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Dedication

I dedicate this dissertation to my love, Rabia. I would not have been able to complete this work without her support and encouragement.

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Finally, any mistakes within the dissertation I claim as my own.

Essays in Applied Econometrics

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This dissertation consists of three essays in applied econometrics that analyze the strategic interactions between individuals and institutions. The first chapter examines the relationship between employee benefits and the performance of startups. Using national longitudinal data on startups, I find that an increase in the share of employee benefits in total compensation packages leads to increased productivity of startups. Results indicate that a 10 percent increase in the share of employee benefits leads to an increase ranging from 1.5 to 3.9 percent in productivity even if the returns to the employee benefits are heterogeneous across startups. I also find that an increase in the share of employee benefits increases the chance of survival of startups.

The second chapter investigates the dynamics of employee screening and transitions from temporary to permanent employment. I analyze unique German data that contains specific information about the dynamics of the transition from temporary to permanent employment, I find that employers screen the abilities of employees only before they hire them. I find no evidence that employers screen the cognitive ability of employees during temporary employment.

The third chapter examines the relationship between housing prices and the availability of curbside parking. Using a policy change in Istanbul as a quasi-experiment, this chapter explores the effect of Istanbul's switch from informal and free curbside parking to formal and paid curbside parking on housing prices. In a differences-in-differences model coupled with a propensity score matching, we find that an exogenous change in the availability of parking leads to a statistically significant decrease in house prices. We estimate that house prices per square meter decrease by 13 percent in the neighborhoods where the city starts charging curbside parking spaces. However, rents stay the same compared to the other neighborhoods.

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Chapter 1

Effects of Employee Benefits on the Performance of Startups

1.1 Introduction

Do Human Resource Management (HRM) practices increase productivity of firm? Among these practices, what are the effects of a change in compensation practices, particularly employee benefits on productivity? Although it is believed that HRM practices have positive effect on productivity (e.g., Bloom and Van Reenen, 2011), the evidence is weak. Previous studies suffer from limited data and identification problems. There has been even less evidence about the impact of a change in compensation practices and employee benefits on productivity. Using data from a unique nationally representative sample of startups (the Kauffman Firm Survey (KFS)), I examine the effects of a change in the share of employee benefits in the total compensation package on the productivity of startups. I find that an increase in the share of employee benefits lead to increased productivity even if the returns to the employee benefits are heterogeneous across startups.

This study focuses on startups for two reasons. First, startups are the main driving force of net job creation in the U.S., even during the 2008 crisis (Haltiwanger et al., 2012). Second, startups are more likely to exhibit greater variation of compensation practices than the established firms. This allows precise estimation of the effects of employee benefits on productivity, and better allows us to observe how firms implement their employee benefits.

To examine the effects of employee benefits on productivity, first, I estimate a labor augmented Cobb-Douglas production function in which the share of employee benefits is incorporated with the two-step estimation approach designed by Black and Lynch (2001). This method allows me to address the biases that result from time-invariant unobserved startup heterogeneity and the endogeneity of capital per employee. Second, in order to avoid endogeneity problems that may arise because I use the share of employee benefits as an explanatory variable, in a control function approach, I use cross state and time variation in state income tax rates as an instrument for the share of employee benefits. Unlike conventional IV methods, the control function approach allows me to address the bias that results from the heterogeneous returns to employee benefits across startups. It allows to recover average treatment effect when startups select on the basis of unobservable heterogeneous returns. I find that a 10 percent increase in the share of employee benefits in the total compensation package leads to an increase ranging from 1.5 to 3.9 percent in productivity.

The present study makes three novel contributions to the literature. First, it is one of the few papers that examine the effects of HRM practices on the startup performance, and to my knowledge it is the only one that looks specifically at the effects of compensation practices and employee benefits on the productivity of startups. Second, novel estimation strategy successfully allows me to identify the effects of employee benefits on productivity. Third, it addresses most of the limitations in the previous work on the effects of HRM practices on the productivity of the firm such as unobserved heterogeneity, endogeneity of capital and employee benefits and heterogeneous returns to these benefits across startups.

1.2 Conceptual Framework

Before examining the effects of employee benefits on the productivity of startups, it is useful to discuss why we might observe increased productivity in the first place and if this is the case, why all other startups do not follow this pattern. Because wages and employee benefits are normal goods, there exists a point where an average employee is willing to exchange wages for benefits. Such an equilibrium point is determined by the valuations of wages and employee benefits of the average employee.[1] Because the employee benefits are not taxable but wages are,[2] an average employee might value a marginal increase in benefits more than an additional dollar in wages for some compensation mixes. For example, Royalty (2000) finds that a marginal dollar spent on an observable facet of health insurance is valued significantly more than one dollar in wages. Thus, total utility derived from the compensation mix would increase if the average employee values an additional dollar in employee benefits more than a dollar in wages when the total compensation (wages and costs of benefits) is constant and the share of employee benefits is increased.[3] Moreover, assuming the employees in startups have the same preferences for employee benefits as employees in established firms do, it is plausible to think that an average employee in a startup would value an increase in the share of benefits more than a wage because the average share of benefits is nearly 10 percent in the KFS sample with the maximum of 22 percent. On the contrary, established firms offer significantly higher shares of benefits in their total compensation packages. Figure 1.1 shows that small firms (with fewer than 100 employees) allocate 25 percent of their total

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[1] Woodbury (1983), Woodbury and Hamermesh (1992) construct prices for wages and employee benefits separately to analyze the trade-off between them.

[2] In this study I exclude the employee benefits, which are mostly taxable such as paid vacations, paid sick days.

[3] Carrington et al. (2002) show that non-discrimination laws limit the variation in the amount of employee benefits offered to employees. As a result, most of the benefits are offered to all employees. Thus, even if some employees do not value benefits, the net gain of offering benefits to all employees could be positive.

compensation for employee benefits while large firms (with more than 500 employees) allocate up to 35 percent. It is also possible that employees value an increase in benefits more than wages because they attach a symbolic value to them as in Akerlof's (1982) gift-exchange models and the social exchange theories in sociology literature (see Rhoades and Eisenberger, 2002). For example, Wagner and Harter (2006) find that employees who feel that their managers/supervisors care about them are more satisfied with jobs, less likely to quit, and work harder. The increased utility from a compensation mix with larger share of employee benefits creates a utility difference between the job and outside jobs with the same total compensation. This would increase the overall attractiveness of the job.[4] Thus, first, the startup could choose from a larger applicant pool, which could enable it to choose more productive employees (Ippolito, 2002). Second, the possibility of losing the more attractive job makes the employees put forth adequate effort (Shapiro and Stiglitz, 1984). Third, the startup could enjoy a lower job turnover rate, which would increase the efficiency (Allen and Clark, 1987; Gruber and Poterba, 1994). As a result, the productivity of the startup might increase because of an increase in the share of employee benefits in the total compensation.[5]

Although the objective of this study is to determine if employee benefits increase productivity, one might want to know why some startups do not offer such benefits to their employees if this is the case. Several explanations have been offered in the literature. First, owners/managers of some firms might have limited information about the advantages of employee benefits (Ichiniowski et al., 1997). For example, recent data analyzed by Fronstin and Helman (2000) show that 37 percent of small business owners are unaware that the

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[4] The distinction between attractive jobs and others is similar to the "dual-labor market theory" by Doeringer and Piore (1971).

[5] Some studies consider the changes in the HRM practices as a "good management" practice, and thus a partial technological change (see Bloom and Van Reenen, 2011). If this interpretation is true, we would again expect to see an increase in productivity.

federal government does not tax health insurance when employers provide it to their employees. Because 55 percent of the owners in the KFS sample have no previous entrepreneurial experience and 25 percent have less than 5 years of overall experience, it is possible that they have limited information about the advantages of employee benefits.[6]

In addition, a survey of 220 "INC 500 Businesses" (an annual list of 500 fastest growing private company in the U.S.) shows that 51 percent of the firms had no business plans when they started the company (Shuman and Seeger, 1986). This suggests that many entrepreneurs have limited information and planning when they start their businesses, including their strategy for employee benefits.

Second, like any other endogenously chosen organizational practice, it might take time to determine the best use of employee benefits. Owners/managers of the startups might observe the related industry and its practices, or experiment with employee benefits. Also, based on an employer survey, Salisbury and Ostuw (2000) show that uncertainty about the steady flow of revenue and the chances of survival are major factors that prevent firms from offering benefits. Thus, the likelihood of offering employee benefits increase with the age of the firm. Figure 1.2 supports this possibility that the median share of employee benefits among startups in the KFS sample has increased from 2004 to 2011.

Third, there are significant differences in employee benefits across industries and firm sizes. Figure 1.3 shows the variation of share of benefits in the total compensation across various industries. There are two main reasons for this variation: first, the expected returns from employee benefits, and second, the costs of benefits, vary across industries and firm sizes. For example, the food services industry (NAICS code: 72) is one of the

[6] There are significant differences between the benefits offered by startups run by owners with previous entrepreneurial experience, on the one hand, and no entrepreneurial experience, on the other. Among the firms with startup capital of less than 100K, the owners with previous startup experience offer on average 2.27 benefits while others offer only 1.66 benefits. Also, owners with more than 5 years of experience offer 1.97 benefits while others offer only 1.37 benefits.

worst industries in terms of benefits because most of the firms hire part-time employees and mostly pay minimum wage, which leaves no room for employee benefits. Also, Figure 1.1 shows the average shares of employee benefits for different firm sizes. There are significant differences between big firms, medium firms, and small firms. This is because of the different costs for different sizes of firms. For example, economies of scale allow big firms to buy cheaper insurance policies than small firms; also their administrative costs are lower.

1.3 Literature Review

Employee benefits have become a crucial part of compensation packages. Starting from 5 percent in the 1950s, it has reached to more than 30 percent of total compensation. Moreover, the increase has not resulted from a decrease in real wages nor has it been dominated by a specific type of firm. The real financial value of benefits has increased steadily and the increase is common to different types of firms. Figure 1.4 shows the increase in the real value of employee benefits for three different firm sizes between 1990 and 2013. For the large firms, the real hourly cost of benefits has increased from $6.12 to $8.68; for medium sized firms it has increased from $3.82 to $5.21; and for small firms the change is from $3.31 to $3.71. Yet, the subject has not attracted the attention it deserves from the economics literature. Previous studies focus on employee preferences on benefits and sorting based on the employee benefits, but not on the possible effects on the performance of the firms (see Oyer, 2008; Royalty, 2000).

A handful of studies analyze the effects of some benefits on labor outcomes, such as job turnover, absenteeism, and satisfaction (see Allen and Clark, 1987; Madrian, 1994; Gruber and Poterba, 1994; Dale-Olsen, 2006; Saltzstein et al., 2001). Although the changes in these labor outcomes might help to increase productivity of the firm indirectly, my approach is to estimate the overall effect on productivity directly by estimating a production function similar to Bartel (1994), Dearden et al. (2006).

A closely related study to mine is Dale-Olsen (2007). Using Norwegian data, he finds that fringe benefits and productivity are positively correlated. The present study differs from his paper in several aspects. First, he assumes homogenous returns to fringe benefits. However, if the employer believes that the expected benefits outweigh expected costs, then he might offer these benefits. Because the costs and the expected benefits are quite different across different-sized firms, it is not plausible to assume homogenous returns. Instead, with a control function approach, I address the bias that results from the heterogeneous returns to benefits by modeling the decision of offering employee benefits. Second, the fringe benefits that he considers are quite different than the ones I consider in this paper: the most common benefits in the U.S., such as health insurance, retirement, life insurance, disability benefits have already been offered by nearly all firms in Norway. This explains why he uses other types of benefits like extended paid sick days or vacations. On the contrary, I exclude such benefits because they are not tax exempt. Third, where I focus on startups, Dale-Olsen (2007) focuses on larger firms with at least 10 employees.

The present study builds upon the few studies that examine the HRM practices in startups. In the literature, Doeringer et al. (1997) examine the adoption of Japanese HRM practices by American startups to understand what type of startups are more likely to implement these practices. Similarly, Litwin and Phan (2013) examine the likelihood of offering health insurance and retirement plan for startups. However, neither of these studies examines any effects of HRM practices on startup performance.

The present study also contributes to the general understanding of the effects of HRM practices on productivity. In that sense, it builds upon the seminal papers including

Huselid (1995), Huselid and Becker (1996), Cappelli and Neumark (2001), Black and Lynch (2001). Although these studies address important biases in the estimation, none of them address the biases that result from the endogeneity of HRM practices and heterogeneous returns to these practices. In this sense, the present study differs from them. In addition, I specifically focus on employee benefits, which have not been analyzed in the literature in the context of HRM practices.

One related paper to mine in terms of econometric methodology is Bauer (2003). Using German data, he uses a similar control function approach to estimate the effects of High Performance Work Organizations (HPWO) practices on productivity. He uses the incidence of HPWO practices in the U.S. as an instrument for the similar HPWO practices in Germany. However, unlike this paper, his instrument in the control function approach does not explicitly help to model the selection process into the implementation of such practices in addition to the cross-country differences.

1.4 Data

This paper uses the longitudinal data from the Kauffman Firm Survey (KFS).[7] The KFS is the biggest source of information about startups in the U.S. It provides detailed financial information including compensation, capital and revenues. The survey started in 2004 and it has seven annual follow-ups. To be eligible as a startup, KFS requires firms not to have an employee identification number (EIN) or a schedule C income or a legal form before 2004. The eligible business should not have paid unemployment insurance or federal taxes before 2004. The business should also have fulfilled at least one of these criteria in 2004. 4,928 startups responded positively to the data collection process with a

[7] For more information about the Kauffman Firm Survey, see Ballou et al. (2008).

weighted average response rate of 43 percent. The response rate is reasonably higher than many studies in the HRM literature (see Huselid and Becker, 1996). In addition to the higher response rate, it is a representative sample of startups in the U.S., unlike previous studies, which are mainly based on specific industries or firms.[8] The panel structure of the sample makes it possible to address econometric problems such as unobserved heterogeneity. Because most of the startups are prone to fail, the attrition bias might cause problems in the estimation. To deal with this problem, I use the estimation approach in Olley-Pakes (1996) in Section 1.7.2.

During the seven follow-ups, 44 percent of the startups have exited the sample either because of firm closures, mergers, acquisitions or simply because they have refused to participate at one point. Refusal rate is about 14 percent during the follow-ups. Probit analysis of the characteristics of the startups that refuse to respond exhibits no significant pattern with respect to their industries. In the seven years since 2004, 26 percent of the firms permanently closed; four percent of them have been acquired or merged with another firm.

After removing the firms that refused to answer at one point, there remain 19,795 observations. I drop the observations with missing revenue, capital, and number of employees. Because the focus of this paper is employee benefits, I also remove the firms with zero employees. Of the remaining observations, 1,496 observations are for the exit periods of the firms. At the end, the analyzed sample has 5,772 remaining number of observations of 1,577 startups. To calculate the per capita variables such as average

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[8] The KFS oversamples the high technology firms. The largest industry, with nearly 25 percent of the total, is the professional, scientific, and technical services (NAICS code: 54); the second largest industry is the retail trade with nearly 11 percent (NAICS code: 44-45); the third largest industry is manufacturing with nearly 10 percent. (NAICS code: 31-33) All other industries comprise less than 10 percent. Therefore, sampling weights provided in the dataset are used in the estimation to take the oversampling of high technology firms into account.

revenue, cost, and capital, I only consider the full-time employees. The omission of part-time employees should not affect the results because it is already rare to offer benefits to part-time employees, especially in startups. For example, only in 372 out of 5,772 observations in the KFS health insurance is offered to part-time employees. Also, the average number of part-time employees per startup is close to one, which would limit the effect of their omission from the analysis. The present analysis uses the unbalanced data in the sample. Missing variables do not seem to cause significant differences in the distribution.

In this paper, I use the confidential version of the KFS, which is available to researchers through a secure data enclave provided by National Opinion Research Center (NORC). To assign some characteristics of the owners to the firm such as education, experience, gender, and race, I use two different options. In the first option, the primary owner who has the largest share gives his characteristics to the firm. In the second option, I use the weighted average of relevant variables among the owners. Results are similar and I only report the first case.

Following Haltiwanger, Lane, and Spletzer (1999), I use log of revenue per employee as a proxy for labor productivity as the dependent variable in the analysis. I adjust other financial variables including compensation (wages and costs of benefits), capital etc. per employee as well.[9] For the main explanatory variable in the regression, the KFS provides eight main separate binary variables of benefits with an additional column

[9] Foster, Haltiwanger, and Syverson (2008) find that the revenue based productivity calculation, which is the most common approach in the literature, might understate the degree of dispersion, particularly for newer firms.

of other benefits. The main benefits are: health insurance, retirement, paid sick days, paid vacations, stock options, bonuses, tuition reimbursements, and flexible schedule.[10]

I also decompose other benefits and extract the life insurance, short and long term disability benefits to use in the estimation. Following Frazis and Loewenstein (2013), I impute the costs of employee benefits by using Employee Compensation Index (ECI) of National Compensation Survey (NCS). ECI is based on a large survey of 18,000 occupations in 4,500 private non-farm firms. ECI is a weighted sum of compensation costs including wages and various benefits separately for different industry and firm sizes. In the present analysis, the average share of the costs of given benefits in the total compensation for given firm size, and industry is retrieved from ECI data and assigned to the binary variables indicating whether that specific benefit is offered to the employee. This way, I create the share of employee benefits in the total amount of compensation for each firm-year observations and therefore the value of benefits are comparable to value of wages.[11]

The instrument, the state income tax rates for the average employee in the KFS sample, is retrieved from the NBER's Taxsim database. The instrument is based on the average employee's income in the KFS sample, and it varies across states and time.[12]

1.5 Econometric Model

To analyze the impact of employee benefits on the productivity of startups, I estimate the following labor augmented Cobb-Douglas production function:

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[10] Because paid sick days and paid vacations are tax exempt from the employee's income, I exclude them from the estimation. Flexible schedule is also excluded because it is not possible to quantify its financial value.

[11] A similar approach to use share of benefits as a part of total compensation is used in Wadhwani and Wall (1991), Dale-Olsen (2007), Woodbury (1983), Woodbury and Hamermesh (1992).

[12] For more information please check: http://users.nber.org/~taxsim/} I present the detailed information about the variables in Table 1.1.

$$Y_{it} = \beta + \gamma k_{it} + \delta X_{it} + a_i S_{it} + u_{it} \tag{1.1}$$

where Y_{it} is the log of revenue per employee, k_{it} is the log of capital per employee, and S_{it} is the share of employee benefits in the total compensation. In addition to the total compensation (wages and cost of benefits), vector X_{it} includes explanatory variables related to the primary owner and the characteristics of startups. Subscript i is used for startups; subscript t is used for time periods. I also include state and industry variables in the regression.

1.5.1 Homogenous Returns

In this section, I suppose that the returns to employee benefits are constant across startups. Even if I make this assumption, estimating the equation (1.1) is challenging due to several potential biases. These biases might arise through unobserved time-invariant startup heterogeneity, endogeneity of capital per employee, exit of inefficient startups, and measurement error. If I take the first differences of equation (1.1), the bias caused by the time-invariant startup heterogeneity would be removed. However, this would also remove the important time-invariant characteristics of the startup and its owners. Therefore, I follow the literature and employ the two-step approach described in Black and Lynch (2001). Briefly, I estimate the production function in step one with fixed effects and then calculate the total factor productivity. Then, I regress the averaged component of this residual over time on the employee benefits. In the first step, I estimate the production function in two different ways. The first approach is the within estimation, which addresses the bias that results from the time-invariant unobserved heterogeneity; the second approach is to use lagged values of capital per employee as instruments in the GMM estimation, which additionally addresses the bias that results from the endogeneity of capital per employee and measurement error. I also include year-industry dummies to the estimation that would allow for different trends for each industry.[13] Then I generate the following predicted values:

$$\hat{Y}_{it} - \hat{\gamma} k_{it} = \beta + \delta X_{it} + a_i S_{it} + u_{it} \tag{1.2}$$

Then I average this value over the years to have an estimate of the time-invariant residual for each startup.

In the second step, I project this average residual on the share of employee benefits, total compensation (wages and costs of benefits) and other characteristics of the startup in addition to the macroeconomic indicators and year-state-industry dummies.

Unobserved startup heterogeneity might create a bias if firm-specific time-invariant part of the error, u_{it}, is correlated with the explanatory variables such as capital per employee, k_{it}. For larger and established firms, the bias is expected to be upwards. However, for startups, which have lower levels of capital per employee at the beginning because of the limited time for capital accumulation through time, the direction of the bias is a priori unclear. It is possible that startups with more-than-average capital recruit better managers and better employees, which would lead to more productivity, thus upward bias. On the other hand, the managers of startups with less-than-average capital may be more risk-seeking and creative than the average manager, especially in management practices, and this could lead to more productivity. This would create a downward bias.

[13] Related studies that estimate the impact of Human Resource Management practices on productivity such as Bloom and Van Reenen (2007) also follow this approach.

Endogeneity of capital per employee can also cause bias for the estimates. Successful firms that accumulate more capital could create upward bias. It is less likely that startups facing negative shocks increase their capital. Thus, we expect a positive bias. However, it has been shown that measurement error in capital could also create downward bias (see Black and Lynch, 2001). Because I construct the measure for capital stock, a significant measurement error is possible. As a result, direction of the overall bias is a priori unknown.

Although the two-step approach addresses the biases that result from the unobserved time-invariant heterogeneity and endogeneity of capital, it does not address the bias that results from the endogeneity of employee benefits in the second step. As Nickell, Nicolitsas, and Patterson (2001) show that firms facing negative productivity shocks are more likely to take risks and more likely to change organizational structure. This would create a downward bias for the estimates of employee benefits. It is also possible that a positive productivity shock might increase the likelihood of implementing a change in compensation practice. However, to my knowledge, there is no evidence of this in the literature. I am going to address the endogeneity of employee benefits in the next section.

1.5.2 Heterogeneous Returns

In this section, I relax the assumption of homogenous returns to employee benefits. Such relaxation is necessary because expected returns and costs for changing employee benefits policies are quite different across startups. There are several reasons for this difference. First, Datta, Guthrie, and Wright (2005) point out that the implementation of HRM practices significantly depend on various industry characteristics, which creates a great variation of HRM practices across industries. This finding is consistent with Figure

1.3, which displays the differences in terms of the average share of employee benefits across various industries. Second, economies of scale make the costs for employee benefits significantly different between big firms and small firms not only in terms of cheaper policies, but also through lower administrative costs. For example, Figure 1.5 shows that the average premium cost for health insurance is significantly different for firms with fewer than 10 employees. Third, there can be tax advantages or regulations in various states that changes the cost of offering employee benefits. Fourth, competition and norms about fairness as shown by Akerlof and Yellen (1984) in a given industry or state could change the expectations toward benefits. That is why expected returns and costs for employee benefits could have a great variation for startups causing the coefficient a_i not to estimate the average returns for all startups. Owners select into different degrees of employee benefits based on the heterogeneous returns to employee benefits.

To illustrate this, consider the coefficient, a_i from equation (1.1). If we relax the assumption of homogenous returns we can write it as α + ψ_i where α = E(a_i). Then we can rewrite the equation as:

$$Y_{it} = \beta + \delta X_{it} + \alpha S_{it} + \overbrace{u_{it} + S_{it}(\underbrace{a_i - \alpha}_{\psi_i})}^{error_{it}} \tag{1.3}$$

If startups with above average returns, when ψ_i> 0, offer more benefits then we would expect an upward bias. However, if many startups with below-average returns offer more benefits only because of lower costs, changes in tax advantages or negative shocks, we would observe a downward bias. Depending on which of these groups dominate the sample, heterogeneity in returns to employee benefits could lead to upward or downward bias.

Conventional IV estimates would be generally inconsistent and fail to identify the average return to employee benefits if E($S_{it}\psi_i|z_{it}$) depends on z_{it}, where z_{it} is the instrument for employee benefits. To solve this problem, I follow a control function approach. It allows to recover average treatment effect when startups select on the basis of unobservable heterogeneous returns. On the contrary, conventional IV method would be able to recover only LATE for a specific subsample that comply with the assignment caused by the instrument. It is not possible to identify which startups comply and which do not.

1.5.2.1 Identification

If the choice of employee benefits is based on firm-specific expected returns, which are unobservable to the econometrician, then the nature of the selection into the employee benefits should be taken into account explicitly. Basically, it means that conditional mean of the error, u_{it} and unobserved heterogeneity in returns, ψ_i need to be modeled and added into the productivity estimation in (1.1).

Following Blundell and Costa Dias (2009), I suppose that the choice of employee benefits depends on the information summarized by the observables z_{it}, S_{it}, X_{it} and the unobservables v_{it} as follows:

$$S_{it} = f(z_{it}, X_{it}, v_{it}) \tag{1.4}$$

If S_{it} is continuous and decision rule *f(.)* is known and invertible, then S_{it}, X_{it}, z_{it} are sufficient to identify v_{it}, which means that conditioning on v_{it} would be the same as conditioning on observables. For consistent estimation, the following assumptions are needed:

A.1. The instrument, z_{it}, state tax rates for the average employee in the KFS, should affect the decision to change the share of employee benefits in the total compensation package. Such a relationship has been documented in the literature. Woodbury and Hamermesh (1992) show that the demand for benefits is responsive to changes in tax price of benefits. In addition, Long and Scott (1982) show that increasing tax rates lead to the increased use of non-wage compensation. Gentry and Peress (1994) give supporting evidence that when employers decide to offer benefits, they respond to tax incentives. Finally, Royalty (2000) finds that an increase in the tax rates increases the chances that the employee is offered health insurance. Table 1.2 presents evidence in favor of the relationship between state tax rates and the share of employee benefits. Results from the first stage regression indicate that a 10 percent increase in state tax rates is associated with a statistically significant modest 0.3 percent increase in the share of employee benefits.

A.2. Conditional on v_{it}, S_{it} would be exogenous in (1.3). This means that after controlling v_{it}, we would have all information about the firm's decision process on offering benefits. This assumption is the main difference between conventional IV and the control function approach.

A.3. Conditional on v_{it}, z_{it} is independent of u_{it} and ψ_i. This is the usual exclusion restriction. As Blundell and Costa Dias (2009) shows the following conditions are sufficient for assumptions A.3. and A.2.

$$E[u_{it}|v_{it}, S_{it}, X_{it}, z_{it}] = E[u_{it}|v_{it}] = h_u(v_{it}) \tag{1.5}$$

$$E[\alpha + \psi_i|v_{it}, S_{it}, X_{it}, z_{it}] = \alpha + E[\psi_i|v_{it}] = h_\psi(v_{it}) \tag{1.6}$$

Exclusion restriction assumption requires that the state tax rates should only affect the productivity through its effect on the share of benefits. However, Royalty (2000) warns that the unobservable state specific factors could be correlated with state tax rates. If startups that consider employee benefits crucial, establish in states with favorable tax laws for employee benefits, then the exclusion restriction might fail. To circumvent this problem, I add state fixed effects, state level unemployment rates and state's unemployment benefits to the estimation to capture the unobservable state characteristics that might be correlated with state tax rates. Additionally, following Royalty (2000), I compare the effect of state tax rates on the likelihood of receiving tax exempt health insurance to the not tax exempt paid sick days to see if the state tax rates on other unobservable state characteristics determine the likelihood of receiving these benefits. Results in Table 1.3 confirm that state tax rates are associated with health insurance but not with paid sick days. This confirms that controlling state fixed effects is sufficient for consistent estimation.

In addition to the unobservable state characteristics, if the income tax rate faced by the average employee is correlated with other taxes that affect input prices such as capital, exclusion restriction might fail. However, Besley and Rosen (1999) show that consumers bear the burden of the taxes that affect input prices. For some commodities, the after-tax price increases exactly by the same amount of tax. Taxes on other commodities are over-shifted; that means a $1 increase in tax leads to more than a $1 increase in its price. Chouinard and Perloff (2004) support this finding. They find that consumers bear the entire burden of state-specific gasoline tax changes. So, even if there is some relationship between income tax rates and other taxes that might affect input prices of the firm, the cost of this increase is reflected to the consumers. If firms had believed that their sales would be affected when taxes cause an increase in prices, thus revenues, they would have covered some portion of the costs of the tax. Based on the evidence, therefore, it is likely that tax rates affect the dependent variable only by its effect on employee benefits.

1.5.2.2 Implementation of the Model

Consider this equation:

$$E[Y_{it}|\upsilon_{it}] = \beta + \alpha S_{it} + S_{it}E[\psi_i|\upsilon_{it}] + E[u_{it}|\upsilon_{it}] \quad (1.7)$$

Briefly, in the control function, E[u_{it}| υ_{it}]=E[$u_{it}|S_{it}$, z_{it}, X_{it}] is explicitly included in the regression function and the return ψ_i is specific as a function of (z_{it}, X_{it}, S_{it}). We can estimate it in a 2-stage process. In the first stage, S_{it} is regressed on all observable variables (X_{it},z_{it}) and the residuals $\widehat{\upsilon_{it}}$ is obtained. In the second step, I add residuals and their interaction with S_{it} in the regression. Conditional on these assumptions, the coefficient α would is expected to reflect the average effect of employee benefits on productivity where returns are heterogeneous.

To illustrate the implementation, assume that the S_{it} is written as follows:[14]

$$S_{it} = \mu_0 + \mu_1 X_{it} + \mu_2 z_{it} + \upsilon_{it} \quad (1.8)$$

Then the conditional mean for the error, u_{it} and the unobserved heterogeneity, ψ_i can be written as:[15]

[14] As Dustmann and Meghir (2005) state, I do not need to assume linearity for identification if the instrument takes at least many discrete values so that it satisfy rank condition, and the conditional means only depend on residuals, not on instruments or variables.

[15] Similar to Dustmann and Meghir (2005), I do not assume any functional form for residuals. They are only derived from the OLS regression.

$$E(u_{it}|z_{it}\upsilon_{it}) = \rho\upsilon_{it} \qquad \mathrm{E}(\psi_i|\mathrm{z_{it}}\upsilon_{it}) = \xi\,\upsilon_{it} \tag{1.9}$$

And they can be plugged in equation (1.7). Then, I test if S_{it} is indeed endogenous by an F-test of ρ = 0, ξ = 0 It's rejected. Based on this, coefficients ρ and ξ allow us to analyze the importance of omitted variable bias and the selection bias in OLS estimates.

1.6 Results

In this section, I present the econometric results concerning the effects of employee benefits on the performance of startups.

1.6.1 Who Offers the Employee Benefits?

In Section 1.5, I discuss various sources of bias for the estimates. The direction of the overall bias a priori is not clear. Thus, I start investigating the effects of employee benefits on the performance of startups by examining the characteristics of startups that offer employee benefits to their employees. I conduct a probit regression using the binary variable indicating whether the startup offers any employee benefits as the dependent variable. In what follows, I describe the independent variables used in the regression. The decision to offer employee benefits depends on the characteristics of primary owners.[16]

Educated or experienced primary owners may know the advantages of employee benefits better. In section 1.2, I present the preferences towards benefits between these different groups of owners with or without education and experience. To capture the effect of these owner characteristics on the likelihood of offering employee benefits, I add

[16] The majority of startups in the sample are owned by a single owner. Hence, the characteristics of the primary owner are important determinants for the decision to offer employee benefits.

dummy variables indicating whether the primary owner is college educated and if he has previous entrepreneurial experience in addition to the standard experience variable.

Moreover, in a related literature, it has been shown that firms using modern information and high technology are more likely to implement organizational changes (see Caroli and Van Reenen, 2001; Athey and Stern, 1998). To see if high technology startups are more likely to offer employee benefits, I add a count variable showing the total number of copyrights, trademarks, and patents of the startup.

The performance and resources of the startups are other factors that might affect their decisions to offer benefits. More resourceful startups such as high technology startups in Silicon Valley may take the risks easily and experiment on different compensation practices including the changes in benefits. Or, primary owners who believe that their startups have competitive advantage in the industry, hence, would be successful in the future could consider offering benefits more seriously. To control these effects, I add variables showing the capital per employee, total compensation per employee (wages and costs of benefits), and a dummy variable showing whether the startup believes that it has competitive advantage in its industry.

Finally, I add two macroeconomic indicators to capture the possibility that the startup might need to offer a comparable compensation package to compete with other firms in the given industry to attract better qualified employees: state unemployment rates and average total compensation in the given industry. Similarly, if the state unemployment rate is already high, a startup might not need to offer a competitive compensation package.[17]

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[17] Because the positive correlation between firm size and the employee benefits is a well-documented fact in the literature (see Oyer, 2008) I do not consider as a separate explanatory variable in the estimation to focus on the per employee variables.

Table 1.4 presents the marginal effects from a probit estimation. The dependent variable is a dummy variable indicating whether the startup offer any employee benefits. Column (1) reports the results for the whole sample; columns (2), (3), and (4) are for the subsamples of corporations, proprietorships, and family firms, respectively. The coefficients displayed are the marginal effects evaluated at the sample averages of the explanatory variables on the probability of receiving any benefit if the explanatory variable increases by one. Column (1) of Table 1.4 confirms that resourceful startups are more likely to offer employee benefits. Capital per employee has statistically significant effect on the probability of offering an employee benefit. Increasing the capital per employee by 10 percent increases the likelihood of providing any benefit by 1.9 percentage points. Also, an increase in the overall spending on compensation correlates with the possibility of offering at least one benefits. A 10 percent increase in the total compensation leads to 2.7 percentage points increase in the likelihood of offering any benefit. If the startup believes that it has the competitive advantage in the industry, the likelihood of offering any benefit increases by 6 percentage points. This result is consistent with the employer surveys. If the firm is confident about its survival and its performance, which is captured by the competitive advantage variable, then it might start considering changing its compensation practices including the decision to offer employee benefits. Results in Column (1) confirm that the primary owner's education and experience are positively associated with the likelihood of offering any benefits. Being a college educated primary owner increases the probability of offering any benefit by 4.7 percentage points; increasing the experience of the primary owner by 10 years increases the probability by 2 percentage point. Owners with previous entrepreneurial experience are 2.7 percentage points likelier to offer benefits than others.

These results have important implications regarding the analysis of the effects of employee benefits on the productivity of the startup. First, results indicate that college educated primary owners are more likely to offer benefits. If these owners are also successful managers, we would expect an upward bias in the estimates for share of employee benefits in the total compensation. Yet, it is also possible that there is no clear association between college education and productivity. Experience of the owner, which could be a clearer indicator of success in the business, has a smaller impact compared to the education on the likelihood of offering benefits. Also there is some evidence for the learning process of owners. The results show that owners who have previous entrepreneurial experience are more likely to offer employee benefits. Second, competitive advantage has the largest coefficient in the probit estimation showing that confidence which can be an indicator for future success of the business, has the largest association with benefits. This shows that startups offer employee benefits depending on their future expectations for the startup. This suggests that heterogeneous returns to the benefits across startups can cause important bias in the estimation.

1.6.2 Effects of Employee Benefits on the Productivity of the Startup?

In this section, I present the econometric results concerning the effects of employee benefits on the productivity of startups. Table 1.5 presents the estimation results of the production function. Columns show the estimates with different methods to address various biases. In all columns, the dependent variable is the log of revenue per employee.

The first column presents the results of OLS estimation as a starting point to examine the importance of potential biases. Results indicate that employee benefits have statistically significant impact on productivity. A 10 percent increase in the share of employee benefits in the total compensation package increases the productivity by 1.6 percent. Capital per employee has an impact with a similar magnitude; total compensation (wages and costs of benefits) per employee seem to have a more pronounced impact on productivity than other factors. A 10 percent increase in total compensation per employee is associated with a six percent increase in productivity. This result is consistent with the efficiency wage literature in which higher wages are correlated with higher productivity. Experience and the age of the primary owner have small impacts on productivity, which suggest that the unobservable time-invariant heterogeneity in startups cause a minor bias, if any.

To check whether the unobserved time-invariant factors have significant impacts on the productivity, I follow the two-step method to estimate the production function designed by Black and Lynch (2001). In this approach, first, production function is estimated with fixed and quasi-fixed effects; the calculated firm specific average residual is then regressed on the share of employee benefits and other explanatory variables affecting the production. Estimation results are presented in Column (2). Almost all coefficients are similar to the OLS estimates, which indicate that the unobservable heterogeneity has a minor effect on the estimates. However, the coefficient on the share of benefits indicates that the OLS estimate is slightly downward biased, which suggests that less successful startups may experiment with employee benefits to solve productivity problems in the startup.

Although the fixed effects approach in the first step addresses the bias that results from the time-invariant unobserved heterogeneity, it does not address the possible bias that results from the endogeneity of the capital per employee. On one hand, successful startups could build up more capital and if capital and revenue per employee are determined simultaneously, there could be upward bias. On the other hand, measurement error in capital per employee could lead to downward bias (Black and Lynch, 2001). Results show that the coefficient for the capital per employee is indeed downward biased, which is consistent with the literature. However, estimates for the share of benefits are not biased. Hansen-Sargent over-identifying restrictions are not rejected by the data, which suggests that there is no misspecification in the model. Estimates from the GMM estimation for capital per employee are significantly larger than its counterparts from the within estimation. This suggests that the bias that results from the measurement error dominates the bias that results from the unobserved time-invariant heterogeneity.

Although the methods designed by Black and Lynch (2001) attempt to address potential biases in the first step, there could be additional bias in the estimates of share of employee benefits due to endogeneity in the second steps. In addition, great variation across startups over various industries and different expectations from employee benefits could cause severe biases in the estimation. In what follows, I focus on the possible biases that result from the endogeneity of employee benefits and heterogeneous returns to these benefits.

I follow the control function approach described in Garen (1984), Blundel and Costa Dias (2009). As the instrument for the share of benefits, I use the cross state and time variation in state income tax rates for the average employee in the KFS sample. Table 1.2 reports the results from the first stage and reduced form estimation. Results indicate that state tax rates cause a significant variation in the share of employee benefits. A 10 percent increase is associated with a 0.3 percent increase in the share of benefits. Column (4) of Table 1.5 presents the results with the control function approach. Results show that employee benefits have a statistically significant impact on the productivity of the startup. A 10 percent increase in the share of benefits lead to a 3.9 percent increase in productivity. The magnitude is also economically meaningful. For example, on average, health insurance premium costs are nearly $5,000 for a small startup. This constitutes 4.5 percent of the total compensation costs. Offering health insurance would increase the average revenue per employee by $11,000 according to the results in Column (4).[18] Results from the control function approach indicate that OLS estimates are largely downward biased. This suggests that the startups that offer benefits mostly do this not because of higher expected returns, but because of other factors. These factors could be decreases in costs or following trends in the industry because of competition. Nevertheless, I do not rule out the possible measurement error in the share of benefits. Because I use industry averages for different firm sizes, if costs are lower in startups then results would be downward biased because of this.

1.6.3 Depression Times

The overall economic environment could affect the implementation of employee benefits into the compensation practices. Owners' confidence for success could decrease during crisis times. Additionally, a negative productivity shock could force the firms to change some of their practices. For example, Dunne, Haltiwanger, and Troske (1997) find that reorganization practices of the workforce in a firm are concentrated in recessions. Hence, different preferences towards employee benefits during recessions could affect the estimation results. To examine whether the return to employee benefits have changed after the recession, I create a binary variable dividing the sample into two groups: between 2004 and 2008, and between 2008 and 2011. Table 1.6 presents the results of the estimation. The first column reports the results of the OLS estimation; the second column reports the results

[18] The magnitude is similar to the previous findings. For example, Huselid and Becker (1996) find that one standard deviation increase in employee skills and organizational structure leads to an approximate $ 4,000 increase in the annual cash flow and $15,000 increase in the market value.

of the two step approach by Black and Lynch (2001); the third column reports the results of the control function approach.[19] With nearly all methods, results suggest that the returns to employee benefits are significantly lower after the 2008 recession. This finding is consistent with the Dunne et al. (1997). If the startups experiment with employee benefits when they face a negative productivity shock, then the ones which increase the number of benefits would be the ones with largest declines in productivity. Because the effects of 2008 recession continue for several years, it is reasonable to see a decrease to the returns to the employee benefits.

1.6.4 Survival

Bloom and Van Reenen (2011) indicate that human resource management practices are positively correlated with survival rates of the firms. To check whether employee benefits increase the chances of survival of startups, I conduct a survival analysis. Because the observations are recorded annually the estimation is on grouped data. Also, the sample only includes observations from the first eight years the KFS. That is why it is right censored. Because of these two limitations, I use discrete time event history model instead of a variant of Cox estimation. Table 1.7 presents the results. The dependent variable is the dummy variable: whether the startup exits from the sample in the given year because of failure. Displayed coefficients are the marginal effects evaluated at the sample means of the explanatory variables on the probability of failure if the explanatory variable increases by 1 unit conditional on the survival in the previous year. The first column shows the results for the whole sample; the second column shows the results when the primary owner is

[19] Because the sample size is quite small, I only consider the fixed effects version of the Black and Lynch (2001).

male; the third column is when the firm is a corporation; the fourth column is for the proprietorships; and the fifth column is for family firms. Results indicate that employee benefits decrease the likelihood of failure of startups. A 10 percent increase in the share of employee benefits in the total compensation decreases the likelihood of failure by 3 percentage points. Startups with large capital per employee are also less likely to fail. In that case, a 10 percent increase is associated with 5 percentage points decrease. Results suggest that the productivity estimates in Table 1.5 might be upward biased because employee benefits also increase the chances of survival for startups. I address this problem in the next section.

1.7 Robustness

1.7.1 Smallest Startups

Oyer (2008) shows that larger firms are more likely to offer employee benefits due to the scale of economies. Figure 1.1 supports this finding by showing that larger firms offer more benefits than smaller firms. If the large startups in the KFS sample are also more productive, then the estimation results could be dominated by these startups, which would create an upward bias. In this section, I consider only startups with fewer than 10 employees ever to check this possibility. Table 1.8 presents the estimation results. The first column reports the results of the OLS estimation; the second and third columns report the results of the two step approach by Black and Lynch (2001); the fourth column reports the results of the control function approach. A similar pattern can be observed when the estimates are compared to Table 1.5, which suggests that the estimates are not dominated by large startups.

1.7.2 Attrition

Because 26 percent of the startups in the KFS sample have been permanently closed, attrition bias might be a serious problem for the estimation. If the startups with larger capital per employee survives despite lower productivity, and if more resourceful startups are more likely to offer employee benefits, coefficients for k_{it} and S_{it} would be downward biased. However, as described in section 1.6, if employee benefits increase the chances of survival for startups, coefficient of S_{it} would be upward biased. To solve this problem, I follow Olley and Pakes (1996). The idea is similar to the two-step approach by Black and Lynch (2001) in the previous section. First, I estimate the production function in step one considering the endogeneity of capital per employee and the exit of low productivity startups. Then, in the second step, I regress the average residual obtained from step 1 on the share of employee benefits along with all other explanatory variables. In the first step, endogeneity is addressed by using investment as a proxy to time-varying unobservable productivity component and predicted survival probabilities as a proxy for attrition. To do this, I regress Y_{it} on a higher order polynomial of capital and investment. I obtain the estimated polynomial. Then, I estimate a probit of survival dummy on that higher-order polynomial consisting of capital, investment and their cross terms. Finally, I add a polynomial that is joint with the predicted probability obtained in the previous step, and the lagged estimated polynomial in the first step. Because I need to use the same coefficient for the capital from the first step, the main regression equation turns into a non-linear regression.

I present the estimation results in Table 1.9. Results indicate that attrition do not create significant bias for the conventional estimates of S_{it}, but as indicated, estimates for capital per employee seem to be downward biased.

1.7.3 Labor Hours

Ideally, to calculate the average productivity, it is better to calculate the average revenue using hours worked than the number of employees. However, the KFS data only allow me to use the number of employees to calculate the average revenue. If the hours worked change dramatically through time, then averaging on employees would be a poor approximation. This is why I use another dataset to show that the average hours per employee in a week do not change dramatically from 2004 to 2011. I use the U.S. KLEMS-Labor Input Data. (see Jorgenson et al., 2012)[20] Figure 1.6 depicts this claim. In this figure, the first column represents the employees between ages of 25-34; the second column represents employees between ages of 35-44. Similarly, the first row shows the situation for college graduates; the second row shows the situation for high school graduates. To be consistent, I use the most common 7 industries in the KFS data, which consist of more than 75 percent of all startups. I cannot use all industries because of data confidentially issues. Figure 1.6 shows the average hours worked in a week over different levels of education and age ranges. Basically, it reveals that the average hours worked in a week does not change significantly through time by the industries used in the study. This is why the number of employees can be used as a proxy for the average labor hours when I average the revenue.

1.8 Conclusion

Lazear (2001) defines the entrepreneur as the "single most important player in modern economy." As the main driving force of the net job creation, it is essential to examine the performance of startups and its determinants. This study is one of the first

[20] The data is publicly available at the address: http://www.worldklems.net/data.htm

studies to examine the effects of compensation practices, particularly employee benefits on the performance of startups. Results suggest that increasing the share of employee benefits in the total compensation leads to increased productivity and enhances the chances of survival of the startup.

Tax advantage status of employee benefits might make a marginal increase in benefits more valuable compared to the additional one dollar in wages for an average employee for some compensation mixes. If this is the case, holding the total compensation constant, an increase in the share of employee benefits might result in increased utility compared to an outside job with the same total compensation cost. This differential utility would make the job more attractive, which would cause existing employees to work harder and allows the startup to select from a pool of better-qualified applicants.

Although this study overcomes most of the limitations in the previous studies, it does not address all the biases. First, because many startups have no or missing employee information in the data, the study loses a considerable amount of variation. Although the means of nearly all of the variables are similar to their counterparts in the whole sample, results might be affected. Second, it is possible that employee benefits have contemporaneous correlation with other good management practices. In that case, the results might be upward biased. Because the data do not offer any reasonable information about such practices, it is not possible to address this bias in this study.

Nevertheless, the estimation strategy lends itself well to drawing more serious conclusions about the effects of employee benefits on the performance of startups. Results highlight the importance of addressing biases that result from the endogeneity of employee benefits and the heterogeneous returns to these benefits. Results indicate that establishing a good mix for compensation between wages and benefits for employees might help startups to increase their productivity and their chances of survival.

Chapter 2

Employer Screening and Transitions from Temporary to Permanent Employment

2.1 Introduction

From 1980s to the 2000s many European countries relaxed their laws about the use of temporary employment. These changes aimed to ease the strict employment protections, in order to help to decrease unemployment. Although some countries such as Spain experienced record rates of gross job creation, most of the new jobs were temporary, replacing permanent jobs. As a result, there has been no significant change in the overall unemployment. (Güell and Petrongolo, 2007). The empirical evidence concerning the effects of these policy changes on unemployment is also ambiguous. (see Bentolila and Dolado, 1994; Saint Paul, 1993). For example, Blanchard and Landier (2002) show that these policy changes had negative effects on unemployment because they induced higher levels of turnover.

In the literature, there are three explanations about the use of temporary employment. First of all, Portugal and Varejao (2009), Ichino et al. (2005), and Booth, Francesconi, and Frank (2002) claim that screening is the main reason that firms use temporary jobs. If this is the case, temporary jobs help to create better job matches between employee and the employer. This explanation is the foundation for "stepping-stone hypothesis". According to this hypothesis, employers screen their employees more effectively during the temporary employment than pre-hiring. Thus, temporary jobs are pathways to permanent jobs. Second, Blanchard and Landier (2002) point out that temporary jobs might be used for churning purposes; they claim that firms might prefer to fire employees at the end of temporary employment and simply recruit again. Third, Bentolila and Bertola (1990) claim that temporary jobs are used as a buffer stock that help firms easily dismiss employees to adjust to economic conditions. Churning and buffer stock use of temporary employment are both harmful for employees and overall economy. From the employee perspective, many spells of unemployment and low-paid entry level jobs are not preferred. From a policy perspective, creation of never-ending temporary jobs is not healthy for the overall economy because there would be more low productivity entry-level jobs, thus, lower productivity and lower output. (Blanchard and Landier, 2002) Hence, is essential to understand the dynamics and possible outcomes of temporary employment, and which, if any, of these three explanations is supported by the data.

This study aims to understand the details of employer screening process and dynamics of temporary employment. Specifically, I ask whether employers use temporary employment as a screening device and if they do, whether temporary employees tend to receive subsequent permanent employment in the same firm. I begin the analysis by examining pre-hiring employer screening. I find that employees with higher cognitive ability are more likely to find permanent jobs. Additionally, graduates of schools such as technician or apprenticeship schools, and university of cooperative education are more likely to find permanent jobs. Because such special schools are supported and screened by employers, one can suggest the existence of pre-hiring employer screening, particularly during the vocational education. However, university and doctorate graduates are less likely to find permanent jobs. Second, I find that firms are more likely to offer employer paid training opportunities to their permanent employees than temporary employees.

Employees with higher cognitive ability are more likely to receive employer paid training in the permanent employment, but one cannot see any significant difference in cognitive ability concerning the employer paid training in the temporary employment. This result is consistent with human capital theory that firms prefer permanent employees to make training investments in. The hypothesis that on-the-job training is used as a screening device is not empirically supported. Third, I find no evidence that employees with higher cognitive ability are more likely to move up to permanent employment. Regardless of their cognitive ability, employees who receive employer paid on-the-job training and who are more experienced are more likely to move up to permanent employment. Finally, there is no evidence that university or doctorate graduates are more likely to experience a transition from temporary to permanent employment. These findings suggest that employers do not use temporary employment as a screening device for cognitive ability.

This paper contributes to the literature in two ways. First, it uses a new German data (ALWA) that uniquely tracks the transition from temporary to permanent employment, coupled with detailed background employee information. The data allows to observe if the employee experience a transition from temporary to permanent employment within the same firm. This way, it clearly tests the hypothesis that employers screen their employees during temporary employment. Because of data limitations, the previous literature was unable to test the existence of employer screening during temporary employment concerning the temporary to permanent transition with the same employer. Second, it jointly examines the employer paid on-the-job training opportunities and the temporary to permanent transitions inside the firm.

This paper adds to the large literature about temporary employment. Booth, Francesconi, and Frank (2002), de Graaf-Zijl, Van den Berg, and Heyma (2011), Güell and Petrongolo (2007) are some examples that find evidence to support the existence of employer screening during temporary employment. However, none of these studies use data that tracks the temporary employee within the same firm. Thus, it is hard to conclude that there exists employer screening for ability during temporary employment.

Portugal and Varajeo (2009) is the only study that I know of that tracks whether the employee stays with his current employer for the permanent job. Using Portuguese data, they find evidence supporting the existence of employer screening. They consider only the employees who worked in a temporary employment for the first year between only a short time period i.e. between 1999-2002. This study builds upon their work by using data with much greater time span and extensive background information. Greater time span allows me to address another shortcoming of their work that I do not use survival methods for cross sectional data. In this method, one needs to assume that all regressors are time invariant. Moreover, resulting survival times do not have the same distribution as the actual time. (Van Es, Klaassen, Oudshoorn, 2000)

In a closely related literature, Andersson, Holzer, and Lane (2005), Kvasnicka (2008), Autor and Houseman (2005), Ichino et al. (2005) examine the effects of being employed in Temporary Help Agencies (THA) on the likelihood of finding a subsequent permanent job. This study differs from the existing literature on the THAs because of two reasons. First, it focuses on employer screening in a given firm. THAs are external institutions that might have different internal goals and dynamics. Second, THAs have minor share in the German labor market. Their share is approximately 1.1 percent in the overall employment. (Klös, 2000, pg.6) To concentrate on employer screening inside the firm, I remove the employees working in THAs from the analysis.

This paper is organized as follows. Section 2.2 describes the data, Section 2.3 presents relevant details about the educational system in Germany. Section 2.4 explains the use of temporary employment in Germany. Section 2.5 presents the results of the empirical analysis, and Section 2.6 concludes.

2.2 Data

In this study, I use the retrospective life history data named "Work and Learning in a Changing world (ALWA)" and its extension ALWA-LiNu collected by the German Institute for Employment Research (IAB). The data was collected between 2007 and 2008. Main data, ALWA, consists of 10,177 German residents born in between 1956 and 1988. It includes all spells of employment of the participants in addition to the secondary and tertiary educational attainment. It also has extensive background information. There are variables about nationality, place of birth, family background information such as the highest level of educational attainment of participants' parents, marital status, number of spoken foreign languages, and informal training received by the participants.

The extension data, ALWA-LiNu, includes 3980 participants among the total sample size of 10,177. Cognitive skill tests have been administered to the participants. For the randomly selected participants, two task booklets were employed in a face-to-face interview. Tests focus on numeracy and literacy skill of the participant. The tests were designed in accordance with ETS protocol.[21] Using the numeracy scores, I standardize the continuous measure of cognitive ability.

In addition to the variables about the employees in the data, there is information about the firm where the employee works including the name of the industry and size of the firm. I present the distribution of industries in the sample in Figure 2.1. The figure shows that the employees in the analysis concentrate on manufacturing and services

[21] For more information about the data please check Antoni and Seth (2012).

industries. To control for the occupational fixed effects, I group the observations by using the ISCO codes in the data. Moreover, I construct the regional unemployment rate with statistics from Federal Statistics Office of Germany.[22]

Observations in the ALWA data are employment spells of the employees. Because there might be several employment spells for the same individual, the number of observations is more than 10,177. When I limit the analysis to the employees who participated in the extension (ALWA-LiNu), the number of observations becomes 11,994. I drop the observations before 1985 because of the federal restrictions on temporary employment (19 percent of the sample). I also exclude observations from East Germany because labor market dynamics there are dramatically different than those in West Germany (17 percent of the sample). For example, the existence of the many federal labor programs concentrating on temporary to permanent transitions might spoil the results (see Matthes, 2002). To understand the effects of education on employment outcomes clearly, I exclude 132 observations that are employment spells prior to the graduation. As I describe in the introduction, in order to focus on temporary employment inside the firm, I remove the employees of Temporary Help Agencies, which decrease the number of observations by 173.

Table 2.1 documents the detailed descriptive statistics. It displays the statistics separately for permanent jobs and temporary jobs.

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[22] For more information about the unemployment statistics in Germany please check: https://www.destatis.de/

2.3 Educational System in Germany

The education system in Germany is segmented and coordinated at each level. After primary school, there are several options for students to choose for their secondary education. Basic school (Hauptschule), Intermediate school (Realschule), Comprehensive school (Gesamtschule), and Gymnasium are the main options.[23] Except Gymnasium, which allows students to proceed to the university directly at the end of 12th year, other schools offer two options. Students can take their certificates at the end of their 10th years and start working. They can also proceed with apprenticeship schools (Berufsfachschule), or after a transitory period they can proceed to university of applied sciences (Fachhochschule), university of cooperative education (Berufsakademie) or standard university.

Although it has become less coordinated (Ryan, 2001), the German educational system is designed to meet the needs of the occupationally segmented structure of the German labor market. (Muller et al., 1998). In this market, strong vocational training including apprenticeships is much valued. These vocational programs help to create a bridge between employees and employers. Employees can have closer relationships with the employers, and this allows the employers to screen them effectively during their vocational training. Furthermore, it is common for graduates of apprenticeship schools to start working in the firm where they worked as an apprentice during their vocational education. Hence, one can consider apprenticeship as a pre-hiring screening device. That's why graduates of vocational degrees can easily find permanent jobs when they finish school. However, for the university graduates this is not the case. For them, the risk of being unemployed after leaving the educational system is higher than graduates from other schools (Winkelmann, 1996). Considering this segmented structure of education is

[23] There are also special needs schools and other very small schools such as Sekundarschule.

essential if one aims to examine the details of employer and employee relationships. It is also critical to control for the educational background because people in Germany tend to stay in the same occupation for which they are trained. (Shavit and Muller, 1998)

2.4 Temporary Employment in Germany

Germany has always been considered to be one of the few countries that offer high levels of employment protection for permanent employees. Dismissal regulations are quite strict. If the labor market regulators think that the reasons for the dismissal are unsubstantiated, the dismissal becomes invalid or the employer needs to pay high severance payments. (Giesecke and Groß, 2004) Some authors believe that this strictness is one of the leading factors that cause employers to use temporary employment. (Kahn, 2007) According to Bielenski et al. (1994), uncertain economic development, avoiding legal problems in the case of dismissals, and longer probation periods are among the other most popular reasons for employers to offer temporary jobs to their employees in Germany.

The Employment Promotion Act of 1985 introduced temporary jobs for the first time in Germany. Initially, these contracts were time-limited (2 years). The law has gone through some modifications in -- 1990, 1994, and 1996. Lastly in 2001, the government removed the time limitation requirement. According to the EUROSTAT (2014) data, in 1987, 11.6 percent of the all contracts were temporary, but in 2008 it was 14.8 percent. This shows that the share of such contracts have increased significantly, thus, they become a crucial part of the labor market. Using the ALWA data, I present the upward trends in the share of temporary employment in Figure 2.2.

Given that the share of temporary employment has increased and most of the employees tend to stay in the same occupation, it is important to understand who works as a temporary employee in the first place, and what makes some of them to move to permanent employment. I examine these issues in Section 2.5.

2.5 Results

In this section, I report the estimation results concerning the employer screening and the dynamics of temporary employment. First, I analyze the pre-hiring employer screening. Second, to understand the process of employer screening during temporary employment, I investigate the employer paid on-the-job training opportunities inside the firm. Finally, I test the existence of employer screening during temporary employment and what kind of employees move up to permanent employment.[24]

2.5.1 Pre-Hiring Employer Screening

To analyze pre-hiring employer screening, I estimate a probit and a linear probability model for the probability that the employment is temporary, as opposed to permanent. Table 2.2 reports the results of these regressions. Column (1) reports the coefficients of probit model; Column (2) reports the marginal effects evaluated at the sample means; Column (3) reports the coefficients of linear probability model.

As discussed in the introduction, employers may use either pre-hiring screening, or screening during temporary employment. Assuming that the employees with higher cognitive ability are usually paid more than their counterparts with lower cognitive ability, the firing costs of the employees with higher cognitive ability would be higher because firing costs are usually correlated with the wage. As a result, firms might prefer to hire

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[24] Because observations are employment spells of the employees, I cluster on employees in all the following regressions.

employees with higher cognitive ability in temporary employment in order to decrease the firing costs. However, employees with higher cognitive ability might also be in high demand by other firms. Thus, firms might consider capturing such employees by giving them permanent jobs. Depending on the choice of the firm, either effect could dominate.

To understand which effect dominates, I include a continuous measure for cognitive ability, which is unobservable to the employer, into the estimation.[25] Results indicate that employees with higher cognitive ability are more likely to work in permanent employment. One standard deviation increase in the cognitive ability decreases the probability of having a temporary job by 2 percentage points. The effect is statistically significant. This finding suggests the existence of pre-hiring employer screening for cognitive ability. However, although pre-hiring screening exists, some employers might prefer additional screening during the temporary employment. I check this possibility in the next section.

As explained in Section 2.3, Germany has a segmented and coordinated education system. Because the employers support and thus closely watch some types of schools in this system, there is a reason to believe that they can screen the graduates of these schools efficiently during the school years. Compared to the apprenticeship school (omitted category), the probability of having a temporary job is 15 percentage points higher for university graduates, and 12 percentage points lower for cooperative education graduates, and nearly 5 percentage points lower for technician school graduates. This finding indicates the existence of pre-hiring employer screening because higher educational attainment such as university and post-university is associated with temporary employment, but the graduates of university cooperative education and technician schools are more likely to find a permanent job. Because cognitive ability might be correlated with the educational

[25] I also control for industry and time fixed effects in the regressions.

attainment, I re-estimate the models used in Table 2.2 without cognitive ability in one case; in another case, I exclude the variables for educational attainment. Results are presented in Table 2.3. Results confirm that higher cognitive ability is associated with lower probability of temporary employment, though the magnitude of its coefficient is smaller. In addition, when I exclude cognitive ability from the estimation, signs and magnitudes of the school variables do not change significantly.

Personal characteristics and preferences of employees might play an important role in the likelihood of having a temporary job. Varejao and Portugal (2004) show that young females with lower educational attainment are more likely to be employed temporarily. In addition, Fernandez and Ortega (2007) show that immigrants are more likely to work in a temporary job. To control for these effects, I include continuous variables for employee's experience and age; binary variables showing if the employee is female, married or German; a count variable for the number of spoken foreign languages; another binary variable showing if the employee received a type of informal training prior to the current employment; and a categorical variable displaying the highest level of education attainment by the employee’s parents into the estimation. Results indicate that more experienced employees are less likely to have temporary jobs. An additional one year of experience is associated with 10 percentage points decrease in the likelihood of temporary employment. If the firms were using temporary employment as a churning device, we would not expect to see such a significant and robust negative relationship between the previous experience and the likelihood of having a temporary employment.

Finally, unemployment rate might be correlated with the use of temporary employment. It is possible that firms prefer to use temporary employment to decrease their firing costs. And if the unemployment is already high, they do not have to offer permanent employment to attract employees. Moreover, if the firms use temporary jobs as a buffer stock device to adjust the changes in the overall economy, then we would expect to see a relationship between unemployment rate and the likelihood of temporary employment. To control for this possibility, I add regional unemployment rates for the corresponding states and time to the estimation. Results show that an increase in the unemployment rate is not associated with a significant decrease in the likelihood of a temporary employment. This finding supports the previous interpretation that it is unlikely that the firms use temporary employment as a buffer stock device.

2.5.2 Who gets on-the-job Training?

In the literature, on-the-job training has been considered as a device for the firms to invest in or to screen their employees. According to the human capital theory, firms tend to prefer permanent employees to making training investments. As Acemoglu and Pischke (1999) show, if firms' payoffs from training are negatively correlated to the probability of workers' exits, they tend to offer training opportunities to their permanent employees. On the other hand, Autor (2001) shows that on-the-job training can be used as a screening device. Firms could screen their employees during the training. For example, firms might test the employee’s motivation, ability, and willingness to learn during the training. According to this view, firms are more likely to offer training to temporary employees than to permanent employees in order to screen them during the on-the-job training.

To examine the relationship between training, types of employment and ability, I estimate a probit and a linear probability model for which the dependent variable is the binary variable indicating whether the employee receives employer paid training opportunity. Table 2.4 reports these results. Column (1) shows the coefficients of the probit regression; Column (2) shows the marginal effects evaluated at the sample averages of other variables; Column (3) shows the results of the linear probability model. Instead of a single variable for the cognitive ability, I interact the cognitive ability with the binary variable showing if the employment is temporary to examine differential use of training in different types of employment.[26] Results indicate that in permanent employment, one standard deviation increase in the cognitive ability leads to 3.8 percentage points increase in the likelihood of receiving employer paid training. However, in temporary employment, I find no evidence that employees with higher cognitive ability are more likely to receive employer paid training. This shows that employers consider cognitive ability as an important factor when they decide who receives employer paid training for permanent employees, but this is not the case for temporary employees. Positive coefficient for the cognitive ability is cancelled out by the negative coefficient of the interaction term. T-test confirms that the effect is insignificant when we consider both of the variables. Results also show that it is less common to receive employer paid training in temporary employment holding the cognitive ability constant. Hence, consistent with Acemoglu and Piscke (1999), employers prefer to offer training to their permanent employees. Results are also consistent with Bartel (1995) who show that employees with higher test scores are more likely to receive training.

Results indicate that previous experience is positively correlated with employer paid training opportunities. However, an increase in the age is associated with a decrease in the likelihood of receiving employer paid training. Firms do not prefer to invest in older employees, but they prefer to train experienced employees to keep them in the firm. Both

[26] As discussed in Norton, Wang, and Ai (2004), interaction terms in probit models cannot be interpreted as in the same way as their counterparts in linear probability model. As a robustness test, I use the *inteff* command in STATA to compute the correct marginal effect of a change in two interacted variables. The marginal effect of the interaction is calculated as -0.06 (compare it to -0.072 in the Table 2.3.) by the *inteff* command, and it is still statistically significant.

of these results are consistent with the human capital theory that employees who have the relevant characteristics to establish a long-lasting, stable job match with the firm receive more training. (see Lynch, 1992)[27]

2.5.3 Transition from Temporary to Permanent Employment

In this section, I analyze the temporary to permanent transitions inside the firm. As I discuss in the previous section, employer paid training opportunity might be used as a device to screen the employees. Thus, I interact the binary variable for the employer paid training with the cognitive ability. Again, I estimate a probit and a linear probability model for which the dependent variable is the binary variable indicating whether the temporary employee move up to a permanent position after his temporary employment as opposed to staying in a temporary employment or becoming unemployed. I exclude the employees who transfer to another firm after the temporary employment from the estimation because it is not possible to distinguish whether they work in a permanent or temporary employment in the new firm. I present the results in Table 2.6.

Column (1) shows the coefficients of probit estimation; Column (2) shows the marginal effects evaluated at the sample averages of other variables; Column (3) shows the results of the linear probability model. I find no evidence that employers screen the cognitive ability of their employees during temporary employment. Results indicate that for both untrained and trained employees, cognitive ability has no significant impact on the likelihood of moving up to a permanent position inside the firm. On the other hand, no matter what level of cognitive abilities they have, trained employees are more likely than

[27] Again, I re-estimate the models used in Table 2.4 without cognitive ability in one case. In another case, I exclude the variables for educational attainment. Results are presented in Table 2.5. Results are quite similar to the Table 2.4.

untrained employees to move to permanent employment after their temporary employment. The effect changes from 16 percentage points to 18 percentage points. This finding shows that firms prefer to keep the employees that they invest in.[28]

Results also indicate that employers do not prefer to move older employees into permanent employment. 10 years of increase in age is associated with a 9 percentage points decrease in the likelihood of transition. Additionally, an additional year of experience is associated with an 18 percentage points increase in the likelihood of moving up to permanent employment holding age constant.

Finally, results show that high-skilled employees are less likely to move up to permanent employment. Graduates from university and university of applied sciences are significantly less likely to move to permanent employment from temporary employment.[29]

Briefly, employers want to retain temporary employees with accumulated experience and on-the-job training in subsequent permanent employment, but there is no evidence that they screen cognitive ability of employees during temporary employment. One explanation might come from Figure 2.1. The figure displays that employees in the ALWA data are concentrated in manufacturing and service industries. It is possible that temporary employees are mainly used for simple tasks, which do not require higher cognitive ability, in these industries.

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[28] The *inteff* command gives the marginal effect of the interaction as 0.02 (compare it to 0.011 in the Table 2.4), and it is still statistically insignificant.

[29] Results are similar when I re-estimate the models used in Table 2.6 without cognitive ability in one case, and without educational attainment in another case. Results are presented in Table 2.7.

2.6 Conclusion

This study contributes to the understanding of temporary employment and the temporary to permanent transitions. Using new and unique data that clearly tracks the dynamics of the transition from temporary employment to permanent employment, I examine the employer screening, dynamics of temporary employment and transitions into permanent employment. Consistent with the literature, I find that unexperienced university graduates with low cognitive abilities are more likely to enter temporary employment than permanent employment. During the employment, employees with higher cognitive ability in permanent jobs are more likely to receive paid training opportunities, but this is not the case for temporary jobs. Finally, I find no evidence that employees with higher cognitive ability are more likely to move to permanent employment during the temporary employment. Receiving employer paid on-the-job training increases the chances of transition into permanent employment during temporary employment. Briefly, I find no evidence that supports the employer screening for cognitive ability and stepping stone hypotheses.

Although this study addresses some of the limitations of the previous literature, it still suffers from several problems. Retrospective data is prone to measurement error issues, especially about the timing of important thresholds such as conversion into permanent employment or the amount of on-the-job training. Small sample size is another problem that might limit the power of statistical inference. Nevertheless, results highlight the importance of identifying channels concerning temporary employment.

Chapter 3

Unbundling Curbside Parking Costs from House Prices and Rents

3.1 Introduction

When parking is free, some people still bear the costs of parking spaces. Shopping malls provide free parking to customers but embed the parking costs in stores' rents and prices of goods and services sold in the mall (Hasker and Inci, 2014; Ersoy, Hasker, and Inci, 2015). Employers provide free parking to employees rather than paying higher wages because parking spaces are not taxed as a benefit in kind (van Ommeren and Wentink, 2012). Cities provide cheap curbside parking to residents but the cost of waiting for parking permits are capitalized in house prices (van Ommeren, Wentink, and Dekkers, 2011).[30] A red flag is raised immediately in economics when the cost of a good or service (in this case parking) is embedded in the price of other goods or services because such a situation may have perverse welfare consequences.

The capitalization of parking costs in property prices is particularly transparent in the cases of premises with on-site parking requirements. Cities typically require developers to provide enough parking spaces on the premises they construct, usually more than one per housing unit. Most developers prefer to provide parking spaces bundled with the

[30] See Inci (2015) for a comprehensive review of similar examples.

property. So, if one buys a property, its parking spaces may seemingly come for free, but in fact their cost is already in the property prices. As Shoup (2005, Ch. 20) argue in detail, if on-site parking requirements are canceled or relaxed, developers do not need to provide so much parking spaces, and provided that the housing market is competitive enough, they pass the cost savings on the customers. Therefore, the property prices drop when parking is unbundled from their prices and sold separately. By making use of a legislation that exempts developers from on-site parking requirements, Manville (2013) finds that bundled parking increases the asking price of an apartment by $22 per square foot and the asking rent by $200. Moreover, a condo with bundled parking is $43,000 more expensive. In their case study of six neighborhoods in San Francisco, Jia and Wachs (1999) find that condominiums with parking units are 13 percent more expensive (see Manville, Beata, and Shoup, 2013).

A related (but more subtle) mechanism is still in charge in the case of curbside parking spaces, although those spaces are not legally bundled with housing units. If curbside parking spaces outside the premises are free, residents use those spaces as their own parking garage, whose cost is embedded in the price of the property in some way or another. Thus, house prices should go down if the city starts charging for curbside parking spaces. In other words, free curbside parking in front of a premise is a privilege for residents. When they are converted into paid parking spaces, this privilege disappears and eventually parking costs get unbundled from house prices. In this paper, we empirically show that this has been the case in Istanbul, where there was a gradual transition from free to paid curbside parking starting toward the end of 2005.

The city of Istanbul was not operating its curbside parking spaces before 2005. Curbside parking was largely free. However, as it occasionally occurs in the absence of a format market, there were self-appointed informal parking attendants on some busy streets who collect parking fees in exchange for *looking after* your car. On December 1st, 2005, the city established a parking company called ISPARK. As of now, it is operating most of the designated curbside parking spaces in the city.[31] The company started operating at different neighborhoods at different times. In this paper, we make use of these variations across neighborhoods and over time to identify the effects of introduction of formal paid curbside parking spaces on house prices and rents. In particular, using a difference-in-differences identification strategy, we show that house prices are 14 percent lower in the neighborhoods where the city started operating parking spaces, while the rents in those neighborhoods are not statistically different from others. Thus, the city's curbside parking reform effectively prevented its residents from using the streets as their own parking garages with no or nominal prices so that house prices decreased. However, the rental market is not sufficiently competitive enough that house owners do not reflect these price decreases to tenants. In other words, it appears that landlords have more market power in the overall rental market in Istanbul. It could also be the case that some of the tenants owning cars either disown their cars or relocate to places where parking is free.

When we look at European and Asian sides of Istanbul separately, we clearly see that the effects we identify are pronounced more in the former. In particular, in the European side, house prices are 15 percent lower in the neighborhoods where the city started operating parking spaces, while the differences between rents are not statistically different. However, we obtain completely new dynamics in the Asian side. In particular, house prices are 4 percent higher in the neighborhoods where the city started operating parking spaces, while rents are 7 percent higher in them. Thus, landlords still have more market power in the Asian-side rental market since they reflect a proportionally higher

[31] Kadikoy, a large town in Istanbul, is operating some of the curbside parking spaces within its borders.

premium in the rents than the appreciation in their house prices.[32] This should not be surprising since many residential buildings in the Asian side are developed relatively later and many of them have their own on-site parking spaces (mostly in the form of surface lots) sufficient to satisfy the parking demand of residents. Hence, the vanishing possibility to use curbside as their own parking garage is relatively less important for Asian-side residents.

The *unbundling effect*, namely the prevention of residents from using curbside as their own parking garage, is the driving force behind the results in our paper. Of course, there are other opposing and enforcing effects in charge, but they appear to be secondary in our data. One of them is the positive effects associated with the transition from informal to formal markets. In a formal parking market, there is a legal formal entity (in our case, ISPARK) that is supposed to meet certain business and quality standards. Thus, the transition to a formal parking market enhances trust and improves quality in the market. In fact, this is one of the marketing pitches of ISPARK. After all, an average parker trusts a formal entity more than he trusts a self-appointed informal parking attendant, who implicitly threatens him to "look after" his car against possible damages. Hence, the transition to a formal parking market in and of itself should increase house prices and rents by making houses around those parking spaces more valuable. We call this effect the *trust-enhancing effect*.

Some details about the informal parking market in Istanbul help us partially clarify why the trust-enhancing effect is secondary in our data. First of all, informal parking attendants have all the incentives to favor residents against nonresidents. They are mostly

32 A similar effect is estimated by Brueckner et al. (forthcoming) in the case unbundling of airline bag fees from the airfares. They find that once airline companies started charging separate fees for bags, average airfare decreased by less than the bag fee itself, which signals the exercise of market power by the airline companies

paid by the residents and the way to prevent residents from making legal complaints against them is to favor residents. In fact, they used to reserve parking spaces for residents by putting barrels or rocks on the curb, which makes those spaces unavailable for others. They were also engaging in price discrimination in favor of residents. A resident typically do not pay for parking every time he parks. He rather makes lump-sum payments to the parking attendants and the per-hour equivalent of these payments appear to be less than what they had to pay immediately after the transition to formal paid parking.[33] The city's action wiped out the informal parking attendants from the market.

The other effect is associated with the transition from free to paid parking spaces. Paid parking decreases demand for parking and thus the level of cruising for parking, which in turn make parking more convenient for everyone (Arnott and Inci, 2006, 2010; Arnott, Inci, and Rowse, 2015).[34] This effect should increase house prices and rents in the neighborhoods with significant cruising for parking. We call this effect the *reduced-cruising effect*. Because we get an overall decrease in house prices and rents, this effect appears to be secondary in our data. To investigate it more, we check what happens to our results when we exclude some old counties in Istanbul, in which cruising for parking is expected to be more of a problem. This time, the results become weaker: house prices are lower by 11 percent while rents are still insignificant. This means that in the excluded counties the unbundling effect is even stronger. This is not surprising. When a formal market does not operate, the informal market develops to serve as a rationing mechanism. Where cruising for parking is severe, the rationing function of the informality becomes

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[33] We do not have data for these informal transactions. We only make this claim based on our own experience. However, we are able to track down lots of news in the media about protests made by some residents right after the start of the transition.

[34] Higher parking fees are not always associated with lower cruising levels. Glazer and Niskanen (1992) show that traffic congestion may sometimes increase in response to higher parking fees because they may extensively increase parking turnover by making parking durations shorter.

more important. We already know that parking attendants tend to favor residents while rationing demand. Thus, the reduced-cruising effect should not be dominant in the overall regressions.[35]

The paper is organized as follows. Section 3.2 describes the data and Section 3.3 explains our estimation strategy. Section 3.4 derives the impact on house prices and rents on the raw unmatched sample. Section 3.5 undertakes a propensity score matching and derives the impacts again on the matched sample. Section 3.6 obtains the impact on house prices and rents over time. To show the robustness of our results, Section 3.7 makes four sensitivity tests: first running a placebo test in order to rule out the possibility that the estimates are driven by unobservable prior trends in neighborhoods, second obtaining the results for European and Asian sides of Istanbul separately, third obtaining the results when older counties, where traffic congestion is severe, are excluded, and fourth obtaining the results with different matching methods. Section 3.8 concludes.

3.2 Data

Our empirical analysis utilizes four different datasets. The first dataset is house prices and (house) rents data for Istanbul collected by the real estate information company REIDIN. For brevity, we call this dataset the housing data hereafter. The data consists of monthly neighborhood averages of house prices and rents in square meters for 278 random-sampled neighborhoods of 38 counties in Istanbul for the period between July 2007 and August 2013. We exclude Adalar, Arnavutkoy, Beykoz, Catalca, Sile, and Silivri in our

[35] One can also claim that the level of cruising is not significant in European countries. Although we know of no specific estimates for Istanbul, van Ommeren, Wentink, and Rietveld (2012) estimate the average cruising-for-parking time to be only 36 seconds in the Netherlands.

analysis because the parking market is negligible in these periphery counties. All prices in our analysis are CPI-adjusted to June 2007 prices.

The second dataset is ISPARK's administrative parking data from which we extract the exact establishment dates of all designated curbside parking locations in Istanbul. For brevity, we call this dataset the parking data hereafter. There are 797 ISPARK curbside parking locations in 206 neighborhoods. Because ISPARK established its first parking location on December 1st, 2005, one potential issue to think about is the fact that the housing data does not cover the first 19 months of operations. We exclude the 275 parking locations that were established before the starting date of our housing data. Most of these initial parking locations were in either major transportation points or busy commercial districts with no to few residential areas. More than 75 percent of these initial locations have no residential areas, and more than 90 percent have more commercial areas than residential areas. Thus, they are unlikely to affect the house prices and rents

After matching the remaining locations with our housing data, we end up with 67 neighborhoods in which ISPARK started operating curbside parking locations within our data period and 194 neighborhoods in which it did not. The former neighborhoods form our treatment group and the latter neighborhoods our control group. There are 246 paid and one free curbside parking locations in these neighborhoods operated by ISPARK.[36] Table 3.1 documents some summary statistics. The first three columns show the average of our dependent variables of house prices and rents in the full sample, Column (4) lists the differences between control and treatment groups in terms of the dependent variables while

[36] Although we do not have data or all off-street parking spaces in Istanbul, we have some data for parking garages and surface lots operated by ISPARK. There are 124 surface lots and 31 multi-story parking garages in our data. The signs and significance in estimations remain largely the same when we include them in our analysis.

column (5) does the same thing after we match the similar neighborhoods based on observable characteristics by using propensity score matching (PSM hereafter).

For future reference, average house price per square meter is 1442.5 Turkish Liras (TRY hereafter) in the whole sample while average rent per square meter is 6.49 TRY. Istanbul lays over two continents, Europe and Asia, separated by Bosporus. Both house prices and rents are higher in the European side (respectively 1451.08 TRY and 6.71 TRY per square meter) than in the Asian side (respectively, 1431.93 TRY and 6.22 TRY per square meter). The Asian side is mostly residential with lots of housing opportunities while the housing market in the European side is relatively tight. Many people prefer living in the Asian side and working in the European side despite the fact that there is extreme traffic congestion on the two bridges connecting the two continents.

The third dataset includes the information about the educational and financial characteristics of neighborhoods collected by the Turkish Statistical Institute (TURKSTAT). We use the fraction of people by their educational attainment in a given neighborhood collected in the scope of Address Based Population Registration System (ADNKS). We also use the fraction of free health care receivers in a neighborhood, which we call the *poverty measure*.[37] We expect to see higher house prices and rents in the neighborhoods with higher educational attainment and lower poverty measure. We further use *population density*, which is the number inhabitants in a neighborhood divided by the total land area of that neighborhood. It should be controlled for since it is highly related to parking demand and availability.

The fourth dataset is the survey conducted by the Istanbul Metropolitan Municipality in 2007 while developing the city's transportation master plan. We use the

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37 This health care program, called green card (Yesil Kart) in Turkey, is a non-contributory health insurance program that aims to provide health services to the poor.

information gathered by this survey in conducting the PSM used in the empirical analysis. The survey documents neighborhood averages of income, rents, and household size. It also includes the number of vehicles and the number of curbside parking spaces in a neighborhood. With these, we calculate *vehicle* and *parking ratios* for each neighborhood. The vehicle ratio is the number of vehicles per person in a neighborhood while parking ratio is the number of parking spaces per person in a neighborhood.

Compared with the average number of vehicles, the parking capacity put into operation by ISPARK is sizeable enough to make a significant effect on the availability of parking in a neighborhood. There are on average 9.29 parking locations operated by ISPARK in a neighborhood and the average capacity of a parking location is 102 cars. According to the survey results in 2007, the average number of people living in a neighborhood is 19,185 while the average number of vehicles is 3,004. This gives 0.16 cars per person, which is consistent with the car ownership rates calculated for Istanbul in other studies.

Table 3.2 documents summary statistics about the independent variables, including educational attainment (ratio of uneducated people and the ratios of primary school, high school, and university graduates),[38] population density, poverty measure, average rent, average income, average household size, vehicle ratio, and parking ratio.

Combining and matching the four different data sources requires tedious tasks because some of the neighborhood names in different datasets are different and the borders of counties and neighborhoods are modified in years. We match our data by overlapping the neighborhoods from different sources on the map by using ArcGIS software. We

38 The TURKSTAT ADNKS has eight educational attainment categories: illiterate, literate but no school completed, primary school, primary education, junior high school or vocational school at the same level, high school or vocational school at the same level, higher education or more, literacy status unknown. We group the first two categories as “Uneducated”, the next three groups as “Primary School.”

modify the data properly if a border change makes it inevitable. In such cases, we assume that inhabitants and vehicles are evenly distributed over the space. If there are more than one parking location in one neighborhood, we use the establishment date of the first one in our analysis. If a parking location is in between two neighborhoods (which is usually the case when one side of a street belongs to one neighborhood and other side to the other neighborhood) we count it in both neighborhoods.

3.3 Estimation Strategy

Within our data period, ISPARK started operating curbside parking locations at different neighborhoods of Istanbul at different times. Exploiting the variation in this quasi-experiment, we analyze the causal impact of the transition from free and informal curbside parking to paid and formal curbside parking on house prices and rent. Specifically, we estimate a difference-in-differences (DD hereafter) model with many groups and time periods:

$$\begin{aligned} &Dependent\ Variables = \alpha + \beta(ISPARK_{it}) + \\ &\gamma(Neighborhood\ Characteristics_{it}) + \theta_i + \tau_t + \epsilon_{it} \end{aligned} \tag{3.1}$$

where the dependent variables are house prices and rents; subscripts *i* denotes neighborhood, *t* denotes time period; α is the regression constant, β is the coefficient we estimate showing the causal impact of curbside parking spaces on house prices and rents. The neighborhood characteristics, whose coefficient is γ, include demographic characteristics of neighborhoods, and educational attainment and financial characteristics of inhabitants in them. θ_i controls for neighborhood fixed effects while τ_t controls for time fixed effects, and finally ϵ_{it} is the time and neighborhood-varying error term capturing unobserved characteristics.

Because some variables are not available for all years, we devise different specifications to fully exploit the relevant information. In all estimation specifications, we use neighborhood and time dummies to control for neighborhood and time fixed effects. To deal with the possibility of serial correlation, we consider applying the methods described in Bertrand, Duflo, and Mullainathan (2004) and Hansen (2007). While their models assume independence across clusters, in our case, high correlation among neighborhoods make it impossible to cluster at the neighborhood level. It is also infeasible to cluster at the county level because then we would end up with only 33 clusters. Wooldridge (2006) shows that there is no theoretical or practical reason to expect efficient results in such a case.

In a DD framework, residents should ideally have no prior information about the timing of the establishments. The newspaper articles we have browsed show that this assumption is largely satisfied in our case. In particular, the establishment of ISPARK locations and subsequent entries to the neighborhoods come at a surprise to the public.[39] Moreover, it would be most ideal if ISPARK established parking locations independently and randomly. Our correspondence with ISPARK staff revealed that the selection of parking locations is not random. The company tries to establish parking locations in the neighborhoods with higher revenue potential. Hence, it is more likely for ISPARK to establish parking locations in the neighborhoods with higher number of cars and high-income residents. If the initial differences between neighborhoods stem from this non-

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39 See also our test in Section 3.6, which supports that the lead variable in a dynamic impact analysis are insignificant, meaning that residents did not anticipate ISPARK's establishment of parking locations in their neighborhoods or at least they did not reflect it into the house prices or rents.

random selection, our estimates will suffer from omitted variable bias. We deal with such a potential problem by matching neighborhoods with or without ISPARK parking locations (in other words, treatment and control groups) by employing a PSM method based on pre-treatment characteristics. In particular, we match the treatment groups with observationally similar control groups before estimating the DD model.[40] We run PSM on the base year by using the municipality's survey in 2007, and then conduct a weighted DD on the units that remain on common support. This estimation design allows us to concentrate on the characteristics that determine "participation."

To ensure that we obtain consistent estimates, we check if there are common trends between control and treatment groups. Figure 3.1 shows the time series patterns of our dependent variables in four panels. The upper panels show the pattern for house prices per square meter while the lower panels the pattern for rents per square meter. Leftward panels show the trends in the whole city while the rightward ones the trends for the county Bahcelievler, which has the largest number of ISPARK parking locations after excluding the pre-2007 locations. These figures clearly show that neighborhoods with or without ISPARK parking locations follow largely similar trends for both house prices and rents and thus the common trend assumption is satisfied.

3.4 Initial Estimations

We are now ready to obtain some initial results. Table 3.3 reports the regression results that uses the original unmatched sample in estimating the impact of ISPARK parking locations on house prices and rents. The first two columns concentrate on the

[40] We match these neighborhoods by using pretreatment information about average rent, income, number of vehicles, number of parking locations, and household size in neighborhoods. The data comes from the municipality's survey in 2007.

impact on house prices. The estimation in column (1) uses the variables available for all years. To better control for financial characteristics in a neighborhood, the estimation in column (2) uses in addition the poverty measure, but this measure is missing for 2011 and 2013. The last two columns do exactly the same in order to obtain the impact on rents. We control for neighborhood and time fixed effects in all estimations in this and subsequent sections.

The results suggest that the establishment of formal paid curbside parking locations in a neighborhood leads to a statistically significant decrease in house prices and rents in Istanbul. Column (1) shows that house prices in the neighborhoods with formal paid curbside parking locations are on average 244.926 TRY per square meter lower than house prices in other neighborhoods. Given that the average house price is 1442.5 TRY per square meter in the whole sample (see Table 3.1), this means that the house prices in the neighborhoods where ISPARK started operating curbside parking spaces are about 17 percent cheaper than the other neighborhoods in Istanbul. The effect becomes slightly higher once we control for poverty measure in column (2).

The impact is relatively smaller for rents. Columns (3) and (4) in Table 3.3 shows that the impact on rents is significant only in the 10 percent confidence interval. In fact, the effect will become statistically insignificant in the PSM analysis undertaken in Section 3.5. Column (3) shows that rents are 0.187 TRY per square meter lower in the neighborhoods where ISPARK started operating curbside parking locations. Given that the average rent is 6.49 TRY per square meter in the whole sample (see Table 3.1), this impact corresponds to nearly 3 percent lower rents in those neighborhoods.

The impact of population density is insignificant in three of the four specifications in this initial estimations but it will become significant and positive once we undertake PSM in Section 3.5. Other independent variables have the expected signs. In particular, increased educational attainment and decreased poverty measure are associated with higher house prices and rents. Note that university graduates are the omitted group in our regressions. Thus, the signs of the other educational attainment categories, which are all negative, are relative to the impact of university graduates. Thus, educational attainment has a positive impact on house prices in a neighborhood.[41]

3.5 Propensity Score Matching Estimations

The ideal requirement in a DD framework is that ISPARK selects parking locations randomly and independently from each other, but it does not. According to the ISPARK officials we interviewed, the decision criteria for selecting parking locations include average income of residents, the density of cars, and the availability of parking possibilities in the neighborhood. One way to obtain healthy estimates in such an environment is to use PSM method, which requires two properties to hold. The first one is unconfoundedness property, which means that treatment is random conditional on observables and selection into the treatment depends only on the observable characteristics. We match neighborhoods by using propensity scores on the Istanbul Metropolitan Municipality's survey conducted in 2007 while they develop the master transportation plan of the city. Because this survey is conducted before the starting point of our data, the measures we use in the propensity scores are not confounded with the outcomes of the treatment.

[41] Although the signs of educational attainment categories are as expected, the ranking between them are convoluted. In particular, one expects the magnitude of the coefficient to decrease as educational attainment increases but it is not in our regressions. The reason for this is that there is not much variation in educational attainment over time and thus although we get the expected signs, we do not identify its impact perfectly. In fact, when we control for only university graduates and drop all other educational attainment categories, neither the coefficient of interest nor its statistical significance change much. Because educational attainment categories are only control variables in our regressions that make use of exogenous changes in parking, only their signs should be taken into account, not their magnitude.

The second property is the balancing property, which requires that the neighborhoods with the same propensity score have the same distribution of observable characteristics used to predict the score. In general, the quality of matching lies in balancing the characteristics between treated and untreated neighborhoods. Figure 3.2 shows that the balancing between these groups is pretty much satisfied. In particular, the differences based on observable pre-treatment characteristics are clearly diminished after matching. In fact, they are no longer statistically significant after matching, suggesting that the matching process helps to reduce the bias associated with observable characteristics.

By using radius matching within caliper of 0.01, we verify that the PSM is successful in making the distributions of the propensity scores for treated and untreated neighborhoods similar.[42] To visualize the validity of the performance of our PSM exercise, Figure 3.3 shows these distributions. This figure clearly reveals that the overlapping condition for the distributions of the propensity scores for treated and untreated groups is satisfied. Thus, the common support assumption holds.

Our DD estimation runs a weighted least squares regression by weighting the observations according to the weights derived from the matching. As shown in Hirano, Imbens, and Ridder (2003), this framework yields a fully efficient estimator. The DD estimator has the following form:

$$DD = \frac{1}{N}\left[\sum_{i\in T}(Y_{i2}^{T} - Y_{i1}^{T}) - \sum_{j\in C}\omega(i,j)(Y_{j2}^{C} - Y_{j1}^{C})\right] \qquad (3.2)$$

[42] Table 3.10 shows that nearest neighbor, kernel with normal distribution, and mahalanobis matching methods give similar outcomes.

where T represents the treatment group and C the control group, Ys are either house prices or rents in a neighborhood, N is the sample size, and $\omega(i, j)$ is the weight obtained from PSM. This estimation method is different from a standard DD estimation in that the weights, $\omega(i, j)$, are derived from the corresponding PSM.

Table 3.4 reports the regression results based on the DD estimation coupled with a PSM design. In this table, we follow the same specification order that we follow in Table 3.3. The impact on house prices is statistically and economically significant. The estimation in column (1) shows that the house prices are 180.478 TRY per square meters lower in the neighborhoods where ISPARK started operating parking locations. Given that the average house price is 1442.5 TRY per square meter in the whole sample (see 3.1), this impact corresponds to 13 percent lower house prices in the neighborhoods where ISPARK started operating parking locations. The impact is only slightly higher (14 percent) when we control for poverty measure in column (2). These two estimations based on a matched sample are somewhat lower than their counterparts in Table 3.3. Such a difference hints an existence of an upward omitted variable bias in the unmatched sample of Section 3.4.

This time, the impact on rents is close to zero and in fact statistically insignificant. This means that house owners do not reflect the decrease in the values of their houses to tenants. There could be two reasons for this. First, the rental market is not sufficiently competitive so that the owners do not need to reflect the decrease in the values of their houses to the rents. Second, tenants owning cars might have relocated from neighborhoods with paid parking to the other neighborhoods (some may even disown their cars) while those who do not have cars move into central locations. As a result, the demand for rental units in the neighborhood does not significantly change in the overall and hence the rents remain largely the same. We have some evidence that sorting might have occurred. According to the surveys done in 2007 and 2012 by Istanbul Metropolitan Municipality, average car ownership in 2012 is 5.1 times higher than that in 2007 in the neighborhoods where ISPARK entered before 2007. However, the same ratio is 5.8 in the neighborhoods where ISPARK entered between 2007 and 2012 and 6.5 in the neighborhoods where ISPARK never entered. Hence, although car ownership increased in all neighborhoods, it increased proportionally more in the neighborhoods where ISPARK entered later or never entered. This is consistent with sorting of people according to their car ownership status.

Later in Section 3.7.2, we look at the house prices and rents in the European and Asian sides of Istanbul separately and find that both house prices and rents are higher in the neighborhoods where ISPARK started operating curbside parking locations in the Asian side. However, the increase in rents is disproportionately higher in percentage terms. In other words, house owners raise rents disproportionately more than the increase in the values of their houses. This suggests that, although the impacts of parking locations on house prices and rents are quite different on each side of the Bosporus, the landlords have more market power in the rental market in both sides of the city.

This time, increased population density is associated with higher house prices and rents in all of the four specifications, as expected. Other independent variables continue to have expected signs. In particular, increased educational attainment and decreased poverty measure are associated with higher house prices and rents.

3.6 Dynamic Impacts

It takes time for house prices and rents to adjust to the new economic regime after the transition to the paid and formal curbside parking because the costs of parking spaces for residents is modest compared to the values of their houses or the rents they pay. We have not so far attempted to identify such a dynamic impact. To incorporate it into our analysis, we now use lags and leads of the treatment variables instead of using a single treatment variable. In particular, we add indicator variables for each of the two quarters before (leads) and four quarters after (lags) ISPARK's establishment of parking locations in a neighborhood. We also add another indicator variable for all periods after the first year.

Table 3.5 reports the estimation results with lags and leads. The first two columns present the results for house prices for the unmatched and matched samples, respectively. The last two columns do the same thing for rents. As expected, the coefficients on leads and the first three lags are statistically insignificant. The insignificance of the estimates of the lead variables signal that residents did not anticipate the establishment of ISPARK parking locations in their neighborhoods. To help visualize, Figure 3.4 displays the point estimates of the coefficients for lags and leads of house prices along with their confidence intervals. The monotonic decrease in house prices starts after six months and reaches its peak after one year.

3.7 Sensitivity tests

Although the magnitude of the decrease in house prices is debatable, it should be clear so far that ISPARK's establishment of parking locations prevented residents from using curbside as their parking garage and in response the house prices decreased over time. That is, the unbundling effect dominates the trust and reduced-cruising effects and thus the overall impact is a negative one. To be able to fully convince ourselves and the reader that the coefficient of interest is negative, we undertake four robustness checks in this section.

We first undertake a placebo experiment to check if any underlying pre-existing trends are influencing the results. Second, we run our estimations separately for European and Asian sides of Istanbul, each of which has different housing dynamics. Third, we run our estimations after excluding old counties in Istanbul, in which cruising for parking is more likely to be a problem. Fourth, we check if our results change when we use different matching methods in our PSM estimations. In all of these robustness checks we continue to get a negative effect of ISPARK's parking locations on house prices, except when we concentrate on the Asian side in isolation.

3.7.1 Placebo Test

Table 3.6 presents the results of the placebo experiment where we use a fake treatment variable one year before the actual treatment of ISPARK. The first two columns show the results for house prices for the unmatched and matched samples, respectively. These estimations show that house prices are not affected by the fake implementation of ISPARK's establishment of parking locations in the neighborhood. The last two columns show the results for rents for the unmatched and matched samples, respectively. Rents are affected positively, which implies that the previously insignificant estimates in Tables 3.3 and 3.4 might be spoiled by a previous upward trend in rents prior to the implementation. There could be two explanations, the first of which is more likely. First, rather than being insignificant, the impact on rents could in fact be negative although its magnitude is still expected to be small. Second, ISPARK might be entering the neighborhoods with higher rents but our interviews with ISPARK staff do not reveal such a motivation. In sum, the placebo test largely confirms the validity of our estimation approach.

3.7.2 European side vs. Asian Side

The city of Istanbul lies over both Europe and Asia and the dynamics of housing markets on each side are quite different from each other. It is especially noteworthy that the Asian side of Istanbul is mostly residential with lots of housing opportunities. As Table 3.2 shows, average house prices and rents are lower in the Asian side. Because the Asian side is developed later on, many buildings have their own parking garages that are usually sufficient to satisfy the parking demand of the residents living in the building. Nevertheless, strangers and visitors of residents are usually prohibited to park in the on-site parking spaces. Thus, curbside parking is mostly used by these two groups. Although there are only two bridges and a metro tunnel connecting the two sides of the city, many people still prefer to live in the Asian side and commute to the European side to work. To check if there are any underlying differences between the two, we now run our separate estimations for Asian and European sides of the city. In the end, we do obtain different impacts of ISPARK parking locations on house prices and rents.

Table 3.7 is the counterpart of Table 3.3 developed on an unmatched sample. Columns (1) and (3) show the effects on house prices in the European side. Column (1) reveals that house prices decrease by 380.093 TRY per square meter in response to ISPARK's establishment of parking locations in a neighborhood. Given that the average house price per square meter is 1451.08 TRY in the European side, this impact corresponds to a 26 percent lower house price in the neighborhoods that ISPARK started operating parking locations. Column (3) shows that this effect is only slightly diminished (23 percent) when we control for poverty measure. Columns (2) and (4) show the effects on house prices in the Asian side. Whether we control for poverty measure or not (columns (4) and (2) respectively), the impact on house prices is statistically insignificant, although it is negative in both specifications.

The rest of Table 3.7 shows the impact on rents. As columns (5) and (7) show, the impact on rents is negative and statistically significant in the European side. Given that the average rent per square meter is 6.71 TRY in the European side (see Table 3.1), these results imply that rents are about 5 percent lower in the neighborhoods that ISPARK started operating curbside parking locations. However, the effect is completely the opposite in the Asian side. As columns (6) and (8) show, the impact on rents is positive and statistically significant. Given that the average rent per square meter is 6.22 TRY in the Asian side, these imply that rents are about 3-4 percent higher in the neighborhoods that ISPARK started operating curbside parking locations in the Asian side.

Table 3.7 is useful in setting the stage, but as we underlined in our baseline regressions, there could be an upward omitted variable bias in these unmatched estimations. Table 3.8 develops the counterpart of Table 3.4, which uses PSM. Column (1) show that house prices are 220.130 TRY per square meter lower in the neighborhoods that ISPARK started operating curbside parking locations. Column (3) shows that this effect is slightly lower when we control for poverty measure. Given that the average house price per square meter is 1451.08 TRY in the European side, both specifications correspond to a 15 percent lower house price. Columns (2) and (4) show the effects on house prices in the Asian side. In these matched-sample estimations, the impact on house prices are statistically significant and this time positive. Column (2) shows that house prices are 47.483 TRY per square meter higher in the neighborhoods that ISPARK started operating curbside parking locations in the European side. Given that the average house price per square meter is 1431.93 TRY in the Asian side, this impact corresponds to a 3 percent lower house price. The effect is slightly higher when we control for poverty measure (58.596 TRY per square meter, 4 percent).

Columns (5)-(8) in Table 3.8 show the impact on rents in the matched-sample estimations. When we look at the European and Asian sides separately, the impact on rents continue to be insignificant in the European side. However, there is now a positive effect in the Asian side. In particular, the estimation in column (5) shows that rents are 0.247 TRY per square meter higher in the neighborhoods where ISPARK\ started operating curbside parking locations in the Asian side. Given that the average rent is 6.22 TRY per square meter in the Asian side, this is a 4 percent lower rent. When we control for poverty measure in column (8), the effect is significantly higher (0.419 TRY per square meter), which corresponds to a 7 percent higher rent.

What do these results tell us? First, we learn that the overall impact of ISPARK's establishment of parking spaces on house prices and rents are derived largely from the locations in the European side. When we concentrate on the Asian side, we see a completely new housing and rental market dynamic. It appears that the transition from informal to formal parking (the trust-enhancing effect) is valued more than the unbundling effect in the Asian side. It is not surprising at all. The residential areas of the Asian side are developed relatively later than those in the European side. Many buildings have their own parking spaces, mostly in the form of surface lots around buildings. Consequently, it is less of an issue that curbside is used as residents' own parking garage. The residents would not reject and perhaps even prefer if a formal institution replaces the self-appointed parking attendants. Thus, the trust-enhancing effect (and perhaps the reduced-cruising effect) dominates the unbundling effect in the Asian side of Istanbul.

3.7.3 Old Counties

The old residential locations of Istanbul are full of historical buildings and monuments. The streets are usually narrower. Traffic congestion is severe and the informal parking market is larger. Thus, to check if the results are largely derived by these neighborhoods, we run our regressions again when we exclude neighborhoods in the old counties of Beyoglu, Eyup, Fatih, and Sisli, all of which are on the European side.

Table 3.9 presents the estimation results on the matched sample. We use the same order of specifications we have in Table 3.4. The estimates are less negative when we exclude the old counties than the estimates in Table 3.4. These results show that the establishment of ISPARK parking locations leads to a 162.8 TRY per square meter decrease in house prices, which corresponds to 11 percent. The counterpart of this percentage was 14 percent in Table 3.4. Cruising for parking in these neighborhoods are likely to be large but for the same reason self-appointed parking attendants, who tend to favor residents, are also more likely to be operating parking locations in these neighborhoods. Thus, the unbundling effect is likely to be large even though the reduced-cruising effect is also large.

3.7.4 Different Matching Methods

In all our matched-sample estimations we have presented so far, we carry out PSM by using radius matching with a caliper of 0.01 as defined in Dehejia and Wahba (2002). Columns\ (2)-(8) in Table 3.10 show that results do not change much when we use other matching methods, including Kernel matching (columns (2) and (6)), nearest neighbor with replacement (columns (3) and (7)), and Mahalanobis covariate matching (columns (4) and (8)). In particular, in all specifications, the impact on house prices is statistically significant and negative while the impact on rents is statistically insignificant, except when we use Kernel matching.

3.8 Conclusion

People think about parking spaces only when they are looking for one, but in fact, whether they realize it or not, parking spaces significantly affect their lives in various dimensions and in many subtle ways. In this paper, we concentrate on the impact of curbside parking spaces on house prices and rents. In particular, we empirically examine what happens to house prices and rents if the city starts charging for curbside parking spaces that were previously either free or operated by self-appointed informal parking attendants. The transition from free parking to paid parking puts downward pressure on house prices and rents while the transition from an informal market to a formal one does the opposite.

Istanbul went through such a transition starting in late 2005. By running various regressions, we obtain various magnitudes of impacts on house prices and rents for Istanbul. Our analysis rests on the quasi-experiment that took place in Istanbul. Before 2005, curbside parking was either free or operated by self-appointed informal parking attendants. The city established a parking operator and started eliminating free and/or informal parking across neighborhoods and over time. By making use of these variations, we estimate the impact of establishment of formal and paid curbside parking locations on the house prices and rents in Istanbul. Using propensity score matching, combined with a difference-in-differences analysis, we find that establishment of formal and paid curbside parking locations in a neighborhood leads to a statistically and economically significant decrease in housing prices while rents remain about the same with other neighborhoods.

There are various dynamics going on in the background that influence the overall impact we obtain in our estimations. First, establishment of formal curbside parking spaces prevented residents from using the curbside as their own parking garages. This unbundling effect looks like the most dominant effect in our data and it should lead to a decrease in house prices. Second, a transition from an informal curbside parking market operated by self-appointed parking attendants to a formal one operated by a legal entity should enhance trust and security and improve quality of service in the market. This trust-enhancing effect should result in an increase in house prices. This appears to be secondary in our data. Third, the transition from free parking to paid parking may potentially decrease cruising for parking.[43] In that case, this effect is expected to decrease house prices. This reduced-cruising effect should have a positive impact on house prices. It looks like this effect, too, is secondary in our data. In the overall, we observe a negative impact on house prices.

The impact on rents is close to zero and mostly statistically insignificant. This, combined with the negative impact on house prices, may signal that the rental market is not sufficiently competitive that house owners do not feel obliged to reflect the decrease in their house values to rents. It may also be the case that tenants with cars either disown their cars or move into locations where parking is still free. In fact, as reported in Section 3.5, we see that car ownership rates increase in time in each and every neighborhood we have in our data although they increase disproportionately more in the neighborhoods that ISPARK entered later or never entered, which signals some sort of spatial sorting going on.

We further examine the impacts in the European and Asian sides of the city separately. This analysis reveals curious outcomes. Although the impact on house prices

43 As explained in the Introduction, cruising for parking does not decrease if higher curbside parking fees disproportionately increases parking turnover. (Glazer and Niskanen, 1992)

continues to be negative and the impact on rents continues to be statistically insignificant in the European side, both impacts become positive and statistically significant in the Asian side. This means that although the trust-enhancing and reduced-cruising effects are secondary in the overall data, they are relatively more important in the Asian side. When we exclude the old counties in the European side where cruising for parking is more likely to be an issue, the negative impacts become smaller in magnitude. This hints the importance of the unbundling effect in the full sample, and especially in the European side.

One may believe in these magnitudes or not. But it should be certain after all our analyses that at least some of the costs of curbside parking spaces are embedded in the house prices. This is very important in its own right. Past work made it clear that unbundling on-site parking spaces from the housing units will decrease house prices, which is obvious because parking units are officially sold bundled with the housing unit. What is more interesting in our case is that despite the fact that curbside parking spaces in front of a residence are not formally bundled with the housing units, they have a pretty significant effect on their prices.

What is the takeaway from this exercise? This paper shows that curbside parking spaces that are not formally bundled with the residences can have a statistically and economically significant effect on their sale or rental prices. We show that a smart policy change that took place in Istanbul virtually unbundled the costs of curbside parking spaces from the house prices. The policy wiped out the informal parking market. It should be clear that parking greatly affects the welfare of many people and so can better parking policy.

Appendix

A.1: Figures

Figure 1.1: Changes in the Share of Employee Benefits

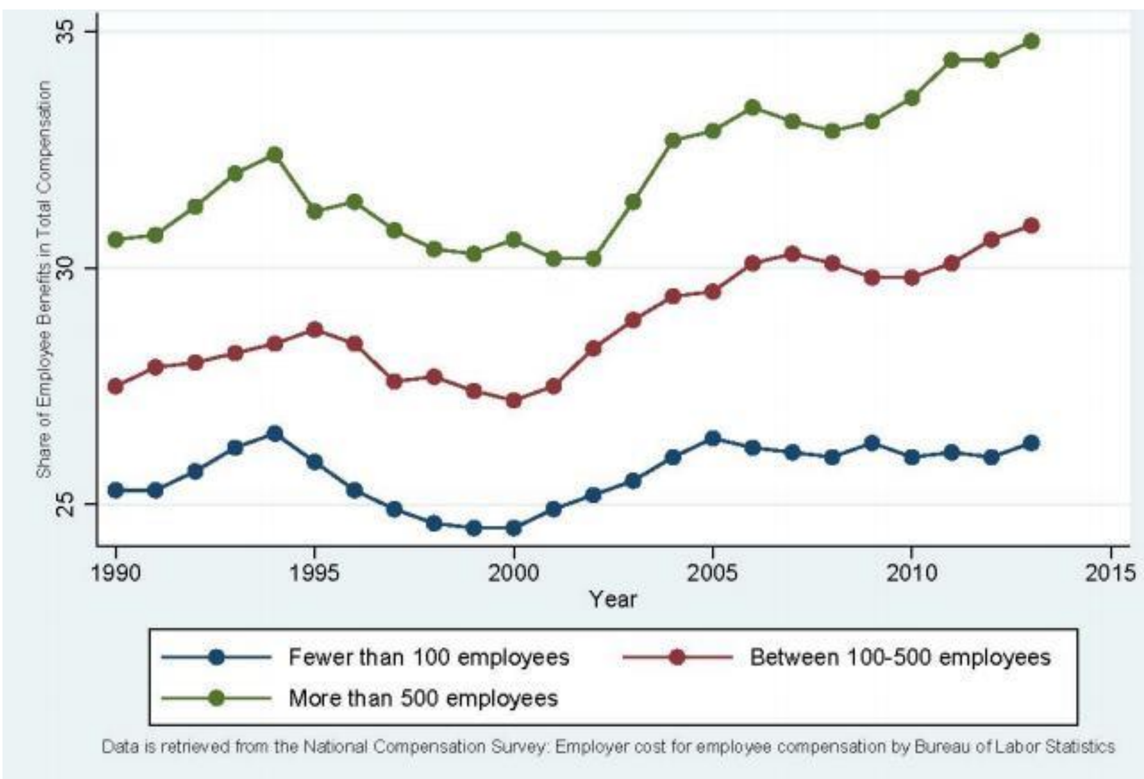


Figure 1.2: Median of Share of Benefits in Compensation

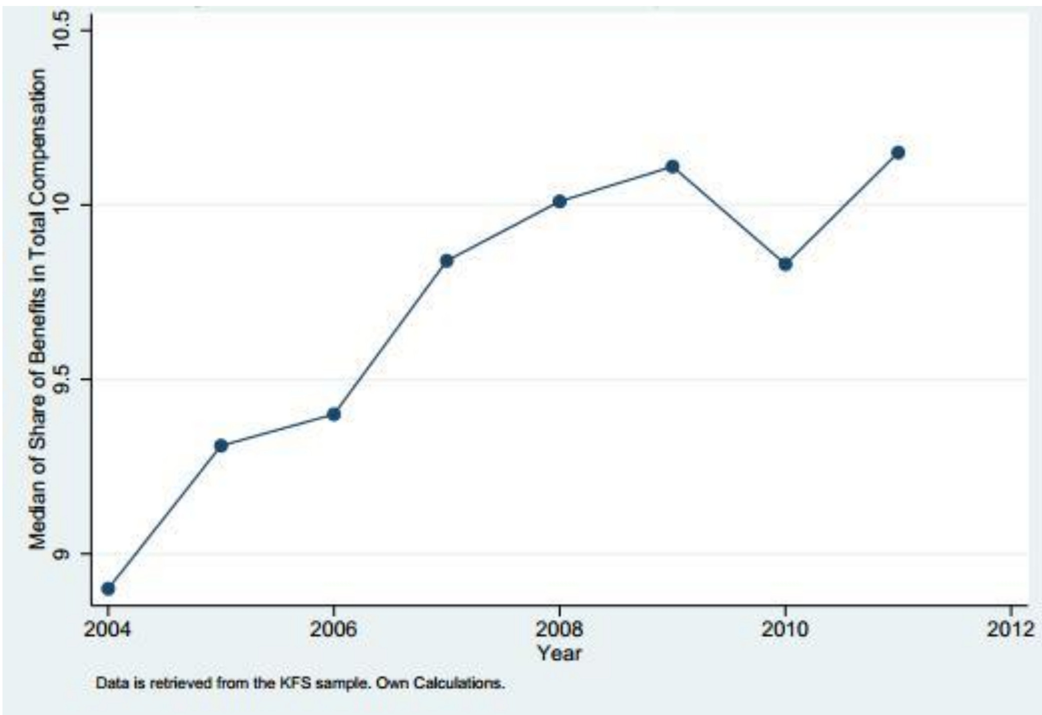


Figure 1.3: Share of Benefits in Total Compensation over Industries

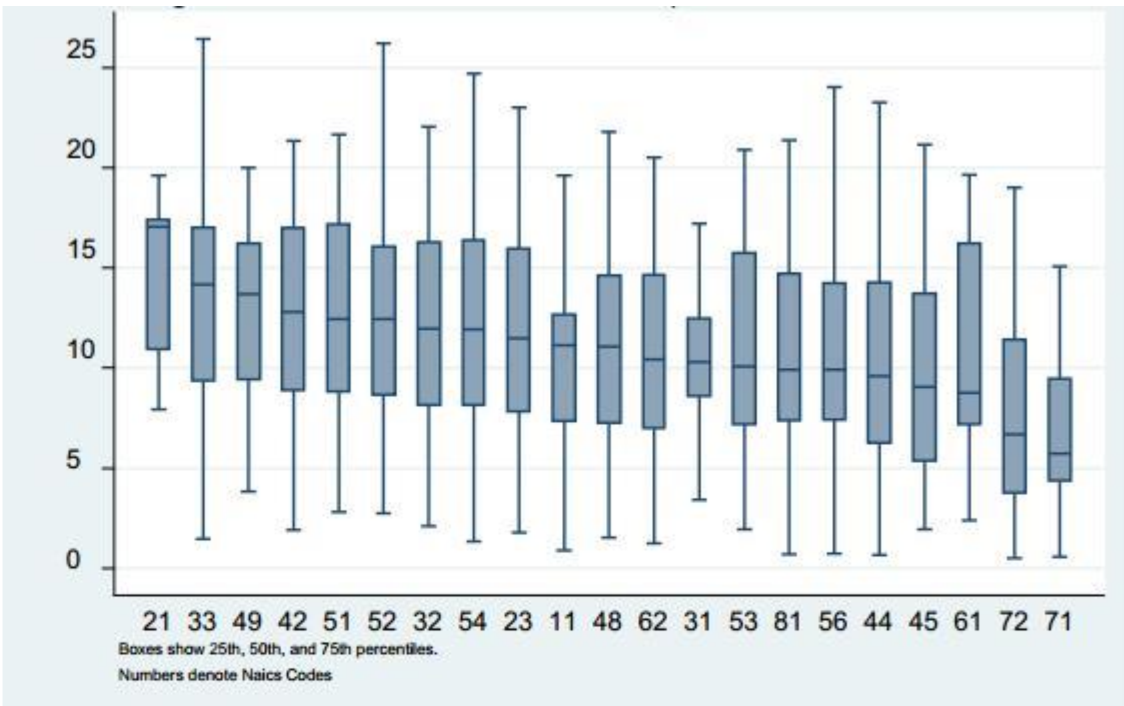


Figure 1.4: Changes in the Real Value of Benefits

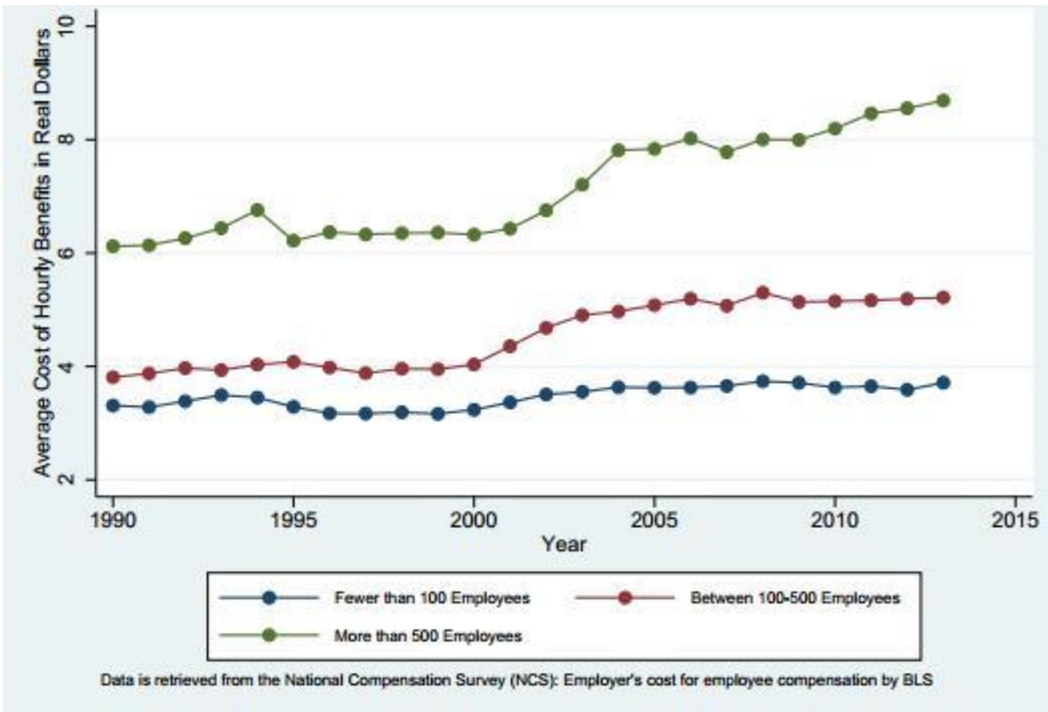


Figure 1.5: Avg. Premium of Health Insurance per Employee

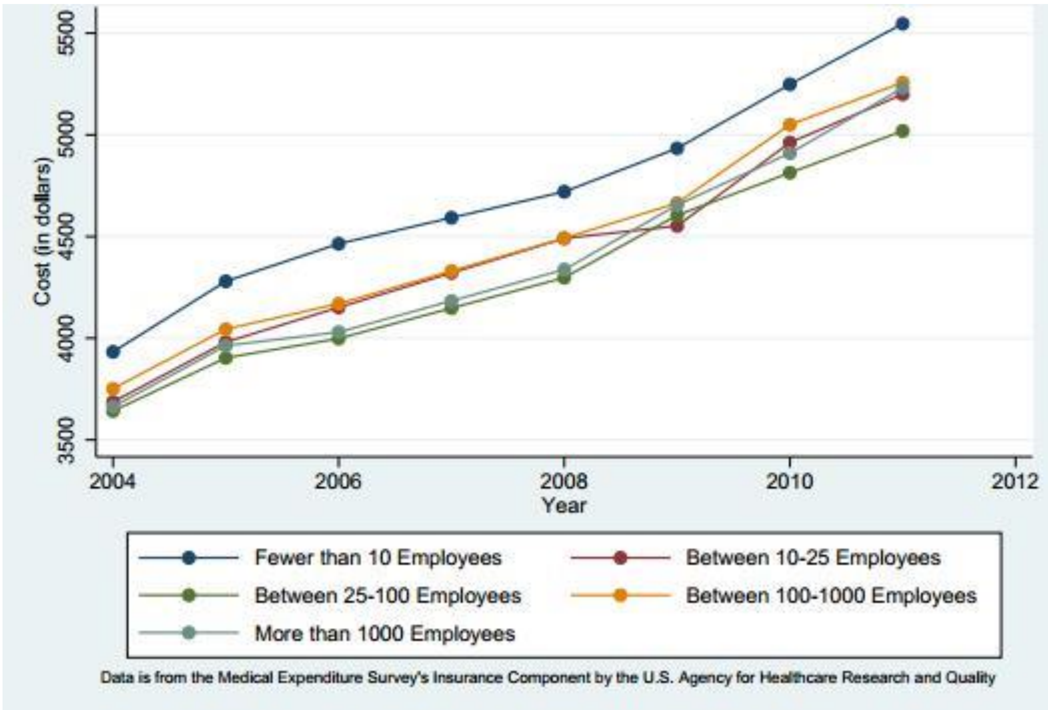


Figure 1.6: Changes in Avg. Weekly Hours by Industries

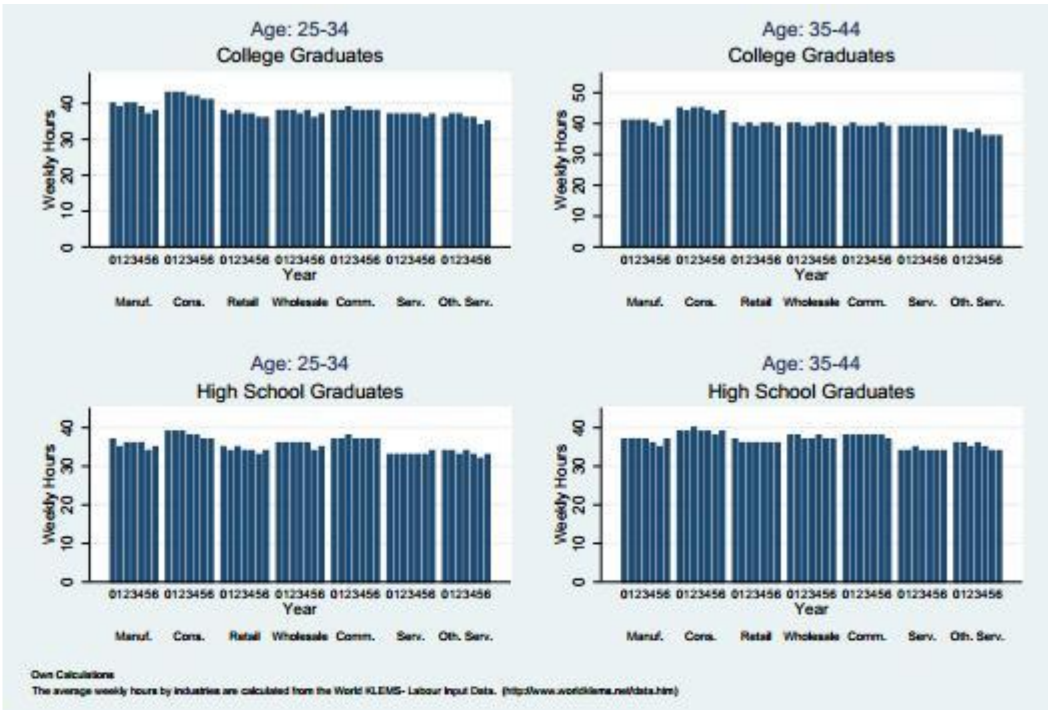


Figure 2.1: Distribution of Industries in the ALWA data

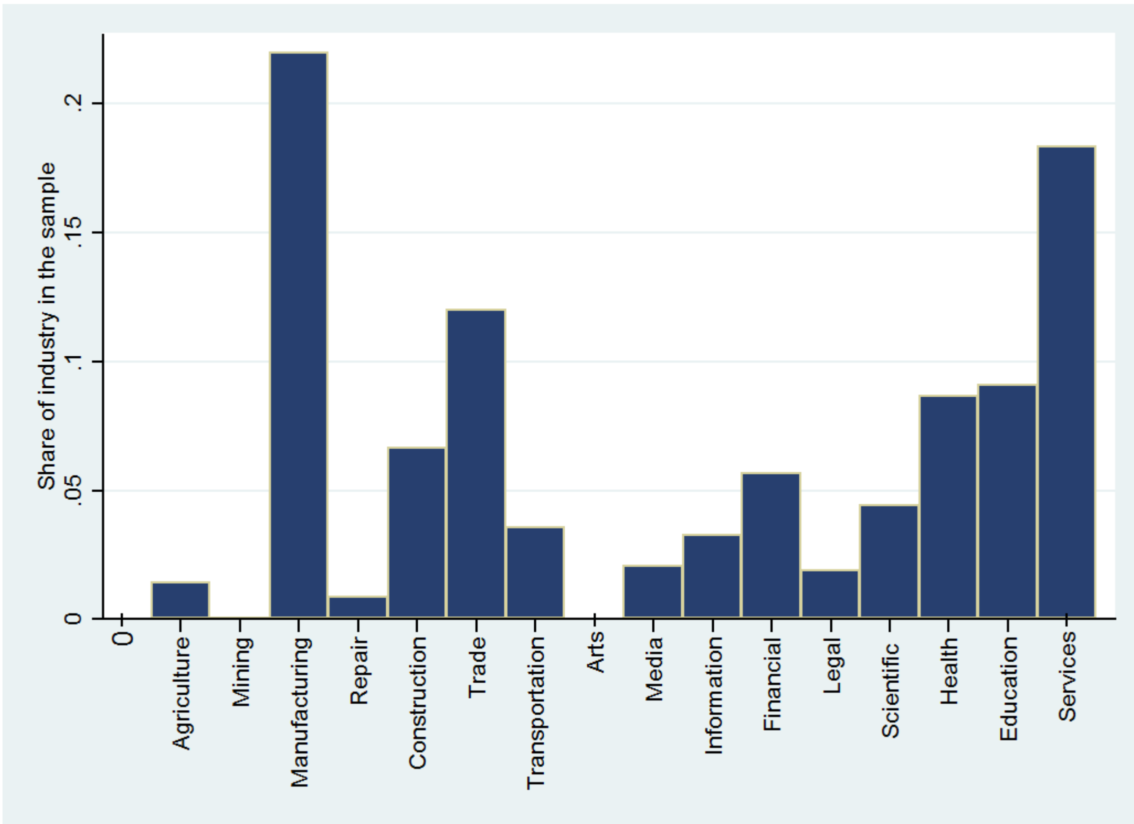


Figure 2.2: Growth of Temporary Employment in Germany in ALWA

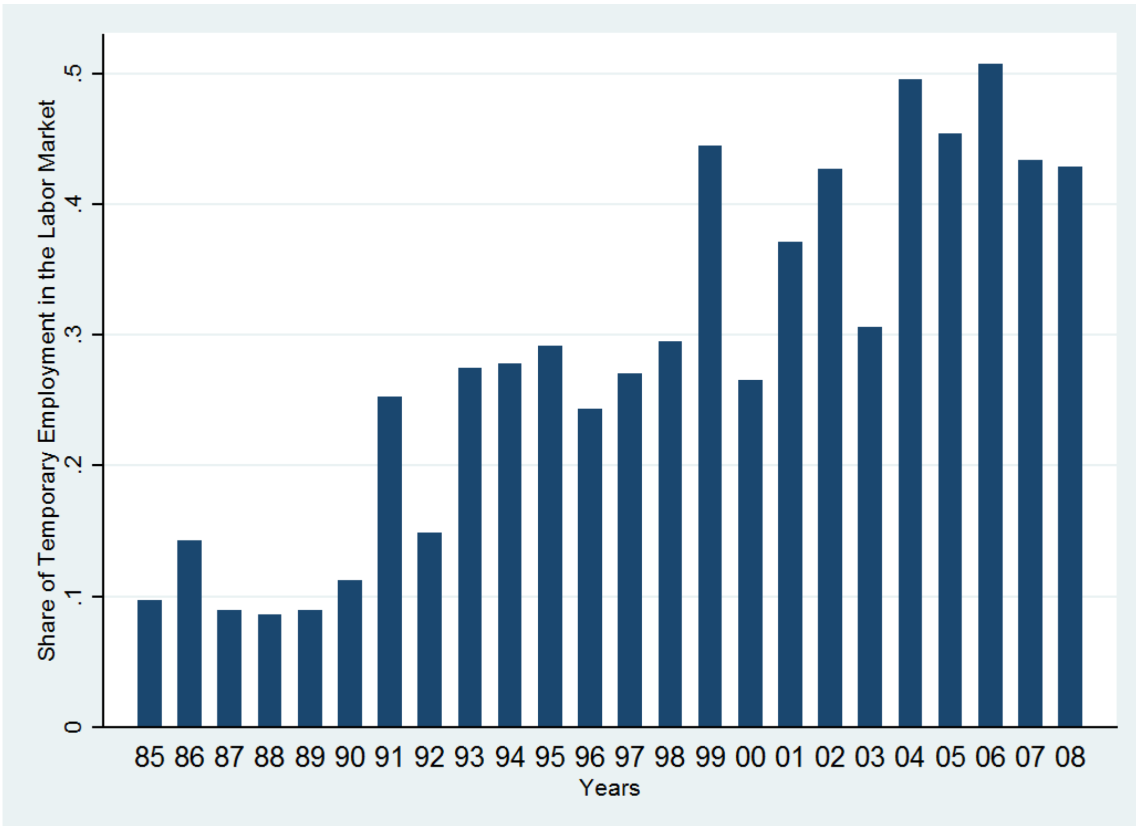


Figure 3.1: Common Trend Assumption

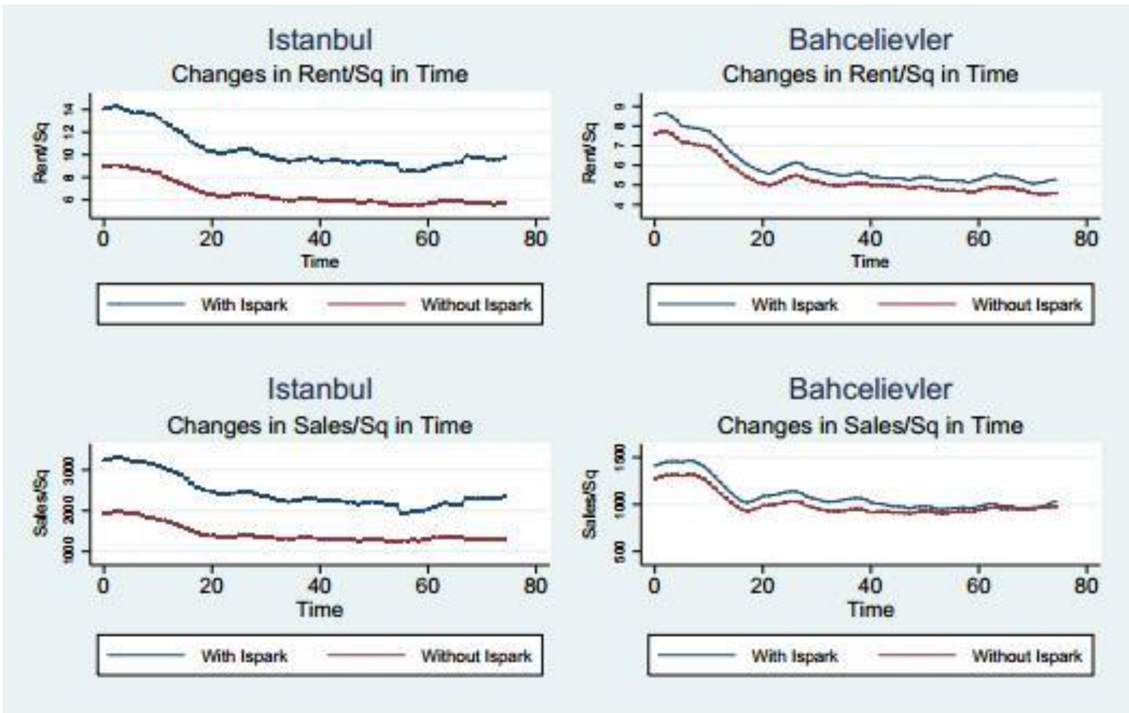
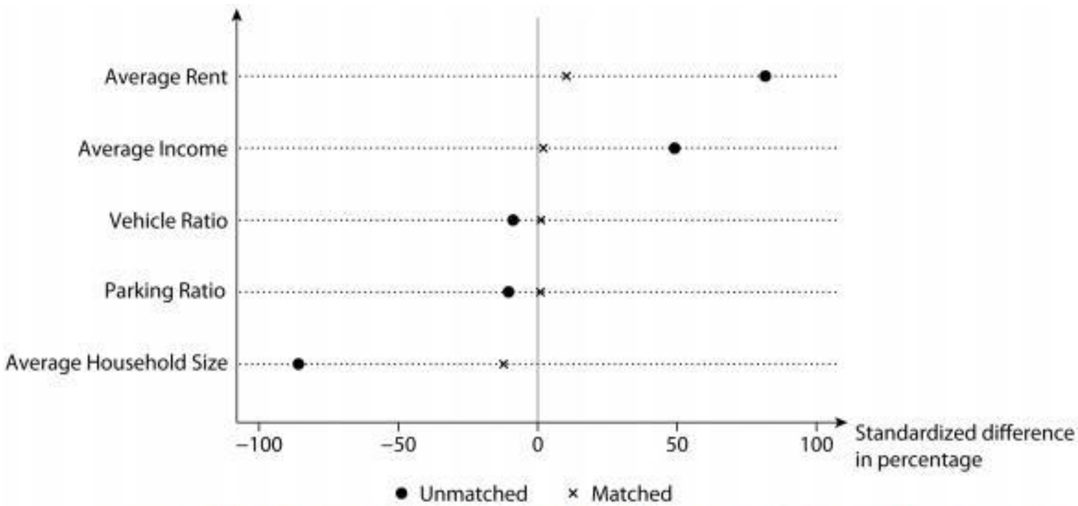


Figure 3.2: Evaluation of Matching Quality



Notes: Vehicle Ratio is the number of vehicles per person in a neighborhood. Parking Ratio is the number of parking spaces per person in a neighborhood.

Figure 3.3: Propensity Score Distributions

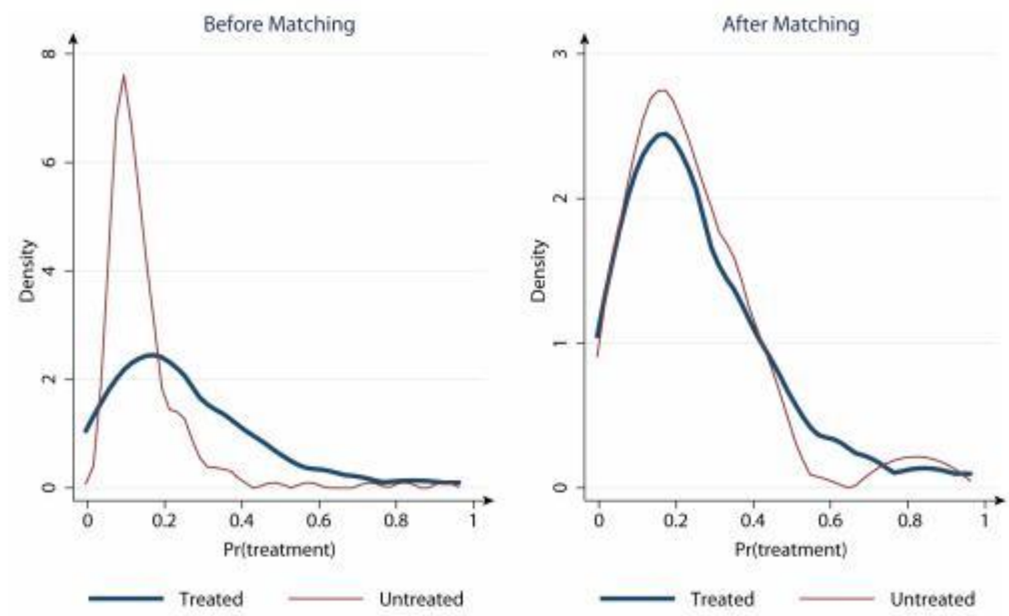
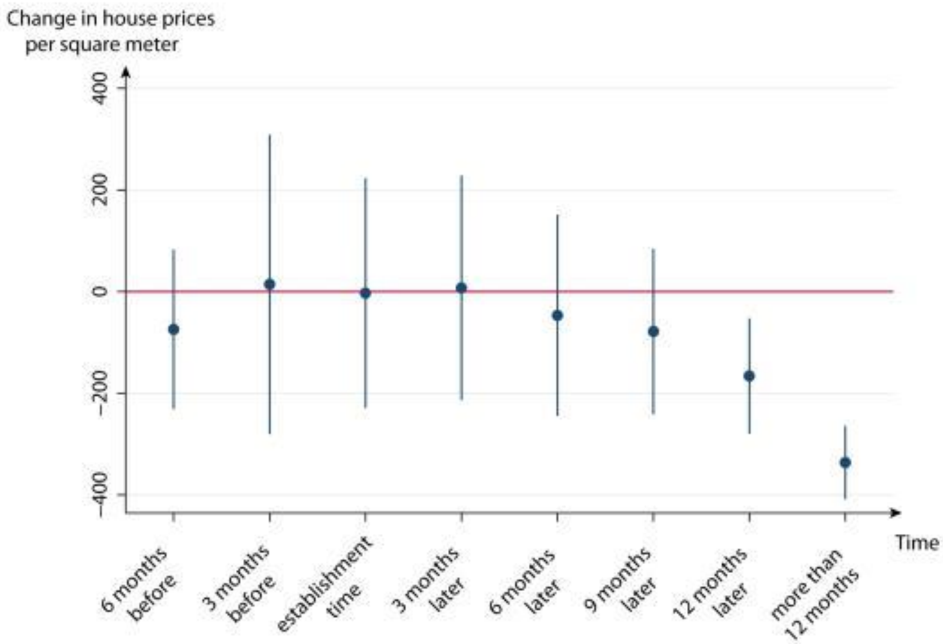


Figure 3.4: Impact of ISPARKs on House Prices over Time



Notes: The figure plots the coefficients for lags and leads of house prices obtained from the matched sample, reported in column (2) of Table 4. The dots represent the point estimates of the coefficients for each of the two quarters before to four quarters after ISPARK's establishment of parking locations. The vertical bands represent the confidence intervals (±1.96 times the standard error of the point estimate).

A.2: Tables

Table 1.1: Descriptive Statistics

	Unbalanced Sample		
Variables	N	Mean	SD
Employee	8658	4.5	12.16
Ln(Revenue/Employee)	6884	10.56	2.8
Ln(Capital/Employee)	7197	9.56	3.03
Ln(Compensation/Employee)	7197	8.21	4.02
Ln(Share of Employee Benefits)	5910	2.44	0.46
Ln(Average Industry Compensation)	18153	10.27	0.72
Ln(Outside Compensation)	18153	10.23	0.65
Ln(State Unemployment Benefit)	18158	9.23	0.23
Ln(State Tax Rate)	17329	0.068	1.24
State Unemployment Rate	19796	6.18	2.22
Total IP	17077	1.79	13.11
% Competitive Advantage	17773	0.57	0.49
Percent of Government Sales	15956	7.52	21.67
% Corporation	19796	0.28	0.44
% Proprietorship	19796	0.34	0.47
Primary Owner's Age	19630	46.11	10.85
Primary Owner’s Experience	19743	13.44	10.9
% Primary Owner’s Education	19796	0.55	0.49
% Primary Owner’s Gender	19796	0.25	0.43
% Family Firms	19796	0.42	0.49

The data is from Kauffman Firm Survey’s confidential-use version. Sample includes years between 2004 and 2011. Sample sizes in years vary because of firm failures and refusals to participate. Share of employee benefits is the ratio of the value of benefits to the total amount of compensation. Values are imputed by the ECEC data from BLS. The binary variables for each employee benefits are matched with the average cost of the employer in the given industry in the given year. Total IP is the total number of copyrights, trademarks, and patents of the firm. State tax rate is calculated by using NBER’s TAXSIM. State unemployment rate is retrieved from BLS. Average industry compensation is constructed by using the average compensation levels of the given industry excluding the given firm. Annual total assets include values of equipment, land, buildings, vehicles, accounts receivables, cash, inventory, and other business property. Average compensation over $ 1 million is removed.

Table 1.2: First Stage and Reduced Form Regressions

	(1) First Stage	(2) Reduced Form
Ln(State Tax Rate)	0.037***	0.017*
	(0.003)	(0.008)
Ln(Capital/Employee)	0.004	0.145***
	(0.003)	(0.011)
Ln(Compensation/Employee)	-0.045***	0.629***
	(0.005)	(0.007)
Firm Characteristics	Yes	Yes
Owner Characteristics	Yes	Yes
Macroeconomic Indicators	Yes	Yes
Industry Dummies	Yes	Yes
State Dummies	Yes	Yes
Year Dummies	Yes	Yes
R-squared	0.187	0.40
Firms	1577	1577
N	5572	5572

The data is from Kauffman Firm Survey's confidential-use version. The first column reports the results of the first-stage of the control function approach; the second column reports the results for the reduced form estimation. Standard errors in parentheses and they are clustered by the firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.3: Probit Models of Health Insurance and Paid Sick Days

	(1) Health	(2) Paid Sick
Ln(State Tax Rate)	0.010*	-0.001
	(0.006)	(0.006)
Ln(Capital/Employee)	0.012	0.017**
	(0.007)	(0.007)
Ln(Compensation/Employee)	0.067***	0.050***
	(0.015)	(0.015)
Firm Characteristics	Yes	Yes
Owner Characteristics	Yes	Yes
Macroeconomic Indicators	Yes	Yes
Industry Dummies	Yes	Yes
State Dummies	Yes	Yes
Year Dummies	Yes	Yes
R-squared	0.07	0.07
Firms	702	702
N	2439	2439

The data is from Kauffman Firm Survey's confidential-use version. The dependent variables in the two columns are dummy variables indicating whether the health insurance and paid sick days are offered to the employee. Reported coefficients are marginal effects, and they are clustered by the firm. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Factors Associated with Employee Benefits

Dependent Variable: Binary Variable for Employee Benefit	(1) Full Sample	(2) Corporations	(3) Proprietorship	(4) Family
Ln(Capital/Employee)	0.019***	0.017***	0.025***	0.015***
	(0.003)	(0.003)	(0.005)	(0.003)
Ln(Compensation/Employee)	0.027***	0.024***	0.036***	0.021***
	(0.006)	(0.005)	(0.008)	(0.005)
Competitive Advantage	0.060***	0.053***	0.080	0.047
	(0.015)	(0.015)	(0.022)	(0.013)
Total IP	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.000)
State Unemployment	-0.01	-0.001	-0.001	-0.001
	(0.003)	(0.002)	(0.004)	(0.002)
Ln(Avg. Industry Compensation)	0.014	0.012	0.018	0.011
	(0.016)	(0.014)	(0.022)	(0.013)
Primary Owner's Experience	0.002**	0.002**	0.003**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Primary Owner's Education	0.047**	0.042**	0.062**	0.036*
	(0.017)	(0.015)	(0.023)	(0.014)
Primary Owner's Business in the Same Industry	0.027	0.024	0.036	0.021
	(0.018)	(0.017)	(0.025)	(0.014)
Primary Owner's Gender	-0.011	-0.009	-0.014	-0.008
	(0.023)	(0.020)	(0.030)	(0.018)
Primary Owner's Age	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Industry Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
Pseudo-R^2	0.11	0.11	0.11	0.11
Firms	726	218	147	227
N	2596	816	471	826

The data is from Kauffman Firm Survey's confidential-use version. The dependent variable is the binary variable indicating whether the firm offers an employee benefit. Reported coefficients are marginal effects. Each column reports the results of a probit regression that uses a different sub-sample. The first column is for the full sample; the second column is for the corporations; the third column is for proprietorships; the fourth column is for family firms. All financial variables are adjusted with industry deflators. Standard errors are in parentheses, and they are clustered by the firm.

Table 1.5: Effects of Employee Benefits on the Productivity of Startups

Estimation Method:	(1) OLS	(2) B&L-FE	(3) B&L-GMM	(4) Control Function
Ln(Share of Benefits)	0.161***	0.192***	0.141***	0.393**
	(0.049)	(0.047)	(0.052)	(0.192)
Ln(Capital/Employee)	0.152***	0.146***	0.534***	0.144***
	(0.015)	(0.023)	(0.212)	(0.011)
Ln(Compensation/Employee)	0.633***	0.471***	0.186***	0.652***
	(0.036)	(0.031)	(0.027)	(0.025)
Selection Effect				-0.115
				(0.086)
Firm's Fixed Effect				-0.04
				(0.269)
Firm Characteristics	Yes	Yes	Yes	Yes
Owner Characteristics	Yes	Yes	Yes	Yes
Macroeconomic Indicators	Yes	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-squared	0.414	0.294	0.11	0.408
Firms	1577	1577	1577	1577
N	5572	5572	5572	5572

The data is from Kauffman Firm Survey's confidential-use version. The dependent variable is log of revenue per employee. Each column reports the results of estimation with a different method. The first column shows the OLS results; the second and the third columns show the results of the two step approach designed by Black and Lynch (2001), fixed effects and GMM respectively; the fourth column shows the results from the control function approach. All financial variables are adjusted with industry deflators. Standard errors are in parentheses, and they are clustered by the firm.

Table 1.6: Effect of Benefits Before and After the Crisis

Dependent Variable: Ln(Revenue/Employee)	(1)	(2)	(3)
Estimation Method	OLS	B&L	CF
Ln(Share of Benefits)	0.248***	0.204***	0.475
	(0.063)	(0.051)	(0.289)
Ln(Capital/Employee)	0.153***	0.146**	0.144***
	(0.020)	(0.023)	(0.017)
Ln(Compensation/Employee)	0.645***	0.463***	0.652***
	(0.036)	(0.031)	(0.029)
Post Crisis	0.995***	0.027	1.45***
	(0.200)	(0.164)	(0.55)
Ln(Share of Benefits)x Post	-0.267***	-0.044	-0.223***
	(0.074)	(0.060)	(0.056)
R-squared	0.409	0.288	0.409
Firms	1577	1577	1577
N	5572	5572	5572
Firm Characteristics	Yes	Yes	Yes
Macroeconomic Indicators	Yes	Yes	Yes
Owner Characteristics	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes
Year Dummies	No	No	No

The data is from the Kauffman Firm Survey's confidential-use version. The dependent variable is log of revenue per employee. Each column presents the estimation results with a different approach. The first column reports the results with the OLS; the second column reports the results with the two-step approach by Black and Lynch (2001); the third column reports the results with the control function approach. All financial variables are adjusted with industry deflators. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7: Survival of the Startups

Dependent Variable: Binary Variable for Exiting the Sample	(1) Full	(2) Male	(3) Corp.	(4) Prop.	(5) Family
Ln(Revenue/Employee)	-0.001	-0.001	-0.001	-0.003	0.000
	(0.002)	(0.002)	(0.002)	(0.004)	(0.001)
Ln(Share of Benefits)	-0.003***	-0.003***	-0.003**	-0.007***	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)
Ln(Capital/Employee)	-0.005***	-0.005***	-0.006**	-0.011***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)
Ln(Compensation/Employee)	-0.003	-0.003	-0.003	-0.007	-0.001
	(0.003)	(0.003)	(0.003)	(0.006)	(0.001)
Comparative Advantage	-0.013**	-0.013**	-0.014**	-0.027*	-0.005**
	(0.006)	(0.006)	(0.007)	(0.014)	(0.002)
State Unemployment	-0.007***	-0.007***	-0.007***	-0.014***	-0.002***
	(0.002)	(0.002)	(0.002)	(0.004)	(0.001)
Ln(Average Industry)	-0.019**	-0.018***	-0.020***	-0.039***	-0.007
	(0.006)	(0.006)	(0.006)	(0.014)	(0.002)
Owner's Characteristics	Yes	Yes	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	No
Year Dummies	Yes	Yes	Yes	Yes	Yes
Pseudo-R^2	0.07	0.07	0.07	0.07	0.07
Firms	1097	1097	1097	1097	1097
N	3651	3651	3651	3651	3651

The data is from Kauffman Firm Survey's confidential-use version. Dependent variable is a binary variable whether the firm exits the KFS sample in a given year. Reported coefficients are marginal effects. Each column reports the results of a discrete time hazard model. Column 1 is for the full sample; column 2 is for the startups with male owners; column 3 shows the results for only corporations; column 4 is for startups with proprietorship status; column 5 shows the results for family startups. Standard errors in parentheses and they are clustered by the firm. Because the no other information can be retrieved in the firm's exit year, all continues variables are lagged values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: Sensitivity test with Smallest Startups

	(1)	(2)	(3)	(4)
Estimation Method:	OLS	B&L-FE	B&L-GMM	Control Function
Ln(Share of Benefits)	0.175***	0.208***	0.265***	0.318
	(0.051)	(0.047)	(0.058)	(0.337)
Ln(Capital/Employee)	0.148***	0.146***	0.534***	0.140***
	(0.015)	(0.023)	(0.212)	(0.014)
Ln(Compensation/Employee)	0.637***	0.482***	0.336***	0.656***
	(0.037)	(0.031)	(0.037)	(0.035)
Selection Effect				-0.140
				(0.078)
Firm Characteristics	Yes	Yes	Yes	Yes
Owner Characteristics	Yes	Yes	Yes	Yes
Macroeconomic Indicators	Yes	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-squared	0.419	0.314	0.105	0.414
Firms	1374	1374	1374	1374
N	4934	4934	4934	4934

The data is from Kauffman Firm Survey's confidential-use version. The dependent variable is log of revenue per employee. Smallest startups are the ones which employ fewer than 10 employees at any time periods. Each column reports the results of estimation with a different method. The first column shows the OLS results; the second and the third columns show the results of the two step approach designed by Black and Lynch (2001), fixed effects and GMM respectively; the fourth column shows the results from the control function approach. All financial variables are adjusted with industry deflators. Standard errors are in parentheses, and they are clustered by the firm. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Effects of Benefits under Attrition

Dependent Variable:	(1) Full	(2) Prop.	(3) Corp.	(4) Family	(5) Crisis	(6) Smallest
Ln(Share of Benefits)	0.153***	0.120***	0.240***	0.127**	0.246***	0.173***
	(0.049)	(0.054)	(0.065)	(0.056)	(0.063)	(0.052)
Ln(Capital/Employee)	0.269***	0.265***	0.265***	0.265***	0.266***	0.266***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Ln(Compensation/Employee)	0.574***	0.573***	0.573***	0.574***	0.587***	0.593***
	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)	(0.034)
Firm Characteristics	Yes	No	No	No	Yes	Yes
Owner Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Industry Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	No	Yes
R-squared	0.294	0.295	0.295	0.294	0.288	0.314
Firms	1577	1577	1577	1577	1577	1577
N	5572	5572	5572	5572	5572	5572

The data is from Kauffman Firm Survey's confidential-use version. Each column reports the estimation results for different subsamples. The estimation method is by Olley and Pakes (1996). The first column is for the full sample; the second column is for the proprietorships; the third column is for the corporations; the fourth column is for the family firms; the fifth column is for the 2008 crisis analysis; the last column is for the smallest firms. All financial variables are adjusted with industry deflators. * p < 0.10, ** p < 0.05, *** p < 0.0

Table 2.1: Descriptive Statistics

	Permanent Employment		Temporary Employment	
Variable	N	Mean(SD)	N	Mean(SD)
Standardized Test Score	3645	-0.04 (0.96)	1576	0.06 (1.07)
Experience	9181	0.65 (0.55)	3704	0.40 (0.47)
% Overtime	9181	0.42 (0.49)	3704	0.37 (0.48)
% Female	9181	0.48 (0.49)	3704	0.51 (0.49)
% Married	9181	0.59 (0.49)	3704	0.45 (0.49)
% Full-Time	8916	0.76 (0.42)	3582	0.68 (0.46)
% German	9181	0.97 (0.15)	3704	0.97 (0.15)
Number of Languages	9181	1.88 (1.05)	3704	2.1 (1.1)
% Informal Training	9181	0.34 (0.47)	3704	0.35 (0.47)
% Apprenticeship School	8295	47.05	3176	36.08
% Vocational Healthcare School	8295	7.80	3176	8.25
% Vocational Teacher School	8295	3.17	3176	3.31
% Technician/Foreman School	8295	9.19	3176	3.24
% University of Cooperative Education	8295	3.13	3176	1.54
% University of Applied Sciences	8295	9.90	3176	10.14
% University	8295	17.42	3176	36.34
% Doctorate	8295	0.08	3176	0.16
% Civil Servant Prep.	8295	2.25	3176	0.94

This table reports the descriptive statistics that we use in the analysis from the data called ALWA and its extension ALWA-LinU. All entries for binary variables show the percentage of employees with a given characteristics in the sample. The unit of observation is the employment spell. Standard deviations are reported in parentheses. Parent education and size of the firm are categorical variables. Parent education takes 6 different values and shows the highest educational attainment of the parents. 40.9 percent attended Hauptschule (Basic School); 24.8 percent attended Realschule (Intermediate School); 24.5 percent attended University as the highest educational attainment. Size of the firm takes 7 different values. 7.5 percent has fewer than 5 employees; 12.9 percent has 5-10 employees; 13.1 percent has 10-20 employees; 23.8 percent has 20-100 employees;10.2 percent has 100-200 employees; 20.9 has 200-2000 employees; 11.2 has more than 2000 employees.

Table 2.2: Determinants of Pre-Hiring Screening

	(1) Probit Coefficient	(2) Marginal Effect	(3) LPM Coefficient
Cognitive Ability	-0.074**	-0.021**	-0.020**
	(0.033)	(0.009)	(0.009)
Experience	-0.376***	-0.108***	-0.097***
	(0.089)	(0.025)	(0.022)
Age	-0.006	-0.002	-0.003
	(0.007)	(0.002)	(0.002)
Female	0.178**	0.051**	0.049**
	(0.071)	(0.020)	(0.020)
Unemployment Rate	-0.335	-0.097	-0.000
	(0.215)	(0.062)	(0.017)
Firm Size	0.077***	0.022***	0.021***
	(0.015)	(0.004)	(0.004)
Vocational Healthcare	0.207*	0.060*	0.058*
	(0.110)	(0.032)	(0.034)
Vocational Teacher	0.217	0.063	0.058
	(0.152)	(0.044)	(0.047)
Technician/Foreman	-0.207*	-0.060*	-0.047*
	(0.113)	(0.033)	(0.024)
University of Cooperative Education	-0.660***	-0.191***	-0.121***
	(0.249)	(0.072)	(0.034)
University of Applied Sciences	0.042	0.012	0.013
	(0.114)	(0.033)	(0.030)
University	0.528***	0.152***	0.158***
	(0.094)	(0.027)	(0.028)
Doctorate	1.551***	0.448***	0.502***
	(0.269)	(0.077)	(0.080)
R-squared	0.095		0.090
Employee-Job Spells	3481	3481	3481
Employees	1324	1324	1324

Dependent variable is the binary variable indicating whether the employee has temporary employment. Standard errors are reported in parentheses. Marginal effects are calculated at the sample means of other independent variables. In addition to the shown variables, I control for informal training received by the employee prior to the job, number of spoken foreign languages, nationality, a binary variable indicating whether the job is full time, marital status, highest parental educational attainment, Parent

(Continued from Table 2.2)

education and size of the firm are categorical variables. Parent education takes 6 different values and shows the highest educational attainment of the parents. Size of the firm takes 7 different values. (Up to 5 employees, 5-10 employees, 11-19 employees, 20-99 employees, 100-199 employees, 200-1999 employees, and more than 2000 employees.) School type: 1 is apprenticeship; School type: 2 is vocational training for healthcare professionals; School type: 3 is vocational teacher training; School type: 4 is technician/foreman training; School type: 5 is university of cooperative education; School type: 6 is university of applied sciences; School type: 7 is standard university degree; School type: 8 is doctorate; School type: 11 is civil servant preparation (not shown). I also control for industry and year fixed effects in all specifications. East Germany and years before 1985 are excluded from the estimation.

Table 2.3: Sensitivity Test for Pre-Hiring Screening

	(1) Without School	(2) ME	(3) Without Cog. Ability	(4) ME
Cognitive Ability	-0.037	-0.011		
	(0.029)	(0.009)		
Experience	-0.409***	-0.122***	-0.371***	-0.107***
	(0.090)	(0.026)	(0.089)	(0.025)
Age	0.005	0.002	-0.005	-0.001
	(0.006)	(0.002)	(0.007)	(0.002)
Female	0.162**	0.048**	0.211***	0.061***
	(0.066)	(0.020)	(0.069)	(0.020)
Unemployment Rate	-0.351*	-0.105*	-0.335	-0.097
	(0.208)	(0.062)	(0.214)	(0.062)
Firm Size	0.086***	0.026***	0.076***	0.022***
	(0.015)	(0.004)	(0.015)	(0.004)
Vocational Healthcare			0.196*	0.058*
			(0.110)	(0.031)
Vocational Teacher			0.206	0.059***
			(0.151)	(0.043)
Technician/Foreman			-0.232**	-0.067***
			(0.112)	(0.032)
University of Cooperative Education			-0.696***	-0.201***
			(0.248)	(0.071)
University of Applied Sciences			0.006	0.001
			(0.111)	(0.020)
University			0.488***	0.141***
			(0.091)	(0.026)
Doctorate			1.491***	0.433
			(0.268)	(0.076)
R-squared	0.071		0.09	
Employee-Job Spells	3829	3829	3481	3481
Employees	1518	1518	1324	1324

Dependent variable is the binary variable indicating whether the employee has temporary employment.

Table 2.4: Determinants of on-the-job Training

	(1) Probit Coef.	(2) ME	(3) LPM Coef.
Cognitive Ability	0.104***	0.038***	0.038***
	(0.038)	(0.014)	(0.012)
Temporary Employment	-0.471***	-0.171***	-0.147***
	(0.065)	(0.024)	(0.019)
Cog. Ability X Temporary Employment	-0.199***	-0.072***	-0.063***
	(0.066)	(0.024)	(0.018)
Experience	0.391***	0.142***	0.128***
	(0.067)	(0.024)	(0.022)
Age	-0.015**	-0.006**	-0.005**
	(0.007)	(0.002)	(0.002)
Unemployment Rate	-0.254**	-0.092**	-0.020
	(0.107)	(0.039)	(0.019)
Firm Size	0.097***	0.035***	0.032***
	(0.015)	(0.006)	(0.005)
Vocational Healthcare	0.011	0.004	0.009
	(0.124)	(0.045)	(0.039)
Vocational Teacher	0.186	0.068	0.057
	(0.172)	(0.062)	(0.056)
Technician/Foreman	0.061	0.022	0.018
	(0.117)	(0.043)	(0.037)
University of Cooperative Education	0.014	0.005	0.011
	(0.152)	(0.055)	(0.054)
University of Applied Sciences	0.041	0.015	0.019
	(0.111)	(0.040)	(0.037)
University	0.107	0.039	0.042
	(0.092)	(0.033)	(0.030)
Doctorate	.	.	0.554***
	.	.	(0.057)
R-squared	0.152		0.186
Employee-Job Spells	3474	3474	3474
Employees	1321	1321	1321

Dependent variable is the binary variable indicating whether the employee received employer paid on-the-job training during his employment.

Table 2.5: Sensitivity Tests for on-the-job Training

	(1) Without School	(2) ME	(3) Without Cog. Ability	(4) ME
Cognitive Ability	0.099***	0.035***		
	(0.034)	(0.012)		
Fixed Contract	-0.402***	-0.144***	-0.467***	-0.169***
	(0.062)	(0.022)	(0.063)	(0.03)
Cog. Ability X Fixed Contract	-0.131**	-0.047**		
	(0.059)	(0.021)		
Experience	0.414***	0.148***	0.391***	0.142***
	(0.065)	(0.024)	(0.066)	(0.024)
Age	-0.015**	-0.005**	-0.015**	-0.005**
	(0.006)	(0.002)	(0.006)	(0.002)
Unemployment Rate	-0.243**	-0.087**	-0.265**	-0.096**
	(0.100)	(0.036)	(0.107)	(0.039)
Firm Size	0.098***	0.035***	0.096***	0.035***
	(0.015)	(0.005)	(0.015)	(0.005)
Vocational Healthcare			-0.039	0.006
			(0.123)	(0.04)
Vocational Teacher			0.179	0.065
			(0.171)	(0.062)
Technician/Foreman			0.080	0.029
			(0.116)	(0.042)
University of Cooperative Education			0.045	0.016
			(0.151)	(0.054)
University of Applied Sciences			0.075	0.027
			(0.109)	(0.039)
University			0.127	0.046
			(0.089)	(0.032)
Doctorate			-	-
			-	-
R-squared	0.154		0.149	
Employee-Job Spells	3825	3825	3474	3474
Employees	1517	1517	1321	1321

Dependent variable is the binary variable indicating whether the employee received training paid by the employer during his employment.

Table 2.6: Determinants of Temp-to-Perm Transitions

	(1) Probit Coef.	(2) ME	(3) LPM Coef.
Training	0.592***	0.136***	0.179***
	(0.150)	(0.033)	(0.044)
Cognitive Ability	-0.135	-0.031	-0.014
	(0.092)	(0.021)	(0.015)
Cog. Ability X Training	0.139	0.032	0.011
	(0.136)	(0.031)	(0.038)
Experience	0.750***	0.172***	0.187***
	(0.169)	(0.041)	(0.052)
Age	-0.032*	-0.007*	-0.009**
	(0.017)	(0.004)	(0.004)
Female	0.238	0.055	0.059*
	(0.147)	(0.034)	(0.035)
Unemployment Rate	0.443	0.102	-0.008
	(0.285)	(0.064)	(0.028)
Firm Size	0.107***	0.025***	0.024***
	(0.038)	(0.009)	(0.008)
Vocational Healthcare	-0.870***	-0.200***	-0.173***
	(0.282)	(0.064)	(0.057)
Vocational Teacher	-0.231	-0.053	-0.060
	(0.293)	(0.067)	(0.082)
Technician/Foreman	0.076	0.017	0.024
	(0.307)	(0.070)	(0.089)
University of Cooperative Education	-0.192	-0.044	-0.084
	(0.657)	(0.151)	(0.172)
University of Applied Sciences	-0.957***	-0.220***	-0.205***
	(0.286)	(0.065)	(0.058)
University	-0.787***	-0.181***	-0.174***
	(0.218)	(0.050)	(0.047)
Doctorate	.	.	-0.409**
	.	.	(0.175)
R-squared	0.257		0.187
Employee-Job Spells	750	750	750
Employees	501	501	501

Dependent variable is the binary variable indicating whether the employee move up to a permanent employment within the same firm from temporary employment.

Table 2.7: Sensitivity Tests for Temp-to-Perm Transitions

	(1) Without School	(2) ME	(3) Without Cog. Ability	(4) ME
Cognitive Ability	-0.072	-0.017		
	(0.077)	(0.018)		
Training	0.683***	0.162***	0.580***	0.134***
	(0.138)	(0.032)	(0.148)	(0.033)
Cog. Ability X Training	0.070	0.017		
	(0.125)	(0.030)		
Experience	0.771***	0.182***	0.753***	0.174***
	(0.162)	(0.040)	(0.168)	(0.040)
Age	-0.038***	-0.009***	-0.029*	-0.006*
	(0.014)	(0.003)	(0.016)	(0.003)
Unemployment Rate	0.295	0.070	0.445	0.103
	(0.262)	(0.061)	(0.281)	(0.061)
Firm Size	0.106***	0.025***	0.106***	0.024**
	(0.034)	(0.008)	(0.037)	(0.008)
Vocational Healthcare			-0.857***	-0.198***
			(0.283)	(0.064)
Vocational Teacher			-0.252	-0.058
			(0.292)	(0.068)
Technician/Foreman			0.021	0.004
			(0.302)	(0.070)
University of Cooperative Education			-0.252	-0.058
			(0.653)	(0.151)
University of Applied Sciences			-0.997***	-0.230***
			(0.280)	(0.064)
University			-0.828***	-0.191***
			(0.209)	(0.048)
R-squared	0.223		0.254	
Employee-Job Spells	854	854	750	750
Employees	580	580	501	501

Dependent variable is the binary variable indicating whether the employee move up to a permanent employment within the same firm from temporary employment.

Table 3.1: Summary Statistics of Dependent Variables

	Full	Treatments	Controls	Estimates	
				Raw	Matched
	(1)	(2)	(3)	(4)	(5)
House Prices	1442.5 (893.6)	2158.7 (1499.5)	1377.7 (785.4)	781.0 (44.9)	481.8 (52.7)
Rents	6.49 (2.96)	8.66 (4.3)	6.30 (2.05)	2.35 (0.12)	1.27 (0.15)
Sample	13740	1140	12600	13740	11244

Treatment refers to the neighborhoods with ISPARKS. Column (4) shows raw differences between treatment and control group; column (5) shows the difference in the matched sample, which is created by the propensity score matching method. Radius matching within caliper is used here, but other methods such as kernel and radius give similar results. Standard deviations in the first four columns and standard error in the last column is shown in parentheses.

Table 3.2: Summary Statistics of the Independent Variables

	Obs.	Mean	Std. Dev.
Uneducated Ratio	13740	9.4	5.7
Primary School Ratio	13740	24.8	8.9
High School Ratio	13740	24.5	5.3
Population Density	13740	13.7	11.6
Poverty Measure	12708	17.0	0.4
Average Rent	3904	527.7	293.6
Average Income	3904	1545.5	703.8
Average HH Size	3904	3.3	0.62
Vehicle Ratio	3904	0.37	4.27
Parking Ratio	3904	1.05	7.2

First set of variables comes from TURKSTAT ADNKS; second set of variables comes from Municipality Survey of Istanbul. Vehicle ratio is calculated by dividing the total number of cars to the population; while parking ratio is the number of parking spaces divided by the population of the neighborhood.

Table 3.3 Impact of ISPARKs on Housing Prices (Unmatched Sample)

	(1) House Prices	(2) House Prices	(3) Rents	(4) Rents
ISPARK	-244.926***	-250.653***	-0.187*	-0.207*
	(37.566)	(39.412)	(0.104)	(0.115)
Uneducated	-4.323**	-41.772***	-0.013***	-0.058***
	(1.695)	(4.671)	(0.005)	(0.020)
Primary School	-17.056***	-5.982***	-0.072***	-0.064***
	(1.429)	(1.792)	(0.004)	(0.007)
High School	-17.267***	-11.851***	-0.023***	-0.013
	(2.875)	(3.313)	(0.008)	(0.009)
Density	0.089	-0.136	-0.001**	-0.002
	(0.164)	(0.347)	(0.001)	(0.001)
Poverty Measure		-0.258***		-0.004***
		(0.032)		(0.001)
R-squared	0.961	0.960	0.964	0.962
N	13740	10908	13740	10908

Dependent variables are house prices per square meter and rents per square meter. *Density* is constructed by Population/Area. Percentage of people with free healthcare (yesil kart) is used as a *poverty measure*. Huber-White SE's are reported in parentheses.

Table 3.4. Impact of ISPARKs on Housing Prices (Matched Sample)

	(1) House Prices	(2) House Prices	(3) Rents	(4) Rents
ISPARK	-180.478***	-197.761***	-0.079	-0.043
	(47.424)	(50.854)	(0.135)	(0.153)
Uneducated	-6.632**	-86.809***	-0.007	-0.202***
	(3.307)	(10.111)	(0.007)	(0.035)
Primary School	-13.936***	-0.147	-0.073***	-0.035***
	(2.982)	(4.081)	(0.006)	(0.011)
High School	-35.443***	-64.770***	-0.006	-0.031*
	(8.120)	(9.939)	(0.016)	(0.018)
Density	1.209**	5.248***	0.003**	0.017***
	(0.520)	(1.157)	(0.001)	(0.003)
Poverty Measure		-0.258***		0.001**
		(0.080)		(0.000)
R-squared	0.961	0.961	0.971	0.969
N	10080	8016	10080	8016

Dependent variables are house prices per square meter and rents per square meter. *Density* is constructed by Population/Area. Percentage of people with free healthcare (yesil kart) is used as a *poverty measure*. Huber-White SE's are reported in parentheses. Propensity score matching is carried out by radius matching with a caliper of 0.01 by Dehejia and Wahba (2002). Idea is to use the all members within a caliper for matching. Results for other algorithms for matching are reported in the Appendix.

Table 3.5. Effects on ISPARKs over Time

	(1) Unmatched House Prices	(2) Matched House Prices	(3) Unmatched Rents	(4) Matched Rents
Ispark $_{t+2}$	-34.162	-72.996	-0.184	-0.209
	(65.662)	(81.401)	(0.171)	(0.247)
Ispark $_{t+1}$	9.221	19.046	0.152	0.287
	(114.131)	(150.916)	(0.373)	(0.467)
Ispark $_{t0}$	-29.018	4.194	0.114	0.261
	(88.559)	(114.524)	(0.321)	(0.394)
Ispark $_{t-1}$	-43.167	13.745	-0.072	-0.002
	(96.865)	(112.830)	(0.270)	(0.335)
Ispark $_{t-2}$	-82.748	-39.313	-0.081	-0.066
	(86.042)	(101.615)	(0.165)	(0.216)
Ispark $_{t-3}$	-105.049	-70.285	0.015	0.061
	(71.341)	(84.621)	(0.100)	(0.127)
Ispark $_{t-4}$	-202.154***	-152.155***	-0.027	0.031
	(56.815)	(58.457)	(0.097)	(0.119)
Ispark $_{t-5}$	-305.514***	-299.135***	-0.373***	-0.348***
	(32.295)	(35.938)	(0.087)	(0.099)
R-squared	0.962	0.963	0.964	0.971
N	13740	10980	13740	10980

Dependent variables are House Prices per square meter and Rents per square meter. All population variables except the average people in a household are recorded in thousands and they have been controlled as well. Huber-White SEs are reported in parentheses. Propensity matching is carried out by radius matching within caliper method.

Table 3.6: Placebo Test

	(1) House Prices	(2) House Prices	(3) Rents	(4) Rents
ISPARK	-12.649	24.836	0.236***	0.334***
	(29.227)	(30.478)	(0.065)	(0.074)
Uneducated	-18.016***	-142.951***	-0.008	-0.333***
	(2.377)	(7.317)	(0.006)	(0.025)
Primary School	-30.284***	12.054***	-0.080***	0.024***
	(2.048)	(3.000)	(0.005)	(0.009)
High School	-23.634***	-17.488***	-0.038***	-0.021
	(2.899)	(3.339)	(0.011)	(0.014)
Density	0.332	4.127***	0.006***	0.023***
	(0.383)	(0.723)	(0.001)	(0.003)
Poverty Measure		-0.184***		0.001**
		(0.059)		(0.000)
R-squared	0.965	0.965	0.973	0.973
N	13740	10980	13740	10980

Dependent variables are House Prices per square meter and Rents per square meter. Population variables except the average people in a household are recorded in thousands. Percentage of people with free healthcare (yesil kart) is used as a poverty measure. Huber-White SEs are reported in parentheses. Placebo treatment is carried out 1 year ago before the actual treatment date. Propensity matching is carried out by nearest neighbor without replacement within caliper method.

Table 3.7: Estimation of European and Asian sides (unmatched)

Panel A:	(1) Europe House Prices	(2) Asia House Prices	(3) Europe House Prices	(4) Asia House Prices
ISPARK	-380.093***	-3.677	-337.717***	-19.910
	(55.771)	(13.798)	(54.386)	(16.118)
Uneducated	-9.843***	2.020	-89.539***	-11.717**
	(2.312)	(1.971)	(6.650)	(5.836)
Primary_School	-39.024***	3.565**	-23.261***	10.526***
	(2.019)	(1.668)	(2.439)	(2.233)
High_School	-8.730**	-25.053***	7.901*	-23.867***
	(4.097)	(2.835)	(4.549)	(3.087)
Density	-0.038	-6.124***	-5.435***	-9.587***
	(0.168)	(0.747)	(0.481)	(0.889)
Poverty_Measure			-1.681***	-0.050
			(0.087)	(0.032)
R-squared	0.970	0.944	0.970	0.942
N	7596	6144	6036	4872

Panel B:	(5) Europe Rents	(6) Asia Rents	(7) Europe Rents	(8) Asia Rents
ISPARK	-0.350**	0.170***	-0.364**	0.266***
	(0.160)	(0.036)	(0.166)	(0.033)
Uneducated	-0.011	-0.013**	-0.082***	-0.098***
	(0.007)	(0.006)	(0.030)	(0.017)
Primary_School	-0.107***	-0.036***	-0.109***	-0.004
	(0.006)	(0.005)	(0.010)	(0.007)
High_School	-0.003	-0.036***	0.023	-0.040***
	(0.012)	(0.008)	(0.014)	(0.009)
Density	-0.003***	0.021***	-0.017***	0.024***
	(0.001)	(0.003)	(0.002)	(0.004)
Poverty_Measure			-0.004***	0.001***
			(0.000)	(0.000)
R-squared	0.967	0.959	0.965	0.957
N	7596	6144	6036	4872

Dependent variables are house prices per square meter and rents per square meter. *Density* is constructed by Population/Area. Percentage of people with free healthcare (yesil kart) is used as a *poverty measure*. Huber-White SE’s are reported in parentheses.

Table 3.8: Estimation of European and Asian sides separately (matched)

Panel A:	(1) Europe House Prices	(2) Asia House Prices	(3) Europe House Prices	(4) Asia House Prices
ISPARK	-220.130***	47.483***	-216.011***	58.596***
	(59.404)	(16.006)	(57.666)	(22.173)
Uneducated	-12.943***	4.819**	-144.866***	-33.129***
	(3.664)	(2.443)	(11.677)	(8.544)
Primary School	-50.899***	14.368***	-16.517***	24.997***
	(3.589)	(2.102)	(5.196)	(3.857)
High School	-19.020**	-30.571***	-8.162	-30.313***
	(8.175)	(5.189)	(10.620)	(5.322)
Density	1.314***	-4.606***	-0.467	-18.056***
	(0.458)	(1.775)	(1.182)	(1.775)
Poverty Measure			-1.837***	-0.388***
			(0.266)	(0.079)
R-squared	0.970	0.962	0.971	0.959
N	6672	4836	5304	3840

Panel B:	(5) Europe Rents	(6) Asia Rents	(7) Europe Rents	(8) Asia Rents
ISPARK	-0.101	0.247***	-0.083	0.419***
	(0.176)	(0.046)	(0.181)	(0.047)
Uneducated	-0.002	-0.002	-0.243***	-0.134***
	(0.010)	(0.007)	(0.041)	(0.024)
Primary School	-0.105***	-0.054***	-0.045***	-0.012
	(0.008)	(0.006)	(0.015)	(0.008)
High School	0.032	-0.001	0.073***	-0.001
	(0.020)	(0.012)	(0.024)	(0.012)
Density	0.005***	0.012**	0.006*	0.014**
	(0.001)	(0.005)	(0.003)	(0.006)
Poverty Measure			-0.003***	0.001***
			(0.001)	(0.000)
R-squared	0.971	0.971	0.970	0.969
N	6672	4836	5304	3840

Dependent variables are house prices per square meter and rents per square meter. *Density* is constructed by Population/Area. Percentage of people with free healthcare (yesil kart) is used as a *poverty measure*. Huber-White SE's are reported in parentheses. Propensity score matching is carried out by radius matching with a caliper of 0.01 by Dehejia and Wahba (2002). Idea is to use the all members within a caliper for matching. Results for other algorithms for matching are reported in the Appendix.

Table 3.9: Sensitivity Test without Old Counties

	(1) House Prices	(2) House Prices	(3) Rents	(4) Rents
ISPARK	-148.856***	-162.837***	-0.046	-0.012
	(42.558)	(44.633)	(0.122)	(0.136)
Uneducated	-7.172**	-108.690***	-0.003	-0.163***
	(3.136)	(9.453)	(0.007)	(0.032)
Primary School	-23.349***	1.414	-0.079***	-0.040***
	(2.771)	(4.087)	(0.006)	(0.011)
High School	-31.222***	-46.153***	0.018	0.019
	(6.769)	(8.368)	(0.014)	(0.017)
Density	1.479***	5.269***	0.004***	0.016***
	(0.427)	(0.891)	(0.001)	(0.003)
Poverty Measure		-0.174**		0.001**
		(0.073)		(0.000)
R-squared	0.963	0.963	0.970	0.968
N	10824	8604	10824	8604

Old counties are Beyoglu, Eyup, Fatih, Sisli where informal parking market was the most severe before ISPARK. Dependent variables are house prices per square meter and rents per square meter. *Density* is constructed by Population/Area. Percentage of people with free healthcare (yesil kart) is used as a *poverty measure*. Huber-White SE's are reported in parentheses. Propensity score matching is carried out by radius matching with a caliper of 0.01 by Dehejia and Wahba (2002). Idea is to use the all members within a caliper for matching.

Table 3.10: Sensitivity Tests with Different Matching Methods

	(1) Radius House Prices	(2) Kernel House Prices	(3) Nearest House Prices	(4) Mahalanobis House Prices
ISPARK	-197.761***	-217.178***	-247.161***	-153.395***
	(50.854)	(61.504)	(51.289)	(52.122)
Uneducated	-86.809***	-72.126***	-78.280***	-135.725***
	(10.111)	(13.247)	(20.469)	(15.770)
Primary School	-0.147	-45.516***	-13.962**	17.834***
	(4.081)	(7.062)	(5.919)	(5.490)
High School	-64.770***	-78.927***	-76.653***	-8.947
	(9.939)	(15.603)	(12.877)	(12.001)
Density	5.248***	10.698***	6.452***	1.342
	(1.157)	(1.598)	(1.465)	(2.260)
Poverty Measure	-0.258***	0.328***	-0.592***	-0.840***
	(0.080)	(0.123)	(0.156)	(0.174)
R-squared	0.961	0.969	0.960	0.960
N	8016	2052	2208	2280

	(5) Radius Rents	(6) Kernel Rents	(7) Nearest Rents	(8) Mahalanobis Rents
ISPARK	-0.043	-0.378*	-0.160	0.031
	(0.153)	(0.209)	(0.155)	(0.158)
Uneducated	-0.202***	0.061	0.133	-0.339***
	(0.035)	(0.078)	(0.106)	(0.041)
Primary School	-0.035***	-0.161***	-0.145***	0.040***
	(0.011)	(0.026)	(0.026)	(0.015)
High School	-0.031*	-0.098***	-0.032	0.122***
	(0.018)	(0.034)	(0.027)	(0.030)
Density	0.017***	0.028***	0.014***	0.029***
	(0.003)	(0.005)	(0.004)	(0.006)
Poverty Measure	0.001**	0.003***	0.001	0.001*
	(0.000)	(0.000)	(0.000)	(0.000)
R-squared	0.969	0.966	0.966	0.973
N	8016	2052	2208	2280

Dependent variables are house prices per square meter and rents per square meter. *Density* is constructed by Population/Area. Percentage of people with free healthcare (yesil kart) is used as a *poverty measure*. Huber-White SE's are reported in parentheses. Propensity score matching is carried out by radius matching with a caliper of 0.01 by Dehejia and Wahba (2002) in Columns 1 and 5, Kernel matching in Columns 2 and 6, nearest neighbor with replacement in Columns 3 and 7, and Mahalanobis covariate matching in Columns 4 and 8 respectively.

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