}_{t,j} \tag{2.1}$$

where i denotes firm, t denotes time, j denotes 2-digit industry, bar denotes unweighted average, H represents the number of firms, s is firm i's domestic market share in industry j and p is a measure of productivity.

The first term in (2.1) is unweighted average productivity in industry j at time t and the second term is a covariance term, which measures cross-sectional allocative efficiency. Ideally, this covariance would be positive, which happens when firms with higher (lower) productivity than average have higher (lower) than average market shares. Besides providing an intuitive and compact measure of allocative efficiency, another advantage of the covariance is that it is more comparable across sectors than average productivity itself, since the first moment differences across sectors are differenced out. It is a cross-sectional measure, but it makes sense to compare its values over time: a higher covariance in year t than in year $t-1$ implies that the economy has improved its allocative efficiency.

Based on equation (2.1), there are two sources of aggregate productivity growth: improvements in productivity of the average firm and improvements in allocative efficiency:

$$P_{t,j} - P_{t-1,j} = (\bar{p}_{t,j} - \bar{p}_{t-1,j}) + \left(\sum_{i=1}^{H_{t,j}} \Delta s_{it,j} \Delta p_{it,j} - \sum_{i=1}^{H_{t-1,j}} \Delta s_{it-1,j} \Delta p_{it-1,j} \right). \quad (2.2)$$

Olley and Pakes [1996] find that in the US telecommunications equipment industry, the source of aggregate productivity growth was the reallocation of output from less productive to more productive plants. Following the deregulation of the industry, the allocation of output improved significantly.

A burgeoning literature of cross-country studies, such as Bartelsman et al. [2005] using a World Bank dataset on firm demographics and productivity,[6] find that there is substantial heterogeneity in static allocative efficiency among countries. Escribano and Guasch [2004] and Haltiwanger and Schweiger [2005] use firm-level data from the World Bank's Productivity and the Investment Climate Survey and come to a similar conclusion.

We use TFP as a measure of productivity. Labor-productivity-based covariance only looks at whether workers and output are allocated in less productive or more productive firms, and this captures only part of allocative efficiency, since workers are not the only input used in production. For the economy to achieve a higher productivity, other inputs such as capital need to be allocated to more productive firms as well. If a firm lays off workers who subsequently find work in more productive firms, but keeps the machinery and equipment these workers used laying idle, this will have a positive impact on labor-productivity-based covariance, but not necessarily on TFP-based covariance. Due to this fact and the problems with the measure of labor mentioned in Appendix B, where a more detailed description of productivity measurement is available, our preferred measure of productivity is TFP.

We define a firm-level measure of allocative efficiency, called micro covariance, as a cross product between the percentage deviation of the firm's market share from the average market share in industry j and the deviation of the firm's log

[6] A detailed technical description of the dataset may be found in Bartelsman et al. [2004a]. It contains several firm demographics and productivity indicators and was prepared using firm-level data.

productivity from the average firm-level log productivity in industry j. That is,

$$\frac{\Delta s_{it,j}}{\bar{s}_{t,j}} \Delta p_{it,j}. \tag{2.3}$$

Escribano and Guasch [2004] define a similar micro covariance measure without dividing by $\bar{s}_{t,j}$. The advantage of the definition in equation (2.3) is that it is independent of scale, since it is a product of a percentage deviation and a log deviation. The measure of allocative efficiency defined in (2.3) is interpreted as the contribution of a firm to aggregate allocative efficiency. If the firm has above average productivity and above average market share, then this measure is positive and shows that the firm contributes positively to aggregate allocative efficiency. If a firm has below average productivity, but above average market share, this measure is negative and implies that there are some imperfections in the economy that allow the less productive firm to keep a higher market share than would correspond to its lower than average productivity.

Both Escribano and Guasch [2004] and Haltiwanger and Schweiger [2005] examine the empirical relationship between cross-country differences in static allocative efficiency and cross-country differences in business climate. They find that an adverse business climate has an adverse impact on static allocative efficiency. Escribano and Guasch [2004] show that simple non-linear models with interaction terms explain almost 30 percent of the efficiency variation between Guatemala, Honduras and Nicaragua.

One drawback to both of these papers is that they use survey data that over-represents medium and large firms and old firms, as shown in Haltiwanger and Schweiger [2004]. In addition, the data consist only of continuing firms; entry and exit cannot be measured. A second potential drawback is that the business climate indicators are based on the qualitative, subjective responses of firm administrators, and measure the perception of firms about the business climate in the country, not the actual business climate. For certain purposes that is precisely what is desired, but the causal interpretation of the link between the perception of the business climate and firm performance is problematic. On the one hand, firms may be convinced that an adverse business climate is the main culprit for their woes and use it as an excuse not to take steps to improve performance. On the other hand, a firm that is doing well will probably be satisfied with the business climate. Perception of the business climate might also depend on who answers the survey and his or her attitude towards the government. The advantage of our chapter is that it uses administrative census firm-level data on state aid and firm characteristics.

2.2.2 Dynamic Allocative Efficiency

Reallocation of output from less productive to more productive firms is facilitated by reallocation of output among continuing firms as well as by entry and exit, as Olley and Pakes [1996] note. Griliches and Regev [1995] decompose the growth of aggregate productivity into contributions of continuing, entering and exiting firms:[7]

$$
\begin{aligned}
\Delta P_t &= \sum_{i \in C} \left(\frac{s_{it} + s_{it-k}}{2} \right) (p_{it} - p_{it-k}) \\
&+ \sum_{i \in C} (s_{it} - s_{it-k}) \left[\left(\frac{p_{it} + p_{it-k}}{2} \right) - \left(\frac{P_t + P_{t-k}}{2} \right) \right] \\
&+ \sum_{i \in N} s_{it} \left[p_{it} - \left(\frac{P_t + P_{t-k}}{2} \right) \right] - \sum_{i \in X} s_{it-k} \left[p_{it-k} - \left(\frac{P_t + P_{t-k}}{2} \right) \right], \quad (2.4)
\end{aligned}
$$

where Δ now represents a change between year $t-k$ and year t,[8] p_{it} is the i-th firm's productivity level, s_{it} is the i-th firm's share of output, C, N, and X are sets of continuing, entering and exiting firms, respectively, and P_t is the aggregate productivity level in year t. The first term is the within-firm effect, which reflects within-firm productivity growth weighted by the average output share. The second term is the between-firm effect, reflecting the gains in aggregate productivity coming from the expanding market shares of high productivity firms, or from low productivity firms' shrinking shares. The last two terms capture the contribution of entering and exiting firms, respectively.

Using the already mentioned World Bank dataset on firm demographics and productivity, Bartelsman et al. [2004b] find that most productivity growth (using a five-year window) is explained by the within-firm component, but the net contribution of entry and exit is far from negligible - it is generally positive and accounts for between 20 to 50 percent of total productivity growth. Schumpeterian "creative

[7]Other versions of this decomposition are shown by Baldwin and Gorecki [1991], Baldwin [1995] and Foster et al. [2001].

[8]Note that in equation (2.1), Δ represents a deviation from an unweighted average, and is a cross-sectional difference.

destruction" is thus occuring, as more productive entrants appear to displace less productive exiting businesses.

However, this does not mean that it is not worth looking at countries with a low contribution of entering and/or exiting firms. In that case, the reallocation of output happens predominantly among continuers, and the fact that the contribution of entering and/or exiting firms is low could indicate there are some important frictions in the economy. Indeed, existing studies find that market institutions play an important role. Disney et al. [2003b] find that increased competition boosts productivity and, on the flip side, that keeping poorly performing plants alive removes an important contribution to productivity growth. Similarly, Olley and Pakes [1996] find that deregulation in the telecommunications industry "improved performance by inducing a reallocation of capital to more productive plants" (p. 1292).

We follow standard conventions in the literature to define continuing, entering and exiting firms. We determine continuing firms, entering, exiting, and one-year firms on the basis of the availability of their accounting data, using the following conventions:[9]

[9] Since some firms have missing data for some years, strictly applying the above rule would result in spurious entry and exit. We account for such missing years in our measurement procedure. For example, if firm A is in the sample in $t-1$ and t, as well as in $t+2$, but not in $t+1$, we treat firm A as a continuing firm in t. We rely on firm birth data in combination with the above rule to identify entering firms. We use the online Business Register and the Official Gazzette to determine whether a firm actually exited or ceased to exist as a result of a merger or acquisition, in which case we do not classify it as an exiting firm.

Firm Type	Definitions
Continuing firm (CO)	Exists in $t-1$, t, and $t+1$
Entering firm (EN)	Exists in t and $t+1$, but not in $t-1$
Exiting firm (EX)	Exists in $t-1$ and t, but not in $t+1$
One-year firm (OY)	Exists in t, but not in $t-1$ and $t+1$

2.2.3 Aid for the Rescue and Restructuring of Firms and Allocative Efficiency

Theories of market selection (for example, Jovanovic [1982] and Caballero and Hammour [1994]) point out the productivity-survival link as a "crucial driver of productivity growth" (Foster et al. [2003]): more productive firms grow while less productive firms shrink and eventually cease to exist. Aid to firms in difficulty can prolong the life span of firms and thus have an impact on allocative efficiency. It may help low productivity firms obtain higher market shares, stifling the market shares of high productivity firms, and have a negative impact on static allocative efficiency. The measure proposed in Section 2.2.1 has the advantage of addressing both the market share and productivity channels of impact on aggregate productivity succinctly, and could as such be a good indicator for the impact of aid. Its disadvantages are that it provides no information on which one of the two channels is actually affected by aid and that it provides no information on the impact of aid on exit.

There are a couple of alternative ways of gauging the impact of aid on reallocation: through its impact on exit decisions or through its separate impacts on the growth of market shares and productivity. We define the growth rate of market shares using the Davis et al. [1996] definitions as:

$$\frac{s_{it,j} - s_{it-1,j}}{0.5 * (s_{it,j} + s_{it-1,j})}.$$

This growth rate measure ranges from -2 to +2 and treats expansion and contraction symmetrically, unlike the conventional growth rate measure. The growth rate of productivity is defined as the difference in log productivity, $p_{it} - p_{it-1}$.

We consider all of the above mentioned indicators in this chapter. Before proceeding to the estimation methodology and results, we describe the data and the relevant background information, that forms the basis of our estimation strategy.

2.3 Data Sources and Institutional Background

2.3.1 Data Sources

The data for our research come from six major sources: PASEF - The Data Analysis Service of the Faculty of Economics at the University of Ljubljana; CSAC - Commission for State Aid Control at the Ministry of Finance of the Republic of Slovenia; the Statistical Office of the Republic of Slovenia (henceforth SORS); the Business Register of Slovenia (henceforth BRS); the Ministry of the Economy, and the Official Gazette of the Republic of Slovenia On-line. We describe each of these data sources below, including the variables and/or information they contain and problems, if applicable.

The PASEF database contains balance sheets and income statements for all businesses (excluding sole proprietors and the banking industry), in Slovenia from 1995 to 2003. For reasons mentioned in the introduction (most of the non-agricultural state aid has been oriented towards manufacturing, special attention paid to manufacturing), we will only use data for manufacturing firms.[10] In addition to accounting data, the database contains a unique 7-digit firm ID number,[11] a 5-digit industry code,[12] information on ownership (co-operative, private, social, state, and mixed) and source of capital (domestic, foreign, or mixed) prior to 2001, the municipality and region in which the firm is located, and the average number of persons in paid employment based on hours worked. According to the *Law on Enterprises*, all firms that are registered in any given year are supposed to provide balance sheets and income statements to the relevant government agency, regardless of whether they were in business the entire year.[13]

Our data on state aid for the rescue and restructuring of firms in difficulty were prepared by CSAC, and cover the period from 1998 to 2003.[14] The data in-

[10]PASEF obtained the data from SORS, which collected these reports until July 2002, and from AJPES (Agency of the Republic of Slovenia for Public Legal Records and Related Services), which collected the data from July 2002 onwards.

[11]This 7-digit number identifies the firm uniquely. It is given to the firm at the time of its registration for the first time and may not be used for a different/new firm once the original firm ceases to exist. However, there are two exceptions: i) if firm A merges with firm B, the combined firm gets a new 7-digit ID, ii) if firm A acquires firm B, the ID of firm B ceases to exist and the new firm has the ID of firm A. Hence, we cannot keep track of mergers and acquisitions from this database.

[12]Refer to Appendix E for the names of the industry codes. The classification used is NACE Rev. 3.

[13]Firms that go into bankruptcy are required to provide this information at most 2 months after their bankruptcy. However, there are on average 5 percent of all firms every year that fail to provide their accounting information and are required to pay a fee according to the *Law on Enterprises*.

[14]Until 2003, firms in difficulty were able to obtain state aid for rescue and restructuring from a

clude registration numbers of the providing agency and aid recipient,[15] date of aid approval, date of aid provision, legal foundation, documentation, legal act, instrument and purpose of aid, and the amount of state aid. The dataset has its flaws and most likely underestimates state aid to firms in difficulty,[16] but according to the CSAC staff, its coverage and quality is getting better every year, and it is the best source available.

We obtain a list of firms considered to be labor intensive in 1996 from the Ministry of the Economy. Labor intensive firms had priority in the aid allocation process, so we use this list to define an indicator for these firms.

From SORS we obtain price indices and data on the registered unemployment rate by region and municipality. The producer price index (PPI) is publicly available for our sample period only at the 2-digit industry level; since 2001 SORS has also published the PPI at the 4-digit industry level, although the data are confidential in some industries due to the small number of firms. The intermediate goods price index (IGPI) and the capital goods price index (CGPI) are available only for the total economy and for Mining and Quarrying (10-14), Manufacturing (15-37), and Electricity, Gas, and Water Supply (40-41).

From the Business Register of Slovenia (BRS) we obtain data on the year in which each business was first registered (i.e., the birth of the firm). For some firms

number of different agencies, depending on their circumstances. Since 2003, ME has been the only agency that can give aid to firms in difficulty (in cooperation with other agencies), which makes the state aid data more transparent.

[15]The latter equals the unique 7-digit firm ID number from the PASEF data.

[16]In earlier years when work on the database started, some aid donors kept poor or no records of the aid they gave to firms. In manufacturing, the coverage is likely to be better than in agriculture, because the ME kept quite good records from 1995 onwards and was one of the two major donors.

the reported birth year varies from year to year; our data assumes that the earliest reported birth year is valid. Large state firms that were broken up into smaller units also create problems; for example, a number of firms that were supposedly registered for the first time in 1999 were parts of larger firms prior to 1999, and hence are not truly entering firms. Similarly, some firms appear to exit in 1998, but were actually merged with their parent company. The detection of such firms is tedious, requires knowledge of events in Slovenia and has to be done on a case-by-case basis. Under these circumstances, we have decided against using the actual age of firms as one of our control variables; instead, we use a dummy variable equal to 1 if a firm is at least 10 years old and 0 otherwise.

The Official Gazette of the Republic of Slovenia On-line was our resource for legislation, regulations, public tenders and other measures pertinent to state aid for the rescue and restructuring of firms in difficulty, as well as information that helped us determine whether an exiting firm actually exited or only initiated bankruptcy procedures.

2.3.2 Institutional Background

In Slovenia, state aid first became officially available after the fall of the Berlin Wall in 1989, when the country took its first steps on the path towards a market economy. Prior to 1989, firms could count on the government to save them if they got into financial trouble, though such aid was not officially called state aid.

In this chapter, our focus is on the rescue and restructuring aid in manufacturing. Such aid can only be given to firms in difficulty and has officially been available since 1995.[17] Until 2003, firms in difficulty were eligible to get state aid from various agencies under certain conditions, defined either in legislation or public tenders,[18] and using a number of different instruments.[19],[20]

Conditions under which rescue and restructuring aid could be granted differed somewhat among different aid agencies. However, there were also a number of common criteria. Only medium and large firms were eligible for such aid, and these were defined using number of workers, revenue and assets (see Appendix D for details). Priority was given to labor intensive firms, where labor intensity was defined as "employing a lot of workers who earn low wages" (according to a personal conversation with Igor Naglič from the Ministry of the Economy), rather than having a high ratio of labor to capital.

Priority was also given to firms with losses in at least two out of three consecutive years and firms with a higher share of debt financing than the industry average.[21] Given that firms in difficulty required a lot of labor restructuring, pri-

[17]A firm is defined to be in difficulty when it is not able, with its own resources or resources it is able to obtain from its owners/shareholders or creditors, to halt negative business trends that without state intervention would threaten the survival of the firm. Newly founded companies, companies formed through liquidation of a previous company and related companies are not entitled to rescue and restructuring aid, unless it is possible to demonstrate that the causes of the difficulties are in the enterprise itself and not the result of arbitrary reallocation of costs between them.

[18]We present an abbreviated version of these conditions here; more detail is available upon request.

[19]These instruments were: a) grants, direct interest subsidies, loan remission, b) tax deferrals, tax exemptions and relief, exemptions and relief on the payment of social security contributions, c) equity investments, conversion of debt into equity participation, d) soft loans from public and private sources, loans to companies in difficulty, e) guarantees for non-commercial and/or commercial risks, payment of guaranteed obligations, and f) other sources of aid.

[20]Most of the aid was given by the Ministry of the Economy (ME) and the Development Corporation of Slovenia (DCS); together, these two agencies accounted for more than 80 percent of all such aid. Other donors were the Ministry for Labor, Family and Social Affairs (MLFSA), the Employment Services of Slovenia (ESS) and the Ministry of Finance (MF).

[21]Depending on the aid program, industry was sometimes defined at the 4-digit level, and

ority was given to firms in regions with an above average unemployment rates or in lower development. Finally, firms with survival and growth potential were given priority, though the legislation and/or public tenders did not specify the criteria according to which potential would be judged. The selection criteria were used primarily to exclude firms from obtaining state aid, and were sometimes applied selectively based on the political preferences of the people involved in the process. However, we will establish below that the official criteria were still relevant.

As a share of GDP, aid allocated was relatively negligible, and this share was generally decreasing over time, but the percentage of firms that received aid was far from negligible in certain sectors, as Figure 2.1 illustrates. In addition, these firms employed about one sixth of workers in manufacturing and also accounted for one tenth of total manufacturing output prior to 2002, as Table 2.1 shows. Within some 2-digit industries, more than 40 percent of workers were employed in aid-receiving firms.[22]

There was a sharp decrease in both the aid allocated and the number of recipients in 2002. New restrictions on state aid for the rescue and restructuring of firms in difficulty were officially adopted in July 2000.[23] These restrictions were adopted to harmonize Slovenian legislation with the EU restrictions, and were part of the process by which Slovenia prepared for membership in the EU (Slovenia joined the

sometimes at the 2-digit level.

[22]The presence of aid thus also likely had an impact on the timing and magnitude of job flows, the analysis of which is not part of this chapter, but will be part of future analysis.

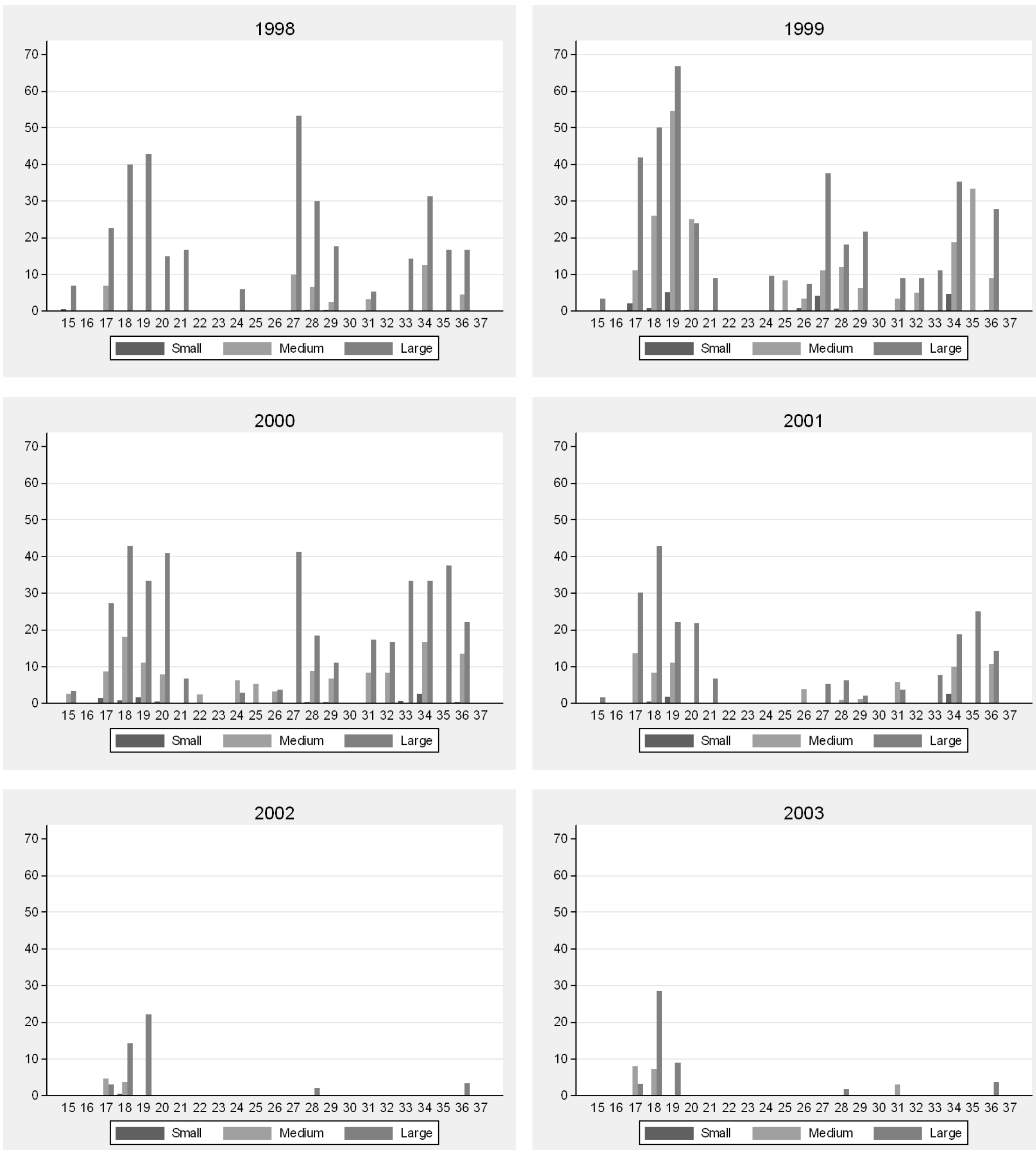
[23]*Decree on the purposes and conditions for the granting of state aid and on the appointment of the ministries responsible for managing individual aid schemes*, The Official Gazette of the Republic of Slovenia, No. 59, 2000, June 30, 2000.

EU on May 1, 2004).[24] Under these new restrictions, aid for resolving financial problems could be granted only once, and aid for restructuring could be repeated only after ten years absent exceptional and unforeseeable circumstances over which the firm has no control. Prior to 2000, several firms in our data received aid multiple times.

Other major changes occured earlier. The process of trade liberalization had begun in the Socialist Federal Republic of Yugoslavia prior to 1991, and Slovenia continued the liberalization of import regimes and the removal of import charges and numerous tariff exemptions when it became independent. The previous protection policy was nontransparent, which led to a number of undesireable outcomes, such as inefficient use of factors of production and inappropriate development of the structure of the economy. Prior to independence, Slovenian producers enjoyed high levels of protection, and nonprice measures (quotas, licenses, etc.) were extremely important. In 1986, free imports represented only 3 percent of import value; this increased to 78 percent by 1990, to 97 percent in 1993 and 98 percent in 1996 (see Majcen and Kaminski [2004], p. 137). The only exceptions in the trade liberalization process were the agriculture, food processing and textile industries. Even prior to 1991, Slovenia's major trading partners were in the EU (Germany and Italy) and a trade agreement between the EU and Slovenia was in effect by January 1, 1997; exports had never been strictly directed towards the Eastern bloc.

[24] Slovenia declared its goal of joining the EU upon its declaration of independence in 1991, it signed the European Agreement in 1996 and negotiations to join the EU officially began in 1998. Hence, changes in legislation were to be expected, and there was presumably no jump in firms' perceptions about the likelihood of Slovenia joining the EU when the actual changes in legislation on state aid occured.

Figure 2.1: Percentage of Firms that Received State Aid for the Rescue and Restructuring to Firms in Difficulty by Firm Size and 2-digit Industry, 1998-2003



Note: Refer to Appendix E for the names of 2-digit industries.
Source: Own calculations based on CSAC and PASEF databases.

Table 2.1: Percentage of Firms that Received Aid - Manufacturing, 1998-2003

Year	Received Aid			Labor Intensive & Received Aid			Medium or Large & Received Aid		
	Firms	Labor	Output	Firms	Labor	Output	Firms	Labor	Output
1998	1.586	15.204	11.251	1.315	14.352	10.689	1.503	15.165	11.218
1999	2.867	16.422	10.395	2.197	14.128	8.603	2.469	16.137	10.313
2000	2.605	18.263	11.344	1.918	15.794	9.372	2.376	18.013	11.284
2001	1.091	9.910	4.605	0.823	9.223	4.170	1.029	9.875	4.598
2002	0.140	2.959	0.748	0.120	2.898	0.743	0.120	2.898	0.743
2003	0.196	3.332	0.783	0.196	3.332	0.783	0.157	3.225	0.759

Source: Own calculations based on CSAC and PASEF data.

Table 2.2: Number of Firms that Received Aid Pre- and Post-2002, Manufacturing

	Size		Labor Intensity	
	Medium or Large	Small	Labor Intensive	Non-Labor Intensive
Pre-2002	354	37	300	91
2002-2003	16	1	16	1

Source: Own calculations based on CSAC and PASEF data.

Slovenia's labor market reform was slow and cautious. The government retained strict employment protection legislation, maintained a costly unemployment benefits system, imposed a heavy tax burden on labor (tax wedge of 48 percent), and kept minimum wages relatively high (40 percent of average wages). The 1991 *Labor Code* remained in power until 2003, with minor modifications, and the 2003 *Labor Code* still makes it hard for employers to lay off workers for "economic reasons" (see Vodopivec [2004] for more details).

The allocation of aid also differed by region,[25] as Table 2.3 illustrates.[26] Regions that received disproportionate amounts of state aid in general had high unemployment rates (Podravska, Pomurska, Zasavska, Spodnjeposavska) and/or were underdeveloped as measured by gross value added per capita (Podravska, Zasavska, Pomurska).[27,28] Regional differences in aid receipt are partly due to differences in the distribution of industry across regions. In addition, some regions were hit harder than others by the disintegration of the Socialist Republic of Yugoslavia and other political changes at the beginning of the 1990s. Hard-hit regions such as Gorenjska, Savinjska, Podravska, Koroska and Zasavska were characterized by large manufacturing firms that had problems adjusting to the market economy and finding new markets. The Pomurska region, meanwhile, has always been the least developed

[25]The capital, Ljubljana, is located in the Osrednjeslovenska region.

[26]Regional classification changed in 2000. Southern parts of the Osrednjeslovenska region were classified together with the Dolenjska region as the Jugovzhodna region. Other regions remained the same. In the chapter, we use the pre-2000 classification.

[27]Gross domestic output per capita by region would be a better measure of development, but SORS has only calculated GDP by region for 1999 and 2001, and their methodology changed in between. We have calculated gross value added per region using the entire PASEF database on all firms in Slovenia, and used the data on population by region from SORS to calculate gross value added per capita.

[28]Gorenjska did not have a higher than average registered uneployment rate and/or a lower than average gross value added per capita; however, the steel industry was concentrated there.

Table 2.3: Ratio of Share of State Aid Received vs. Share of Output Produced by Region, 1998-2003

	Year					
Region	1998	1999	2000	2001	2002	2003
Dolenjska	0.038	0.207				
Gorenjska	0.595	1.513	0.736	0.870	7.830	1.488
Goriska	0.155	0.313	0.648	0.142	0	1.490
Jugovzhodna			0.722	0.078	0	0.068
Koroska	0.474	2.179	1.208	0.312	4.931	0
Notranjsko-kraska	0.086	0.280	0.699	0	0	0
Obalno-kraska	2.180	0.559	0.173	0.805	0	7.662
Osrednjeslovenska	1.548	1.388	0.884	1.733	0	0.201
Podravska	1.239	0.804	2.928	1.154	1.027	0.121
Pomurska	0.141	0.105	0.274	0.017	0.401	9.763
Savinjska	0.133	0.378	0.599	0.051	0.010	0.076
Spodnjeposavska	0.934	0.676	1.235	0.754	0.058	1.483
Zasavska	0.180	1.681	1.409	0.012	0	0

Source: Own calculations based on CSAC and PASEF data.

region in Slovenia, with mostly agricultural activity and limited opportunities even prior to the 1990s.

2.4 Estimation Methods

Evaluating the impact of state aid on static and dynamic allocative efficiency is difficult due to the lack of a counterfactual and because of selection bias. The former asks what would have happened if the firm had been subject to an alternative policy (for example, if a firm received aid, what would have happened to its behavior if it had not received aid, and if the firm did not receive aid, what would have happened to its behavior had it received aid). The latter arises from the fact that firms that receive aid usually differ from firms that do not receive aid along other dimensions. The average treatment effect (ATE) is defined as the difference of the expected outcomes with aid and without aid:

$$ATE = E\left(Y^1 \left| D = 1\right.\right) - E\left(Y^0 \left| D = 0\right.\right), \tag{2.5}$$

where Y^1 indicates outcome with aid, Y^0 indicates outcome without aid and D is a binary indicator equal to 1 if a firm receives aid. This estimate includes the effect on firms for which aid was never intended. The average treatment effect on the treated (ATT) focuses explicitly on the firms that received aid and is defined as:

$$ATT = E\left(Y^1 \left| D = 1\right.\right) - E\left(Y^0 \left| D = 1\right.\right). \tag{2.6}$$

Under the assumption of homogeneous treatment effects, ATT is identical to ATE, but this is not the case under the assumption of heterogeneous treatment effects (Blundell and Costa Dias [2000]).

In general, experimental evaluation is the preferred method to estimate treatment effects; however, given that firms are not assigned randomly to a group that receives aid and a control group that does not receive aid, experimental data are not available. We have to use non-experimental evaluation estimators and rely on the information on how firms actually performed after some of them received aid and others did not. Non-experimental treatment effects estimators can be grouped under two categories based on how they handle selection bias. The first category contains estimators that rely on selection on observables and the second category contains estimators explicitly allowing selection on unobservables (Caliendo and Hujer [2005]).

2.4.1 Selection on Observables

OLS regression implicitly relies on the assumption of selection on observables.[29] The equation of interest can be written as:

$$y_{it} = \beta_0 + \beta_1 AID_{it-1} + \mathbf{X_{it-1}}\boldsymbol{\gamma} + u_{it}, \tag{2.7}$$

where i denotes firm, t denotes time, y is the outcome variable of interest (micro covariance, growth of market share or growth of productivity), AID is a dummy variable defined according to:

$$AID_{it}{}^{30} = \begin{cases} 1 & \text{if firm receives aid in } t; \\ 0 & \text{otherwise,} \end{cases} \tag{2.8}$$

and $\mathbf{X}$ is a vector of controls.

The assumption needed to identify the average treatment effect of aid is that conditioning linearly on $\mathbf{X}$ suffices to eliminate selection bias. This assumption might fail and lead to biased estimates. In our case, the direction of bias is not clear, since different scenarios are possible:

1. Self-selection among firms: applying for state aid is a time- and money-

[29] Another estimator assuming selection on observables is the matching estimator. It is based on an identifying assumption that conditional on $\mathbf{X}$, the outcome is independent of AID, but it does not assume any particular functional forms as the linear regression model does.

[30] We decided to use an indicator for aid instead of amounts of aid because there are a number of cases in which aid was given to a holding company, and there is no further information available on which firms within a holding company received aid.

consuming process, since by law applying firms need to prepare a restructuring plan. If only inherently good firms apply, then the OLS coefficient will be biased upwards.

2. If firms that are inherently bad are more likely to seek out political connections and if connections increase the likelihood of receiving aid conditional on applying, and if firms are more likely to apply when the likelihood of success is high, the OLS coefficient will be biased downwards.

3. If the government prefers to give aid to firms that it suspects to be inherently bad, but that are located in regions with high unemployment rates and thus potential social problems, the OLS coefficient will be biased downwards.

Whether firms are inherently "good" or inherently "bad" is not observable, so estimators that account for selection on unobservables might be more appropriate.

2.4.2 Selection on Unobservables

The underlying instrumental variables identification strategy is to find a variable that determines treatment participation but does not influence the outcome equation. The model can be written as:

$$\begin{aligned} y_{it} &= \beta_0 + \beta_1 AID_{it-1} + \mathbf{X_{it-1}}\boldsymbol{\gamma} + u_{it} \\ AID_{it-1} &= \phi_0 + \mathbf{Z_{it-1}}\boldsymbol{\eta} + \mathbf{X_{it-1}}\boldsymbol{\varphi} + \epsilon_{it-1} \end{aligned} \tag{2.9}$$

where $\mathbf{Z}$ is a vector of instrumental variables and the rest is the same as in equation (2.7).

IV requires the existence of at least one independent variable Z affecting AID, but not directly affecting the outcome. The vector Z thus has to have a non-zero coefficient in the decision rule in equation (2.9), and it must be uncorrelated with the error terms u and ϵ given $\mathbf{X}$. A good instrument is hard to come by, since it has to predict participation, but not otherwise influence the outcome equation. If the instrument is correlated with ϵ, the IV estimate will be biased.[31]

Our identification strategy involves using variables that affect the likelihood of getting aid prior to 2002, but not afterwards when aid became less available. These variables may be directly correlated with outcomes, but β_1 can still be identified as long as the direct impact of these variables on outcomes did not change in 2002. We exploit the variation in aid receipt driven by three factors: (1) differential likelihood of getting aid pre- and post-2002, (2) differential likelihood of getting aid for labor intensive firms versus non-labor intensive firms, and (3) differential likelihood of getting aid for medium and large firms versus small firms. We discuss each of these factors in turn.

First, firms were more likely to get aid prior to 2002. As described in section 2.2.3, new restrictions on state aid were officially adopted in 2000, but they actually had a major effect on aid availability only when the largest donor, the

[31]Under the constant treatment effects assumption (i.e., the impact of treatment is the same for every unit in the population) the IV estimate represents a population average. In a heterogeneous treatment effects world, where the instrument is correlated with the impact and monotonicity holds, the IV estimator gives the Local Average Treatment Effect (LATE), defined as a mean impact of treatment on compliers (those who respond to the instrument by taking the treatment when they otherwise would not) (Angrist et al. [1996]).

Development Corporation of Slovenia (DCS), ceased to exist in 2002. These restrictions, as well as the closure of DCS, were "dictated" by the process of joining the EU and are hence a source of exogenous variation. However, other things changed over this period that could lead to differences in the outcomes of interest. For example, substantial changes in the *Bankruptcy Code* were implemented in July 1999, and although that had an immediate impact on the number of bankruptcies, the response was again delayed, because the courts in Slovenia are relatively slow and therefore bankruptcy takes time. Using pre-post 2002 variation thus may not satisfy the exclusion restriction by itself.

Second, labor intensive firms were given priority in the aid allocation process, and as Table 2.2 shows, they were over 3 times more likely to get aid than non-labor intensive firms. Labor intensive firms were defined by the Ministry of the Economy (ME) as firms employing a lot of workers who earn low wages. We use a list of labor intensive firms dated in 1996 obtained from the ME to identify such firms in our sample. While some firms that potentially fit this criterion after 1996 are not defined as labor intensive, our definition has the advantage of keeping the measure of labor intensity free from possible whims of politicians in later years and thus free from additional selection bias. An indicator variable for labor intensive firms might be a potential instrument. However, it fails the exclusion restriction, since transition and/or competition may have induced more downsizing of labor intensive firms and thus may have had an impact on outcomes directly.

Third, medium and large firms were almost 10 times more likely to get aid than small firms. The eligibility criteria stated that only medium and large firms could receive aid, but in reality, some small firms also received it (either directly or through the holding companies to which they were related).[32] An indicator variable for medium and large firms might be a potential instrument, but it fails the exclusion restriction. Existing studies show that size of firms has an impact on the likelihood of exit (for example, Baily et al. [1992] and Disney et al. [2003a]) and also indicate that size matters for allocative efficiency.[33].

The interaction of a pre-2002 dummy variable and an indicator for labor intensive firms, controlling for baseline differences, is a valid instrument if it has no direct effect on the outcomes of interest, which is plausible, since there are no indications that the direct effect of labor intensity should have changed pre/post 2002. Similarly, the interaction of a pre-2002 dummy variable and an indicator for medium and large firms, controlling for baseline differences, is a valid instrument, if the effect of size of firms on the outcomes of interest did not change pre/post 2002.

To summarize before proceeding to the estimation results, we define our two instrumental variables, Z_1 and Z_2, in the following way:

$$Z_{1,it} = \begin{cases} 1 & \text{if a labor intensive firm \& year prior to 2002;} \\ 0 & \text{otherwise,} \end{cases} \tag{2.10}$$

[32]In Slovenia, the firm size is defined according to the *Law on Enterprises* (see Appendix D for details). Since the official firm size definition changed significantly in 2002, we measure size according to the 1997 criteria to avoid blurring the impact of aid on outcomes.

[33]Haltiwanger and Schweiger [2005] find that in some countries, small firms have higher static allocative efficiency while in other countries, larger firms have higher static allocative efficiency

and

$$Z_{2,it} = \begin{cases} 1 & \text{if a medium or large firm \& year prior to 2002;} \\ 0 & \text{otherwise.} \end{cases} \tag{2.11}$$

Before proceeding to estimation results, we need to mention that although AID is a binary variable, we use a linear probability model in the first stage and not a probit or logit model. The reason for this lies in the low incidence of aid, especially after 2002, as illustrated in Tables 2.1 and 2.2. Since the probability of receiving aid drops close to zero after 2001, maximum likelihood has a hard time identifying the coefficients on the instruments and it wants to push them towards either plus or minus infinity. Angrist [2001] argues that for binary endogenous regressors, "if the covariates are sparse and discrete, linear models and associated estimation techniques like 2SLS are no less appropriate for LDV's than for other kinds of dependent variables" (p. 3).

2.5 Estimation Results

2.5.1 Aggregate Properties and Summary Statistics

In aggregate terms, the static allocative efficiency of Slovenian manufacturing, calculated as a weighted average of 2-digit industry aggregate covariances terms using TFP and time-invariant value-added weights, was positive, as Table 2.4 illustrates. The covariance measure remained roughly constant until 2001, when it declined by 6.8 percent, then started to improve.[34]

[34] Labor productivity based covariance (not reported here, but available upon request) was positive and increasing over the period.

Our estimates are roughly comparable to other available estimates for Slovenia. Bartelsman et al. [2004b] find that the labor-productivity-based Olley and Pakes [1996] covariance in Slovenian manufacturing was negative from 1992-1997 and positive from 1998-2001, and that it was increasing throughout the period. Results are similar for TFP. The magnitudes differ, because their sample is only a subsample of ours, since they were interested in three- and five-year changes in productivity growth, whereas we are looking at one-year changes. In addition, we use services as one of the factors of production, whereas they only use labor, capital and materials.

Growth of TFP was positive from 1999 onwards (see column (4) of Table 2.4). De Loecker and Konings [2003] estimate the growth rate of TFP to be 0.2 percent on average in the period 1998-2001. However, they use "employment market shares" rather than "sales market shares" (p. 20) in addition to using a production function with only labor and capital as inputs, which makes it harder to compare the results. Using employment shares instead of sales shares as weights in Slovenia is bound to produce different results, since there were significant differences between the two in a number of 2-digit industries.

Column (5) of Table 2.4 shows the unweighted exit rates of firms in manufacturing from 1998 to 2003. The exit rate is defined as follows:[35]

$$\text{exit rate}_{uw,t} = \frac{\text{No. of exiting firms between } t \text{ and } t+1}{1/2\,(\text{Total no. of firms in } t-1 + \text{Total no. of firms in } t)} \tag{2.12}$$

[35] Since an exiting firm in t exists for the last time in t, exiting firms exit at some time between t and $(t+1)$.

The exit rate increased to about 2.9 percent in 1999, dropped to about 2.0 percent in 2001, and then increased again. These rates are relatively low compared to exit rates in manufacturing in most OECD and transition countries, as reported by Bartelsman et al. [2004b]. Bartelsman et al. [2004b] report the exit rate for Slovenia to be around 4 percent in manufacturing firms with at least 1 employee in the period 1992-2000. De Loecker and Konings [2003] find exit rates in the period 1998-2000 to be about 1 percentage point higher than what we report here. We trust our results more, since we were very careful in measureing true exit and did not rely solely on the availability of the accounting data, which has missing firm-year observations.

Table 2.5 shows summary statistics of the variables used in the estimation for the overall sample, by aid receipt and by firm status. We define a dummy variable equal to 1 if the firm-level share of debt financing (the ratio of the sum of long-term provisions, long-term financial liabilities, current liabilities and accrued costs, expenses and deferred revenues to the sum of total liabilities and equity) is higher than the firm's 4-digit industry average and equal to 0 otherwise. We measure the importance of firm in its region as the firm's percentage of total regional employment.

The standard deviation of the micro covariance is very high relative to its mean (the coefficient of variation is 8.38 overall, 8.28 in firm-years with aid and 14.42 in firm-years without aid). The growth rate of market share is on average negative, but more so for firm-years with aid. TFP growth, on the other hand, is positive for firm-years without aid and negative for firm-years with aid. The standard deviations of these variables indicate that there is a lot of heterogeneity among firms.

Table 2.4: Olley-Pakes Covariance and Exit Rates in Manufacturing, 1998-2003

Year	Weighted TFP (1)	Unweighted TFP (2)	Covariance (3)	TFP Growth (4)	Exit Rate (5)
1998	2.819	2.702	0.117	-0.039	0.024
1999	2.825	2.710	0.115	0.022	0.029
2000	2.875	2.758	0.117	0.061	0.028
2001	2.894	2.785	0.109	0.041	0.020
2002	2.907	2.789	0.118	0.024	0.022
2003	2.935	2.781	0.155	0.050	0.029

Source: Own calculations based on PASEF data.

Table 2.5: Summary Statistics - Overall, by Aid Receipt and by Firm Status

Firm-Years	Overall		Without Aid		With Aid		Continuers		Entrants		Exiters	
Mean [Std. Dev.]	Obs	(1)	Obs	(2)	Obs	(3)	Obs	(4)	Obs	(5)	Obs	(6)
Dependent Variables (Outcomes)												
Micro Covariance (TFP)	29335	0.144	28927	0.145	408	0.105	27807	0.134	926	0.302	602	0.374
		[1.207]		[1.202]		[1.514]		[1.219]		[0.991]		[0.891]
Growth of Market Share	27704	-0.011	27305	-0.010	399	-0.075	27125	-0.003			579	-0.392
		[0.450]		[0.451]		[0.384]		[0.439]				[0.737]
Growth of TFP	27704	0.013	27305	0.013	399	-0.003	27125	0.016			579	-0.158
		[0.434]		[0.436]		[0.255]		[0.424]				[0.762]
Explanatory Variables												
Aid	29335	0.014	28927	0	408	1	27807	0.015	926	0.002	602	0
		[0.117]		[0]		[0]		[0.120]		[0.046]		[0]
TFP	29335	2.920	28927	2.921	408	2.832	27807	2.934	926	2.662	602	2.606
		[0.840]		[0.841]		[0.776]		[0.824]		[1.092]		[1.038]

Continued on next page.

Table 2.5: Summary Statistics - Overall, by Aid Receipt and by Firm Status (continued)

Firm-Years	Overall		Without Aid		With Aid		Continuers		Entrants		Exiters	
Mean [Std. Dev.]	Obs	(1)	Obs	(2)	Obs	(3)	Obs	(4)	Obs	(5)	Obs	(6)
					Dependent Variables (Outcomes)							
Labor Intensive Firms	29335	0.149	28927	0.140	408	0.775	27807	0.153	926	0	602	0.181
		[0.356]		[0.347]		[0.418]		[0.360]		[0]		[0.385]
Labor Intensive Firms	29335	0.104	28927	0.095	408	0.735	27807	0.106	926	0	602	0.150
x Pre-2002		[0.305]		[0.293]		[0.442]		[0.308]		[0]		[0.357]
Medium and Large Firms	29335	0.220	28927	0.210	408	0.907	27807	0.226	926	0.064	602	0.174
		[0.414]		[0.408]		[0.291]		[0.418]		[0.244]		[0.380]
Medium and Large Firms	29335	0.139	28927	0.129	408	0.868	27807	0.143	926	0.045	602	0.130
x Pre-2002		[0.346]		[0.335]		[0.339]		[0.350]		[0.208]		[0.336]
Old Firms (10+ Years)	29335	0.384	28927	0.382	408	0.542	27807	0.398	926	0	602	0.302
		[0.486]		[0.486]		[0.499]		[0.490]		[0]		[0.460]
Loss in at least 2 out	29335	0.146	28927	0.142	408	0.414	27807	0.147	926	0	602	0.341
of 3 Consecutive Years		[0.353]		[0.349]		[0.493]		[0.354]		[0]		[0.474]
Share of Debt Financing higher	29335	0.576	28927	0.575	408	0.625	27807	0.569	926	0.692	602	0.728
than 4-Digit Industry Average		[0.494]		[0.494]		[0.485]		[0.495]		[0.462]		[0.446]
Regional Unemployment	29335	0.301	28927	0.299	408	0.434	27807	0.299	926	0.308	602	0.377
Rate higher than Average		[0.459]		[0.458]		[0.496]		[0.458]		[0.462]		[0.458]
Percentage of Regional Labor	29335	0.122	28927	0.110	408	0.985	27807	0.125	926	0.049	602	0.106
Force Employed by the Firm		[0.657]		[0.579]		[2.560]		[0.670]		[0.028]		[0.364]

Source: Own calculations based on CSAC and PASEF data.

TFP is on average higher for firm-years without aid. 77.5 percent of firm-years with aid are labor intensive, compared to 14 percent of firm-years without aid. 90.7 percent of firm-years with aid occur in medium or large firms, compared to 21 percent of firm-years without aid. Percentages of old firms, firms with losses in at least 2 out of 3 consecutive years and firms with a higher share of debt financing than the 4-digit industry average are higher for firm-years with aid. 43.4 percent of firm-years with aid are located in regions with a higher than average unemployment rate, and these firms also employ a higher percentage of their region's labor force.

Summary statistics for continuing, entering and exiting firms are shown in columns (4)-(6) of Table 2.5. It is interesting to note that both entering and exiting firms have a higher micro covariance than continuing firms, and there is also less heterogeneity among them in that respect (the coefficient of variation is 3.28 for entering firms and 2.38 for exiting firms, compared to 9.09 for continuing firms). Growth of market share is close to zero for continuing firms; it is significantly lower for exiting firms. Exiting firms also have lower TFP growth and lower TFP on average, though there is a lot of heterogeneity among them. The share of labor intensive firms is a bit higher among exiting firms than among continuing firms, but the share of medium and large as well as old firms is smaller. 34.1 percent of exiting firms have losses in at least 2 out of 3 consecutive years, and they have more debt than continuing and entering firms. Exiting firms are more likely to be located in regions with a higher than average uneployment rate. Overall, Table 2.5 shows that there is substantial heterogeneity among firms.

As explained in Section 2.4, we estimate the impact of state aid on allocative efficiency, controlling for two digit industry, region, year, and the growth rates of 2-digit industry output and regional gross value added in all estimations. Coefficients on industry, region, calendar year and growth rates are not reported to save space, but are available upon request. In all estimations, standard errors are clustered at the firm identification number level to take into account the panel nature of the data. Explanatory variables are lagged one period in Tables 2.6, 2.7, 2.8 and 2.9.

2.5.2 Impact of State Aid on Exit Decisions

As shown in column (5) of Table 2.4, exit rates were relatively low in Slovenia. The exit margin was thus not the one where the bulk of the reallocation happened. Due to the fact that none of the aid-receiving firms exited during 1998-2003, we cannot estimate the impact of state aid on exit decisions directly. However, we can estimate the probability of exit using a probit model of exit on a sample of firm-years without aid and then use the estimated coefficients to calculate the predicted probability of exit for firm-years with aid based on their fundamentals. The gap between this implied probability and the actual (zero) incidence of exit among firms receiving aid will reveal something about the impact of aid on exit. Average marginal effects from estimating a probit model are shown in Table 2.6.

The estimated coefficients are in general in line with the predictions from the theories of market selection and other empirical studies: more productive firms and medium and large firms are less likely to exit, whereas firms with consecutive losses are more likely to exit. Economic fundamentals thus did matter in the firm

Table 2.6: Exit Estimation, Average Marginal Effects

	Exit
TFP	-0.012***
	[0.001]
Labor Intensive Firms	0.026***
	[0.008]
Medium and Large Firms	-0.014***
	[0.004]
Old Firms (10+ Years)	-0.003
	[0.002]
Loss in at least 2 out of	0.030***
3 Consecutive Years	[0.004]
Share of Debt Financing higher	0.013***
than 4-Digit Industry Average	[0.002]
Percentage of Regional Labor	-0.001
Force Employed by the Firm	[0.001]
Regional Unemployment Rate	-0.002
Higher than Average	[0.010]
Observations	23836
Firms	5812
Clustered standard errors in brackets. Control variables: 2-digit industry and growth of its output, region and growth of regional GVA, year. *significant at 10%, **significant at 5%, ***significant at 1%.	
Predicted probability of exit for firm-years without aid	0.025
Predicted probability of exit for firm-years with aid	0.036

Source: Own calculations based on CSAC and PASEF data.

decision to exit. However, the predicted probability of exit is 2.5 percent for firm-years without aid and 3.6 percent for firm-years with aid. This result indicates that according to their economic fundamentals these firms were more likely candidates for exit, but they did not exit, suggesting that aid delayed exit and kept inefficient firms alive.

2.5.3 Impact of State Aid on Static Allocative Efficiency

Regression (1) in Table 2.7 is based on OLS estimation of the following version of equation (2.7),

$$\frac{\Delta s_{it,j}}{\bar{s}_{t,j}}\Delta p_{it,j} = \beta_0 + \beta_1 AID_{it-1} + \mathbf{X_{it-1}}\boldsymbol{\gamma} + u_{it},$$

where i denotes firm, t denotes time, $\frac{\Delta s_{it,j}}{\bar{s}_{t,j}}\Delta p_{it,j}$ is a measure of static allocative efficiency, AID is a dummy variable equal to 1 if firm receives aid and 0 otherwise, and $\mathbf{X}$ is a vector of controls. These estimates assume selection on observables.

Aid has a negative and significant impact on static allocative efficiency: receipt of aid is associated with a 0.29 percent drop in micro covariance.[36] This is consistent with the notion that state aid hinders the efficient allocation of resources to more productive businesses.

IV estimates in column (2) of Table 2.7 allows for selection on unobservables and use the interactions between a pre-2002 dummy variable and dummy variables for labor intensive firms and for medium and large firms as instrumental variables for AID. They are based on:

$$\frac{\Delta s_{it,j}}{\bar{s}_{t,j}}\Delta p_{it,j} = \beta_0 + \beta_1 AID_{it-1} + \mathbf{X_{it-1}}\boldsymbol{\gamma} + u_{it},$$

$$AID_{it-1} = \phi_0 + \mathbf{Z_{it-1}}\boldsymbol{\eta} + \mathbf{X_{it-1}}\boldsymbol{\varphi} + \epsilon_{it}.$$

The IV estimate of the coefficient on aid in column (2) of Table 2.7 is negative

[36]We obtain this magnitude by dividing the estimated coefficient by the standard deviation of micro covariance from Table 2.5, and multiplying the result by 0.01.

and statistically significant, and is almost 4 times higher than the OLS estimate: receipt of aid is associated with a 1.09 percent drop in micro covariance. This result indicates that the OLS estimate of the impact of aid (column (1)) is biased towards zero.

The first stage estimates, reported in column (2a) of Table 2.7, indicate that firms with losses in at least two out of three consecutive years, firms whose share of debt financing is higher than their 4-digit industry average and firms employing a higher percentage of their region's labor force were more likely to get aid, as were labor intensive and medium and large firms prior to 2002. This is an important result in itself, because it indicates that the allocation of aid was not "ad-hoc" and that official criteria were followed. The instruments are significant, have the expected sign, and are uncorrelated with the error process (i.e., the joint hypotheses of correct model specification and the orthogonality conditions cannot be rejected).

The micro covariance measure depends on both the productivity and market share of the firm, so a logical next step is to separate these two channels. However, we do not run regressions with levels of productivity or market share or their deviations from industry averages as dependent variables. Due to the nature of unobserved heterogeneity, it would be much harder to achieve identification for such regressions using levels. This is especially true in the case of market shares. In the following two sections, we look at the impact of aid on the growth of market share and on the growth of TFP instead to learn more about the channel through which aid affects static allocative efficiency.

Table 2.7: Static Allocative Efficiency Estimation: OLS and IV

	OLS	IV	
	(1)	(2)	(2a)
			1^{st} stage: Aid
Constant	-0.008	-0.033	-0.029***
	[0.046]	[0.048]	[0.004]
Aid	-0.355***	-1.315***	
	[0.131]	[0.508]	
Labor Intensive Firms	-0.206	-0.172	0.001
	[0.126]	[0.132]	[0.004]
Labor Intensive Firms			0.040***
x Pre-2002			[0.010]
Medium and Large Firms	0.496***	0.526***	-0.002
	[0.113]	[0.112]	[0.002]
Medium and Large Firms			0.045***
x Pre-2002			[0.007]
Old Firms (10+ Years)	0.059	0.064	0.004
	[0.040]	[0.040]	[0.003]
Loss in at least 2 out of	0.035	0.059**	0.024***
3 Consecutive Years	[0.030]	[0.030]	[0.004]
Share of Debt Financing higher	-0.056**	-0.052*	0.004**
than 4-Digit Industry Average	[0.027]	[0.027]	[0.002]
Percentage of Regional Labor	0.183	0.199	0.017***
Force Employed by the Firm	[0.141]	[0.141]	[0.004]
Regional Unemployment Rate	0.117*	0.111	-0.001
Higher than Average	[0.068]	[0.069]	[0.009]
Observations	23226	23226	23226
Firms	5558	5558	5558
R-squared	0.05		0.11
Partial R-squared			0.011
F-test			69.06
p-value of Hansen J statistic		0.152	

Clustered standard errors in brackets. Control variables: 2-digit industry and growth of its output, region and growth of regional GVA, year. *significant at 10%, **significant at 5%, ***significant at 1%.

Source: Own calculations based on CSAC and PASEF data.

2.5.4 Impact of Aid on Growth of Market Share

The regression in column (1) of Table 2.8 is based on the following version of equation (2.7):

$$\frac{s_{it,j} - s_{it-1,j}}{0.5 * (s_{it,j} + s_{it-1,j})} = \beta_0 + \beta_1 AID_{it-1} + \mathbf{X_{it-1}} \boldsymbol{\gamma} + u_{it},$$

where i denotes firm, t denotes time, AID is a dummy variable defined as in (2.8), and $\mathbf{X}$ is a vector of controls. The OLS specification assumes selection on observables. The estimated OLS coefficient on AID is positive, but insignificant. Estimated coefficients on the indicators for labor intensive firms, old firms, firms with losses in 2 out of 3 consecutive years firms whose share of debt financing is higher than their 4-digit industry average are all negative and significant, while the estimated coefficient on the indicator for medium and large firms is positive and significant.

The IV estimates in column (2) of Table 2.8 use the interactions between a pre-2002 dummy variable and a dummy variables for labor intensive firms and for medium and large firms as instruments for aid. The IV estimate of the coefficient on aid is positive and statistically significant, and the point estimate is much larger than that obtained under the assumption of selection on observables. This suggests that aid receiving firms had higher market share growth than they would have had had they not received aid. The instruments are significant and have the expected sign, and the instruments are uncorrelated with the error process (i.e., the joint hypotheses of correct model specification and the orthogonality conditions cannot be rejected).

Table 2.8: Growth of Market Share Estimation: OLS and IV

	OLS	IV	
	(1)	(2)	(2a)
			1^{st} stage: Aid
Constant	0.071***	0.087***	-0.029***
	[0.017]	[0.017]	[0.004]
Aid	0.012	0.616***	
	[0.020]	[0.184]	
Labor Intensive Firms	-0.043***	-0.064***	0.001
	[0.011]	[0.013]	[0.004]
Labor Intensive Firms			0.040***
x Pre-2002			[0.010]
Medium and Large Firms	0.032***	0.013	-0.002
	[0.009]	[0.011]	[0.002]
Medium and Large Firms			0.045***
x Pre-2002			[0.007]
Old Firms (10+ Years)	-0.066***	-0.070***	0.004
	[0.007]	[0.007]	[0.003]
Loss in at least 2 out of	-0.086***	-0.101***	0.024***
3 Consecutive Years	[0.010]	[0.011]	[0.004]
Share of Debt Financing higher	-0.013**	-0.016**	0.004**
than 4-Digit Industry Average	[0.006]	[0.006]	[0.002]
Percentage of Regional Labor	0.001	-0.009*	0.017***
Force Employed by the Firm	[0.003]	[0.005]	[0.004]
Regional Unemployment Rate	0.006	0.010	-0.001
Higher than Average	[0.040]	[0.040]	[0.009]
Observations	23182	23182	23182
Firms	5549	5549	5549
R-squared	0.02		0.11
Partial R-squared			0.011
F-test			69.05
p-value of Hansen J statistic		0.174	

Clustered standard errors in brackets. Control variables: 2-digit industry and growth of its output, region and growth of regional GVA, year. *significant at 10%, **significant at 5%, ***significant at 1%.

Source: Own calculations based on CSAC and PASEF data.

2.5.5 Impact of Aid on Growth of TFP

TFP is another channel through which static allocative efficiency can be affected by aid. The regression reported in column (1) of Table 2.9 is based on the following version of equation (2.7):

$$p_{it} - p_{it-1} = \beta_0 + \beta_1 AID_{it-1} + \mathbf{X_{it-1}}\boldsymbol{\gamma} + u_{it},$$

where i denotes firm, t denotes time, p is firm-level TFP, AID is a dummy variable defined as in (2.8), and $\mathbf{X}$ is a vector of controls. The OLS specification assumes selection on observables. The estimated OLS coefficient on AID is positive, but statistically insignificant. Estimated coefficients on indicators for old firms and firms with losses in 2 out of 3 consecutive years are negative and statistically significant, while the estimated coefficient on the percentage of the regional labor force employed by the firm is positive and significant.

The IV estimate of the coefficient on aid, reported in column (2), is negative and lower than that obtained under the assumption of selection on observables but is statistically insignificant. The joint hypotheses of correct model specification and the orthogonality conditions cannot be rejected. Overall, these results suggest that aid did not have a significant effect on TFP growth.

2.5.6 Summary of Results and Macroeconomic Implications

Table 2.10 contains the estimates of the coefficient on aid in the models with micro covariance, growth of market share and growth of TFP as outcomes, using the OLS and IV estimators.[37] Both estimation methods yield a negative and statistically significant impact of aid on micro covariance. However, the IV point estimate is

[37]For the IV estimator, we present results using both instruments mentioned in section 2.4.2, and the results are similar in both magnitude and level of significance if only one of them is used.

Table 2.9: Growth of TFP Estimation: OLS and IV

	OLS	IV	
	(1)	(2)	(2a)
			1^{st} stage: Aid
Constant	-0.022**	-0.025**	-0.029***
	[0.011]	[0.012]	[0.004]
Aid	0.003	-0.119	
	[0.012]	[0.153]	
Labor Intensive Firms	0.004	0.009	0.001
	[0.007]	[0.009]	[0.004]
Labor Intensive Firms			0.040***
x Pre-2002			[0.010]
Medium and Large Firms	0.005	0.009	-0.002
	[0.006]	[0.008]	[0.002]
Medium and Large Firms			0.045***
x Pre-2002			[0.007]
Old Firms (10+ Years)	-0.019***	-0.018***	0.004
	[0.006]	[0.006]	[0.003]
Loss in at least 2 out	-0.024***	-0.021**	0.024***
of 3 Consecutive Years	[0.009]	[0.010]	[0.004]
Share of Debt Financing higher	0.005	0.006	0.004**
than 4-Digit Industry Average	[0.005]	[0.005]	[0.002]
Percentage of Regional Labor	0.004**	0.006***	0.017***
Force Employed by the Firm	[0.002]	[0.003]	[0.004]
Regional Unemployment Rate	-0.026	-0.027	-0.001
Higher than Average	[0.034]	[0.034]	[0.009]
Observations	23182	23182	23182
Firms	5549	5549	5549
R-squared	0.01		0.10
Partial R-squared			0.011
F-test			69.05
p-value of Hansen J statistic		0.956	

Clustered standard errors in brackets. Control variables: 2-digit industry and growth of its output, region and growth of regional GVA, year. *significant at 10%, **significant at 5%, ***significant at 1%.

Source: Own calculations based on CSAC and PASEF data.

much larger than the OLS point estimate.[38]

The IV estimator allows selection on unobservables and is as such preferable

[38] The results are in general similar for labor productivity-based measures (available upon request), but given the measurement problems described in Appendix B, we prefer TFP-based measures.

Table 2.10: Summary of Results: Estimated Treatment Effects of Aid

	OLS	IV
Micro Covariance	-0.355***	-1.315***
	[0.131]	[0.508]
Growth of Market Share	0.012	0.616***
	[0.020]	[0.184]
Growth of TFP	0.003	-0.119
	[0.012]	[0.153]

Clustered standard errors in brackets. Control variables: 2-digit industry and growth of its output, region and growth of regional GVA, year. *significant at 10%, **significant at 5%, ***significant at 1%.

Source: Own calculations based on CSAC and PASEF data.

to the OLS estimator, since the process of aid allocation is not completely transparent and it is likely that some unobserved factors affecting aid receipt also affect outcomes. Correcting for selection is thus important.

The IV estimates indicate that aid had a negative impact on static allocative efficiency, and it could be that the restricted availability of aid after 2002 was behind the improvement in the Olley and Pakes [1996] covariance, beginning in 2002 (see column (4) of Table 2.4).

It appears that aid had an impact on the micro covariance through its impact on market shares, and not so much through productivity. With the observed zero exit rate of firms that received aid, this suggests that aid prolonged the life span of these firms, and enabled them to have higher market shares than they would have had otherwise. In short, aid for rescue and restructuring of firms appears to have been distortive. Recent events indicate that it might have been better to "pull the plug" earlier on weak firms in industries such as textiles, clothing and leather, prior to being "forced" to do so by the EU, in order to give workers a better chance of finding new jobs elsewhere, or to start their own businesses or acquire new qualifications (Damijan and Polanec [2003], Damijan [2003], Grgič [2005]). Our estimates appear to be in accordance with this view.

To estimate the impact of state aid on aggregate allocative efficiency, a general equilibrium model is needed, taking into account the fact that in the absence of aid less productive firms would not be able to keep their market share and would eventually exit, and more productive firms would be able to grow faster. At this point, this is beyond the scope of this chapter and is intended for future work. However, to give some idea, we use the coefficients from column (2) in Table 2.7 to create two counterfactuals: one in which all the firms receive aid and one in which none do.

Table 2.11 shows the "simulated" aggregate covariance term from the Olley and Pakes [1996] decomposition of aggregate productivity under the assumptions of no firms receiving aid, all firms receiving aid and the actual number of firms receiving aid (see Table 2.1 for details on the percentage of firms that received aid). If everyone receives aid, the impact on the aggregate covariance term is enormous compared to the case in the absence of aid. Using the actual aid-receiving firms, the aggregate covariance term is about 1 to 30 percent lower than the no-aid aggregate covariance, depending on the year.

Table 2.11: Aggregate Static Allocative Efficiency in Manufacturing under Different Assumptions on Who Receives Aid, 1999-2003

	Percentage of Firms Receiving Aid		
Year	0	100	Actual
1999	0.128	-1.129	0.101
2000	0.136	-1.115	0.092
2001	0.140	-1.123	0.102
2002	0.138	-1.098	0.126
2003	0.143	-1.099	0.141

Source: Own calculations based on CSAC and PASEF data and Table 2.7.

2.6 Conclusion

The literature on the effect of institutions on firm performance is relatively small, because micro level data on both firm performance and market institutions are not widely available. In this chapter, we use a unique census micro level data set on a potentially very distortive market institution, namely state aid for firms in difficulty. We examine its impact on the static and dynamic allocative efficiency of Slovenian manufacturing in the period from 1998-2003.

We use an Olley and Pakes [1996]-inspired measure of static allocative efficiency, which takes into account both productivity and the allocation of inputs and outputs across businesses. This measure is comparable across firms and sectors, and it provides an informative and compact measure of allocative efficiency that could be used by policy makers when allocating aid to firms. Even though Slovenia is among the most successful transition economies, analysis reveals that aid had a negative and significant impact on static allocative efficiency.

Aid also appears to have postponed the exit of firms, since none of the firms that received aid exited, even though the predicted mean probability of exit based on fundamentals was higher for receipients than for firms that did not receive aid. The growth rate of market share was higher for aid-receiving firms, which suggests that aid had a distortive effect on the market structure, because it allowed less efficient firms to grow faster than more efficient firms and thus shifted the burden of structural adjustment onto firms that managed without aid.

To estimate the impact of state aid on aggregate allocative efficiency and productivity growth, a general equilibrium model is needed, taking into account the fact that in the absence of aid less productive firms would not be able to keep their market share and would eventually exit, while more productive firms would be able to grow faster. This is beyond the scope of this chapter and is intended for future work. Future work also includes looking at the impact of state aid on investment and job flows, given that aid-receiving firms employed up to 18 percent of all workers in manufacturing.

There are a couple of limitations that need to be mentioned. First, these estimates assume homogenous treatment effects (i.e., the impact of aid is assumed to be constant across firms). While this may be appropriate as a first step in the direction of studying the treatment effects of aid, we plan to relax this assumption and allow the impact of aid to differ across firms (at least for treated and untreated firms). Second, the impact of aid might depend on where the firm is in the distribution of productivity or micro covariance. Since only a small percentage of firms actually received aid, however, the data do not allow the estimation of more sophisticated models, such as a quantile regression model, for example. Third, the period of analysis is relatively short.

Working with firm-level datasets presents many challenges, since such data usually have restricted access and are not readily available or even prescreened for errors as some household level datasets are. There is a lot of heterogeneity among firms. Missing firm-year observations and measurement error are part and parcel of every firm-level dataset and the quality of estimates critically depends on the way these are dealt with. In addition, aid affected only a small number of firms, and estimating treatment effects pushes the data quite hard. However, it is an instructive exercise and in time, the quality of micro level data on both firms and institutional measures is likely to improve as we learn more about both.

Chapter 3

Assessing the Job Flows Across Countries: The Role of Industry, Size and Regulations[1]

3.1 Introduction

Over the past decade, a growing body of evidence has accumulated suggesting that the reallocation of factors of production - including labor - plays a major role in driving productivity growth (see e.g. Olley and Pakes [1996], Griliches and Regev [1995], Foster et al. [2001], Foster et al. [2002] and Bartelsman et al. [2004b]). New firms enter the market and create new jobs, while other unprofitable firms exit the market contributing to job destruction. Incumbent firms are in a continuous process of adaptation in response to the development of new products and processes, the growth and decline in markets and changes in competitive forces (Davis and Haltiwanger [1999]). Market conditions and institutional factors play a major role in shaping the magnitude of job flows and their characteristics (Davis et al. [1996]). For example, smaller businesses are inherently more dynamic, in part because they tend to be young ventures and adjust through a learning-by-doing process (Dunne et al. [1988], Dunne et al. [1989]). In addition, some industries have inherently higher job flows (Foster et al. [2002] report that the job flows in the United States

[1]This chapter draws heavily on a joint paper with John Haltiwanger and Stefano Scarpetta with the same title.

retail sector are 1.5 times higher than in the manufacturing sector) than others in all countries, given the smaller size of their typical business and lower inherent entry costs.

Technological and market driven factors are coupled by a host of regulations in driving job flows. For example, regulations affecting start up costs or bankruptcy procedures are likely to affect firm turnover and the associated labor mobility. Likewise, employment protection legislation may stifle labor reallocation by raising labor adjustment costs. Assessing the role of regulations in affecting job flows, over and above that played by technological and market driven factors is of great importance. While labor reallocation is indeed important to promote productivity growth, it is also painful for the affected workers, who face significant search and other adjustment costs (see, e.g., Mortensen and Pissarides [1999a], Mortensen and Pissarides [1999b] and Caballero and Hammour [2000b]). Several models predict that labor regulations reduce gross job flows (e.g. Bertola [1992], Hopenhayn and Rogerson [1993]), but the empirical evidence is still inconclusive. While several empirical papers find a negative effect of employment protection legislation on unemployment (Bentolila and Bertola [1990], Nickell and Layard [1999]), the effects on job reallocation are more nuanced (Bertola and Rogerson [1997], Boeri [1999]): countries with different types of labor regulations are observed to have fairly similar gross job flows. The lack of a causal relationship between regulations and gross job flows at the aggregate level may be due to different elements. Stringent labor regulations may be associated with other regulatory and institutional factors that also affect job flows. For example, Bertola and Rogerson [1997] argue that countries with strict regulations also tend to have institutions that restrict the ability of firms to adjust wages in response to a shock (e.g. centralized wage bargaining). A more fundamental problem is that cross country analyses of job flows may be flawed by severe omitted variable problems and measurement errors, including differences in the distribution of activity across industries and size of firms as well as different cut-off points in the enterprise surveys from which job flow data are obtained.

In this paper, we draw from a harmonized and integrated firm-level dataset including 16 developed, emerging and transition economies. With these data, we explore the industry and size dimensions of the job flows in detail and relate them to institutional differences across countries.[2] To give a preview of our results, we find that countries share a number of features of job flows along the industry and size dimensions. All countries are characterized by large job flows. These vary significantly and systematically across industries, pointing to technological and market-driven factors, but especially across firms of different sizes. However, there are notable cross-country differences even after controlling for industry and size effects. Thus, we develop a formal test of the links between hiring and firing regulations and job flows in this chapter, and also test for the robustness of our results to the inclusion of other regulations affecting business operations. We use a difference-in-difference approach whereby we identify an industry and size class's baseline job reallocation

[2]To our knowledge, the only other paper that econometrically analyzes the effects of labor regulations on gross job flows across countries is Micco and Pages [2004]. Their paper exploits sectoral gross job flows data for manufacturing for 18 countries. We extend their work by also including the service industry for a subset of countries and, more importantly, by controlling for industry specific differences in firm size. In addition, our data allow distinguishing between jobs flows generated by the entry and exit of firms and those generated by the reallocation of labor by incumbent firms. As shown in the paper, this sheds additional light on labor reallocation and the role of regulations in labor and product markets.

from the United States data. Under the assumption that regulations in the United States are among the least restrictive in our sample, the baseline should proxy for the technological and market driven job turnover in the absence of policy induced adjustment costs. Under the additional assumption that this technological and market driven demand for labor reallocation carries over to other countries, we assess whether industries that require more labor mobility are disproportionately affected by regulations that raise adjustment costs. The advantage compared with standard cross-country/cross-industry empirical studies is that we exploit within country differences between industry/sizes based on the interaction between country and industry/size characteristics. Thus, we can also control for country and industry/size effects, thereby minimizing the problems of omitted variable bias and other misspecifications. Interestingly, we find support for the general hypothesis that hiring and firing costs reduce turnover, especially in those industries that require more frequent labor adjustment. Regulations also distort the patterns of industry/size flows. Within each industry, medium and large firms are more severely affected by stringent labor regulations, while small firms are less affected, probably because they are partially exempt from such regulations or can more easily circumvent them. Moreover, stringent labor regulations have more of an impact on job flows by small and medium entering and exiting firms, as well as continuing firms of all sizes, whereas product market regulations are more important for shaping the job flows of large entering and exiting firms, and do not play much of a role for continuing firms.

The remainder of the chapter is organized as follows. Section 3.2 presents our harmonized firm-level dataset and discusses the different concepts we have used to characterize labor reallocation. Section 3.3 analyzes the main features of job flows, highlighting the role of firm dynamics, industry and size compositions. Section 3.4 presents the results from the analysis of variance. Section 3.5 introduces the difference-in-difference approach used in the econometric analysis and discusses the empirical results for the baseline and policy augmented specifications of the job flow equations. It also describes a battery of robustness tests. Finally, Section 3.6 provides some concluding remarks.

3.2 Data

Our analysis of job flows draws from a harmonized firm-level database that involves 16 industrial, developing and emerging economies (Germany, Finland, France, Italy, Portugal, United Kingdom and United States, Estonia, Hungary, Latvia, Slovenia, Argentina, Brazil, Chile, Colombia and Mexico) and covers the 1990s (time period covered varies by country - see Table 3.1).[3] The data collection was conducted by an active participation of local experts in each of the countries, and involved the harmonization of key concepts to the extent possible (such as entry and exit of firms, job creation and destruction, and the unit of measurement), as well as the definition of common methods to compute the indicators (see Bartelsman et al. [2005] for details).[4]

[3]The database also includes Indonesia, South Korea and Taiwan (China) as well as the Netherlands, Canada, Denmark, Romania and Venezuela, but annual data on job flows are not available for these countries or are not fully reliable.

[4]Micco and Pages [2004] compiled a dataset from different country sources covering 2-digit manufacturing sector information for 18 countries. Their dataset does not include transition countries, and does not allow differentiating job flows by firm status and firm size for all the countries.

Table 3.1: Data Sources Used for Firm Demographics and Job Flows

Country	Source	Period	Max. industry coverage (number of industries)	Threshold
OECD				
Finland	Business register	1988-1998	All (17)	Emp $\geq$ 1
France	Fiscal database	1989-1997	All (17)	Turnover: Man: Euro 0.58m Serv: Euro 0.17m
Germany (West)	Social security	1977-1999	All but civil service, self employed (11)	Emp $\geq$ 1
Italy	Social security	1986-1994	All (19)	Emp $\geq$ 1
Portugal	Employment-based register	1983-1998	All but public administration (19)	Emp $\geq$ 1
United Kingdom	Business register	1980-1998	Manufacturing (10)	Emp $\geq$ 1
United States	Business register	1988-1997	Private businesses (19)	Emp $\geq$ 1
LAC				
Argentina	Register, based on Integrated System of Pensions	1995-2002	All (19)	Emp $\geq$ 1
Brazil	Census	1996-2001	Manufacturing (13)	Emp $\geq$ 1
Chile	Annual Industry Survey (ENIA)	1979-1999	Manufacturing (13)	Emp. $\geq$ 10
Colombia	Annual Manufacturing Survey (EAM)	1982-1998	Manufacturing (13)	Emp. $\geq$ 10
TRANSITION				
Estonia	Business register	1995-2001	All (19)	Emp $\geq$ 1
Hungary	Fiscal register (APEH)	1992-2001	All (19)	Emp $\geq$ 1
Latvia	Business register	1996-2002	All (18)	Emp $\geq$ 1
Mexico	Social security	1985-2001	All (17)	Emp $\geq$ 1
Slovenia	Business register	1992-2001	All (19)	Emp $\geq$ 1

The key features of the micro-data underlying the analysis are as follows:

Unit of observation: Data used tend to conform to the following definition: "an organizational unit producing goods or services which benefits from a certain degree of autonomy in decision-making, especially for the allocation of its current resources" (EUROSTAT [1998]). Generally, this will be above the establishment level.

Size threshold: While some registers include even single-person businesses (firms without employees), others omit firms smaller than a certain size, usually in terms of the number of employees (businesses without employees), but sometimes in terms of other measures such as sales (as is the case in the data for France). Data used in this study exclude single-person businesses. However, because smaller firms tend to have more volatile firm dynamics, remaining differences in the threshold across different country datasets should be taken into account in the international comparison.

Industry coverage: Special efforts have been made to organize the data along a common industry classification (ISIC Rev.3) that matches the OECD-Structural database (STAN). In the panel datasets constructed to generate the tabulations, firms were allocated to the single STAN industry that most closely fit their operations over the complete time-span.

The firm-level and job flows data come from business registers (Finland, United

Kingdom and United States, Estonia, Latvia, Slovenia), social security databases (Germany, Italy, Mexico) or corporate tax rolls (Argentina, France, Hungary) (Table 3.1). Annual industry surveys are generally not the best source for firm demographics, due to sampling and reporting issues, but have been used nonetheless for Brazil, Chile, and Colombia. Data for Portugal are drawn from an employment-based register containing information on both establishments and firms. All these databases allow firms and jobs to be tracked over time because addition or removal of firms from the registers reflects the actual entry and exit of firms.

We define four size classes based on the number of firm' employees: 1- 19 workers, 20-49 workers, 50-99 workers, and 100 or more workers. We define the job creation rate, job destruction rate, net employment growth, job reallocation rate, and excess job reallocation rate (also by firm status: continuing, entering and exiting firms) as follows (see also Davis et al. [1996]):

Job Creation Rate:	$pos_{sict} = \frac{\sum_{i \in SC^{+}} \Delta E_{sict}}{0.5(E_{sict} + E_{sic,t-1})}$
Job Destruction Rate:	$neg_{sict} = \frac{\sum_{i \in SC^{-}} \Delta E_{sict}}{0.5(E_{sict} + E_{sic,t-1})}$
Job Creation Rate (Entry):	$pos_{EN,sict} = \frac{\sum_{i \in SC^{+},EN} \Delta E_{sict}}{0.5(E_{sict} + E_{sic,t-1})}$
Job Destruction Rate (Exit):	$neg_{EX,sict} = \frac{\sum_{i \in SC^{-},EX} \Delta E_{sict}}{0.5(E_{sict} + E_{sic,t-1})}$
Net Employment Growth:	$net_{sic} = pos_{sic} - neg_{sic}$
Job Reallocation Rate:	$sum_{sic} = pos_{sic} + neg_{sic}$
Excess Job Reallocation Rate:	$exc_{sic} = pos_{sic} - \lvert neg_{sic} \rvert$

where i represents industry, s represents size class, c represents country, t represents time and E denotes employment. Capital letters S and C refer to a set of size classes or countries, respectively. The symbol Δ denotes the first-difference operator, $\Delta E_t = E_t - E_{t-1}$. We take averages of *pos* and *neg*, and then calculate *net*, *sum* and *exc*.

3.3 Basic Facts about Job Turnover in Industrial and Emerging Economies of Latin America and Central and Eastern Europe

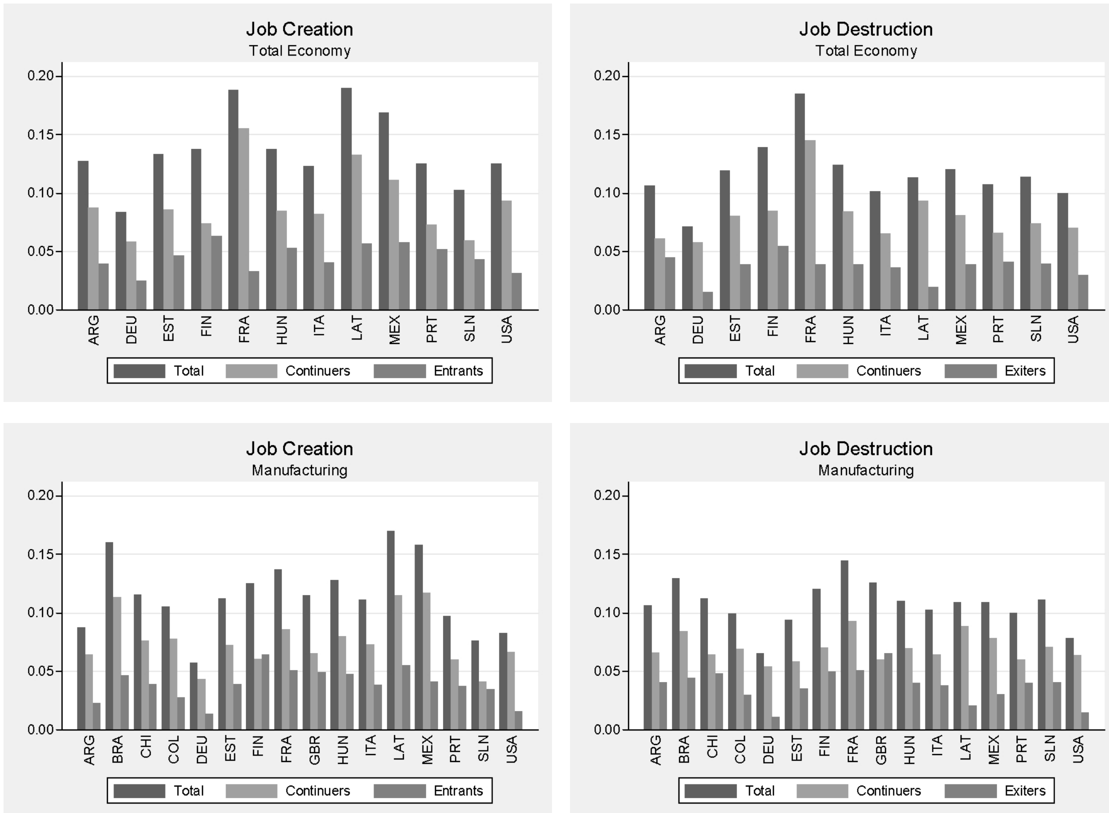
This section explores the main stylized facts emerging from our analysis across countries, industries and firm size: 1) the large magnitude of job flows in all countries, 2) the significant role that firm entry and exit play in total job flows, 3) the different job turnover across firms of different sizes, and 4) the similarities in the industry ranking of job turnover across countries. We review these stylized facts in turn below to motivate our multivariate analysis aimed at assessing the possible role of labor market regulations for job turnover and the magnitude and efficiency of the allocation of labor.

3.3.1 Large Job Turnover in All Countries

Table 3.3 presents summary statistics for job flows across industry, size classes and countries, for the total economy. Figure 3.1 summarizes country level job flows and compares them across countries.

The first noticeable fact emerging from this cross country comparison is the large magnitude of job flows in all countries. Gross job flows (the sum of job creation and job destruction) range from about 25 percent of total employment on average

Figure 3.1: Decomposition of Job Creation and Destruction by Continuing, Entering and Exiting Firms, 1990s, Total Economy and Manufacturing



Source: Own calculations based on harmonized firm-level database.

in the OECD countries, to 29 percent in Latin American countries and to about 30 percent in the transition economies. By contrast, net employment changes were very modest if not nil in the OECD and the Latin America samples, while the transition economies recorded a significant net job growth in the period covered by the data, after the substantial job losses of the early phases of the transition.

3.3.2 Firm Dynamics Plays a Major Role in Total Job Flows

The second main stylized fact emerging from our analysis of job flows is the strong contribution of the creative destruction process. Indeed, entering and exiting

Table 3.3: Average Job Flows in the 1990s, Overall and by Region, Total Economy

OVERALL					
Variable	Obs	Mean	Std. Dev.	Min	Max
Job Creation Rate	1048	0.147	0.067	0.000	0.647
Job Destruction Rate	1048	0.131	0.062	0.000	0.419
Net Employment Growth	1048	0.015	0.065	-0.299	0.419
Job Reallocation Rate	1048	0.278	0.112	0.000	0.875
Excess Job Reallocation Rate	1048	0.231	0.098	0.000	0.732
Job Creation Rate (Entry)	1048	0.055	0.043	0.000	0.357
Job Destruction Rate (Exit)	1048	0.046	0.029	0.000	0.216
OECD					
Job Creation Rate	448	0.127	0.046	0.033	0.288
Job Destruction Rate	448	0.127	0.060	0.029	0.411
Net Employment Growth	448	0.000	0.046	-0.282	0.148
Job Reallocation Rate	448	0.254	0.096	0.072	0.57
Excess Job Reallocation Rate	448	0.223	0.085	0.058	0.472
Job Creation Rate (Entry)	448	0.045	0.030	0.003	0.195
Job Destruction Rate (Exit)	448	0.045	0.028	0.000	0.216
LAC					
Job Creation Rate	300	0.148	0.061	0.033	0.431
Job Destruction Rate	300	0.140	0.066	0.041	0.419
Net Employment Growth	300	0.008	0.053	-0.214	0.286
Job Reallocation Rate	300	0.288	0.114	0.086	0.785
Excess Job Reallocation Rate	300	0.248	0.103	0.066	0.732
Job Creation Rate (Entry)	300	0.056	0.040	0.000	0.227
Job Destruction Rate (Exit)	300	0.053	0.032	0.003	0.152
TRANSITION					
Job Creation Rate	300	0.174	0.088	0.000	0.647
Job Destruction Rate	300	0.128	0.061	0.000	0.385
Net Employment Growth	300	0.046	0.087	-0.299	0.419
Job Reallocation Rate	300	0.303	0.123	0.000	0.875
Excess Job Reallocation Rate	300	0.227	0.109	0.000	0.608
Job Creation Rate (Entry)	300	0.070	0.056	0.000	0.357
Job Destruction Rate (Exit)	300	0.039	0.025	0.000	0.135

Source: Own calculations based on harmonized firm-level database.

firms account for about 30-40 percent of total job flows. Within the OECD sample, the entry of new firms played a particularly strong role in total job creation in Finland in the 1990s (46 and 51 percent of total job creation in total economy and manufacturing, respectively), Slovenia (42 and 46 percent of total job creation) and

Portugal (41 and 38 percent of total job creation). At the same time, the exit of obsolete firms also accounted for a significant fraction of overall job destruction, particularly so in Argentina (42 and 38 percent of total job destruction), Finland (39 and 41 percent of total job destruction) and Portugal (38 and 40 percent of total job destruction). In transition countries, entry was more important in the early years of transition and exit in the second half of the 1990s, both for the total economy and in manufacturing.[5]

The large job flows in the transition countries are not surprising. The process of transition started in the early 1990s and it included downsizing of existing firms as well as new firms emerging as the economies were moving towards a market economy. Indeed, 40.2 percent of jobs were created by entering firms in transition countries, compared to 35.4 percent in the OECD countries. In addition, job destruction due to exit represented 35.4 percent of total job destruction in the OECD countries, but only 30.5 percent in transition countries. Findings are similar if we focus only on industries within manufacturing.

3.3.3 Small and Large Firms Contribute the Most to Job Flows

Small firms account for the vast majority of total firm dynamics in all countries in our sample. However, their contribution to overall job reallocation, while still important, is less dominant. Figure 3.2 presents job reallocation rates by firm size classes. In general, job reallocation is highest in firms with less than 20 employees,

[5] This was especially so in Slovenia, a lot of entry occurred in the early 1990s, since private firms were few and far in between prior to that; exit did not keep up with that early on and was relatively low compared to OECD and other transition countries.

and the lowest in firms with 100+ employees. In the United States, job turnover declines monotonically with firm size, and the decline is particularly marked among large units (100+). Latin American countries follow similar patterns to those of the United States, while the European countries, with the exception of France, have a less marked drop of job reallocation among larger units. The transition countries, on the other hand, show a steeper slope in smaller size classes, especially in the early years of transition.

The analysis of size specific job reallocation rates should be complemented with a decomposition of the overall job reallocation into that due to firms of different sizes. Tables 3.4 and 3.5 present the percentage of job creation/destruction/reallocation in each size class as a share of total job creation/destruction/reallocation for total economy and manufacturing, respectively:

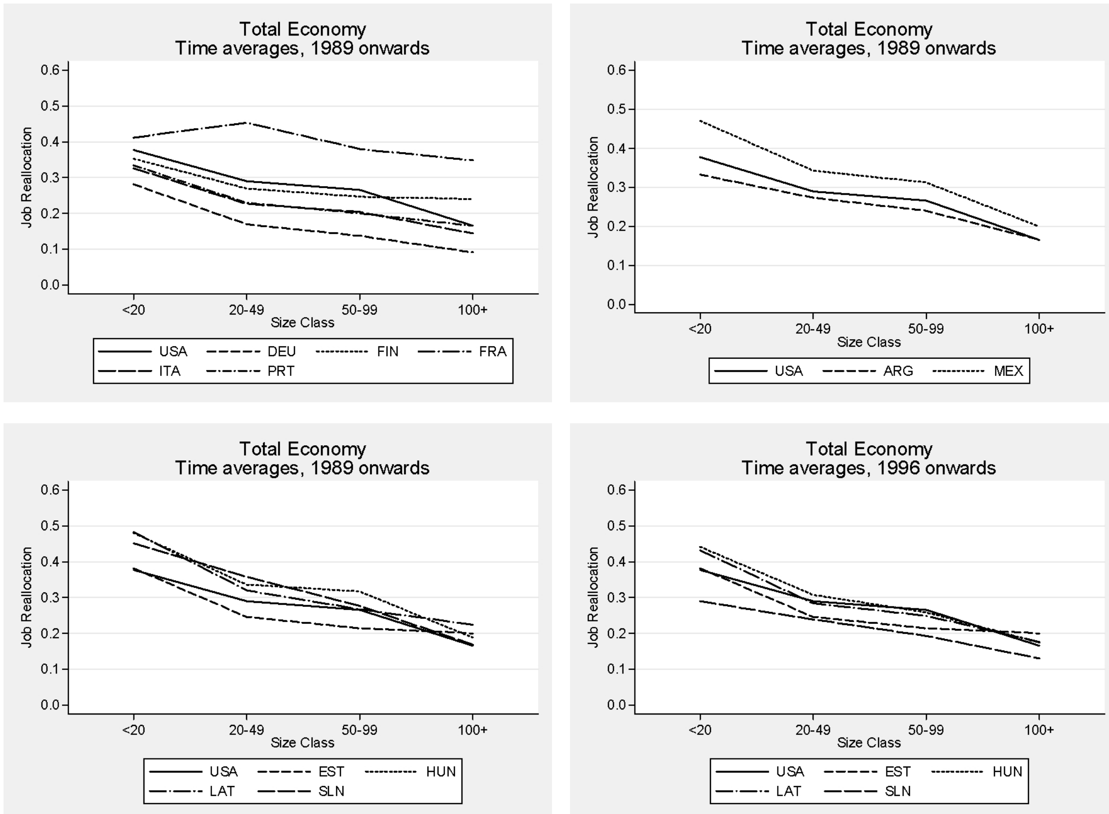
$$pX_{sic} = \frac{X_{sic}}{X_{ic}} \tag{3.1}$$

where i denotes industry, s denotes size class and c denotes country. X stands for POS, NEG or SUM, where POS is the number of jobs created, NEG the number of jobs destroyed and SUM the total number of jobs reallocated (created+destroyed).[6]

In manufacturing, the highest share of jobs was created/destroyed/reallocated by firms in the largest size class, 100+. At the same time, however, the second

[6]Note that for Chile, Colombia and France, we do not observe some of the smallest firms (in the first two countries, we do not observe firms with less than 10 workers, and for France, firms with sales below a certain threshold are excluded from the sample).

Figure 3.2: Job Reallocation across Firms of Different Sizes, Total Economy



Source: Own calculations based on harmonized firm-level database.

most important size class in terms of job reallocation is firms with less than 20 employees. In fact, it seems that the number of jobs created/destroyed/reallocated has a U-shaped relationship with size class in manufacturing. The importance of the smallest size class increased in transition countries over time, and the importance of the largest size class decreased.

At the level of total economy, the highest share of jobs was created/destroyed/-reallocated in the smallest size class in a number of countries (Germany, Italy, Portugal, Argentina, Estonia, Latvia), followed by the largest size class. Again, a similar pattern is observed for transition countries: the smallest size class gained in importance over time, while the largest size class declined in importance.

Table 3.4: Percentage of Job Flows in a Certain Size Class, Total Economy, 1990s

	Gross Job Reallocation				**Job Creation**				**Job Destruction**			
Country	<20	20-49	50-99	100+	<20	20-49	50-99	100+	<20	20-49	50-99	100+
Germany	0.467	0.140	0.093	0.300	0.440	0.149	0.102	0.309	0.51	0.129	0.082	0.278
Finland	0.394	0.103	0.067	0.436	0.419	0.088	0.055	0.438	0.369	0.12	0.080	0.431
France	0.173	0.133	0.110	0.584	0.130	0.085	0.103	0.682	0.220	0.185	0.119	0.477
Italy	0.522	0.130	0.073	0.276	0.492	0.142	0.085	0.280	0.568	0.116	0.059	0.256
Portugal	0.457	0.153	0.097	0.292	0.471	0.152	0.094	0.283	0.449	0.152	0.099	0.300
United States	0.315	0.131	0.087	0.467	0.279	0.132	0.089	0.499	0.361	0.130	0.085	0.423
Argentina	0.397	0.154	0.106	0.342	0.367	0.158	0.112	0.362	0.433	0.147	0.097	0.322
Mexico	0.377	0.137	0.099	0.386	0.319	0.137	0.103	0.442	0.462	0.138	0.094	0.307
Estonia (1990s)	0.365	0.172	0.125	0.337	0.414	0.167	0.114	0.306	0.318	0.180	0.139	0.363
Hungary (1990s)	0.273	0.134	0.118	0.475	0.296	0.144	0.107	0.453	0.251	0.125	0.127	0.497
Latvia (1990s)	0.383	0.141	0.104	0.371	0.390	0.137	0.101	0.372	0.376	0.150	0.112	0.363
Slovenia (1990s)	0.227	0.088	0.100	0.585	0.293	0.100	0.090	0.517	0.169	0.076	0.112	0.643
Estonia (late 1990s)	0.365	0.172	0.125	0.337	0.414	0.167	0.114	0.306	0.318	0.180	0.139	0.363
Hungary (late 1990s)	0.317	0.142	0.108	0.433	0.337	0.149	0.106	0.408	0.294	0.132	0.111	0.463
Latvia (late 1990s)	0.421	0.143	0.107	0.328	0.437	0.139	0.107	0.317	0.398	0.150	0.109	0.343
Slovenia (late 1990s)	0.287	0.104	0.099	0.510	0.328	0.121	0.084	0.467	0.244	0.085	0.116	0.555

We do not observe firms with sales below a given threshold in France.

Source: Own calculations based on harmonized firm-level database.

Table 3.5: Percentage of Job Flows in a Certain Size Class, Manufacturing, 1990s

	Gross Job Reallocation				**Job Creation**				**Job Destruction**			
Country	<20	20-49	50-99	100+	<20	20-49	50-99	100+	<20	20-49	50-99	100+
Germany	0.344	0.136	0.098	0.422	0.307	0.141	0.110	0.442	0.399	0.135	0.088	0.378
Finland	0.199	0.093	0.073	0.635	0.205	0.088	0.065	0.642	0.201	0.099	0.083	0.618
France	0.258	0.156	0.109	0.477	0.227	0.139	0.105	0.530	0.286	0.175	0.113	0.426
United Kingdom	0.198	0.116	0.102	0.583	0.209	0.116	0.103	0.572	0.183	0.113	0.101	0.604
Italy	0.427	0.142	0.078	0.353	0.421	0.154	0.082	0.343	0.445	0.133	0.074	0.348
Portugal	0.306	0.193	0.137	0.364	0.335	0.197	0.132	0.337	0.289	0.186	0.138	0.386
United States	0.161	0.116	0.096	0.626	0.146	0.119	0.099	0.635	0.180	0.114	0.094	0.612
Argentina	0.331	0.164	0.115	0.389	0.318	0.174	0.123	0.385	0.346	0.155	0.108	0.392
Brazil	0.288	0.145	0.100	0.466	0.290	0.162	0.105	0.443	0.297	0.127	0.092	0.484
Chile	0.069	0.163	0.158	0.610	0.051	0.154	0.154	0.640	0.091	0.174	0.163	0.572
Colombia	0.126	0.172	0.163	0.538	0.095	0.160	0.161	0.585	0.162	0.186	0.165	0.487
Mexico	0.258	0.124	0.103	0.515	0.201	0.115	0.100	0.584	0.343	0.137	0.106	0.414
Estonia (1990s)	0.227	0.172	0.142	0.459	0.246	0.180	0.146	0.429	0.206	0.164	0.137	0.493
Hungary (1990s)	0.159	0.121	0.111	0.609	0.165	0.135	0.117	0.583	0.154	0.107	0.106	0.633
Latvia (1990s)	0.431	0.155	0.110	0.305	0.451	0.157	0.120	0.272	0.400	0.154	0.092	0.354
Slovenia (1990s)	0.100	0.072	0.100	0.728	0.146	0.091	0.102	0.661	0.069	0.058	0.102	0.771
Estonia (late 1990s)	0.227	0.172	0.142	0.459	0.246	0.180	0.146	0.429	0.206	0.164	0.137	0.493
Hungary (late 1990s)	0.172	0.128	0.109	0.591	0.177	0.136	0.111	0.576	0.169	0.119	0.108	0.604
Latvia (late 1990s)	0.453	0.146	0.107	0.293	0.467	0.147	0.120	0.265	0.434	0.146	0.085	0.336
Slovenia (late 1990s)	0.128	0.082	0.108	0.682	0.173	0.112	0.106	0.609	0.099	0.062	0.11	0.729

We do not observe firms with less than 10 workers in Chile and Colombia, and firms with sales below a given threshold are excluded from the sample in France.

Source: Own calculations based on harmonized firm-level database.

3.3.4 Large Disparities in Job Flows Across Industries

To assess the possible role of policy and institutions in shaping the magnitude and effectiveness of job flows, we need to identify the intrinsic need for job mobility that certain industries may have compared to others. Certain industries are exposed to greater variability in demand; may be more exposed to macro shocks; and may be facing a higher pace of technological progress that imposes more frequent retooling of the production process and the associated adjustment of the workforce.

To illustrate the cross industry variation in job flows, we highlight the U.S. industries with the highest (wood) and the lowest (transport equipment) job flows within manufacturing, as well as the trade and restaurants sector (see Table 3.6). In wood, the job reallocation rate was 26 percent in the United States, and ranged from only 13 percent in Germany to 37 percent in Brazil. In the United States, incumbent firms were responsible for more than 70 percent of job reallocation, whereas in Great Britain, 53 percent of reallocation was due to entry and exit of firms. In transport equipment, the job reallocation rate was 11.9 percent in the United States, and ranged from 8.3 percent in Germany to 34 percent in Latvia. In Mexico, incumbent firms were responsible for more than 85 percent of job reallocation, whereas in Slovenia, almost 53 percent of reallocation was due to entry and exit of firms. In trade and restaurants, job reallocation ranged from 22.2 percent in Slovenia after 1996 to 38.8 percent in France. In all countries, reallocation in this industry was mostly due to incumbent firms, but this share differs among countries.

Table 3.6: Cross-Industry Variation in Job Flows

	HIGH - WOOD			LOW - TRANSPORT EQUIPMENT			TRADE AND RESTAURANT		
Country	Gross Job Reallocation	Continuers	Entry & Exit	Gross Job Reallocation	Continuers	Entry & Exit	Gross Job Reallocation	Continuers	Entry & Exit
EU & USA									
Germany	0.130	0.105	0.027	0.083	0.071	0.012			
Finland	0.252	0.156	0.096	0.249	0.135	0.113	0.264	0.158	0.106
France	0.248	0.146	0.102	0.238	0.174	0.064	0.388	0.305	0.083
United Kingdom	0.289	0.132	0.154	0.199	0.109	0.089			
Italy	0.215	0.141	0.074	0.125	0.091	0.034	0.259	0.161	0.098
Portugal	0.226	0.121	0.105	0.197	0.135	0.061	0.260	0.146	0.114
United States	**0.260**	**0.185**	**0.074**	**0.119**	**0.108**	**0.010**	**0.256**	**0.176**	**0.080**
LAC									
Argentina	0.224	0.134	0.090	0.197	0.156	0.041	0.271	0.151	0.121
Brazil	0.370	0.236	0.134	0.228	0.162	0.066			
Chile	0.287	0.151	0.136	0.272	0.163	0.109			
Colombia	0.223	0.133	0.090	0.187	0.135	0.052			
Mexico	0.346	0.228	0.118	0.234	0.200	0.033	0.311	0.182	0.129
TRANSITION, 1990s									
Estonia	0.242	0.140	0.102	0.166	0.117	0.050	0.295	0.194	0.101
Hungary	0.290	0.176	0.114	0.244	0.186	0.058	0.375	0.238	0.137
Latvia	0.292	0.219	0.074	0.330	0.243	0.087	0.298	0.222	0.076
Slovenia	0.191	0.119	0.072	0.252	0.118	0.133	0.263	0.161	0.103
TRANSITION, late 1990s									
Estonia	0.242	0.140	0.102	0.166	0.117	0.050	0.295	0.194	0.101
Hungary	0.262	0.159	0.102	0.259	0.193	0.065	0.338	0.211	0.127
Latvia	0.266	0.192	0.074	0.348	0.280	0.068	0.277	0.208	0.070
Slovenia	0.165	0.109	0.056	0.194	0.107	0.087	0.222	0.146	0.076

Source: Own calculations based on harmonized firm-level database.

Table 3.7: Pairwise Correlations with the U.S. Job Flows, Total Economy (Unbalanced Panel)

	Gross Job Reallocation	Excess Job Reallocation	Job Creation by Entering Firms	Job Destruction by Exiting Firms
OECD	**0.7057**	**0.6577**	**0.5851**	**0.6900**
Germany	0.8183	0.8074	0.7815	0.8525
Finland	0.6852	0.6025	0.0509	0.4277
France	0.4745	0.3531	0.5845	0.7815
United Kingdom	0.8471	0.8247	0.7129	0.7737
Italy	0.5954	0.5782	0.5504	0.7031
Portugal	0.8134	0.7804	0.8301	0.6012
LAC	**0.8290**	**0.7773**	**0.7848**	**0.8024**
Argentina	0.7670	0.7214	0.7851	0.7527
Brazil	0.9048	0.8383	0.9035	0.7768
Chile	0.7264	0.5556	0.6013	0.7632
Colombia	0.9121	0.8835	0.8780	0.8534
Mexico	0.8345	0.8878	0.7562	0.8660
TRANSITION, 1990s	**0.7057**	**0.6961**	**0.623**	**0.4413**
Estonia	0.6036	0.6554	0.4761	0.1641
Hungary	0.8168	0.8157	0.8174	0.6911
Latvia	0.6616	0.6962	0.5919	0.5960
Slovenia	0.7406	0.6172	0.6065	0.3140
Late 1990s	**0.6771**	**0.6981**	**0.5859**	**0.4500**
Estonia	0.6036	0.6554	0.4761	0.1641
Hungary	0.7911	0.7970	0.8070	0.6622
Latvia	0.5919	0.6644	0.5886	0.6108
Slovenia	0.7216	0.6755	0.4718	0.3629

Source: Own calculations based on harmonized firm-level database.

Table 3.8: Rank Correlations with the U.S. Job Flows, Total Economy (Unbalanced Panel)

	Gross Job Reallocation	Excess Job Reallocation	Job Creation by Entering Firms	Job Destruction by Exiting Firms
OECD	**0.7007**	**0.6330**	**0.5445**	**0.7030**
Germany	0.8186	0.8154	0.7950	0.8789
Finland	0.6450	0.5269	-0.0089	0.5028
France	0.5083	0.3688	0.5654	0.7423
United Kingdom	0.8672	0.7937	0.6713	0.8168
Italy	0.5880	0.5515	0.5443	0.5999
Portugal	0.7770	0.7418	0.6996	0.6773
LAC	**0.8371**	**0.7908**	**0.8035**	**0.8121**
Argentina	0.8611	0.8255	0.7897	0.7774
Brazil	0.8868	0.7913	0.8956	0.7828
Chile	0.6743	0.5619	0.6358	0.7608
Colombia	0.8996	0.8812	0.8624	0.8586
Mexico	0.8636	0.8940	0.8342	0.8810
TRANSITION, 1990s	**0.7174**	**0.6978**	**0.6240**	**0.4702**
Estonia	0.6785	0.6186	0.5161	0.2981
Hungary	0.8200	0.8108	0.7676	0.7223
Latvia	0.6304	0.7137	0.5481	0.5560
Slovenia	0.7407	0.6479	0.6640	0.3045
Late 1990s	**0.6925**	**0.6874**	**0.5832**	**0.4807**
Estonia	0.6785	0.6186	0.5161	0.2981
Hungary	0.7925	0.7711	0.7529	0.6955
Latvia	0.5854	0.6671	0.5945	0.5792
Slovenia	0.7136	0.6927	0.4691	0.3498

Source: Own calculations based on harmonized firm-level database.

3.3.5 The Correlation of Industry/Size Job Flows Across Countries

We next look at the correlation of industry/size level job flows across countries. A strong influence of market-driven and technological factors in shaping industry job flows should result in a strong correlation across countries. However, as we will see below and as stressed in previous studies (e.g. see Micco and Pages [2004]), industry/size job flows are also influenced by the policy and institutional environment. Lack of correlation may not therefore imply that market-driven and technological factors do not play a significant role, but rather that policy and institutions distort job flows. Job flows are part-and-parcel of the creative destruction process, and an unfavourable institutional environment will cause this process to stagnate (Caballero and Hammour [2000a]). To minimize the possible interference of the policy environment, we also present the rank correlation of industry job flows, which may provide a better proxy for the true correlation if the policy environment affects levels but not the rank order of industry/size flows.

Table 3.7 presents the industry/size pairwise level correlations, using the U.S. as the benchmark, for several flow indicators: gross job reallocation, excess job reallocation, job creation by entering firms and job destruction by exiting firms. We use 2-digit industry and four size classes. It is noticeable that the cross-country correlations are very high for most countries. Focusing on gross job reallocation, the correlation between the EU average and the United States is 0.71; that between Latin American countries and the United States is 0.83 and that for transition countries is 0.71. Rank correlations (Table 3.8) are slightly lower than levels correlations for some Latin American countries and higher for the others, but are on average still the highest among regions. Correlations are on average higher if we focus only on manufacturing (not reported here). Industry/size-level correlations with the U.S. are particularly strong for some Latin American countries, e.g. Brazil (0.90) and Colombia (0.91), despite the very different degree of economic development, as well as for Great Britain (0.84). Some of the lowest correlations are found for some EU countries, in particular France (0.47).[7]

It is also interesting to see that transition economies had a much stronger correlation of their job flow pattern by industry and size class with the United States in the sample that covers the entire 1990s, than in the sample focusing on the 1996-2001 period. This could be surprising, since the early phases of transition were characterized by massive job reallocation and the unique need to change the structure of the economy from central planning to the market based system. One working hypothesis that we develop later in the chapter is that after the initial phases of transition, these countries have moved towards the flow patterns observed in EU countries, with whom they share several policy and institutional factors.

[7]We cannot compare the reported results directly with Micco and Pages [2004], since our analysis includes the size dimension in addition to the industry dimension. However, we also conducted the analysis excluding the size dimension (not reported here, but available upon request from the authors), and we find that the pairwise correlation of with U.S. gross job reallocation is highest for Mexico (0.91), followed by Brazil (0.84) and Great Britain (0.74). They find the correlation to be the highest with Canada, Great Britain and New Zealand, but our sample covers different time-period.

3.4 Analysis of Variance

In the previous section, we have explored the different dimensions of the job flow data across countries, industries and size classes. The next logical step is to assess the relative importance of these different dimensions in explaining the overall variance in our dataset. Tables 3.9 and 3.10 present the analysis of variance of job flows, for the unbalanced total economy[8] and manufacturing samples, respectively. We consider industry, size, country and industry*size effects separately, and, in addition, differentiate the analysis of variance by region (OECD, transition, Latin America).

It is noticeable that technological and market structure characteristics that are reflected in the industry-specific effects explains only 6.8 percent of variation in overall cross-country gross job reallocation (Table 3.9), although they account for a higher share in Latin America (23.3 percent). By contrast, differences in the size structure of firms explain as much as 40.0 percent of the total variation in cross-country gross job reallocation in all regions, and play an even more important role in transition countries at the beginning of the 1990s. This fact is again in accordance with the characteristics of transition, as already mentioned in the previous section. Even country effects explain more of the variation in gross job reallocation than the industry effects, except in Latin America, so even though there are similarities among countries within a region, there is still variation between them. Overall, the combined industry*size effects can explain the bulk of the variation in gross job

[8] The total economy sample is unbalanced in the sense that it covers manufacturing only for United Kingdom, Brazil, Chile and Colombia - see Table 3.1 for details.

reallocation: 55.6 percent overall, 55.8 percent in OECD countries, 73.3 percent in Latin American countries and 72.3 percent in transition countries (66.9 percent, if we look only at the second half of the 1990s).

Gross job reallocation consists of job creation and job destruction, so we now turn to these two categories of job flows for further insight. We also further distinguish job creation by new firms and by incumbents and job destruction by exiting firms and by those that survive but downsize (we only report results for job creation by new firms and job destruction by exiting firms; other results are available upon request from the authors). A number of interesting features emerge:

- *Industry effects* explain about 6.7 percent of variation in job creation and 6.1 percent of variation in job destruction, but there are significant differences among the three regions. Industry effects account for a much larger share of the overall variation (30.8 percent) in job creation in Latin America, slightly less than half of this in OECD countries, and only 7.3 percent in transition countries. In the early phases of transition, creation of jobs occurred across all industries, whereas they were more concentrated in certain industries in OECD and especially in Latin America: 14.4 percent of variation in job destruction in Latin America can be explained by industry effects, but only 8.9 percent in OECD countries.
- *Size effects*. Both in the case of job creation and job destruction, size effects alone account for a significant share of the total variation (30.0 and 41.0 percent, respectively). Looking at results by region reveals that size effects can account for 54.0 percent of variation in job creation in transition countries,

but only 28.6 percent of variation in job destruction. In Latin America, the results are the opposite: size effects can account for 63.0 percent of variation in job destruction, but only for 21.4 percent of job creation.

- *The role of entry and exit of firms.* Size heterogeneity plays a particularly strong role in explaining the variation of job creation by new firms and job destruction by exiting firms. Size heterogeneity is particularly important in Latin America, where it accounts for 59.5 percent of the heterogeneity in job creation by new firms and 70.0 percent of the variation in job destruction by exiting firms. In the OECD countries, size heterogeneity plays a smaller role in both job creation and destruction by entering and exiting firms. In the transition economies there is a strong difference between job creation and destruction. The variation of job creation by entrants is strongly influenced by size heterogeneity, while the importance of size effects for variation in job destruction by exiters is relatively small.

How should one interpret these different sources of variability of job flows? Not surprisingly, in all regions size heterogeneity looms large among new firms depending on market conditions, but also upon regulations that may affect the optimal size of entry. This seems particularly the case in Latin America in which industries with many new micro entrants coexist with those where entry size is larger. But size heterogeneity also explains a significant fraction of the variance in job destruction due to firm exit: some industries see large failures of small young businesses while others see the decline of more mature large firms. By contrast, in transition economies there is more variability in the size structure of new firms than among those that exit the market. A large number of new businesses entered the market filling different niches of activities that were largely underdeveloped under central planning, while job destruction involved firms of different sizes more evenly, with the closure of many large obsolete firms but also of many relatively small new ventures. It is also noticeable that in the transition economies, country effects account for 20.3 percent of variation in job destruction by exiting firms, but only 6.5 percent of variation in job creation by entering firms. This is suggestive of cross-country differences in the enterprise restructuring and its impact on firm closure and downsizing.[9]

To summarize, the analysis of variance of job flows suggests a significant role for the size composition - a factor that was not considered in previous studies - as well as differences across and within regions. Technological and market structure characteristics (e.g. the industry effects) seem to play a relatively smaller role in explaining cross-country differences in job flows.

[9]See Haltiwanger and Vodopivec [2003] and World Bank [2004].

Table 3.9: Analysis of Variance, Total Economy (Unbalanced Panel)

	Job Creation	Job Destruction	Net Employment Growth	Gross Job Reallocation	Excess Job Reallocation	Job Creation - Entry	Job Destruction - Exit
INDUSTRY EFFECTS							
All	0.0670	0.0613	0.0554	0.0675	0.0538	0.0164	0.0500
OECD	0.1492	0.0892	0.1164	0.1104	0.0509	0.0229	0.0706
LAC	0.3076	0.1438	0.1568	0.2327	0.1655	0.1159	0.1049
Transition (1990s)	0.0644	0.0931	0.1525	0.0341	0.0877	0.0486	0.0938
Transition (late 1990s)	0.0731	0.0665	0.1350	0.0344	0.0790	0.0399	0.0827
SIZE EFFECTS							
All	0.3003	0.4100	0.0021	0.4706	0.4591	0.4325	0.3373
OECD	0.3027	0.3738	0.0605	0.4139	0.4468	0.4439	0.3127
LAC	0.2142	0.6300	0.2557	0.4777	0.5093	0.5950	0.7000
Transition (1990s)	0.5400	0.2861	0.1443	0.6149	0.4706	0.4858	0.1236
Transition (late 1990s)	0.4309	0.2488	0.0708	0.5268	0.4945	0.4412	0.1441
COUNTRY EFFECTS							
All	0.2138	0.1252	0.1975	0.1648	0.1435	0.1453	0.1996
OECD	0.1576	0.2009	0.1113	0.2019	0.1885	0.1253	0.2829
LAC	0.3041	0.0419	0.1808	0.1588	0.1276	0.1133	0.0255
Transition (1990s)	0.0570	0.0867	0.0974	0.0512	0.0865	0.0653	0.2031
Transition (late 1990s)	0.0997	0.0445	0.0681	0.0851	0.0933	0.0645	0.1719
INDUSTRY*SIZE EFFECTS							
All	0.3861	0.4964	0.0904	0.5558	0.5263	0.4624	0.4097
OECD	0.4888	0.5041	0.2421	0.5579	0.5215	0.5018	0.4053
LAC	0.5574	0.8079	0.5062	0.7326	0.6998	0.7364	0.8478
Transition (1990s)	0.6856	0.4685	0.3998	0.7233	0.6186	0.5956	0.3004
Transition (late 1990s)	0.5978	0.4736	0.3417	0.6692	0.6493	0.5676	0.3189

Source: Own calculations based on harmonized firm-level database.

Table 3.10: Analysis of Variance, Manufacturing

	Job Creation	Job Destruction	Net Employment Growth	Gross Job Reallocation	Excess Job Reallocation	Job Creation - Entry	Job Destruction - Exit
INDUSTRY EFFECTS							
All	0.0126	0.0432	0.0431	0.0207	0.0129	0.0093	0.0484
OECD	0.0377	0.0681	0.1729	0.0358	0.0136	0.0135	0.0691
LAC	0.0397	0.0429	0.0626	0.0371	0.0172	0.0196	0.0464
Transition (1990s)	0.0344	0.072	0.0902	0.0257	0.0577	0.0402	0.0655
Transition (late 1990s)	0.0387	0.0469	0.0695	0.0251	0.0529	0.0244	0.0666
SIZE EFFECTS							
All	0.3307	0.4572	0.0046	0.5231	0.4903	0.4120	0.3555
OECD	0.4202	0.4786	0.0727	0.5254	0.5053	0.4083	0.3252
LAC	0.3112	0.6997	0.2919	0.5946	0.5737	0.6780	0.7441
Transition (1990s)	0.5315	0.2608	0.1302	0.5940	0.4678	0.4327	0.1031
Transition (late 1990s)	0.4188	0.2257	0.0660	0.5116	0.5086	0.3937	0.1217
COUNTRY EFFECTS							
All	0.2627	0.1217	0.2310	0.1868	0.1783	0.1620	0.2351
OECD	0.1937	0.1710	0.0757	0.1981	0.2164	0.1680	0.3753
LAC	0.454	0.0538	0.2244	0.2157	0.1874	0.1446	0.0388
Transition (1990s)	0.0458	0.1033	0.0947	0.0508	0.1062	0.0589	0.2157
Transition (late 1990s)	0.1113	0.0449	0.0999	0.0761	0.1112	0.0608	0.1919
INDUSTRY*SIZE EFFECTS							
All	0.3649	0.5265	0.0811	0.5641	0.5171	0.4371	0.4274
OECD	0.4862	0.5894	0.3134	0.5930	0.5408	0.4505	0.4171
LAC	0.3724	0.7695	0.4003	0.6519	0.6081	0.7143	0.8235
Transition (1990s)	0.6548	0.4303	0.3295	0.7029	0.5831	0.5407	0.2536
Transition (late 1990s)	0.5563	0.4489	0.2741	0.6605	0.6390	0.5214	0.2797

Source: Own calculations based on harmonized firm-level database.

3.5 Empirical Analysis

3.5.1 The Estimation Model

In this section, we develop a formal test of the causal relationship between regulations in the labor market and job flows. We base our empirical analysis on two important results discussed in the previous sections: 1) a significant share of the total variance in job flows observed in the data is explained by industry*size effects, and 2) there is a high correlation of industry/size job flows across countries. These two results are consistent with the hypothesis that the distribution of idiosyncratic profit shocks impacting desired employment and the adjustment costs impacting the adjustment to such shocks varies systematically by industry and size class. Some industries show much more churning of firms in all countries, and likewise, small businesses are more volatile than large businesses in all countries.

While industry and size effects play a major role, they are far from the entire story. Adjustment costs governing responses to idiosyncratic shocks vary not only by industry and size, due to underlying market and technological factors, but also across countries, due to differences in institutions. To the extent that institutions vary more by country than industry and size, our working hypothesis is that the impact of institutions that impede adjustment in any given country will be more binding on industry/size cells with the greatest propensity for reallocation in that country. In this section, we develop a formal test of this working hypothesis. That is, we explore the links between the regulatory environment in which firms operate and job turnover that exploits these observed industry/size variations through a difference-in-difference approach (see Rajan and Zingales [1998]).[10] The test is constructed as follows: we identify an industry/size propensity for job reallocation from the United States data. Under the assumption that regulations in the labor and goods markets in the United States are among the least restrictive in our sample, variation in job reallocation across industry/size cells in the United States should proxy for the technological and market driven differences in job reallocation in the absence of policy induced adjustment costs. Under the additional assumption that these technological and market driven differences in the demand for job reallocation carry over to other countries, we assess whether industry/size cells that have a greater propensity for job reallocation are disproportionally affected by regulations that raise adjustment costs. This would imply that, ceteris paribus, industry/size cells with more volatile idiosyncratic profit shocks and more frequent adjustment of factors should be more strongly affected by regulations raising adjustment costs than those industry/size cells with less volatile idiosyncratic profit shocks and less frequent adjustment. The advantage of our approach compared to standard cross-country/cross-industry empirical studies is that we exploit within country differences between industry/size cells based on the interaction between country and industry/size characteristics. Thus, we can also control for country and industry/size effects, thereby minimizing problems of omitted variable bias and other misspecifications.

To estimate our model we of course need an appropriate measure of underlying

[10]The difference-in-difference approach has already been used in the corporate literature (e.g., Classens and Laeven [2003]), in the analysis of firm dynamics (Klapper et al. [2004]) and in the analysis of job flows (Micco and Pages [2004]).

market and technology driven differences in reallocation by industry/size cells. Since the United States is generally considered to be the country with the least restrictive regulations in the labor market and often in the goods market, we use United States industry/size job flows as the benchmark for the propensity for job reallocation.

Our different model specifications used in the empirical analysis can be summarized as follows:

i) *baseline specification*

$$JFlow_{sic} = \beta_0 + \beta_1 USJFlow_{si} + \sum_{c=1}^{C} \gamma_c D_c + \epsilon_{sic} \tag{3.2}$$

where D_c are country c $(c = 1, \ldots, C)$ dummies, $USJflow_{si}$ is the U.S. job flow variable in size class s and industry i, and ϵ is the iid error term. This specification will give us a sense about the link between cross industry/size differences in gross job flows between the United States and other countries in our sample.

ii) *cross-sectional analysis of regulation*

$$JFlow_{sic} = \beta_0 + \beta_1 USJFlow_{si} + \beta_2 Regulation_c + \sum_{m=1}^{M} \delta_m D_m + \epsilon_{sic} \tag{3.3}$$

We have now added a regulatory variable that only varies across countries and thus requires removing the country dummies. To partially control for the omitted fixed effect, we can introduce regional dummies $(D_m, m = 1, \ldots, M)$, although we have shown before that there is significant heterogeneity within each region.

iii) *difference-in-difference with interaction*

$$JFlow_{sic} = \beta_0 + \beta_1 USJFlow_{si} + \beta_2 \left(USJflow_{si} Regulation_c\right) + \sum_{c=1}^{C} \gamma_c D_c + \epsilon_{sic} \tag{3.4}$$

Here we examine whether the difference in industry/size job flows between high and low volatility industry/size cells is smaller in highly regulated countries compared to the U.S. benchmark. By including the regulatory variable only in interaction with the U.S. job flow measure, we can control for unobserved country fixed effects.

The multivariate version of this specification, in which we consider more than one regulatory variable together, can be written as follows:

$$JFlow_{sic} = \beta_0 + \beta_1 USJFlow_{si} + \sum_{k=1}^{K} \beta_{2,k} \left(USJflow_{si} Regulation_{c,k}\right) + \sum_{c=1}^{C} \gamma_c D_c + \epsilon_{sic} \tag{3.5}$$

where $k = 1, \ldots, K$ is the number of regulatory variables used.

The measure of job flows used in the empirical analysis is the sum of job creation and job destruction rates (*sum*). In Appendix G, we also report the same specifications discussed above for excess job reallocation, i.e. the difference between the sum and the (absolute value of) net employment change. As shown in Appendix G, the results are largely unaffected by the use of this alternative measure of job flows.

All our variables are time averages over the available annual observations. The sample is unbalanced and covers fewer years for some countries than others (see Table 3.1). Time averaging allows us to reduce the possible impact of business cycle fluctuations in the years for which we have the data and the possibility that such fluctuations were not synchronized (and thus could be captured by common time dummies). We also consider two sample periods: 1) 1989 to 2001, and 2) the same sample for OECD and Latin American countries and the sample from 1996 onwards for the transition economies. The choice of the second sub-sample for the transition economies is motivated by two interrelated factors. First and as discussed in the previous section, the initial years of the transition process (1991-1995) were characterized by unprecedented reallocation of labor - and other factors of production - across industries, firms and locations. The magnitude and direction of the observed flows were only temporary and, indeed, job flows declined towards the standard of the OECD countries, and also became more balanced within each industry/size cell. Second, the early years of transition were characterized by major regulatory reforms to conform countries' institutional settings to those of market economies. For these two reasons, focusing on the second half of the 1990s for the transition economies is more appropriate in our comparative analysis of job flows.

3.5.2 Regulations in Labor and Product Markets

Before moving into the analysis of the empirical results, we briefly discuss our regulatory indicators. We consider synthetic indicators of the stringency of regulations in the labor and product markets, as well as the degree of enforcement of laws and regulations. Our primary source for these is the "Economic Freedom of the World (EFW)" database (see Gwartney and Lawson [2004]). This has been developed under the auspices of the Canadian Fraser Institute with the aid of a worldwide network of economists and research institutes. In particular, we use indicators referring to hiring and firing practices, regulation of business activities and integrity of the legal system.

Despite other indicators available in the literature for developing and emerging economies (e.g., the World Bank Doing Business database), the EFW tracks changes in regulations over time and is thus more suitable for our analysis of job flows that have indeed been influenced by policy changes over the period covered by our data (see Table 3.11 for details on the regulatory variables).

The EFW indicator of hiring and firing restrictions is measured on a scale from 0 to 10, with 10 being the worst (most restrictive). The average of this indicator is the highest in transition countries (5.70), followed by the OECD sample (5.43) and Latin America (4.68). This synthetic indicator passes simple validation tests: for example, its correlation with a similar indicator of employment protection legislation developed by the OECD is 0.85, statistically significant at the 1 percent level.[11]

[11]We check the robustness of our results by using an alternative measure of employment protection legislation, the OECD EPL index. Since this measure is not available for Latin America and transition countries in the early 1990s, we augmented it in two ways. First, for transition

Table 3.11: Institutional Variables, 1990s

Variable	Mean	Std. Dev.	Min	Max
OVERALL				
Hiring and Firing Practices	5.261	1.515	2.878	7.700
Law&Order adj. Hiring and Firing Practices	4.113	2.019	0.000	7.209
Business Regulations	3.490	1.389	1.100	5.900
Law&Order adj. Business Regulations	2.490	1.233	0.000	4.600
Law and Order	2.280	2.818	0.000	10.000
EU & USA				
Hiring and Firing Practices	5.427	1.804	2.878	7.400
Law&Order adj. Hiring and Firing Practices	5.084	1.559	2.878	6.600
Business Regulations	3.074	1.682	1.100	5.600
Law&Order adj. Business Regulations	2.822	1.349	1.100	4.600
Law and Order	0.469	1.121	0.000	3.000
LAC				
Hiring and Firing Restrictions	4.679	0.943	3.230	5.740
Law&Order adj. Hiring and Firing Restrictions	2.249	1.642	0.000	4.431
Business Regulations	4.206	1.297	2.617	5.900
Law&Order adj. Business Regulations	1.811	1.321	0.000	3.320
Law and Order	4.949	2.769	2.280	10.000
TRANSITION				
Hiring and Firing Restrictions	5.696	1.705	3.586	7.700
Law&Order adj. Hiring and Firing Restrictions	4.742	1.846	3.079	7.209
Business Regulations	3.323	0.669	2.650	4.200
Law&Order adj. Business Regulations	2.757	0.716	1.776	3.486
Law and Order	1.763	1.119	0.637	3.300

Source: Own calculations based on harmonized firm-level database and Gwartney and Lawson [2004].

In the sensitivity analysis, we also consider an EFW synthetic indicator of regulations in the product market. Regulations affecting markets for goods and services have a strong impact on the degree of competition and the pace and effectiveness of reallocation of resources, including labor. Thus, more restrictive regulations that stifle product market competition are also likely to influence job flows. The business regulation indicator is a simple average of five different indicators: price controls;

countries we used data on EPL collected by Haltiwanger et al. [2003]. Second, for Latin America we imputed EPL by regressing a measure of hiring and firing practices from the Fraser Institute on EPL for transition and OECD countries and then using the estimated coefficient to calculate EPL. EPL is measured on a scale from 0 to 4, with 4 being the worst (most restrictive). It is on average the strictest in OECD (2.35) and the least strict in Latin America (1.73).

administrative conditions and new business; time with government bureaucracy; starting a new business; and irregular payments. These five indicators are designed to identify the extent to which regulatory restraints and bureaucratic procedures limit competition and the operation of goods and services markets. Business regulation is measured on a scale from 0 to 10, with 10 being the most restrictive. This indicator is on average the highest in Latin America (4.21), followed by transition countries (3.32) and OECD (3.07).

The EFW indicator of law and order is measured on a scale from 0 to 10, with 10 being the worst. The average of this indicator is highest in Latin America (4.95), followed by transition countries (1.76) and the OECD sample (0.47). Appendix F contains more detailed definitions of the variables used in our analysis.

3.5.3 The Baseline Specification

In our empirical investigation, we start with a baseline specification in which we only include the U.S. job flow benchmark and the country dummies (equation (3.2). We then test for differences in the estimated coefficient of the U.S. job flow benchmark across the three regions for which we have data (OECD, Latin America and transition economies). Further, we allow the coefficient of the U.S. job flow variable to vary by firm size class.

Table 3.12 presents the results for these three alternative specifications and for the two samples discussed above (1989-2001 for all countries, and restricted to 1996-2001 for transition economies). As expected, the estimated coefficient of the U.S.

job flow is highly significant, confirming the bivariate correlation analysis discussed above. However, the estimated coefficient is significantly less than one, suggesting that, other things being equal, the responsiveness to market and technologically driven factors that affect reallocation in the U.S. is less than one. This finding is interesting by itself since it suggests that market driven and technological factors are not perfectly correlated across countries. Or put differently, it is consistent with the view that countries around the world have factors that impede the reallocation process.[12]

If we then allow the coefficient on the U.S. job flow to vary by region (EU, Latin America and transition economies), we notice that there is a closer link between cross industry/size differences in gross job flows between the United States and the Latin American countries than between the United States and the European countries. If we restrict the analysis to the 1996-2001 period for the transition economies, we see that the estimated coefficient on U.S. job flows (column (5)) declines to a level that is not statistically different from that of the EU countries. In other words, as the process of economic transformation has progressed, the patterns of cross-industry/size job flows in transition economies have diverged from those in the U.S.

The next step in our preliminary analysis is to differentiate the coefficient on the U.S. job flow by firm size. Perhaps not surprisingly, we find that the coefficient is the highest for the smallest size class (1-19 employees) and declines monotonically for the larger size classes. In other words, the patterns of cross industry job flows in the

[12] Appropriate caution needs to be used in interpreting the magnitude of the coefficient since measurement error can drive the coefficient below one. Still, we find it interesting that this coefficient is, in general, less than one, and that the pattern of variation in the magnitude of this coefficient across regions and size classes is consistent with our interpretation.

United States and other countries are more similar among small firms than among larger firms, possibly because small firms are exempted from certain regulations and/or can more easily avoid other regulations. Hence, small firms show patterns of cross-industry job flows more similar to those in the U.S. - the least regulated economy. For larger firms, regulations are likely to be more binding, especially in those industries that are inherently more volatile.

3.5.4 Regulations and Job Flows

The next step in our analysis is to look at the possible impact that labor regulations have on observed job flows (Table 3.13). We focus on the restricted sample for the transition economies as discussed above. The first specification (column (1)) is a simple cross-country estimate in which we include the U.S. job flow benchmark and the labor regulation indicator, but we do not interact the latter with the U.S. benchmark. These results are only preliminary, not least given the possible omitted variable bias due to the exclusion of country fixed effects. The estimated coefficient of the synthetic indicator of the stringency of hiring and firing regulations is negative and statistically significant at the 1 percent level. This result is largely unchanged if we allow the coefficient on the U.S. job flow benchmark to vary across the three regions (column (2)).

Table 3.12: Job Flows - A Baseline Difference-in-Difference Analysis

	1990s			**1990s, transition late 1990s**		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-0.0348***	0.0004	0.0524***	0.1153***	0.1376***	0.1810***
	[0.0100]	[0.0118]	[0.0118]	[0.0095]	[0.0109]	[0.0107]
USA SUM	0.7097***			0.6621***		
	[0.0183]			[0.0173]		
USA SUM *EU		0.5860***			0.5746***	
		[0.0288]			[0.0274]	
USA SUM *Transition		0.8282***			0.6878***	
		[0.0325]			[0.0308]	
USA SUM *LAC		0.7493***			0.7493***	
		[0.0329]			[0.0312]	
USA SUM *<20 Workers			0.5628***			0.5385***
			[0.0227]			[0.0215]
USA SUM *20-49 Workers			0.3975***			0.3875***
			[0.0317]			[0.0301]
USA SUM *50-99 Workers			0.3157***			0.3169***
			[0.0351]			[0.0333]
USA SUM *100+ Workers			0.1764***			0.2090***
			[0.0566]			[0.0537]
Observations	935	935	935	940	940	940
Adjusted R-squared	0.69	0.70	0.74	0.69	0.69	0.73

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. All regressions include country dummies. USA SUM: industry/size job reallocation in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America.

Source: Own calculations based on harmonized firm-level database.

Table 3.13: Job Flows and the Role of Labor Regulations (Difference-in-Difference Analysis)

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.1815***	0.2062***	0.0930***	0.0360***	0.0016	0.0513***
	[0.0341]	[0.0354]	[0.0290]	[0.0138]	[0.0100]	[0.0140]
USA SUM	0.6588***		0.8417***	0.7047***	0.8602***	0.8541***
	[0.0426]		[0.2010]	[0.0835]	[0.1016]	[0.0490]
USA SUM *EU		0.5660***				
		[0.0390]				
USA SUM *Transition		0.6876***				
		[0.0466]				
USA SUM *LAC		0.7501***				
		[0.1050]				
EPL	-0.0191***	-0.0190***				
	[0.0042]	[0.0042]				
USA SUM *EPL			-0.032			
			[0.0311]			
USA SUM *EPL (Adj)					-0.0452**	
					[0.0182]	
USA SUM *EPL *EU				-0.0211		
				[0.0138]		
USA SUM *EPL (Adj) *EU						-0.0484***
						[0.0097]
USA SUM *EPL *Transition				-0.0057		
				[0.0146]		
USA SUM *EPL (Adj) *Transition						-0.0361***
						[0.0113]

Continued on next page.

Table 3.13: Job Flows and the Role of Labor Regulations (Difference-in-Difference Analysis) (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
USA SUM *EPL *LAC				0.0127		
				[0.0182]		
USA SUM *EPL (Adj) *LAC						-0.0450**
						[0.0183]
Observations	940	940	940	940	940	940
Adjusted R-squared	0.55	0.56	0.69	0.69	0.69	0.69

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. Columns (1) and (2) include region dummies. Columns (3)-(6) include country dummies. USA SUM: industry/size job reallocation in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America. EPL is the index of stringency of hiring and firing regulations. EPL (Adj) is the indicator of hiring and firing adjusted to take into account different degrees of enforcement of regulations (see main text).

Source: Own calculations based on harmonized firm-level database.

The next step is moving to the difference-in-difference analysis by focusing on the variation of job flows across industry/size classes within each country. Column (3) presents the basic model with the U.S. job flow benchmark and its interaction with the hiring and firing labor regulation variable, plus country fixed effects (as in equation (3.4) above). We find that the interaction term is negatively signed but not statistically significant at the conventional level. This result holds even if we differentiate the effect of labor regulations by region.

Previous research (see, e.g., Caballero et al. [2004], Heckman and Pages [2004]) suggests that the degree of enforcement of labor regulations - as well as other regulations - varies across our sample of countries that include the OECD, Latin American and transition economies. Not only are some firms and jobs not registered in Latin America and increasingly in the transition economies and some Southern European countries, registered firms may also not fully comply with the existing rules and regulations. As an indication of the different degree of enforcement of laws and regulations, we consider the law and order indicator from the Fraser Institute (based on the Political Risk Component I (Law and Order) from the International Country Risk Guide, from 0 to 10, 10 being the worst).[13] The indicator shows the highest compliance with laws and regulations in the OECD sample of countries (average of 0.56), followed with the transition economies (average of 1.76) and by the Latin American countries (average of 4.96).

To control for possibly differing degrees of enforcement of laws and regulations

[13]Micco and Pages [2004] also make an attempt at controlling for different degrees of enforcement of regulations by using an indicator of rules of laws and government effectiveness (see Kaufmann et al. [2004]). We used the Fraser index of law and order because it is available for the time period for which our job flows data are available for the different countries.

we adjust our regulatory variable as follows (R is the regulatory variable):[14]

$$R_{c,adj} = \left(1 - \frac{Law\&Order}{10}\right) \cdot R_c \tag{3.6}$$

Columns (5) and (6) in Table 3.13 show the estimated effect of the interaction between the U.S. job flow and the adjusted labor regulation variable without and with differentiation by region. It is indeed noticeable that, once we control for the difference in the degree of enforcement across countries, the interaction between hiring and firing regulations and U.S. job flows becomes strongly significant overall (column (5)), and in each of the sub regions (column (6)) when we allow the coefficient of the interaction to vary. In other words, once we control for enforcement, we find that intrinsically more volatile industries and size classes present lower levels of gross job turnover relative to the less volatile industries and size classes in countries with more stringent hiring and firing regulations. It is also interesting to notice that once we control for the enforcement of labor regulations, the estimated coefficient of the technology variable (the U.S. job flow benchmark) is closer to unity. Thus, a significant fraction of less than perfect correlation in the magnitude of job flows in the countries in the sample with the United States can be explained by restrictive labor regulations that raise labor adjustment costs.

How sizeable is the estimated impact of labor regulation on job flows? Given our estimation approach, we consider the effect of labor regulations in reducing job reallocation between two industries at the extremes of the labor flexibility require-

[14]There is no indication in Gwartney and Lawson [2004] that the original regulatory variables consider the enforcement of regulations in addition to the statutes.

ment. Using the coefficient on the interaction term in column (5) of Table 3.13, we estimate that the difference in job reallocation between industry/size cells with a high flexibility requirement (90^{th} percentile of the flexibility distribution in the United States) and industry/size cells with a low flexibility requirement (10^{th} percentile of the same distribution) will be 4.5 percentage points lower in a country with the highest index of hiring and firing regulations compared to the United States, the country with the least restrictive regulations. Considering that the average job reallocation rate is around 25 percent in the sample used in the regression, the estimated impact is indeed sizeable.[15]

3.5.5 The Differential Effects of Regulations on Small and Large Firms

The next step in our analysis is to look at the possibly different effect of labor regulations on job flows of firms of different sizes. Table 3.14 presents regressions in which we estimate the coefficient on the interaction between the benchmark U.S. job flow and the hiring and firing regulatory indicator for firms of different sizes. Column (1) considers the hiring and firing indicator without controlling for the different degree of enforcement of laws and regulations. Interestingly, once the in-

[15] The estimated value is obtained as follows:

$$\beta\,[(USJflow_{90^{th}} - USJflow_{10^{th}})\,(HF_{max} - HF_{min})]$$

where β is the estimated coefficient, and $USJflow$ and HF are the job reallocation in the United States and the indicator of hiring and firing regulations corrected for the degree of enforcement, respectively. Micco and Pages [2004], using a similar approach, estimated an impact of 5.7 percentage point. Their country sample and period of observation were different from ours but the results are close.

teraction effect is allowed to vary across firm size classes, the estimated effect is negatively signed and statistically significant at the conventional level for all size classes. Moreover, the estimated impact of stringent regulations on the variance of job flows across industries increases with firm size. As hypothesized above, smaller firms are often either exempt from certain regulations or can more easily stay below the radar screen of regulators and law enforcement authorities. The estimated negative impact of labor regulations on job flows is almost twice as strong in large firms (more than 100 employees) compared to micro units (fewer than 20 employees).

Column (2) of Table 3.14 presents a similar specification in which we control for the different degree of enforcement of regulations. Controlling for such effects yields larger coefficients and a larger magnitude of the impact of labor regulations on job flows. As in the previous case, the estimated effect of labor regulations increases with the size of firms.[16]

Appropriate care and caution is required to interpret the interaction effects estimated in Table 3.14 with respect to employer size. Recall that small businesses systematically have higher job reallocation rates than larger businesses in all countries including the U.S. benchmark. As such, the results in Table 3.13 imply that industry/size cells with a higher U.S. benchmark will have the flow reduced by labor market regulations that are enforced. For Table 3.14, this implies that in comparing coefficients across size class interactions, the magnitudes are comparable for a given U.S. benchmark rate. That is, the absolute effect is larger for large businesses than

[16]Also in this case, the results are robust to the use of the excess labor reallocation. See Appendix G for more details.

small businesses for a given U.S. benchmark rate. But given that small businesses have a higher U.S. benchmark rate this variation tends to work in the opposite direction.

The final step in our analysis is aimed at assessing the robustness of our results to the inclusion of regulations in the goods and services markets in our specification. As discussed above, regulations in different markets tend to be highly correlated, i.e. countries that impose strict rules of hiring and firing also tend to impose more restrictive regulations on the goods and services markets. There are also specific aspects of product market regulations that can influence job flows over and above labor regulations. For example, since a significant fraction of overall job flows is due to the entry and exit of firms, regulations affecting the start up of a new business, as well as bankruptcy rules that affect the exit of low performing units, may affect job flows. Likewise, regulations affecting price setting by firms and their relations with the public administration and their clients can all influence incentives for firms to expand, adopt new technologies and adjust their workforce.

Columns (3) and (4) of Table 3.14 show the results of estimating the job flow regressions controlling for our synthetic indicator of business regulations. We correct both labor and product market regulations by the degree of enforcement proxied by the law and order indicator. In column (3), we do not differentiate the interactions between U.S. reallocation and regulations by firm size, while we do so in the last column of the table. Including the interaction between product market regulations and U.S. job flows does not dramatically alter our results. Whether we differentiate the impact of regulations by firm size or not, the estimated effects of the interaction

Table 3.14: Job Flows by Firm Size - the Role of Labor and Product Market Regulations (Difference-in-Difference Analysis)

	(1)	(2)	(3)	(4)
Constant	0.1225***	0.0753***	0.1150***	0.0660***
	[0.0126]	[0.0131]	[0.0109]	[0.0147]
USA SUM	0.8379***	0.8579***	0.8401***	0.8371***
	[0.0700]	[0.0409]	[0.0988]	[0.0435]
USA SUM *EPL (Adj)			-0.0546**	
			[0.0203]	
USA SUM *EPL *<20 workers	-0.0499***			
	[0.0124]			
USA SUM *EPL (Adj) *<20 workers		-0.0632***		-0.0540***
		[0.0090]		[0.0139]
USA SUM *EPL *20-49 Workers	-0.0739***			
	[0.0129]			
USA SUM *EPL (Adj) *20-49 Workers		-0.0895***		-0.0649***
		[0.0100]		[0.0188]
USA SUM *EPL *50-99 Workers	-0.0853***			
	[0.0131]			
USA SUM *EPL (Adj) *50-99 Workers		-0.1012***		-0.0793***
		[0.0104]		[0.0206]
USA SUM *EPL *100+ Workers	-0.0997***			
	[0.0148]			
USA SUM *EPL (Adj) *100+ Workers		-0.1140***		-0.0537*
		[0.0133]		[0.0319]
USA SUM *Bus. Reg. (Adj)			0.0235	
			[0.0255]	
USA SUM *Bus. Reg. (Adj) *<20 Workers				-0.0096
				[0.0225]
USA SUM *Bus. Reg. (Adj) *20-49 Workers				-0.037
				[0.0309]
USA SUM *Bus. Reg. (Adj) *50-99 Workers				-0.0321
				[0.0338]
USA SUM *Bus. Reg. (Adj) *100+Workers				-0.1003*
				[0.0530]
Observations	940	940	940	940
Adjusted R-squared	0.73	0.73	0.69	0.73

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. All regressions include country dummies. USA SUM: industry/size job reallocation in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America. EPL is the index of stringency of hiring and firing regulations. EPL (Adj) is the indicator of hiring and firing adjusted to take into account different degrees of enforcement of regulations (see main text). Bus. Reg. is the indicator of the stringency of business regulations. Bus. Reg. (Adj) is the same indicator adjusted to take into account different degrees of enforcement of regulations.

Source: Own calculations based on harmonized firm-level database.

between U.S. job reallocation and labor regulations remain negatively signed and highly statistically significant, while the coefficients on the product market regulations are generally not statistically significant. However, once we differentiate effects by firm size, we notice that the only statistically significant effect of product market regulations is among large businesses (greater than 100 employees). Moreover, controlling for product market regulations reduces the estimated impact of labor regulations for those firms. In other words, for large firms product market regulations play an important role in curbing labor reallocation over and above labor regulations. Intermediate firms (those in between 20 and 99 employees) seem to be the most adversely affected by stringent labor regulations that raise labor adjustment costs. In terms of magnitude, note that stringent labor market regulation is associated with a 0.5 percent drop in job reallocation for micro, small and medium firms, and a 0.2 percent drop in job reallocation for large firms. Stringent product market regulation, on the other hand, has the largest impact on job reallocation by large firms: it is associated with a 0.4 percent drop.[17]

3.5.6 Do regulations influence the various margins of labor reallocation differently?

So far we have focused on the effects of regulations in labor and product markets on overall job reallocation. In this section we want to explore whether such regulations have a different impact on the different margins of reallocation,

[17]We obtain these magnitudes by calculating the derivative of the coefficient with respect to enforcement adjusted regulatory variables and dividing by the standard deviation of gross job reallocation.

namely on job flows due to the entry and exit of firms in the market and those due to reallocation among incumbents (see Table 3.15).[18] Column (1) shows that the cross industry/size patterns of job reallocation by entering and exiting firms in the Latin American countries (see column (1) of Table 3.14) are very similar to those observed in the United States. This drives the result observed without distinguishing between different types of firms. Indeed, the coefficient on U.S. job reallocation for continuing firms in Latin America is only half of that for entering and exiting firms (see column (2) of Table 3.15). For the EU and transition countries, there are no significant differences in their cross-industry/size patterns of job flows between entering/exiting and continuing firms compared to those in the U.S.

Column (3) of Table 3.15 shows the results of estimating the job flow regressions for entering and exiting firms, controlling for labor and product market regulations corrected by the degree of enforcement and differentiating the impact of both by firm size, whereas column (4) does the same for continuing firms. The results suggest a negative and statistically significant effect of labor market regulation (interacted with U.S. job reallocation) on labor mobility generated by entering and exiting firms for all but large firms. The coefficients are also more than twice as large in magnitude as the corresponding coefficients in column (4) of Table 3.14, and they are about the same magnitude for micro, small and medium entering and exiting firms. However, in order to correctly assess the magnitude of the impact,

[18]We focus on the combined flows due to entry and exit of firms because of the very high correlations between entry and exit across industries in most countries. This, in turn suggests that entries and exits are largely part of a creative destruction process in which entry and exit reflects within sector reallocation reflecting idiosyncratic differences across firms within sectors (see Bartelsman et al. [2004b] for evidence based on the same dataset used in this paper, as well as Geroski [1991], Baldwin and Gorecki [1991]).

Table 3.15: Job Flows by Firm Size, Entering, Exiting and Continuing Firms - the Role of Labor and Product Market Regulations (Difference-in-Difference Analysis)

	Entry & Exit	Continuers	Entry & Exit	Continuers
	(1)	(2)	(3)	(4)
Constant	-0.0074	0.0241**	0.0232***	0.0610***
	[0.0054]	[0.0094]	[0.0058]	[0.0116]
USA SUM			1.0809***	0.4742***
			[0.0454]	[0.0615]
USA SUM *EU	0.5730***	0.5118***		
	[0.0307]	[0.0372]		
USA SUM *Transition	0.6835***	0.6133***		
	[0.0345]	[0.0418]		
USA SUM *LAC	0.9982***	0.4942***		
	[0.0341]	[0.0427]		
USA SUM *			-0.1542***	-0.0018
EPL (Adj) *<20 workers			[0.0137]	[0.0179]
USA SUM *			-0.1483***	-0.0418*
EPL (Adj) *20-49 Workers			[0.0212]	[0.0219]
USA SUM *			-0.1636***	-0.0557**
EPL (Adj) *50-99 Workers			[0.0277]	[0.0226]
USA SUM *			-0.1148	-0.0722**
EPL (Adj) *100+ Workers			[0.0738]	[0.0304]
USA SUM *			0.1010***	0.0007
Bus. Reg. (Adj) *<20 Workers			[0.0220]	[0.0288]
USA SUM *			0.0034	0.0404
Bus. Reg. (Adj) *20-49 Workers			[0.0347]	[0.0357]
USA SUM *			-0.0208	0.0546
Bus. Reg. (Adj) *50-99 Workers			[0.0450]	[0.0368]
USA SUM *			-0.2452**	0.0599
Bus. Reg. (Adj) *100+Workers			[0.1205]	[0.0504]
Observations	946	934	946	934
Adjusted R-squared	0.69	0.55	0.75	0.58

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. All regressions include country dummies. USA SUM (Entry & Exit): industry/size job reallocation due to entering and exiting firms in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America. EPL is the index of stringency of hiring and firing regulations. EPL (Adj) is the indicator of hiring and firing adjusted to take into account different degrees of enforcement of regulations (see main text). Bus. Reg. is the indicator of the stringency of business regulations. Bus. Reg. (Adj) is the same indicator adjusted to take into account different degrees of enforcement of regulations.

Source: Own calculations based on harmonized firm-level database.

we need to remember that the magnitude of job reallocation varies significantly by size class. Taking that into account, note that stringent labor market regulation has the biggest impact on job reallocation by micro entering and exiting firms: it is associated with a 0.6 percent drop in job reallocation by such firms. The impact on small, medium and large firms is lower: stringent labor market regulation is associated with a 0.3, 0.3 and 0.1 percent drop in job reallocation, respectively.

The estimated effects of product market regulation (interacted with U.S. job reallocation) on job flows by entering and exiting firms is not significant for small and medium firms, and is negative and significant for large firms, while it is surprisingly positive for micro firms. The opposite effect of product market regulation on small and large firms is consistent with the idea that regulations tend to promote labor adjustment among small firms but reduce adjustment among large ones. For continuing firms, labor market regulation is more important than product market regulation, as the results in column (4) of demonstrate. The coefficients are smaller in magnitude than the ones in column (4) of Table 3.14, but the basic result holds: the estimated impact of stringent regulations on the variance of job flows across industries increases with firm size. Stringent labor market regulation is associated with a 0.2 percent drop in job reallocation by continuing large, medium and small firms.[19]

These results confirm the importance of labor market regulations in shaping labor adjustment patterns, particularly so in those industries and size classes where technological and market factors require more frequent employment changes. However, controlling for other regulations influencing firm behavior also influences job flows. In addition, labor market regulations are especially important for entering

[19] Impact is the largest for medium firms, followed by large and small firms, but it is rounded to 0.2 percent for all of them.

and exiting firms, especially for micro, small and medium firms, which presumably face more hardship in adjusting to changing market conditions (for example, demand) than large firms and find labor market regulations (such as firing costs) too restrictive. Even though small firms are often either exempt from certain regulations or can more easily stay below the radar of regulators, this appears to be easier for continuing small firms than for entering or exiting small firms.

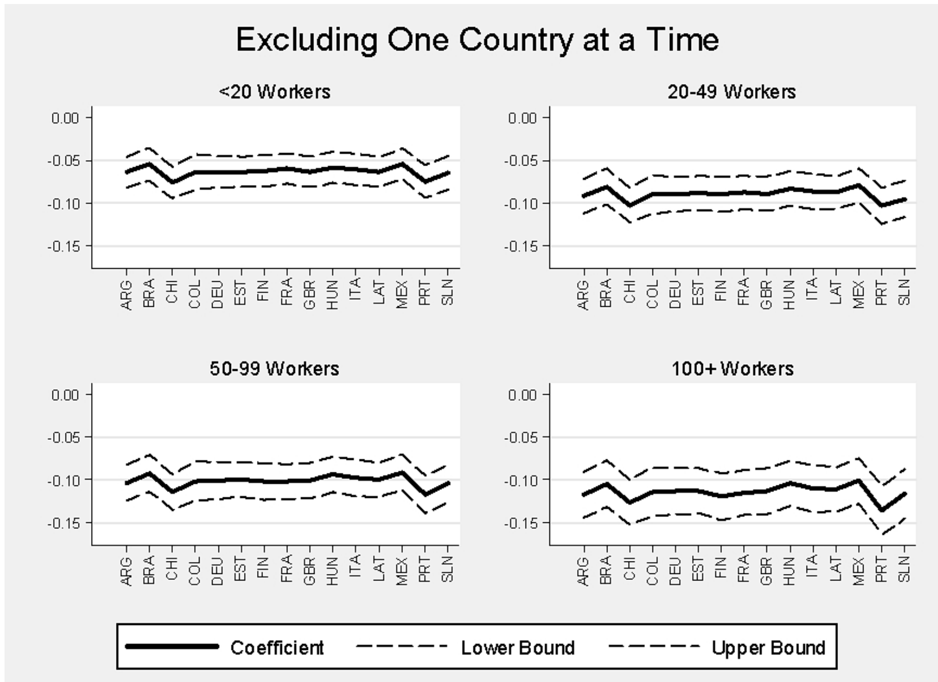
3.5.7 Sensitivity Analysis

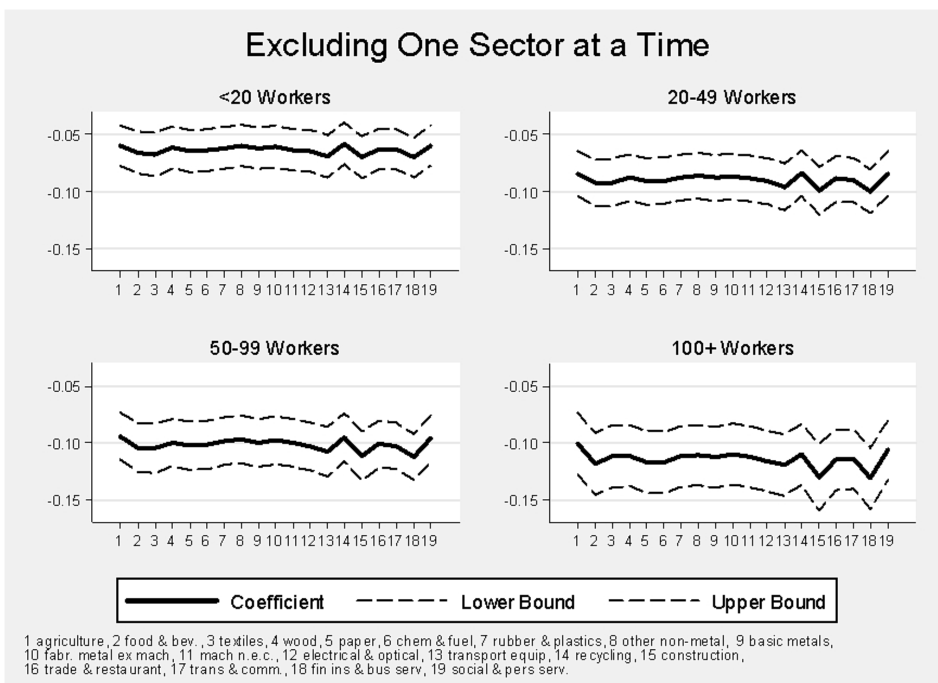
In the empirical analysis, we control for country, industry and size effects, as well as for unobservable effects using a difference-in-difference approach. Moreover, we test the robustness of results for hiring and firing regulations by including other regulatory variables. However, the use of quasi panel data may still run the risk that results are driven by the inclusion of a specific country or industry in the sample that drives the results in a given direction. The use of an unbalanced panel on the industry dimension makes this risk potentially more serious.

To test for the robustness of results to changes in the sample, we re-estimate our two preferred specifications - columns (2) and (4) in Table 3.14 - removing one country or one industry at a time from the sample. Figures 3.3 and 3.4 present the estimated coefficients on enforcement-adjusted hiring and firing regulations interacted with job reallocation in the United States, differentiated by size classes, in the specification without and with control for business regulations.

The results show a remarkable stability of the estimated coefficient for the

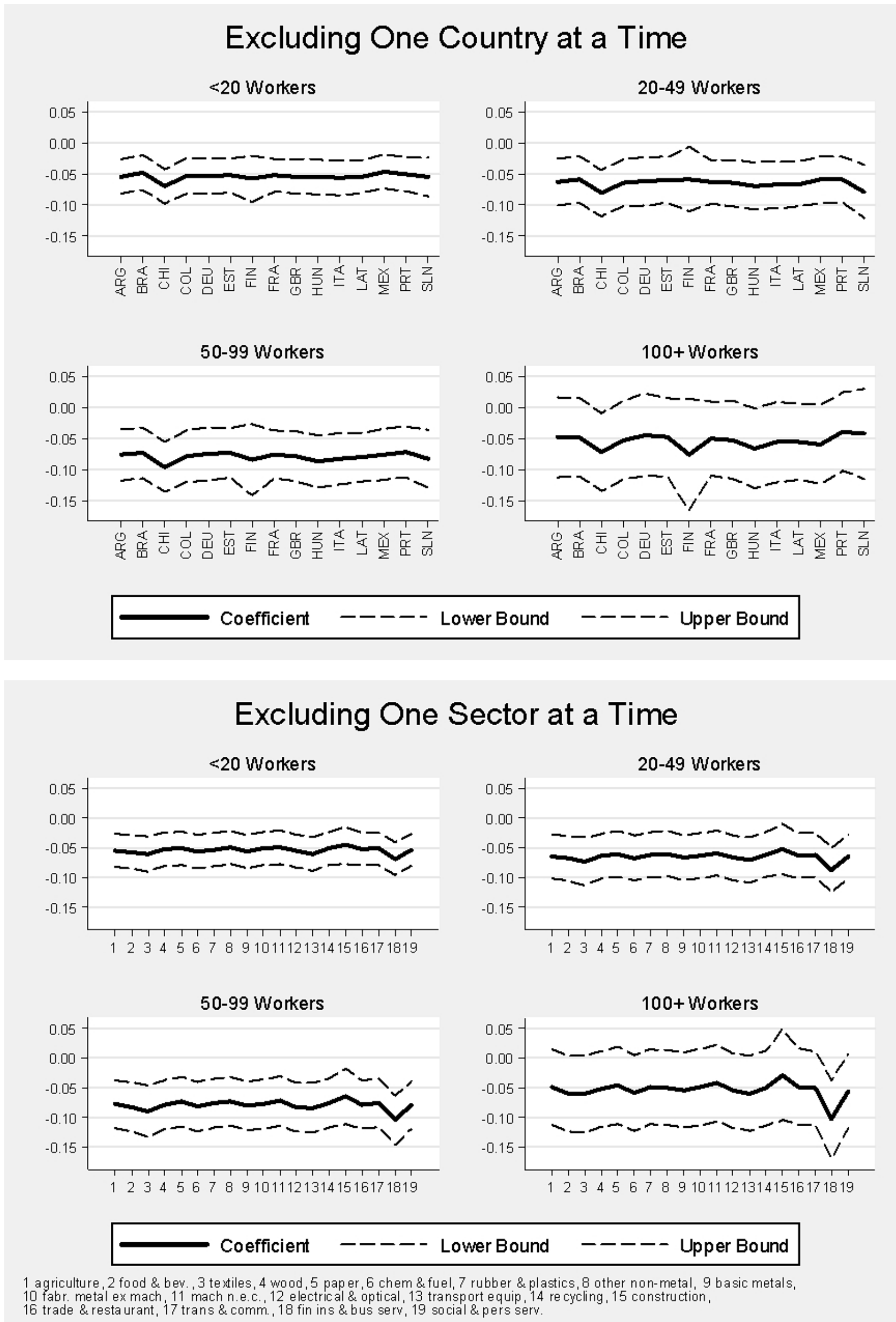
Figure 3.3: Sensitivity Analysis: Estimated Coefficient on Enforcement Adjusted Hiring and Firing Regulations Interacted with U.S. Job Reallocation and 95% Confidence Intervals, Excluding One Country or One Sector at a Time, Labor Market Regulations (Column (2) from Table 3.14)





Source: Own calculations based on harmonized firm-level database.

Figure 3.4: Sensitivity Analysis: Estimated Coefficient on Enforcement Adjusted Hiring and Firing Regulations Interacted with U.S. Job Reallocation and 95% Confidence Intervals, Excluding One Country or One Sector at a Time, Labor and Product Market Regulations (Column (4) from Table 3.14)



Source: Own calculations based on harmonized firm-level database.

interaction term to changes in the sample along the country or the industry dimension. The point coefficient estimates for the interaction term are always negatively statistically significant. The most sensitive coefficients are those for the largest size class - 100 or more employees - where the exclusion of Portugal or Chile leads to a stronger estimated effect of regulations. Not surprisingly given the unbalanced nature of the sample, the exclusion of finance and business activities as well as construction tend to strengthen the estimated negative effects of regulations on job reallocation.

3.6 Conclusion

This chapter exploits a rich new database with harmonized data on job flows that vary by country, industry and size class. We find that all countries in our sample exhibit sizeable annual gross job flows. Industry and size class effects together account for a very large share of the overall variability in job flows across country, industry and size class cells (e.g., over 50 percent of the variation in the summary measure of job reallocation is accounted for by industry and size effects interacted together). Interestingly, the most important factor here is employer size. Small businesses exhibit a substantially higher pace of job creation and destruction and this pattern is pervasive across industries and countries. Moreover, industry effects play a large role as well. Taken together, it is clear that some form of technology, cost and demand factors that are common across countries account for the bulk of the variation in job flows. Nevertheless, even after controlling for industry/technology and size factors, there remain significant differences in job flows across countries that could reflect differences in business environment conditions.

Our harmonized firm-level dataset for a sample of 16 industrial and emerging economies over the past decade allows us to look at one of the factors shaping the business environment - regulations on the hiring and firing of workers. To minimize the possible endogeneity and omitted variable problems associated with cross-country regressions, we use a difference-in-difference approach. The empirical results suggest that stringent hiring and firing regulations (and their consistent enforcement) reduce job turnover, especially in industry and size class cells that inherently exhibit more job turnover. To capture the latter, we use the United States patterns as a benchmark to identify and quantify industry/size class cells with inherently higher job turnover. Regulations also distort the patterns of flows across industry and size classes within a country. Interestingly, even though medium and large firms have lower average flows, holding the magnitude of the U.S. benchmark rates constant, medium and large firms are more severely affected by stringent labor regulations within a country. Small firms are less affected (for a given pace of reallocation in the U.S. benchmark), probably because they are in some cases exempt from such regulations or can more easily circumvent them. It is also interesting to point out that both labor and product market regulations have different effects on the different margins of firm and job mobility. Thus, labor regulations effect disproportionally the entry and exit of firms and the associated job mobility, while stringent business regulations seem to affect more the entry and exit of larger businesses and the associated job reallocation.

Much work remains to be done to understand the implications of our findings. Our findings provide evidence that stringent labor regulations have an impact on reallocation dynamics. It is a much larger step to demonstrate that stringent labor regulations have an adverse impact on the efficient allocation of labor in a manner consistent with the predictions of Hopenhayn and Rogerson [1993]. To explore the latter, we need to measure not only reallocation but also productivity at the micro level. A number of studies have found that allocative efficiency is important for understanding differences across time, industries and countries in the level and growth of productivity (see, e.g., Foster et al. [2001] and Bartelsman et al. [2005]). Putting those findings together with those in this chapter certainly suggests that stringent labor market regulations may have an important adverse impact on allocative efficiency and in turn productivity levels and growth. However, much work (including additional data infrastructure development) is needed to bring all of the pieces together to explore these important issues.

Chapter 4

Impact of Changes in Wage Setting Policies on the Structure of Wages in Slovenia[1]

4.1 Introduction

It is a well established empirical fact that worker flows are large relative to job flows and that job flows underlie a big fraction of worker flows (see, e.g., Davis and Haltiwanger [1999]). However there are worker flows above and beyond those needed to accomodate job flows. Empirical evidence also shows that within-firm productivity growth accounts for the majority of aggregate productivity growth (see, e.g., Foster et al. [2001] and Bartelsman et al. [2005]). Putting these two empirical facts together, it is possible that within-firm productivity growth is driven by worker reallocation, which might reflect match quality turnover (i.e., workers leave firms because the jobs are not challenging enough for them or firms lay off workers because they cannot handle the job). Wage structure may induce workers to either stay with the firm or leave: if a match between a worker and the firm is bad, lower wage may induce the worker to leave.

Research on the structure of wages has a long history, but recent availability of comparable longitudinal micro-level datasets across countries has brought about

[1] This chapter draws heavily on a joint paper with John Haltiwanger and Milan Vodopivec with the same title.

a renaissance in both theoretical and empirical work on this subject (see, for example, Heckman et al. [2003]). Overviews of empirical work on differences in wages and changes in the wage structure can be found in Autor and Katz [1999] or, more recently, Autor et al. [2005]. In the 1940s and 1950s, empirical work on the wage structure focused on occupation and industry wage differentials. In the 1960s and 1970s, differences in wages by education and potential experience came to the forefront as a result of increased availability of micro-level datasets with information on earnings and worker characteristics. Mincer [1974] found that more educated workers have higher earnings and that wage-experience profiles are upward sloping and concave. Availability of longitudinal matched employer-employee data took this research further; Abowd and Kramarz [1999] have shown that it is important to use appropriate estimation techniques to eliminate biases resulting from omitting unobservable firm and person fixed effects from the specification.

Over the past decade or two, changes in the economic systems of transition countries provided an interesting "natural experiment" on the impact of institutional factors on the wage structure and its changes. Socialist governments constrained the labor supply mechanism: it was everyone's duty to work and jobs were provided for everyone; firing was not allowed. Moreover, "economy-wide wage rates were assigned for all classes of jobs" (Orazem and Vodopivec [1994], p. 1). The collapse of the command economy and subsequent transition to the market economy brought about significant changes in the wage structure. Existing studies on the wage structure in transition economies rely on sample micro-level data (for example, Münich et al. [2005] and Flanagan [1995] for the Czech Republic, Jones and Ilayperuma Simon

[2005] for Bulgaria, and Orazem and Vodopivec [2000] for Estonia and Slovenia) and focus mostly on a year or two prior to and after the beginning of transition. The above mentioned studies find that market forces become more important in the wage determination process and consequently that the wage structure changes as transition progresses.

However, transition did not end in the early 1990s for most of these countries. Indeed, there were significant changes in employment protection legislation, unemployment benefits, collective bargaining systems and union density in the late 1990s in most transition countries (see, for example, Haltiwanger et al. [2003]) and all of these could potentially have an impact on the wage structure. None of these studies covers the late 1990s. The only exception is Vodopivec [2004] for Slovenia, who uses a matched employer-employee dataset, but only estimates a standard Mincer [1974] model. In this chapter, we use the same dataset, covering the period from 1992 to 2000, and we extend the standard wage structure analysis by including firm and person fixed effects in the model. In addition, we exploit changes in the wage setting system in 1997 to compare the wage structure in 1992-1996 with the one in 1997-2000. We find that there were significant changes in the wage structure in the late 1990s, with the most educated groups gaining the most. We show that worker characteristics, either observable or unobservable, become more important in explaining the variance of real wages in the late 1990s, reflecting the change in the wage determination system.

The outline of this chapter is as follows. In Section 4.2, we describe the institutional background of wage setting in Slovenia. Data used are described in

Section 4.3. Estimation methods and results are discussed in Section 4.4. Section 4.5 concludes.

4.2 Institutional Background of Wage Setting[2]

The late 1980s and 1990s were a period of fundamental political and economic changes in Slovenia. These changes started in 1988, when Slovenia was still a part of Yugoslavia, with the Yugoslav *Law on Enterprises* that transferred decision-making rights from workers to equity owners, which resulted in significant changes in both employment and wage policies. Transition continued after Slovenia declared independence in 1991.

On the employment front, firms were given the right to lay off workers since 1988. However, large costs were associated with layoffs. One way for firms to reduce or eliminate these costs is the use of fixed-term contracts, which were introduced in the *Labor Code* of February 1991. These contracts were limited neither in the number of successive contracts nor in the maximum cumulated duration. The use of fixed-term contracts has increased over time. Another low- or no cost way to adjust labor is to induce workers to leave by giving them lower wages than they could get elsewhere. Despite allowing relatively liberal use of fixed-term contracts, Slovenian employment protection legislation has been among the most restrictive in Europe (see Haltiwanger et al. [2003], Riboud et al. [2002]).

Under self-management (pre-1988), the government set the firm's wage bill and the workers set individual wages within each firm. The objective was to even

[2]This section draws heavily on Haltiwanger and Vodopivec [2003].

out differences in wage pay among firms, as well as within firms.[3] As a result, Yugoslav firms had extremely compressed wage scales. This system was replaced by a three-component system in Slovenia in 1991, consisting of the *Labor Code*, collective bargaining, and incomes policy (Haltiwanger and Vodopivec [2003], p. 258).

The *Labor Code* of 1991 let wages be determined by employers within the framework set by collective agreements. The first general bargaining collective agreement was ratified in August 1990, and it was followed by other general and industry collective bargaining agreements.[4] General collective bargaining agreements prescribe the components of the wage (basic wage, wage supplements, supplements for individual success and supplements based on firm success) and determine fringe benefits (duration of vacation, reimbursement of transportation to work, meals, etc.). Wages and fringe benefits of managerial workers are normally set in individual contracts, which are much more flexible.

The largest component of a worker's pay is the basic wage, which is usually determined as a multiple of the minimum basic wage, set by the collective agreements. This basic wage depends on the category to which a worker belongs on the basis of his/her highest attained educational level. There are nine categories total, and the basic wage for the highest category was set at three times that of the lowest category since 1991, although some industry collective bargaining agreements set higher ratios. Table 4.1 shows these ratios for the general collective bargaining

[3]Haltiwanger and Vodopivec [2003] note that "the pay of the highest pair manager was 4.54 times that of the lowest paid worker" in a firm with more than one thousand workers.

[4]On December 31, 1996, there were 24 valid collective bargaining agreements (Pirš [1996]).

agreement and select industry collective bargaining agreements in 1997 (there were very few changes in these ratios during the period of our analysis). Until 1997, firms in bad financial standing had the right to reduce basic wage levels.

Incomes policies continued to be an important component of the wage setting system until 1997. In 1991, the law tied the growth of the wage bill to the growth of the cost of living and limited managerial salary to fifteen times the minimum wage (Orazem and Vodopivec [1994]). After 1992, the growth of the wage bill was agreed upon in collective bargaining agreements. Since 1997, the only limitation on wage growth was the requirement that the annual growth of pay based on individual contracts should be matched by the growth of the payroll of the workers covered by collective agreements.

The wage-setting system in Slovenia is quite rigorous and complicated, but it does allow firm- and worker-specific deviations from the wage guidelines set in collective bargaining agreements, should the success of the firm and/or worker warrant them. Given that the wage-setting policies changed in 1997 in two respects (firms in bad financial standing no longer had the right to reduce basic wage levels and limitations on the wage growth were significantly reduced), an interesting question is whether this had any impact on the structure of wages and the importance of the person characteristics (both observed and unobserved) and firm fixed effects. This is precisely what we attempt to answer in this chapter. But first, we describe the data used and its properties.

Table 4.1: Ratios for Basic Wage Scale in the General Collective Agreement and Select Industry Collective Agreements in 1997

Category		General	Paper	Coal	Electricity
1	Simple work (no training)	1.000	1.000	1.000	1.000
2	Less demanding work (short training, completed elementary education)	1.100	1.150	1.287	1.200
3	Medium demanding work (up to two year professional/vocational education)	1.230	1.300	1.406	1.320
4	Demanding work (up to two-and-a-half-year professional/vocational education)	1.370	1.450	1.582	1.550
5	More demanding work (3 years of professional/vocational education, with a foreman exam, or 4-5 years of such education)	1.550	1.700	1.771	1.750
6	Very demanding work (2 years of college level education)	1.850	2.200	2.154	2.120
7	Extremely demanding work (4-5 years of college level education)	2.100	2.600	2.170	2.740
8	Most demanding work (master degree)	2.500	3.300	3.650	3.950
9	Exceptionally important and most demanding work (doctorate)	3.000	3.800	4.347	4.500

Source: Pirš [2000].

4.3 Data Description

We use a matched employer-employee dataset, compiled from three unusually rich administrative databases covering virtually all Slovenian workforce participants and all business subjects. The first is a database on workers, containing employee characteristics, employment history and earnings information. The second is a firm-level database, containing business registry information. The third is a firm-level database with data from balance sheets and income and loss statements. Common identifiers allow us to combine the records from these three datasets. The resulting dataset covers the period from 1992 until 2000.[5]

4.3.1 Employee Dataset

Employee dataset is maintained by the Statistical Office of the Republic of Slovenia (SORS), as a part of the Statistical Register of Employment (SRDAP). It is based on the records from the Pension, Disability and Medical Insurance Register, augmented with: a) data from statistical surveys on the recipients of undergraduate and graduate degrees, b) data from the Central Population Register (CRP) at the Ministry of the Interior, and c) data from the Business Register of Slovenia (PRS), now maintained by the Agency of the Republic of Slovenia for Public Legal Records and Related Services (AJPES). Data are collected by the Health Insurance Institute of Slovenia (ZZZS) from firms or self-employed persons. Records in SRDAP are updated monthly: all the changes need to be reported within 8 days of their occurence.

[5] Data are also available for 2001, but the coverage is much worse than in previous years, so we exclude it from our analysis.

Data on the recipients of undergraduate and graduate degrees are obtained from the colleges and universities annually.

This dataset contains information on the start and end of employment (if applicable), employee and job characteristics (gender, birth year, level of education attained, level of professional training, type of shift, type of employment, vocation), start and end of the reference period of earnings, earnings, and hours worked in the reference period, including hours worked in overtime.

4.3.2 Employer Datasets

Business Register of Slovenia and firm-level accounting dataset are maintained by the Agency of the Republic of Slovenia for Public Legal Records and Related Services (AJPES) since July 15, 2002. Prior to that, they were maintained by SORS.

Firms are required to submit form PRS-1 at the time of their creation, change of their parameters, and their cessation, if applicable, by to Law on Enterprises. They are required by law to submit form PRS-2 at the time of the creation of an establishment within the firm, change in the establishment's parameters and its cessation, if applicable. According to the *Law on Enterprises*, all firms that are registered in any given year are supposed to provide balance sheets and income statements to the relevant government agency, regardless of whether they were in business the entire year or not.

Firm identification numbers are unique and are not recycled. However, they do not allow transparent tracking of mergers and acquisitions. This is possible with the additional information from the type of change.

Business register dataset includes information on the date of creation of the establishment, industry, municipality, ownership, origin of establishment's capital and date of the first entry to the registry. Date of creation was acquired from the current PRS online (`http://www.ajpes.si/prs`) for firms that are still active and were missing this information. Firm-level accounting dataset contains balance sheets and income statements for all businesses (excluding sole proprietors and the banking industry).

4.3.3 Sample Selection and Definition of Variables

The three datasets described above in principle cover all formal sector workers and firms in Slovenia, including sole proprietors,[6] which were excluded from the analysis, as were the workers employed by them. The reason for the exclusion is that sole proprietors in some cases appear to "adjust" reported earnings of their employees downward to reduce their old-age contributions.[7]

There were a number of workers that either held two jobs at the same time or switched employers during the year. When two jobs were held at the same time, we kept only their primary job, defined as the job in which they worked the most hours, either regular or overtime, and in which they were permanently employed. When a job switch occured during the year, we calculated the employment spell at

[6]We do not have accounting data for them, although they exist as well.

[7]These concerns were also mentioned by Orazem and Vodopivec [1994] as a reason for excluding self-employed and workers in private enterprises.

each job in that year and kept the observation with the highest earnings and the longest employment spell.

Our main question is whether the structure of wages changed during the period from 1992 to 2000. Since we have the information on hours worked during regular time and in overtime, we can calculate hourly wages. We define the nominal gross hourly wage of worker i in year t, nw_{it}, as:

$$nw_{it} = \frac{earn_{it}}{h_{R,it} + 1.3 * h_{O,it}}$$

where $earn_{it}$ is the sum of gross wage and salary income,[8] $h_{R,it}$ denotes the number of hours worked during regular time and $h_{O,it}$ denotes the number of hours worked in overtime. Since hours worked in overtime are paid 30 percent more than the hourly wage according to the general collective bargaining agreement, we use a factor of 1.3 to make the number of hours worked in overtime comparable to the number worked during regular time. [9]

Because monthly inflation rates were high especially during the first few years of our sample,[10] nominal earnings and prices in the first half of the year differed significantly from those in the last half of the year. We thus calculate average consumer price indices for the months in which earnings are observed, and then calculate real wages by deflating nominal wages with these indices.

[8]Sum of gross wage and salary income includes personal income tax (payed by employee), payroll tax (paid by employer) and social security contributions (paid by both employer and employee).

[9]This factor includes personal income tax (payed by employee), payroll tax (paid by employer) and social security contributions (paid by both employer and employee).

[10]In 1992, the annual inflation rate was 207 percent.

Education is measured as the highest attained level of education. Specifically, we form six groups: unfinished elementary school, finished elementary school, finished vocational school, finished high school, finished 2-year college and finished 4-year college, masters or doctorate. Potential experience ($EXPER$) is measured on the basis of the average number of years it takes to attain each level of education:

$$EXPER_{it} = \begin{cases} AGE_{it} - 15 & \text{if unfinished elementary school} \\ AGE_{it} - 16 & \text{if finished elementary school} \\ AGE_{it} - 18 & \text{if finished vocational school} \\ AGE_{it} - 19 & \text{if finished high school} \\ AGE_{it} - 22.5 & \text{if finished 2-year college} \\ AGE_{it} - 24.7 & \text{if finished 4-year college or more} \end{cases}$$

where AGE_{it} is age of worker i in year t.

Tenure associated with a job at employer k is calculated as the difference between year t and the year in which the employment spell at employer k started. Workers in our dataset are either in permanent or fixed-term employment, and we define a dummy variable equal to one if they have a fixed-term appointment and zero otherwise.

We also observe type of shift at the person level. There are five shift types: a) one 8-hour shift, b) two 8-hour shifts, c) three 8-hour shifts, d) four persons work at one job in 24 hours, and e) "turns" of 12 hours or more, followed by more than 24 hour break.

We excluded observations with exceptionally low or exceptionally high hourly wages and observations with more than 50 years of potential experience from the sample. We also excluded observations for which data on earnings, type of employment, type of shift and level of education attained were not available. The excluded observations accounted for less than two percent of the entire sample.

Table 4.2 presents the official figures for employment from the Statistical Office of Slovenia from 1992-2000, and compares them to the number of people employed in our sample; columns (1) and (4) are relevant for this comparison. Our sample covers 74.41 percent of all persons in paid employment in firms in 1992, the coverage increases to 97.33 percent in 1996 and then falls again to 74.58 percent by 2000. The sample covers the entire economy, with the exception of the banking industry.

Table 4.2: Persons in Paid Employment in Enterprises and Other Organizations and by Private Employers

	Persons in Paid Employment			
	(1)	(2)	(3)	(4)
Year	In Firms	By Sole Proprietors	Total	Sample
1992	658922	33157	692079	490312
1993	629016	36553	665568	506515
1994	605496	41840	647336	518377
1995	594394	47558	641952	531082
1996	581106	53545	634651	565618
1997	593086	58140	651226	564910
1998	591653	60827	652480	556836
1999	606928	64043	670971	545238
2000	615493	67549	683042	459035

Source: SORS and own calculations.

4.3.4 Summary Statistics

Some interesting facts emerge from the summary statistics in Table 4.3. Log real hourly wages have increased during the period from 1992 to 2000, but it seems that, contrary to expectations, their variation has not. On average, workers stayed in the same job for 3.4 years, but since the data is censored on December 31, 2000, tenure may be higher if we extended our analysis beyond this date. Overall, 84.1 percent of workers finished at most high school, with the number being higher in 1992-1996, and lower in 1997-2000. The share of workers with at least a 4-year college degree increased by 1.5 percentage points in 1997-2000 compared to 1992-1996. The shares of workers with unfinished or finished elementary school has decreased, indicating that the workforce was more educated at the end of 1990s than it was in the early 1990s.

It is also interesting to note two factors that could be interpreted as a result of adjusting to the strict labor market legislation: an increase in the share of workers who worked at least one hour in overtime[11] and an increase in the share of workers in fixed term appointments, rather than in permanent employment. In 1997-2000, the share of workers who worked at least one hour in overtime was 2.8 percentage points higher than in 1992-1996. More dramatically, the share of workers in fixed term appointments increased by 13.4 percentage points in 1997-2000, compared to 1992-1996.

[11] According to the general collective bargaining agreement, overtime work is limited to 20 hours per month and 180 hours per year.

Table 4.3: Summary Statistics

	1992-2000		1992-1996		1997-2000	
Variable	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Log(Real Hourly Wage)	5.6856	0.5059	5.6325	0.5032	5.7507	0.5017
Experience	17.9856	9.9833	17.8629	9.9701	18.1363	9.9974
Tenure	3.3787	2.7264	2.7011	1.9009	4.2112	3.2968
Tenure Squared	18.8489	25.8727	10.9093	13.7285	28.6029	32.9794
Elementary School	0.1779	0.3824	0.1802	0.3843	0.1751	0.3801
Vocational School	0.3110	0.4629	0.3089	0.4621	0.3135	0.4639
High School	0.2629	0.4402	0.2546	0.4357	0.2731	0.4456
2-Year University	0.0757	0.2645	0.0753	0.2639	0.0761	0.2652
4-Year University or more	0.0836	0.2768	0.0768	0.2662	0.0920	0.2890
Two 8-Hour Shifts	0.2205	0.4146	0.2236	0.4167	0.2167	0.4120
Three 8-Hour Shifts	0.0447	0.2066	0.0441	0.2054	0.0453	0.2080
4 persons work at one job in 24 hours	0.0098	0.0987	0.0096	0.0974	0.0101	0.1002
Turns of 12 hours or more, followed by more than 24 hour break	0.0173	0.1304	0.0199	0.1396	0.0141	0.1179
Fixed Term Appointment	0.2334	0.4230	0.1734	0.3786	0.3071	0.4613
Overtime	0.2788	0.4484	0.2664	0.4421	0.2940	0.4556
Incomplete Year	0.1966	0.3974	0.2071	0.4052	0.1838	0.3873
Number of observations	4737923		2611904		2126019	

Source: Own calculations based on matched employer-employee database.

Table 4.4: Number of Years in the Sample and Number of Firms Worked At (Number and Percentage of Observations)

Years	Firms 1	2	3	4	5	6	7	8	Total
1	100479								100479
	2.12								2.12
2	145454	34070							179524
	3.07	0.72							3.79
3	159900	72018	12612						244530
	3.37	1.52	0.27						5.16
4	239752	99540	30488	3852					373632
	5.06	2.10	0.64	0.08					7.89
5	345960	130150	45310	10125	1275				532820
	7.30	2.75	0.96	0.21	0.03				11.25
6	259356	151338	56670	16332	3150	312			487158
	5.47	3.19	1.20	0.34	0.07	0.01			10.28
7	301756	192689	74249	20860	4823	714	56		595147
	6.37	4.07	1.57	0.44	0.10	0.02	0.00		12.56
8	369848	271640	93296	24240	5320	1000	128	0	765472
	7.81	5.73	1.97	0.51	0.11	0.02	0.00	0.00	16.16
9	858699	441657	123048	29259	5328	963	171	36	1459161
	18.12	9.32	2.60	0.62	0.11	0.02	0.00	0.00	30.80
Total	2781204	1393102	435673	104668	19896	2989	355	36	4737923
	58.70	29.40	9.20	2.21	0.42	0.06	0.01	0.00	100.00

Source: Own calculations based on matched employer-employee database.

Table 4.4 tabulates the observations by the number of years a worker is in the sample and the number of firms he or she works at while in the sample. 30.80 percent of workers were in the sample during the entire period from 1992-2000, and 58.70 percent of workers never switched their employers. However, the remaining 41.30 percent of workers have worked for at least 2 or more employers during 1992-2000.

4.4 Empirical Analysis

4.4.1 The Estimation Model

To assess how the wage structure in Slovenia changed in the late 1990s compared to the early 1990s, we first estimate the standard Mincer [1974] model, with observable worker characteristics and controls only, which we will refer to as our baseline specification:

$$\ln w_{it} = \mathbf{X_{it}}\boldsymbol{\beta} + \mathbf{Z_{it}}\boldsymbol{\gamma} + \epsilon_{it} \tag{4.1}$$

where $\ln w_{it}$ is the natural logarithm of the real hourly wage of worker i at time t. $\mathbf{X_{it}}$ denotes a vector of observable individual characteristics, including an intercept, a set of dummy variables indicating different levels of formal education, years of potential work experience (and its square, triple and quadruple) and years of tenure (and its square). $\mathbf{Z_{it}}$ denotes a vector of controls, including a dummy variable indicating if the individual is in a permanent or a fixed-term contract position, a set of dummy variables for the type of shift, a dummy indicating whether the individual worked overtime, a dummy variable indicating if the individual worked less than a full year, and a set of annual dummy variables. These job-related circumstances could have an impact on the amount of workers' remuneration: for example, firms might be willing to offer a lower wage to workers employed on the basis of fixed-term contracts than to workers in permanent employment, even though both types of workers work in the same jobs and have comparable person characteristics.

However, specification in model (4.1) does not take into account firm-specific or industry-specific deviations from the wage guidelines. In order to control for these, we include either 2-digit industry or firm fixed effects in the model:

$$\ln w_{it} = \varphi_{J(i,t)} + \mathbf{X_{it}}\boldsymbol{\beta} + \mathbf{Z_{it}}\boldsymbol{\gamma} + \epsilon_{it} \tag{4.2}$$

where $\varphi_{J(i,t)}$ denotes either a 2-digit industry fixed effect or a firm fixed effect. We cannot include both industry and firm fixed effects in the model at once, because we defined 2-digit industry for each firm as the mode of 2-digit industry over 1992-2000. Note that in this model, $\mathbf{X_{it}}$ does not include an intercept, as it is absorbed in the industry or firm fixed effects.

Abowd et al. [2002] specify a statistical model that takes into account not only firm fixed effects (heterogeneity), but also person fixed effects:

$$\ln w_{it} = \theta_i + \varphi_{J(i,t)} + \mathbf{X_{it}}\boldsymbol{\beta} + \mathbf{Z_{it}}\boldsymbol{\gamma} + \epsilon_{it} \tag{4.3}$$

where θ_i denotes person fixed effects and $\mathbf{X_{it}}$ contains time-varying individual and/or firm characteristics. This vector does not include the level of education attained, which is now part of θ_i, and it does not include an intercept. Estimation of this model requires algorithms based on iterative conjugative gradient method to deal with the high dimensionality of the problem. Identification of person and firm fixed effects requires that some of the individuals in the sample switch employers, as it uses graph theory to determine groups of connected individuals and firms (Abowd et al. [2002], p. 3). As Table 4.4 illustrates, our sample fulfills this requirement.

We estimate models (4.1), (4.2) and (4.3) using the data from 1992-2000, and for the two sub-periods, 1992-1996 and 1997-2000. We also ask how much variation in log real hourly wages can be explained by the respective components in these three models: industry and firm fixed effects, person fixed effects (both observable and unobservable), time-varying person characteristics and controls (as applicable).

4.4.2 The Baseline Specification - Observable Worker Characteristics

We start with a baseline specification, the standard Mincer [1974] model of log real hourly wages. We estimate the model using the entire sample, and then we split the sample into two subsamples, 1992-1996 and 1997-2000. We also estimate the model using the entire sample, allowing all the coefficients to vary in the two subperiods. Table 4.5 presents the results for this specification.

As expected, returns to potential experience are positive, in accordance with the findings from other studies (for example, Orazem and Vodopivec [2000]). On average, one year of additional experience brought about a 4.2 percent increase in real earnings in 1992-1996 and a 6.0 percent increase in 1997-2000. This result could indicate that firms valued work experience more in the later period, since the general collective bargaining agreement prescribed the minimum rate of return to seniority of only 0.5 percent for every year of work experience. These returns are higher than the ones Orazem and Vodopivec [2000] find for 1992.

The wage - potential experience profile is concave, and it became even more concave in 1997-2000 compared to 1992-1996, especially for those with less than 25

Table 4.5: Wage Model - Observable Worker Characteristics

	1992-2000	1992-1996	1997-2001	Change
Intercept	4.8551***	4.8485***	4.9121***	
	[0.0014]	[0.0018]	[0.0020]	
Experience	0.0470***	0.0409***	0.0583***	0.0174***
	[0.0003]	[0.0004]	[0.0004]	[0.0006]
$\text{Experience}^2/100$	-0.2846***	-0.2306***	-0.3834***	-0.1526***
	[0.0028]	[0.0038]	[0.0040]	[0.0056]
$\text{Experience}^3/1000$	0.0860***	0.0711***	0.1151***	0.0441***
	[0.0010]	[0.0014]	[0.0014]	[0.0020]
$\text{Experience}^4/10000$	-0.0090***	-0.0077***	-0.0119***	-0.0041***
	[0.0001]	[0.0002]	[0.0002]	[0.0002]
Tenure	0.0305***	0.0329***	0.0217***	-0.0111***
	[0.0002]	[0.0003]	[0.0003]	[0.0004]
$\text{Tenure}^2/100$	0.1272***	-0.0341***	-0.0602***	-0.0260***
	[0.0020]	[0.0040]	[0.0025]	[0.0047]
Elementary	0.0814***	0.0899***	0.0784***	-0.0115***
	[0.0007]	[0.0009]	[0.0011]	[0.0015]
Vocational	0.2494***	0.2554***	0.2511***	-0.0042***
	[0.0007]	[0.0009]	[0.0011]	[0.0014]
High school	0.5125***	0.5152***	0.5176***	0.0024
	[0.0007]	[0.0009]	[0.0011]	[0.0015]
University (2 years)	0.8315***	0.8225***	0.8519***	0.0294***
	[0.0009]	[0.0012]	[0.0013]	[0.0018]
University (4 years)	1.1618***	1.1390***	1.1933***	0.0543***
	[0.0009]	[0.0012]	[0.0013]	[0.0018]
Fixed Term Appointment	-0.0546***	-0.0299***	-0.0726***	-0.0428***
	[0.0005]	[0.0008]	[0.0006]	[0.0010]
Overtime	0.0961***	0.1015***	0.0910***	-0.0105***
	[0.0004]	[0.0005]	[0.0005]	[0.0008]
Incomplete year	-0.0925***	-0.0964***	-0.0871***	0.0093***
	[0.0005]	[0.0007]	[0.0007]	[0.0010]
Shift effects	YES	YES	YES	
Year effects	YES	YES	YES	
Industry effects	NO	NO	NO	
Firm effects	NO	NO	NO	
Observations	4737923	2611904	2126019	
R-squared	0.4695	0.4282	0.5090	

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. Omitted group of education are workers with unfinished elementary school. The model also includes controls for the type of shift and year fixed effects.

Source: Own calculations based on matched employer-employee database.

years of potential experience. For those with more than 25 years of potential experience but less than 35 years of potential experience, the marginal returns are almost the same in both sub-periods, while returns decreased in 1997-2000 for those with more than 47 years of potential experience. This indicates that marginal returns to a year of potential experience rise for the least experienced but fall for the most experienced. Panel A of Figure 4.1 shows precisely this pattern.

Firm tenure also had a positive impact on wages - an additional year of "service" at the same firm brought about a 3.1 percent increase in earnings during 1992-2000, 3.3 percent during 1992-1996 and 2.2 percent during 1997-2000. These results indicate that seniority in the firm became less valued (while perhaps individual skills became more important) in the late 1990s. The wage - tenure profile is concave as well, meaning that marginal returns to a year of tenure rise less than proportionately with the length of tenure.

Average returns to education rose more for the most educated groups relative to the least educated groups. Consistent with findings from Orazem and Vodopivec [1994], we find that those with four years of university education or more gained the most in relative earnings, followed by those with two years of university education. Returns to education increased by 5.6 percent for the first group and by 1.0 percent for the second relative to those who did not finish elementary school. Those with finished elementary school or vocational school actually lost 1.1 and 0.4 percent relative to those who did not finish elementary school. These results indicate that the wage scale was not as compressed in the late 1990s as it was at the beginning of transition.

4.4.3 Industry and Firm Fixed Effects

The next step in our analysis is to look at the impact of industry and firm fixed effects using model (4.2). Firms are allowed to deviate from the prescribed minimum basic wage and the basic wage differs by industry, since there are a number of industry collective bargaining agreements in force. We again estimate the model for the entire period, and for the two sub-periods.

Table 4.6 contains the results for the Mincer [1974] model with 2-digit industry fixed effects (refer to Appendix E for the list of 2-digit industries). Compared to the model without industry fixed effects, the returns to potential work experience are larger in 1992-1996, about 4.6 percent for each additional year of potential experience, but slightly smaller in 1997-2000. The wage - potential experience profile is still concave, as Panel B of Figure 4.1 illustrates, and only workers with 25 to 35 years of potential experience do worse in 1997-2000 than in 1992-1996. Tenure with the firm has a positive impact on log real wage in both sub-periods, and the wage - tenure profile is concave.

Average returns to education still follow the same general pattern as in Table 4.5, but they are lower. One possible explanation is that some industries attract more educated workers or have on avergae a better educated workforce than others, and the omission of industry fixed effects causes the coefficients on education to pick up some of this correlation.

On the other hand, it is also plausible that there are differences not only between industries but also between firms in the types of workers they are able

Table 4.6: Wage Model - Observable Worker Characteristics and Industry Fixed Effects

	1992-2000	1992-1996	1997-2001	Change
Experience	0.0487***	0.0445***	0.0578***	0.0156***
	[0.0003]	[0.0004]	[0.0004]	[0.0006]
Experience^2/100	-0.2962***	-0.2618***	-0.3753***	-0.1262***
	[0.0026]	[0.0036]	[0.0038]	[0.0053]
Experience^3/1000	0.0899***	0.0819***	0.1121***	0.0334***
	[0.0009]	[0.0013]	[0.0013]	[0.0019]
Experience^4/10000	-0.0096***	-0.0091***	-0.0116***	-0.0028***
	[0.0001]	[0.0002]	[0.0002]	[0.0002]
Tenure	0.0325***	0.0395***	0.0222***	-0.0177
	[0.0002]	[0.0003]	[0.0003]	[0.0004]
Tenure^2/100	-0.1523***	-0.1211***	-0.0668***	0.0630***
	[0.0019]	[0.0038]	[0.0024]	[0.0045]
Elementary	0.0765***	0.0818***	0.0753***	-0.0094***
	[0.0007]	[0.0009]	[0.0011]	[0.0014]
Vocational	0.2329***	0.2359***	0.2368***	-0.0056***
	[0.0007]	[0.0008]	[0.0010]	[0.0013]
High school	0.4544***	0.4560***	0.4599***	-0.0050***
	[0.0007]	[0.0009]	[0.0011]	[0.0014]
University (2 years)	0.7793***	0.7742***	0.7940***	0.0189***
	[0.0009]	[0.0012]	[0.0013]	[0.0017]
University (4 years)	1.0937***	1.0716***	1.1221***	0.0479***
	[0.0009]	[0.0012]	[0.0013]	[0.0017]
Fixed Term Appointment	-0.0458***	-0.0179***	-0.0681***	-0.0394***
	[0.0005]	[0.0007]	[0.0006]	[0.0009]
Overtime	0.0939***	0.1004***	0.0856***	-0.0105***
	[0.0004]	[0.0005]	[0.0005]	[0.0007]
Incomplete year	-0.0753***	-0.0777***	-0.0691***	0.0123***
	[0.0005]	[0.0007]	[0.0007]	[0.0010]
Year effects	YES	YES	YES	
Industry effects	YES	YES	YES	
Firm effects	NO	NO	NO	
Observations	4737923	2611904	2126019	
R-squared	0.5260	0.4919	0.5630	

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. Omitted group of education are workers with unfinished elementary school. The model also includes controls for the type of shift and year fixed effects.

Source: Own calculations based on matched employer-employee database.

to attract. Table 4.7 presents the results of Mincer [1974] model estimation with firm fixed effects. The returns to potential experience are even larger than in the model with industry fixed effects, overall and in both sub-periods, but the difference between the two subperiods is now only 1.1 percentage points.

It is interesting to note, however, that the returns to education for all groups relative to the least educated group are lower than in the baseline model and the model with industry fixed effects. In addition, all groups gain relative to those with the least education in 1997-2000 compared to 1992-1996, with the most educated workers gaining the most, 5.6 percentage points. Figure 4.2 shows the change in returns to education by the highest educational level attained in 1997-2000 compared to 1992-1996, for models (4.1) and (4.2).

These findings suggest that there indeed is a firm fixed effect present in the wage structure, but person fixed effects are likely to be important as well, for which an Abowd et al. [2002] model estimation is necessary. This is what we do next.

4.4.4 Firm and Person Fixed Effects

Firms are also allowed to make worker-specific deviations from the wage guidelines set in collective bargaining agreements if they want to reward the worker for his/her success. Hence, it is necessary to include not only observable worker heterogeneity (for example, level of education attained), but also unobservable worker heterogeneity in the model. The approach pioneered by Abowd et al. [2002] allows us to estimate both firm and person fixed effects, and our sample fulfills the require-

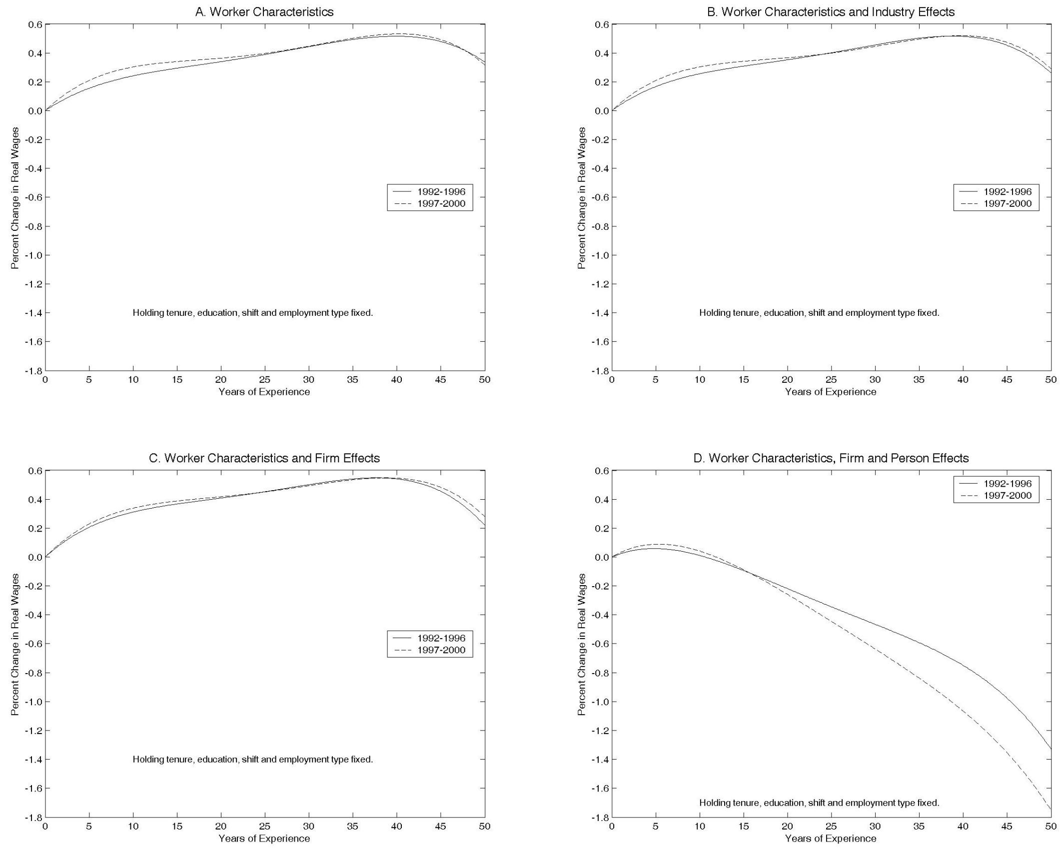
Table 4.7: Wage Model - Observable Worker Characteristics and Firm Fixed Effects

	1992-2000	1992-1996	1997-2001	Change
Experience	0.0557***	0.0553***	0.0621***	0.0108***
	[0.0002]	[0.0003]	[0.0003]	[0.0005]
Experience^2/100	-0.3318***	-0.3313***	-0.3834***	-0.0743***
	[0.0021]	[0.0028]	[0.0032]	[0.0043]
Experience^3/1000	0.0979***	0.1000***	0.1115***	0.0162***
	[0.0008]	[0.0010]	[0.0011]	[0.0015]
Experience^4/10000	-0.0104***	-0.0108***	-0.0115***	-0.0010
	[0.0001]	[0.0001]	[0.0001]	[0.0002]
Tenure	0.0212***	0.0095***	0.0222***	0.0013***
	[0.0002]	[0.0003]	[0.0002]	[0.0003]
Tenure^2/100	-0.1423***	-0.0325***	-0.1178***	-0.0589***
	[0.0016]	[0.0033]	[0.0022]	[0.0037]
Elementary	0.0721***	0.0742***	0.0712***	0.0033***
	[0.0006]	[0.0007]	[0.0009]	[0.0011]
Vocational	0.2310***	0.2345***	0.2299***	0.0093***
	[0.0005]	[0.0007]	[0.0009]	[0.0011]
High school	0.4406***	0.4470***	0.4397***	0.0045***
	[0.0006]	[0.0007]	[0.0009]	[0.0011]
University (2 years)	0.7729***	0.7754***	0.7804***	0.0260***
	[0.0007]	[0.0009]	[0.0011]	[0.0014]
University (4 years)	1.0519***	1.035***	1.0803***	0.0543***
	[0.0008]	[0.0010]	[0.0011]	[0.0014]
Fixed Term Appointment	-0.0783***	-0.0748***	-0.0852***	-0.0239***
	[0.0004]	[0.0006]	[0.0006]	[0.0008]
Overtime	0.0324***	0.0345***	0.0271***	-0.0303***
	[0.0004]	[0.0005]	[0.0005]	[0.0006]
Incomplete year	-0.0470***	-0.0591***	-0.0360***	0.0201***
	[0.0004]	[0.0005]	[0.0006]	[0.0008]
Year effects	YES	YES	YES	
Industry effects	NO	NO	NO	
Firm effects	YES	YES	YES	
Observations	4737923	2611904	2126019	
R-squared	0.7053	0.7161	0.7298	

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. Omitted group of education are workers with unfinished elementary school. The model also includes controls for the type of shift and year fixed effects.

Source: Own calculations based on matched employer-employee database.

Figure 4.1: Returns to Potential Experience, 1992-1996 and 1997-2000

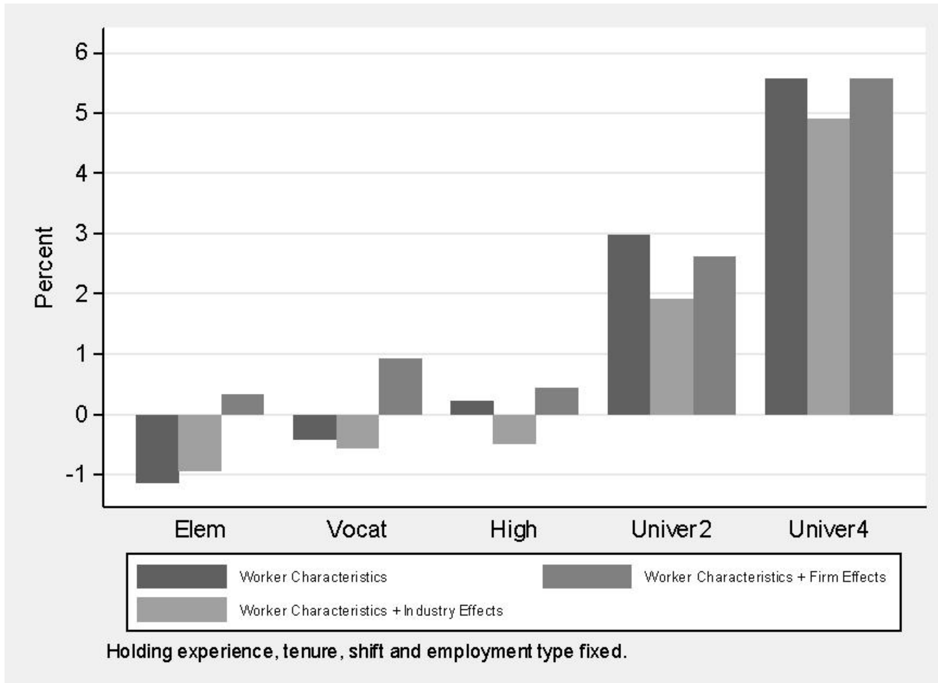


Source: Own calculations based on matched employer-employee database.

ment needed for such estimation to be possible (some workers switch employers), as we have shown in Table 4.4.

As mentioned above, the Abowd et al. [2002] approach uses graph theory to determine mutually exclusive groups of connected individuals and firms. Table 4.8 shows the result of applying this algorithm to our data. The largest group contains 97 percent of the sample in 1992-2000, 96 percent of the sample in 1992-1996 and 92 percent of the sample in 1997-2000. This measure can be interpreted as a measure of worker mobility. Abowd et al. [2002] find this measure to be around 88 percent for France and 99 percent for the State of Washington. Mobility is higher than we

Figure 4.2: Change in Returns to Education (Relative to Those with Unfinished Elementary School), 1992-1996 to 1997-2000



Source: Own calculations based on matched employer-employee database.

expected for Slovenia, but this can be explained by the fact that a lot of churning occurred especially in the early 1990s when there was a lot of entry of small firms, but also later on, as large firms were either downsizing or closing down.

Panel D of Figure 4.1 shows that the wage - potential experience profile is still concave, as in the previous models, but the shape of this profile differs a lot from previous estimations. Returns to potential experience are slightly positive and increasing until about 5-10 years of potential experience, and then turn downwards and become negative at around 13 years of potential experience. This downward slope is steeper in 1997-2000. It appears that the estimated coefficients from previous models picked up something else besides potential experience, related to unobservable person fixed effects (such as ability of workers), which we are now able to estimate

Table 4.8: Results of the Grouping Algorithm

	Largest group	Second largest group	Average of all other groups	Total of all groups
1992-2000				
Observations	4615460	38	5.4	4737923
Persons	865091	38	1.3	895750
Firms	62929	1	1.0	86739
Groups	1	1	22785	22787
Estimable effects	928019	38		959702
1992-1996				
Observations	2517404	60	3.4	2611904
Persons	719027	60	1.5	759506
Firms	38040	1	1.0	66502
Groups	1	1	27597	27599
Estimable effects	757066	60		798409
1997-2000				
Observations	1959527	1152	4.3	2126019
Persons	651454	303	1.7	716417
Firms	34657	1	1.0	75181
Groups	1	1	38610	38612
Estimable effects	686110	303		752986

Source: Own calculations based on matched employer-employee database.

separately.

4.4.5 Analysis of Variance

In the previous sections, we have shown that the wage structure changed in the late 1990s compared to the early 1990s. The big winners of the transition appear to be the most educated workers. However, it is also interesting to analyze the variance structure of log real hourly wages and explore whether there has been a change in the explanatory power of observable and unobservable worker characteristics, industry and firm fixed effects in the late 1990s compared to the early 1990s. As we explained in section 4.2, there are differences among industries both in the level of the basic wage and the wage scale, the wage-setting mechanism allows firm- and person-specific deviations, and there was a change in the wage determination process in 1997, which could have had an impact on the explanatory power of these characteristics.

The variance of log real hourly wages from model (4.1) can be decomposed in the following way:

$$
\begin{aligned}
Var\left(\ln w_{it}\right) &= Var\left(\mathbf{X_{it}}\boldsymbol{\beta}\right) + Var\left(\mathbf{Z_{it}}\boldsymbol{\gamma}\right) + Var\left(\epsilon_{it}\right) \\
&\quad + 2Cov\left(\mathbf{X_{it}}\boldsymbol{\beta}, \mathbf{Z_{it}}\boldsymbol{\gamma}\right) + 2Cov\left(\mathbf{X_{it}}\boldsymbol{\beta}, \epsilon_{it}\right) + 2Cov\left(\mathbf{Z_{it}}\boldsymbol{\gamma}, \epsilon_{it}\right)
\end{aligned} \tag{4.4}
$$

Similar decompositions of the variance of log real hourly wages follow from models (4.2) and (4.3).

As a first step, we look at how much of the variation in log real wage can be explained by industry or firm fixed effects only. As Table 4.9 shows, 2-digit industry effects accounted for 17.20 percent and firm fixed effects for 46.56 percent of variation in log real wage in 1992-1996. The importance of 2-digit industry fixed effects increased by 3.75 percentage points and that of firm fixed effects by 0.71 percentage points in 1997-2000.

Table 4.9: Analysis of Variance

R-squared	1992-1996	1997-2001
Industry effects	0.1720	0.2095
Firm effects	0.4656	0.4727

Source: Own calculations based on matched employer-employee database.

Table 4.10: Variances of Wage Components and Share of Variance of Log Real Hourly Wages Due to Wage Components Using Models (4.1), (4.2) and (4.3)

	Worker Characteristics		Worker Characteristics and Industry Effects		Worker Characteristics and Firm Effects		Firm and Person Effects	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1992-1996	1997-2000	1992-1996	1997-2000	1992-1996	1997-2000	1992-1996	1997-2000
R-squared	0.4282	0.5090	0.4919	0.5630	0.7161	0.7298	0.8448	0.8400
Variances								
Var(ln w_{it})	0.2532	0.2517	0.2532	0.2517	0.2532	0.2517	0.2532	0.2517
Var($\mathbf{X_{it}}\boldsymbol{\beta}$)	0.0215	0.0177	0.0222	0.0175	0.0158	0.0174	0.0493	0.0990
Var($\mathbf{Z_{it}}\boldsymbol{\gamma}$)	0.0065	0.0072	0.0052	0.0052	0.0038	0.0032	0.0101	0.0056
Var$\left(\varphi_{J(it,industry)}\right)$			0.0170	0.0146				
Var$\left(\varphi_{J(it,firm)}\right)$					0.0773	0.0591	0.0544	0.0373
Var(θ_i)							0.2329	0.3085
Var($\theta_{i,observable}$)	0.0986	0.1134	0.0861	0.0983	0.0825	0.0919	0.0516	0.0565
Var($\theta_{i,unobservable}$)							0.1813	0.2519
Var(ϵ_{it})	0.1448	0.1236	0.1286	0.1100	0.0719	0.0680	0.0393	0.0403
Share of variance in ln w_{it} accounted for by the variance of								
$\mathbf{X_{it}}\boldsymbol{\beta}$	0.0848	0.0702	0.0875	0.0693	0.0625	0.0691	0.1948	0.3932
$\mathbf{Z_{it}}\boldsymbol{\gamma}$	0.0255	0.0285	0.0207	0.0207	0.0150	0.0128	0.0398	0.0223
$\varphi_{J(it,industry)}$			0.0671	0.0582				
$\varphi_{J(it,firm)}$					0.3052	0.2348	0.2146	0.1481
θ_i							0.9197	1.2258
$\theta_{i,observable}$	0.3895	0.4507	0.3400	0.3904	0.3257	0.3651	0.2037	0.2247
$\theta_{i,unobservable}$							0.7160	1.0011

Source: Own calculations based on matched employer-employee database.

Table 4.10 shows the variance decomposition of log real wage, using models (4.1), (4.2) and (4.3).[12] $\mathbf{X_{it}}$ contains experience and tenure and their squares, triples and quadruples (last two for experience only), and intercept in model (4.1). $\mathbf{Z_{it}}$ contains dummy variables for type of shift, type of employment, incomplete year, overtime work, and year effects. Person fixed effect, θ_i is decomposed into observable and unobservable person fixed effect. Observable person fixed effect refers to the educational level attained.

Columns (1) and (2) refer to model (4.1), which includes only observable worker characteristics and controls. In this model, the share of variance of log real hourly wage due to the variance in observable worker characteristics decreases by 1.5 percentage points in the late 1990s compared to the early 1990s. This pattern is still present in columns (3) and (4), which refer to model (4.2) with industry fixed effects, but not to the models with firm and/or person fixed effects (columns (5)-(8)). In fact, in the model with person and firm fixed effects, the share of variance of log real hourly wage due to the variance in observable worker characteristics increases by almost 20 percentage points. Firm fixed effects also account for a significant amount of variation in log real hourly wage, but their share drops by 7 percentage points in the late 1990s compared to the early 1990s.

Which component accounts for the most variation in log real hourly wage? Person fixed effects in both sub-periods. Models (4.1) and (4.2) only include observable person fixed effect, the level of education attained, whereas model (4.3) allows us to estimate total person fixed effect, observable and unobservable. The

[12]Covariance terms are omitted from the table, but are available upon request from the authors.

share of variance of log real hourly wage due to observable person fixed effect increases in 1997-2000 compared to 1992-1996 in all model specifications, with increase being the highest when using model (4.1). An overwhelming share of the variation in log real hourly wage comes from the unobservable person fixed effect, especially so in 1997-2000.

Given that unobservable person fixed effects explain almost all of the variation in log real hourly wage, it is interesting to look at the correlation coefficients among the wage components. The correlation between the person fixed effects and firm fixed effects is of particular interest, since it indicates whether good workers are employed by good firms. As Table 4.11 shows, this coefficient is positive and significant[13] in both sub-periods, but it is also very small in magnitude (0.03 in 1992-1996 and 0.02 in 1997-2000).

These results indicate that although firm effects are important, worker characteristics, especially unobserved person fixed effects, became more important in the late 1990s. Given the nature of the change in wage determination system in 1997, this is not surprising, especially in conjunction with the increase in returns to the most educated group of workers. Those workers are namely most likely to have individual contracts, and these were not limited as much in the late 1990s as in the early 1990s.

[13] All of the correlation coefficients in Table 4.11 are significant.

Table 4.11: Correlation Coefficients among Wage Components in a Model with Person and Firm Fixed Effects

	$\ln w_{it}$	$\mathbf{X_{it}}\boldsymbol{\beta}$	$\mathbf{Z_{it}}\boldsymbol{\gamma}$	$\varphi_{J(it,firm)}$	θ_i	$\theta_{i,observable}$	$\theta_{i,unobservable}$	ϵ_{it}
1992-1996								
$\ln w_{it}$	1.0000							
$\mathbf{X_{it}}\boldsymbol{\beta}$	-0.1305	1.0000						
$\mathbf{Z_{it}}\boldsymbol{\gamma}$	0.1156	-0.1102	1.0000					
$\varphi_{J(it,firm)}$	0.4844	-0.0250	0.0027	1.0000				
θ_i	0.7110	-0.5463	-0.0233	0.0341	1.0000			
$\theta_{i,observable}$	0.5324	0.1067	-0.0047	0.0535	0.4707	1.0000		
$\theta_{i,unobservable}$	0.5219	-0.6761	-0.0240	0.0102	0.8823	0.0000	1.0000	
ϵ_{it}	0.3255	-0.0364	-0.0360	-0.0029	-0.0458	0.0255	-0.0655	1.0000
1997-2000								
$\ln w_{it}$	1.0000							
$\mathbf{X_{it}}\boldsymbol{\beta}$	-0.0991	1.0000						
$\mathbf{Z_{it}}\boldsymbol{\gamma}$	0.1607	-0.1566	1.0000					
$\varphi_{J(it,firm)}$	0.3912	-0.0232	0.0602	1.0000				
θ_i	0.7186	-0.5928	0.0741	0.0220	1.0000			
$\theta_{i,observable}$	0.5838	0.0958	0.0157	0.0803	0.4281	1.0000		
$\theta_{i,unobservable}$	0.5185	-0.7014	0.0746	-0.0137	0.9037	-0.0000	1.0000	
ϵ_{it}	0.2301	-0.0939	0.0108	-0.0312	-0.0909	0.0412	-0.1202	1.0000

Source: Own calculations based on matched employer-employee database.

4.5 Conclusion

This chapter exploits a rich matched employer-employee dataset for Slovenia, covering almost the entire universe of workers in the 1990s. Transition in Slovenia started in 1988, and continued throughout the 1990s. Most significant changes in the labor market occured in the early 1990s, but we find that additional small "deregulations" in the form of significantly reduced limitations on the wage growth in 1997 also had a significant impact on the wage structure. Most notably, there was an increase in returns to experience and in returns to the most educated workers. Returns to experience increased more than in the early years of transition, whereas returns to education increased less compared to 1987-1991 (see Orazem and Vodopivec [1994] for the latter).

The magnitude of the returns and the changes in the wage structure depends on the model of log real hourly wages. Specifically, variance in person fixed effects accounted for more than 90 percent of variation in log real hourly wages, and their explanatory power increased in the late 1990s. Firm fixed effects are important as well, but not nearly as much as person fixed and time-variant effects. In fact, their importance decreases in the late 1990s, and the correlation between person and firm fixed effects, although positive, is very small. To caricature, it matters where you work, but it matters much more how good you are at what you do.

In future work, we plan to use the estimated firm and person fixed effects to examine the impact of worker matching on productivity growth. When a match is made between a worker and a firm, its productivity will depend on both the firm effect, the worker effect and the quality of the match. Firms learn about the latter through experience, and separating bad matches will have a positive impact on their productivity.

Chapter 5

Conclusion

This thesis examines the role of institutions in productivity and reallocation dynamics. Specifically, we examine the impact of institutions on allocative efficiency, job flows and wage structure. All of these can have an impact on aggregate productivity and its growth. Aggregate productivity and its growth namely depend not only on how productive businesses are on average, but also on whether more productive businesses are the ones that have a higher market share.

Using a firm-level dataset with accounting information and information on which firms received state aid for the rescue and restructuring of firms in difficulty, we first examine the impact of such aid on allocative efficiency in Slovenian manufacturing in Chapter 2. We measure allocative efficiency using an Olley and Pakes [1996]-inspired micro covariance measure. The impact of state aid on allocative efficiency is difficult to evaluate due to the lack of a counterfactual and because of selection bias. To deal with these, we use treatment effects estimators that assume selection on observables and estimators that explicitly allow for selection on unobservables. In the latter models, we exploit the fact that medium and large firms and labor intensive firms were eligibile for aid but the availability of aid dwindled in 2002 as a result of adjusting to the EU legislation to identify the impact. The empirical analysis reveals a couple of interesting results. First, aid-receiving firms survived longer than they would have had they not received aid, since their economic fundamentals indicate a higher probability of exit than for non-aid-receiving firms, but none of them ceased to exist until the end of 2003. Second, aid hindered the efficient static allocation of resources. Third, aid-receiving firms grew faster than they would have in the absence of aid. Taken together, these results suggest that aid shifted the burden of structural adjustment onto more efficient firms who managed without it. It had a negative impact on both the static and dynamic allocative efficiency.

Reallocation of outputs and inputs from less productive to more productive businesses is facilitated by reallocation of inputs and outputs among incumbent businesses as well as by entry and exit. Existing empirical evidence shows that most of the aggregate productivity growth comes from within-firm productivity growth, but the net contribution of entry and exit is far from negligible (see, e.g., Bartelsman et al. [2004a]). Hence, it is also important to know the impact of institutional environment on job flows and the structure of wages, which can induce good matches (between workers and firms) to leave the firm.

In Chapter 3, we exploit a cross-country database with harmonized data on job flows that vary by industry and size. We find that industry and size together account for a very large share of the overall variability in job flows across country, industry and size cells, with firm size being the most important factor. However, even after controlling for industry/technology and size factors, there remain significant differences in job flows across countries that could reflect differences in business environment conditions. We look at the regulations on hiring and firing of workers, one of the factors shaping the business environment. To minimize the possible endogeneity and omitted variable problems associated with cross-country regressions, we use a difference-in-difference approach. The United States' patterns of job turnover are used as a benchmark to identify and quantify the industry/size cells with inherently higher job turnover, assuming that their patterns are the least distorted by regulation. Empirical results suggest that stringent hiring and firing regulation, and especially their consistent enforcement, reduce job turnover as well as distort the patterns of flows across industry and size classes within a country. Stringent labor regulations within a country affect medium and large firms more severely than small firms, probably because small firms are either partially exempted by such regulations or can more easily circumvent them.

In Chapter 4, we make the first step towards understanding one of the possible explanations for within-firm productivity growth. When a match is made between a worker and a firm, its productivity will depend on both the firm effect, the worker effect, and on the quality of the match. As mentioned, the within component of dynamic productivity decomposition accounts for the bulk of productivity growth, and the within effects may be driven by the worker reallocation, reflecting changes in the worker match quality. One mechanism firms have for attracting and keeping good matches are the wages. It is therefore important to understand the structure of wages. In Slovenia, transition brought about significant changes in employment and wage policies, and we have a rich matched employer-employee database that allows us to study the impact of these changes on the structure of wages. Under self-management, wage scales were extremely compressed. The wage setting system established in 1991 allowed firm- and worker-specific deviations from the wage guidelines set in collective bargaining agreements, but the incomes policies suppressed the growth of wages, especially of the managerial workforce, until 1997. We exploit this change in the wage setting system to compare the wage structure in the early 1990s to the wage structure in the late 1990s. Our results suggest that the wage structure changed significantly in the late 1990s compared to the early 1990s, regardless of the model of wages assumed. However, it also reveals that it is extremely important to include person fixed effects in the model, as their variation accounts for the bulk of the variation in log real hourly wages, and their importance increased over time. Firm fixed effects are important as well, but much less than person fixed effects.

We do not consider welfare implications of institutions in any part of our analysis, we focus only on their role in productivity and reallocation dynamics. It is possible that institutions have a negative impact on productivity and reallocation dynamics, but help improve welfare. For example, state aid has a negative impact on static allocative efficiency, but since it allowed firms to survive longer, jobs in these firms were kept. Strict firing regulations could in theory have a similar impact on employment. Some of the firms might have gone bankrupt had they not received aid, but are doing well in the longer run. Analysis of the impact of aid taking into account more than a one period lag could reveal that the impact of aid on productivity and reallocation dynamics is positive in the longer run. Once the data become available, we intend to re-examine the impact of aid on productivity and allocative efficiency over time.

More work remains to be done to understand and combine the implications of our findings. Our findings provide evidence that institutions (availability of state aid to firms in difficulty, labor regulations, wage setting system) have an impact on productivity and reallocation dynamics separately. It is a much larger step to combine the impact of institutions on productivity and reallocation dynamics in one model to describe the precise channel through which efficient allocation of resources affects aggregate productivity growth. To explore the latter, we need to combine our productivity estimates from Chapter 2 with the firm and person fixed effects estimates from Chapter 4, further exploiting our matched employer-employee database. As mentioned, a number of studies have found that allocative efficiency is important for understanding differences across time, industries and countries in the level and growth of productivity and that the within component accounts for the majority of productivity growth (see, e.g., Foster et al. [2001] and Bartelsman et al. [2005]). Our findings suggest that firms reward good workers with higher wages, but we do not know whether this has a positive impact on firm productivity and consequently on the allocative efficiency in the economy. Exploration of these issues is intended for future work.

Appendix A

State Aid in the Members of the European Union and Accession Candidates

Table A.1: State Aid in the EU Excluding Agriculture, Fisheries and Transport, 2000-2002

	% of GDP	Rank	Per capita, €	Rank	% Sectoral‡
Old members					
Belgium	0.37	17	83	12	3
Denmark	0.72	7	228	3	0
Germany	0.56	10	147	6	34
Greece	0.31	18	60	15	0
Spain	0.55	11	104	9	33
France	0.42	15	108	8	40
Ireland	0.45	14	160	5	51
Italy	0.38	16	89	11	4
Luxemburg	0.26	19	93	10	8
Netherlans	0.19	23	43	21	2
Austria	0.21	22	59	16	4
Portugal	0.55	11	130	7	61
Finland	0.17	24	46	20	2
Sweden	0.16	26	47	19	16
Great Britain	0.17	24	36	22	30
New members					
Cyprus†	2.85	3	405	1	77
Czech Republic†	2.80	4	187	4	90
Estonia†	0.11	27	5	27	0
Hungary†	1.04	6	56	17	58
Latvia†	0.26	19	10	25	45
Lithuania†	0.24	21	10	25	96
Malta†	3.86	1	404	2	95
Poland†	1.29	5	63	14	76
Slovenia†	0.69	8	70	13	27
Slovakia†	0.47	13	22	23	76
Accession candidates			**2000-2003**		
Bulgaria†	0.60	9	11	24	76
Romania	3.30	2	50	18	67

†2000-2003. ‡Including rescue and restructuring aid.

Source: State Aid Scoreboard, Spring 2004, and State Aid Scoreboard online.

Appendix B

Sample Selection and Measurement of Productivity

TFP is calculated as a residual using the standard Cobb-Douglas production function with constant returns to scale and using capital, labor, material and services as inputs. The focus of our work is the impact of state aid on static and dynamic allocative efficiency in manufacturing. State aid data is only available from 1998 onwards, so the period under study is restricted to the period from 1998 to 2003.

B.1 Sample Selection and Measurement of Inputs

Available measures for inputs and outputs are somewhat problematic, but not much can be done given the information available. Our measure of capital, $K_{it,j}$, includes buildings, structures, nonresidential construction, roads, machinery, transport and other equipment, but does not include buildings, machines and equipment under operating leases, and it is impossible to estimate the extent of leasing since this information is not collected separately in Slovenia.[1] The lease payments show up in the cost of services of the lessee, S, and the value of the leased buildings,

[1] Balance sheet includes a category called "Capital revaluation adjustment", but this reflects adjustment of financial investment only. Book values that are reported in the balance sheet are adjusted for depreciation and revaluation until 2001 according to the following formula:

$$\begin{aligned}\text{Book value}_t &= \text{Revaluation coefficient} \, * \\ &\quad * \left(\text{Book value}_{t-1} - \text{Depreciation by prescribed rates by groups of fixed assets}\right).\end{aligned}$$

The revaluation coefficient was roughly equal to the retail price index before 1998 and to the CPI since then.

machines and equipment shows up in K of the lessor. Aggregate measure of capital stock at the total economy level is thus not problematic, but any measure at a more disaggregated level will be fraught with this problem, since the leasing supply firms are not necessarily classified under the same industry as the leasing demand firms.

Labor, $L_{it,j}$, refers to workers employed in accordance with the *Law on Labor Relations*, regardless of whether they are in permanent or temporary employment. The number of workers is calculated based on the hours worked, i.e., total hours worked by workers as defined above are divided by the number of hours worked per worker in a year (about 2000 hours, depending on the required hours per day (usually 8) and the number of working days in a given year). L does not include the number of hours worked by students, who have a special status in Slovenia, and there are some indications that some firms were using students as a part of their labor force extensively to reduce the cost of labor, since a worker employed in accordance with the *Law on Labor Relations* costs the firm 1.9-times more than a student worker.[2] L also does not include workers who work on the basis of a contract for work or a copyright contract. Separate data on these types of employment are not available at the firm or industry level. Cost of labor, used in the calculation of factor elasticities, does not include the cost of labor of such workers; it only includes wages and benefits of workers employed in accordance with the *Law on Labor Relations*. Instead, the

[2] A worker receiving 100.000 SIT net pay costs the firm 115.010 SIT if the worker is a student, and 219.191 SIT if the worker is a regularly employed worker. The difference is due to taxes and contributions. In a survey of 134 firms, 26 percent of them said they do not need more workers due to student labor, and this was especially true for firms with less than 10 or more than 100 workers (Stanković [2004]). New *Income Tax Code*, in effect since January 1, 2005, is attempting to discourage firms from employing students on positions which actually require a full-time regularly employed worker.

cost of student workers and workers employed on the basis of a work contract or a copyright contract appears under the cost of services, S.

As follows from the above, services, $S_{it,j}$, include a number of items that should ideally be included in capital, labor, and labor cost, such as rents and student labor. Services also include transport costs, the cost of unfinished goods produced by other firms,and costs of maintenance, marketing and insurance. Detailed information on these is not available.

As a result of the accounting standards which account for the above mentioned problems with measuring inputs, there are a number of firms in my sample with zero capital and/or zero workers and/or zero materials and/or zero services, but it is possible that these zeros are not actually zeroes. Instead of excluding such firms from further analysis, we adjust the production function as explained in subsection B.2.

The number of PASEF firms by year is shown in column (1) of Table B.1. We then eliminate firms that are missing variables needed for the calculation of productivity. First, we exclude firms with zero or negative output (Q). Then we exclude firms for which capital (K), labor (L), cost of services (S), cost of materials (M) and cost of labor are all zero. This reduces our sample by about 10 percent.

Within the remaining sample, on average about 15 percent of firms employ no workers, 5 percent report zero capital, and only 0.4 percent report zero cost of services. The final sample used to estimate the impact of aid is reported in the final column of Table B.1. This sample is smaller than the sample in column (2) of Table B.1 because we exclude firms in the 1^{st} and 99^{th} percentile of the productivity distribution.

Table B.1: Sample Selection

	Number of firms		Share of firms with			
Year	(1)	(2)	(3)	(4)	(5)	
Year	Raw	After	$K=0$	$L=0$	$S=0$	Sample
1998	6495	5787	0.037	0.154	0.006	4791
1999	6402	5769	0.036	0.153	0.007	4779
2000	6338	5765	0.042	0.143	0.006	4798
2001	6265	5805	0.049	0.137	0.006	4860
2002	6359	5893	0.055	0.138	0.002	5006
2003	6558	5967	0.059	0.141	0.002	5101

Source: Own calculations based on PASEF data.

B.2 Measurement of Productivity

For firms with non-zero inputs, TFP is calculated according to the following equation:

$$lnTFP_{it,j} = lnQ_{it,j} - \bar{\alpha}_{K,j} lnK_{it,j} - \bar{\alpha}_{L,j} lnL_{it,j} - \bar{\alpha}_{M,j} lnM_{it,j} - \bar{\alpha}_{S,j} lnS_{it,j}, \quad \text{(B.1)}$$

where

i - firm, t - time, j - 2-digit industry

$Q_{it,j}$ Net sales revenue + change in inventories

$K_{it,j}$ Book value of fixed assets

$L_{it,j}$ Average number of workers based on the hours worked

$M_{it,j}$ Cost of supplies and material

$S_{it,j}$ Cost of services

For firms with zero labor, zero capital, and zero labor and capital, we calculate the production function and hence TFP using the inputs that were available and interpret the inputs as composite inputs. Equation (B.1) is thus modified according to:

$$lnTFP_{it,r} = lnQ_{it,r} - \bar{\alpha}_{K,r} lnK_{it,r} - \bar{\alpha}_{M,r} lnM_{it,r} - \bar{\alpha}_{S,r} lnS_{it,r} \tag{B.2a}$$

$$lnTFP_{it,r} = lnQ_{it,r} - \bar{\alpha}_{L,r} lnL_{it,r} - \bar{\alpha}_{M,r} lnM_{it,r} - \bar{\alpha}_{S,r} lnS_{it,r} \tag{B.2b}$$

$$lnTFP_{it,r} = lnQ_{it,r} - \bar{\alpha}_{M,r} lnM_{it,r} - \bar{\alpha}_{S,r} lnS_{it,r} \tag{B.2c}$$

where r denotes 1-digit industry[3] and the rest is the same as in (B.1). Equation (B.2a) refers to zero labor firms, equation (B.2b) to zero capital firms, and equation (B.2c) to zero capital and labor firms.

Measures of factor elasticities, $\bar{\alpha}_{K,j}$, $\bar{\alpha}_{L,j}$, $\bar{\alpha}_{M,j}$, and $\bar{\alpha}_{S,j}$, were estimated at the 2-digit industry level (and $\bar{\alpha}_{K,r}$, $\bar{\alpha}_{L,r}$, $\bar{\alpha}_{M,r}$, and $\bar{\alpha}_{S,r}$ at the 1-digit industry level) using factor cost shares with imputed user cost of capital[4] and under the assumption of constant returns to scale. Factor shares are averaged across the years 1996-2003, using the number of firms in 2-digit industry j (1-digit industry r) as weights to minimize the measurement errors.[5]

We experimented with OLS estimation of the production function as an alternative way of computing factor elasticities and TFP. We found that the rank ordering and the quantitative variation in firm-level TFP were not very sensitive to the estimation methodology used to calculate TFP, despite the variation in the factor elasticities - the correlation coefficients between the various pairs of measures

[3]These were calculated at the 1-digit industry level to minimize measurement error due to outliers, since there were only a few such firms in some 2-digit industries.

[4]Refer to Appendix C for details on the calculation of the user cost of capital.

[5]This approach is based on the strong assumptions that factors are paid their marginal products.

were 0.80 or higher. This finding is similar in spirit to the finding of Van Biesebroeck [2004] who finds that the distributional properties of firm-level TFP are reasonably robust to a wide variety of estimation methods.

LP is calculated in a standard way as $lnLP_{it} = ln\frac{Q_{it}}{L_{it}}$. As before, we exclude firms with LP below the 1^{st} and above the 99^{th} percentile from further analysis.

Using the described methodology and following Baily et al. [1992], the level of productivity in industry j in year t is calculated as follows:

$$P_{t,j} = \sum_{i \in j} s_{it,j} p_{it,j} \tag{B.3}$$

where $s_{it,j}$ is the share of the i-th firm's output in industry j's output in year t in current prices $(\frac{Q_{it,j}}{Q_{t,j}})$ and $p_{it,j}$ is either log of TFP or log LP of firm i in industry j at time t.

Appendix C

User Cost of Capital

User cost of capital is calculated at the level of 2-digit industry according to the following formula (slightly adjusted formula from Marchetti and Nucci [2001]):

$$ucK_{t,j} = \frac{P_t^I}{P_t^P} \frac{1 - etr_t}{1 - taxr_t} \left\{ \left(sstdebt_{t,j} * r_t^{stl} + sltdebt * r_t^{ltl} \right) * (1 - taxr_t) \right.$$
$$\left. + seq_{t,j} * r_t^e + \left[sstr_{t,j} * \delta^{str} + (1 - sstr_{t,j})\, \delta^{mach} \right] - \frac{P_t^I - P_{t-1}^I}{P_{t-1}^I} \right\},$$

where t denotes time, j denotes 2 - digit industry, ucK is user cost of capital, P^I is capital goods price index, P^P is producer price index, etr is effective corporate tax rate, $taxr$ is corporate tax rate (25%), $sstdebt$ is share of short-term debt in liabilities and equity, r^{stl} is real interest rate on short-term debt, $sltdebt$ is share of long-term debt in liabilities and equity, r^{ltl} is real interest rate on long-term debt, seq is share of equity in liabilities and equity, r^e is real rate of return on government bonds, $sstr$ is share of buildings, plants and property in tangible assets, δ^{str} is rate of depreciation of buildings, plants and property (2.5%), δ^{mach} is rate of depreciation of machinery and equipment (15%). Shares of short term debt, long term debt and equity in liabilities are calculated at the level of 2-digit industry, excluding firms with negative equity (not a measurement error or a mistake, just a consequence of relatively low equity requirements for the registration of the firm).

Interest rate on short term debt (r^{stl}) is taken to be equal to the average commercial banks' nominal interest rate on short term working capital loans to firms (tolar indexation clause). Interest rate on long term debt (r^{ltl}) is taken to be equal to the average commercial banks' nominal interest rate on long term loans for capital assets (tolar indexation clause). Source for both of these is the Monthly Bulletin of the Bank of Slovenia, and they are available at an annual level. Interest rate on equity (r^e) is usually set equal to the risk-free rate - normally interest rate on 10-year government bonds. However, these data are available only from 2002 onwards for Slovenia, as there were no comparable bonds issued before that (some were issued with a euro clause). Prior to 2002, we set the interest rate on equity to be equal to the interest rate on 181 days - 1 year time deposits (since the interest rates on these and on the 10-year government bonds were almost identical in 2002 and 2003).

Effective tax rates are taken from Table 24 on page 66 in Gabrijelčič [2005].

Appendix D

Definition of Small, Medium, and Large Firms According to Article 51 of the Law on Enterprises

In the period 1993-2001, the following criteria were used to define the size of firms in the preparation of the annual accounting reports (Article 51 of the *Law on Enterprises*):

- Average number of persons in paid employment,
- Total annual revenues, and
- Average value of assets at the beginning and the end of the business year.

In 2002, these criteria were slightly modified (now Article 52 of the *Law on Enterprises*) and are the following:

- Average number of persons in paid employment,
- Net sales revenue (rather than total revenue as before) in the previous business year, and
- Value of assets at the end of the business year.

The numbers and amounts used are represented in Table D.1. In every case, the firm has to satisfy at least two of the above criteria to be put in a certain size class.

Table D.1: Criteria for Classifying Firms as Small, Medium and Large According to the Law on Enterprises

Criteria	**Period**	**Small**	**Medium**	**Large**
Average number of persons in paid employment	1993-1996 & 1997-2001	At most 50	At most 250	- Exceeds at least two of the criteria for medium firms - Banks, insurance companies, and firms that must prepare consolidated annual accounting reports
Annual revenue	1993-1996	less than 200.000.000 SIT	less than 800.000.000 SIT	
	1997-2001	less than 280.000.000 SIT	less than 1.100.000.000 SIT	
Average value of assets	1993-1996	at most 100.000.000 SIT	at most 400.000.000 SIT	
	1997-2001	at most 140.000.000 SIT	at most 550.000.000 SIT	
Average number of persons in paid employment	from 2002 onwards	At most 50	At most 250	- Neither a small nor a medium firm - Banks, insurance companies, and firms that must prepare consolidated annual accounting reports
Net sales revenue		less than 1.000.000.000 SIT	less than 4.000.000.000 SIT	
Value of assets at the end of the business year		at most 500.000.000 SIT	at most 2.000.000.000 SIT	

Source: Official Gazette of the Republic of Slovenia On-line.

We applied these criteria to our data to check if the existing variable in the database is indeed in accordance with the *Law on Enterprises* criteria, and found that there are a number of discrepancies. Most strikingly, there are more small and large firms, and fewer medium firms, than the existing variable in the database would suggest.

Since this was the case, we contacted AJPES to find out what the procedure for assigning a value of this variable to firms was. Prior to 2002, firms classified themselves as small, medium, or large firms, and APP (Agency for Payments, predecessor of AJPES) checked their classification "manually" by checking the firms' classification and then checking the value of the underlying criteria. APP never changed the classification of the firms by itself - it only did so after contacting the firms and discussing the issue with them. This was supposedly the procedure, but it is unclear or unknown whether all APP subsidiaries strictly followed it and if they checked the firms' self-classifications in the first place. It is thus likely that there are misclassifications in the existing variable due to a "human error".

We were told that firms would often classify themselves as small even if they were actually medium firms because small firms were not obliged to have their annual reports audited, and medium and large firms were obliged to do so. Hence, it it unclear to us why the number of small firms actually increases. From 2002 onwards, AJPES uses a software to check for possible misclassifications, so there should be no more misclassifications assignable to human errors.

Appendix E

List of NACE Rev. 3 Codes and Their Names

Table E.1: List of NACE Rev. 3 Codes and Their Names

NACE	Name
Agriculture, Hunting and Forestry	
1	Agriculture, Hunting and Related Service Activities
2	Forestry, Logging and Related Service Activities
Fishing	
5	Fishing, Fish Farming and Related Service Activities
Mining and Quarrying	
10	Mining of Coal and Lignite, Extraction of Peat
12	Mining of Uranium and Thorium Ores
13	Mining of Metal Ores
14	Other Mining and Quarrying
Manufacturing	
15	Food, Beverages, and Animal Feeds
17	Textiles
18	Clothes; Tanning and Treatment of Fur
19	Leather and Leather Products
20	Wood and Wood Products
21	Pulp, Paper, and Paper Products
22	Publishing and Printing
23	Coke, Petroleum Products and Nuclear Fuel
24	Chemicals, Chemical Products, and Man-Made Fibers
25	Rubber and Plastic Products
26	Other Non-Metal Mineral Products
27	Metals
28	Metal Products, Except Machinery and Equipment
29	Machinery and Equipment
30	Office Machinery and Computers
31	Electrical Machinery
32	TV and Radio Sets, and Equipment
33	Medical, Precision, and Optical Instruments
34	Motor Vehicles and Trailers
35	Other Transport Equipment
36	Furniture; Manufacturing n.e.c.
37	Recycling

Continued on next page.

Table E.1: List of NACE Rev. 3 Codes and Their Names (continued)

NACE	Name
Electricity, Gas and Water Supply	
40	Electricity, Gas, Steam and Hot Water Supply
41	Collection, Purification and Distribution of Water
Construction	
45	Construction
Wholesale and Retail Trade	
50	Sale, Maintenance & Repair of Motor Vehicles, Retail Sale, Motor Fuels
51	Wholesale Trade & Commission Trade, Except of Motor Vehicles and Cycles
52	Retail Trade, Save Motor Vehicles, Repair,
52	Personal & Household Goods
Hotels and Restaurants	
55	Hotels and Restaurants
Transport, Storage and Communication	
60	Land Transport, Transport via Pipelines
61	Water Transport
62	Air Transport
63	Supporting and Auxiliary Transport Activities, Travel Agencies
64	Post and Telecommunications
Financial Intermediation	
65	Financial Intermediation, Except Insurance and Pension Funding
66	Insurance and Pension Funding, Except Compulsory Social Security
67	Activities Auxiliary to Financial Intermediation
Real Estate, Renting and Business Activities	
70	Real Estate Activities
71	Renting Machinery, Equipment w/o Operator, Personal & Household Goods
72	Computer and Related Activities
73	Research and Development
74	Other Business Activities
Public Administration and Defence, Compulsory Social Security	
75	Public Administration and Defence, Compulsory Social Security
Education	
80	Education
Health and Social Work	
85	Health and Social Work
Other Community, Social and Personal Service Activities	
90	Sewage and Refuse Disposal, Sanitation and Similar Activities
91	Activities of Membership Organizations n.e.c.
92	Recreational, Cultural and Sporting Activities
93	Other Service Activities

Appendix F

Definitions of Institutional Variables

Table F.1: Definitions of Institutional Variables

Variable	Definition
Hiring and Firing Practices	Flexibility in hiring and firing (5B(ii)) from Fraser Institute, hiring and firing practices of companies are determined by private contract (World Economic Forum: Global Competitiveness Report); scale [0,10], 10 being the worst.
Business Regulations	Regulation of business activities (5c) from Fraser Institute (World Economic Forum: Global Competitiveness Report); scale [0,10], 10 being the worst.
Law and Order	Integrity of Legal System (2e) from Fraser Institute, which is based on Political Risk Component I (Law and Order) from the International Country Risk Guide; scale [0,10], 10 being the worst.

Source: Gwartney and Lawson [2004].

Appendix G

Results for Excess Job Flows

Table G.1: Job Flows - A Baseline Difference-in-Difference Analysis

	1990s			**1990s, transition late 1990s**		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.1048***	0.1351***	0.1513***	-0.0279***	0.0022	0.0256**
	[0.0094]	[0.0106]	[0.0119]	[0.0090]	[0.0106]	[0.0118]
USA EXC	0.6900***			0.6795***		
	[0.0186]			[0.0181]		
USA EXC *EU		0.5602***			0.5624***	
		[0.0292]			[0.0287]	
USA EXC *Transition		0.7596***			0.7223***	
		[0.0335]			[0.0322]	
USA EXC *LAC		0.7878***			0.7854***	
		[0.0329]			[0.0323]	
USA EXC *<20 Workers			0.5973***			0.5867***
			[0.0270]			[0.0259]
USA EXC *20-49 Workers			0.4793***			0.4501***
			[0.0376]			[0.0360]
USA EXC *50-99 Workers			0.4102***			0.3829***
			[0.0429]			[0.0410]
USA EXC *100+ Workers			0.3491***			0.3311***
			[0.0741]			[0.0712]
Observations	933	933	933	937	937	937
Adjusted R-squared	0.66	0.68	0.69	0.68	0.68	0.71

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. All regressions include country dummies. USA EXC: industry/size job reallocation in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America.

Source: Own calculations based on harmonized firm-level database.

Table G.2: Job Flows and the Role of Labor Regulations (Difference-in-Difference Analysis)

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.1649***	0.1946***	-0.0217*	-0.0104	-0.006	0.0056
	[0.0278]	[0.0292]	[0.0113]	[0.0130]	[0.0113]	[0.0131]
USA EXC	0.6769***		0.8363***	0.6473***	0.8892***	0.8457***
	[0.0516]		[0.2100]	[0.0888]	[0.1267]	[0.0507]
USA EXC *EU		0.5542***				
		[0.0449]				
USA EXC *Transition		0.7208***				
		[0.0566]				
USA EXC *LAC		0.7893***				
		[0.1196]				
EPL	-0.0193***	-0.0192***				
	[0.0035]	[0.0035]				
USA EXC *EPL			-0.0279			
			[0.0322]			
USA EXC *EPL (Adj)					-0.0479*	
					[0.0225]	
USA EXC *EPL *EU				-0.0137		
				[0.0147]		
USA EXC *EPL(Adj) *EU						-0.0496***
						[0.0100]
USA EXC *EPL *Transition				0.0101		
				[0.0156]		
USA EXC *EPL(Adj) *Transition						-0.0270**
						[0.0119]

Continued on next page.

Table G.2: Job Flows and the Role of Labor Regulations (Difference-in-Difference Analysis) (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
USA EXC *EPL *LAC				0.0319*		
				[0.0190]		
USA EXC *EPL(Adj) *LAC						-0.0248
						[0.0185]
Observations	937	937	937	937	937	937
Adjusted R-squared	0.58	0.59	0.68	0.69	0.68	0.69

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. Columns (1) and (2) include region dummies. Columns (3)-(6) include country dummies. USA EXC: industry/size job reallocation in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America. EPL is the index of stringency of hiring and firing regulations. EPL (Adj) is the indicator of hiring and firing adjusted to take into account different degrees of enforcement of regulations (see main text).

Source: Own calculations based on harmonized firm-level database.

Table G.3: Job Flows and the Role of Labor and Product Market Regulations (Difference-in-Difference Analysis)

	(1)	(2)	(3)	(4)
Constant	0.0332***	0.0456***	0.0004	0.0490***
	[0.0128]	[0.0140]	[0.0081]	[0.0161]
USA EXC	0.8424***	0.8897***	0.8605***	0.8604***
	[0.0769]	[0.0436]	[0.1181]	[0.0464]
USA EXC *EPL(Adj)			-0.0619**	
			[0.0254]	
USA EXC *EPL *<20 workers	-0.0432***			
	[0.0137]			
USA EXC *EPL(Adj) *<20 workers		-0.0612***		-0.0696***
		[0.0100]		[0.0167]
USA EXC *EPL *20-49 Workers	-0.0653***			
	[0.0144]			
USA EXC *EPL(Adj) *20-49 Workers		-0.0846***		-0.0876***
		[0.0112]		[0.0226]
USA EXC *EPL *50-99 Workers	-0.0772***			
	[0.0148]			
USA EXC *EPL(Adj) *50-99 Workers		-0.0977***		-0.1140***
		[0.0119]		[0.0255]
USA EXC *EPL *100+ Workers	-0.0823***			
	[0.0178]			
USA EXC *EPL(Adj) *100+ Workers		-0.0980***		-0.0881**
		[0.0167]		[0.0433]
USA EXC *Bus. Reg. (Adj)			0.0342	
			[0.0320]	
USA EXC *Bus. Reg. (Adj) *<20 Workers				0.0245
				[0.0270]
USA EXC *Bus. Reg. (Adj) *20-49 Workers				0.0151
				[0.0369]
USA EXC *Bus. Reg. (Adj) *50-99 Workers				0.0385
				[0.0417]
USA EXC *Bus. Reg. (Adj) *100+Workers				-0.0074
				[0.0711]
Observations	937	937	937	937
Adjusted R-squared	0.71	0.71	0.68	0.71

Standard errors in brackets. *significant at 10%, **significant at 5%, ***significant at 1%. All regressions include country dummies. USA EXC: industry/size excess job reallocation in the United States. EU denotes the OECD European countries. Transition denotes the countries in Central and Eastern Europe. LAC denotes the countries in Latin America. EPL is the index of stringency of hiring and firing regulations. EPL (Adj) is the indicator of hiring and firing adjusted to take into account different degrees of enforcement of regulations (see main text). Bus. Reg. is the indicator of the stringency of business regulations; Bus. Reg. (Adj) is the same indicator adjusted to take into account different degrees of enforcement of regulations.

Source: Own calculations based on harmonized firm-level database.

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