Incidence, Optimal Use and Rationale of Place-Based Job Creation Programs

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Abstract

First, this paper empirically evaluates the incidence of the Japanese place-based job creation program, which is rarely studied in Japan. The program increases employment, especially in the agricultural, retail trade and service sectors that most treated cities promote. Second, this paper theoretically demonstrates that this program's optimal subsidy is to provide cities with improving the establishment of cropland leasehold rights and to empirically verify this result. Third, this paper assesses the rationale of this program and does not observe a strong reduction in sales, workers and establishments in the neighboring cities of the treated city.

Keywords: place-based policy, job creation, optimal subsidy, rationale, externality effect

JEL code: J23, J68, R23, H22, H23

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1. Introduction

This paper analyzes the job creation program in Japan. Place-based policies are currently conducted in many countries such as Europe, the U.S. and Asian countries. Governments that hope to help residents in disadvantaged areas base their equity criteria in the face of differences in local labor market outcomes across cities and regions. Most previous studies have emphasized assessing the subsidy's impact on job creation, and most academic research and official government documents evaluate how the policies affect local employment, usually with the goal of computing the number of job created per dollar/yen spent. However, this emphasis is not enough. The placed-based policies are efficiency only under some conditions including imperfect labor mobility, a market failure, missing insurance/credit constraints, public good and so on. Furthermore, if place-based polices positively affect treated cities, but the externality effect on other cities results in a negative impact, the program is not rational.

Kline and Moretti (2014a) argue that the job growth resulting from place-based policy induces migration into the treated community and increases living costs, thus, the benefit from a subsidy turns to land owner under the perfect mobility. From this perspective, Busson, Gregory and Kline (2013) analyze the federal urban empowerment zone program. Busson, Gregory and Kline (2013) note that if most agents are inframarginal in their commuting and residential decisions, the deadweight loss will be small, and local workers will reap the benefits of place-based interventions. If agents have nearly identical preferences, the deadweight loss will be substantial, and government expenditures will be capitalized into land rents. They use confidential micro data and find that the empowerment zone program increases employment and wages without triggering corresponding increases in population and local cost of living. This paper evaluates precisely the incidence of the place-based job creation program in Japan including the resident's mobility.

Furthermore, this paper considers the optimal use of this program's subsidy. Kline and Moretti (2013) show that the targeting of less-productive areas implicit in the place-based subsidy is efficient when the hiring cost is excessive using a theoretical model. They say that the subsidy is optimal even when labor is perfectly mobile across cities. Few papers have empirically studied the optimal use of place-based policy and market failure. It is important to consider the optimal

use because the budgets of policy programs are always limited. This paper theoretically demonstrates and empirically examines the optimal use of place-based job creation programs in Japan with the market failure that comes from hiring costs[1].

The place-based job creation programs that this paper analyzes are conducted in rural areas where the number of vacancies per a job seeker is lower than the national average. These areas have an aging population problem and face global competition. Aging owners put less effort into filling vacancies because they might consider closing their establishments. Aging landowners constrain young workers who have no land but have the will to work in agricultural sectors. The transfer of rights for agricultural land (sale/leasing) has been historically regulated. The establishment of leasehold rights is now allowed to promote effective land use, but the establishment of leasehold rights varies by region. This paper demonstrates that the areas with improving the establishment of leasehold rights should have higher subsidies from the place-based job creation programs. In other words, the communities where the leasehold rights are well established can use the subsidy of the place-based job creation program efficiency. Given the aging population, this paper makes a simple model and verifies empirically the results.

Although this program is efficient due to market failure, we need to consider whether the program is a zero-sum game. In other words, this paper assesses the rationale of this program. If consumption demand increases in the treated cities, thus increasing the number of jobs created, but the consumption demand decreases in neighboring cities, thus resulting in a decrease in the number of jobs, the gain in the treated cities is cancelled out by the loss in the neighboring cities, and the total gain at the national level is zero. Bartik (1991) argues that the place-based policy increases the productivity of firms in the treated zones and contributes to economic growth at the national level, not just at the local level. Freedman (2012) estimates the effect of new markets tax credit using regression discontinuity design and shows the spillover effect on neighboring is small. Ham, Swenson, İmrohoroğlu and Song (2011) find the positive impact of state enterprise zones, federal empowerment zones and federal enterprise community programs on local labor markets using the difference in difference estimation and insignificant spilover impact to neighboring

[1] Higuchi (2001), Nakata (2007) and Azetsu and Fukushige (2009) have evidenced high hiring costs in the Japanese labor market.

census tracts. In contrast, Chirinko and Wilson (2008) shows that the U.S. states' investment tax credit increases investment at the treated states, but the neighboring states' capital price increases (and investments go down). Wilson (2009) shows that R&D tax credit increases the own state's R&D, but the decreases in R&D in other states cancel out this increasing. He demonstrates that the own state's magnitude of the elasticity is the same with that of other states. Goolsbee and Maydew (2000) also show that the reducing the payroll tax weight increases the employment in the own state, but decreases the employment in other states. Roughly speaking, the studies that consider the effect on other states' price show the negative externalities. Freedman (2012) notes that prior evidence on spatial spillovers associated with geographically targeted incentive programs is mixed.

In either case, few empirical studies address this issue about job creation programs. This paper considers the impact on neighboring cities of the treatment group. Determining whether the increase in consumer demand in the treated cities is cancelled out by the loss in neighboring cities makes sense in this place-based job creation program, as this program offers how-to seminars for local establishments to help them attract consumers to their local goods, in many cases using local specialty agricultural products.

Kline and Moretti (2014a) also discuss the related rationale of the agglomeration economy. Moving workers from city *b* to city *a* caused by that city *a* is designated some place based policy will yield an agglomeration gain in city *a* and a loss in city *b*. Social welfare can be raised if the gain is greater than the corresponding loss. At a social optimum, the gains and the losses cancel each other out exactly. Kline and Moretti (2014b) show that whether this cancelling out occurs depends on the elasticity of local productivity with respect to density. If this elasticity is constant, the gains of reallocating a worker from one community to another are always cancelled out. They show serious flaws in the agglomeration rationale for spatially progressive subsidies in U.S. manufacturing.

This paper's contributions to academic literature include a theoretical and empirical demonstration of optimal use and an examination of the programs' rationale. As the fundamental issue, however, a detailed empirical evaluation of Japanese place-based policy for job creation has rarely been conducted prior to this study. One reason for this research gap is a lack of data. The place-based job creation programs that this paper analyzes have the advantage of being able to

provide data, being a financial program and being able to estimate a pure effect. Some policy programs do not publish detailed data. Some policy programs are place-based, but all prefectures are targeted. Some policy programs, e.g., programs conducted by the Ministry of Agriculture, Forestry and Fisheries and the Ministry of Land, Infrastructure, Transport and Tourism, seek to develop local areas, not just create jobs. Major place-based policy for job creation is a designated structural reform district, and many studies discuss this policy in Japan. However, a designated structural reform district is exempted regulations and is not financial benefit, such as a tax reduction[2] or subsidy. Since 2016, the government has reformed designated structural reform districts and designed new special zones that reduce taxes. Prior to the effect of these new special zones, the results presented in this manuscript will provide some milestones regarding the efficiency from the place-based policy.

To evaluate the incidence of the place-based job creation program, this paper makes mention of a control group. Neumark and Kolko (2010) carefully discuss how to make a control group in their estimation of California's enterprise zone program. They argue that some previous studies' broad control groups, e.g., estimating the effects of enterprise zones in a number of states compared with areas in states outside the enterprise zones, make no sense. In addition, they argue that matching census tracts with enterprise zones that use propensity score matching does not account for unobservable sources of job growth differences. Similar to Billings (2009), they designate a narrow buffer just outside the enterprise zone as the control group. However, the place-based job creation program that this paper examines includes seminars on how to attract people to buy the local products from the treatment, making it possible to push neighboring cities' consumer demand far away from neighboring cities' markets. Therefore, this paper does not use Neumark and Kolko's above-described approach. Following Busso and Kline (2007)[3], Neumark and Kolko

[2] Financial special zone, IT industry special zone and distribution special zone in Okinawa prefecture benefit tax reduction, but labor market condition of Okinawa prefecture such as unemployment insurance system differ from other prefectures because U.S. military base covers land area at a great rate.

[3] This refers to an unpublished paper, entitled, "Do Local Economic Development Programs Work? Evidence from the Federal Empowerment Zone Program", which was referenced by Neumark and Kolko (2010). The definition of the control group appears to be the same as that used in the method reported by Busson, Gregory and Kline (2013).

(2010) also use those added later to enterprise zones as control groups. Busson, Gregory and Kline (2013) use rejected and future applicants to the empowerment program as a control group. This paper makes a control group like that of Busson, Gregory and Kline (2013).

The aims of this paper are as follows. First, this paper empirically and precisely evaluates the incidence of place-based job creation programs in Japan. Second, this paper theoretically demonstrates that this subsidy's optimal use is providing cities with improving of the establishment of land leasehold rights, if labor market failure exists. This paper then estimates whether these cities benefit from a larger effect of this program. Third, this paper assesses this program's rationale, i.e., it determines whether the program is a zero-sum game. This paper examines whether the loss of sales, workers and establishments in the cities neighboring the treated cities cancels out the gain in the treated cities.

The remainder of this paper is organized as follows. Section 2 describes the place-based job creation programs that this paper evaluates. Next, Section 3 presents the empirical approach for examining the incidence of the programs. Section 3 also explains the data, displays the fundamental evidence using figures, and explains the estimation results. Section 4 presents the theoretical model used to consider this program's efficiency and optimal use and also shows the empirical approach and estimation results. Section 5 analyzes the rationale of the programs. Section 6 presents the robustness check of the program incidence results from Section 3. Section 7 presents the conclusions and discussion.

2. The place-based job creation program

This place-based job creation program[4] seeks to support to the municipal job creation policy in areas with few job opportunities. Local authorities design the job creation program and

[4] *Chiiki koyo suishin jigyo* in Japanese.

compete for money subsidized by the Ministry of Health, Labour and Welfare. They are recommended to design the program in relation to their industry promotion policy and related policies for local regeneration by other ministries. Local authorities can apply this subsidy at the city level, but only cities that have lower levels of job vacancies per a job seeker below the national average level can use this subsidy.

The subsidy amounts to a maximum of two million dollars per year[5]. Local authorities use this subsidy for job training for local unemployment, seminars about developing business for local establishments and activities to help find jobs and fill vacancies. In addition, designated cities can obtain up to half a million dollars for business activities that induce local employment growth, such as the creation of new local brand goods (many cities use local specialty agricultural products to create local brand goods) or the expansion of new business opportunities. This program lasts for three years. According to a public announcement, 81.4% and 66.6% of cities conducted activities in the agricultural and tourism sectors, respectively, in a program started in 2008. This program started in 2007, and the government decided the new policy for local regeneration at that time.

3. Incidence of the program

3.1 Empirical model

First, this paper examines the incidence of the place-based job creation program explained in the above section. To evaluate its incidence, this paper uses the difference-in-differences method. Bell, Blundell and Reenen (1999) notes that the treatment and control groups should react similarly against exogenous macro shocks; therefore, some of the previous studies have selected time periods that present similar macro-environments with the periods before and

[5] If neighboring cities apply jointly, this subsidy can be up to three million dollars per year. Amounts depend on a program plan. However, a plan is required to cost under 15 thousand dollars per a job.

after an event change and compare these time periods with the periods before and after an event change. However, this method obtains the average effect observed in the treatment group but cannot obtain the average population effect. Instead of this method, Busson, Gregory and Kline (2013) solve this issue by selecting a control group. They make regions that were previously rejected but allowed into designated empowerment zones in subsequent rounds their control group. They demonstrate that, after some basic adjustments, the pretreatment levels and trends in the control group closely mirror those of the treatment group. In this paper, the treatment group includes cities that started the program in 2007, 2008 or 2009. The control group includes cities that started the program in 2010, 2011, 2012 or 2013 and did not conduct the program from 2007 to 2009. The empirical model is as follows:

$$Y_{it} = \alpha_0 + \alpha_1 d_i + \alpha_2 TC_i + \alpha_3 d_i TC_i + \alpha_4 X_{it} + \varepsilon_{it} \quad (1),$$

where Y_{it} is a log of outcomes in city *i* at time *t*. This paper uses (1) the number of workers who *live in* the targeted city (treatment/control group) and (2) the number of workers who *work in* the targeted city as outcomes. This paper also estimates the effect by sector: (3) the number of workers in the agricultural sector, the wholesale, retail trade and service sector, the manufacturing sector, and the financial sector. This paper predicts that we will observe a significant positive effect in the agricultural sector and the wholesale, retail trade and service sector because many treated cities conduct trial business, such as the creation of local brand goods using regional specialty products or the expansion of business opportunities to sell local specialties in these sectors. These programs also implement job training and seminars for local establishments in these sectors. The effect on manufacturing is unclear because the manufacturing sector is weak in many treated cities. This paper also predicts that we will not observe significant effects in the financial sector because this program does not directly intervene to create jobs in the financial sector. Furthermore, this paper analyzes whether this program induces labor mobility. Therefore, this paper examines the effect on (4) the population, (5) the number of households, (6) the population inflow and (7) the population outflow. Unfortunately, there is no data about wages and housing costs at the city level, especially in small cities where the treatment or control groups are located. However, the impact

of housing costs is minimal in these areas due to the current population decreases in Japan. This paper observes sales volume instead of wage in Section 5. d_i is the time dummy that equals zero before the period from 2007 to 2009 and 1 after 2010. TC_i is the treatment group dummy, which equals 1 if city i is the treatment group and is otherwise zero. α_3 is the estimated effect of the place-based job creation programs. X_{it} are control variables, such as the lag share of manufacturing in total number of workers, the change in population density over the 2000-2005 period and the unemployment rate in the base year (2000).

To examine the factors that cause different program effects, this paper estimates difference-in-difference-in-differences using the following model:

$$Y_{it} = \beta_0 + \beta_1 d_i + \beta_2 TC_i + \beta_3 F_i + \beta_4 d_i TC_i + \beta_5 d_i F_i + \beta_6 F_i TC_i + \beta_7 d_i TC_i F_i + \beta_8 X_{it} + \mu_{it} \quad (2),$$

where F_i is a factor dummy variable that equals (1) 1 if the proportion of people over the age of 65 is more than 32% of the total population in 2000 and zero otherwise, (2) 1 if the working age population is less than 58% of the total population in 2000 and zero otherwise, and (3) 1 if the population per square kilometer is more than 176.65 and less than 439.825 and zero otherwise. Thirty-two percent is almost equivalent to the prediction for the national average aging rate in 2030[6]. A city with a large aging population has less consumer demand. A large aging population also means less labor supply. This paper examines whether the effect of this program is less in cities with a large aging population. The aim of using the working age population rather than the labor force participation rate is to roughly capture the negative effect of too few young residents (e.g., only one class in each grade at school, resulting in less competition, less peer effect, less information about career options from friends, less influence and less opportunities for younger generations to play an important role in the community). This paper also predicts that the programs will affect cities with lower population densities less because lower population density does not

[6] According to predictions by the National Institute of Population and Social Security Research in January 2012, the aging rate in 2030 will be 31.6%, 30.9% and 32.3% by neutral birth rate, high birth rate and low birth rate, respectively (the mortality rate is neutral for every case).

benefit from an agglomeration economy. An overly high population density also has its disadvantages; therefore, in this paper, the third quartile of population density (when the population per square kilometer is more than 176.65 and less than 439.825) equals 1. The estimations of the model (1) and (2) are clustered by city[7].

3.2 Data

This paper obtains data from population censuses in 2000, 2005 and 2010. Japan's population census has been conducted almost every five years. It covers all Japanese territories and surveys numerous topics (e.g., work status, place of work, population by age and number of households). This paper assigns cities that started the program in 2007, 2008 or 2009 to the treatment group and puts cities that started the project in 2010, 2011, 2012 or 2013 (and did not take part in the program from 2007 to 2009) in the control group. Local authorities design the job creation program and compete for subsidies. The Ministry of Health, Labour and Welfare choses the treated cities based on the possibility of job creation via the proposed plan. These cities include a few cities with huge populations; thus, this paper excludes cities with populations over the 90th percentile. After excluding huge cities over the 90th percentile, there are 167 cities in the treatment group and 81 cities in the control group. The population distribution in 2000 is shown in Figure 1. There is no information about why cities in the control group did not use the program in earlier years. There are several possible explanations, including the following: the city was rejected; it was not eligible to apply; it was unwilling to apply for the program; or it did not have information about the program. Table 1A shows descriptive statistics of pretreatment sample, and Table 1B provides p-values for the t-test of the null hypothesis that average pretreatment levels and trends of the treatment group and control group are equal. Although Table 1B shows that the levels and trends of the treatment group and control group are similar over the 2000-2005 period, some minor differences arise, e.g., in the manufacturing share level, the unemployment level and trend in population density. This paper excludes these differences through control variables. As for

[7] If cities jointly conduct this program, those cities are identified in the same cluster.

population inflow and outflow, this paper uses data from Basic Resident Registration, for which mayors are responsible.

Figure 2 shows the change in outcomes for the treatment group and control group. Figure 2a indicates that the normalized logged numbers of workers who *live in* the treatment/control group decreases, and the degree of decreasing is the same between the treatment and control groups from 2000 to 2005. However, after the starting program, the treatment group's decreasing trend is slowed, while the control group's trend is steeper. Figure 2b shows the normalized logged numbers of workers who *work in* the treatment/control group. We can find the same evidence here. The two groups' decreasing trends are the almost same from 2000 to 2005, and the decrease is sharper in the control group after 2005. Figures 2c and 2d show the trends for the normalized logged population and logged number of households, respectively. Both group's logged populations are stagnant from 2000 to 2005; however, the control group's logged population decreases sharply after 2005, while the treatment group's logged population decreases slightly. The population census in 2005 records a decrease in total population for the first time since the Second World War. The normalized logged number of households increases in the both groups, but it stagnates in the control group and increases slightly in the treatment group after 2005.

Figures 2e–2h indicate the normalized logged number of workers by sector. In the control group, the agricultural sector and the wholesale, retail trade and service sector show steeper downward trends after 2005. The manufacturing and financial sectors show almost similar trends in the treatment and control groups after starting the program. There is no data about wages and housing costs at the city level in the treatment and control groups. However, instead of wages, this paper observes sales volume in Section 5. In addition, the housing costs might not increase dramatically, as the population decreases at the national level and the treatment and control groups are in rural areas.

After the difference in difference estimation, this paper considers a regression discontinuity design. Figure 3 shows the friction of cities conducted this place-based job creation program around job vacancies per job seeker threshold. The regional data at the city level of job vacancies per job seeker are provided by the Ministry of Health, Labour and Welfare. The data are collected by each job placement agency that administers multiple cities. The fraction

of cities that participate in this program decreases at a cutoff ratio of 0.9 job vacancies per job seeker in 2008; 0.9 is the ratio required to be eligible for this program's subsidies[8].

3.3 Results

Table 2 shows the estimation results of α_3 , the coefficient of the cross term between time dummy and treatment group dummy, using Model (1). The first row shows the results for the logged number of workers who *live in* the cities in the treatment or control group. The second row shows the results for the logged number of workers who *work in* the treatment/control group. Column (1) is the result without control variables. Column (2) is the result with the following control variables: a lag share of manufacturing workers in the total number of workers and a trend in population density. Column (3) is the result with a control variable for the unemployment rate in the base year (2000) in addition to control variables for a lag share of manufacturing and a trend in population density. All coefficients are significantly positive. The place-based job creation program increases the number of local jobs by approximately 5%. The effect on workers who *work in* the treated cities is slightly greater than the effect on workers who *live in* the treated cities, which means that this program is intensely local. The gains from local establishments creating jobs or matching local establishments with unemployment through this program are slightly greater than the gains of residents who take job training through this program.

The third row and below show estimation results by sector. As mentioned in Section 2, most cities use this program in the agricultural and tourism sectors. Therefore, this paper predicts that this program's impacts on the agricultural sector and the wholesale, retail trade and service sector are greater than its impacts on other sectors. In contrast, this program may have no impact on the financial sector, which is far-removed from this program's activities. As predicted, the estimation results indicate that the place-based job creation program increases the number of workers in the agricultural sector by approximately 11%. The program most positively affects the

[8] The national average level of job vacancies per a job seeker was 0.88 in 2008.

agricultural sector. The program increases the workers in the wholesale, retail trade and service sector by approximately 5%, as shown in Column (1) and Column (2), which is a smaller (but positive) effect than that on the agricultural sector. However, if the model is controlled for the unemployment rate in the base year, the effect is insignificant because the increase in workers in the wholesale, retail trade and service sector is higher in cities with higher unemployment rates in the base year[9]. In contrast, the program does not affect the manufacturing and financial sectors. The coefficients are insignificant from Column (1) to Column (3). To confirm the concentration of manufacturing sector (and other sectors), this paper considers the distribution of workers in each sector, but there is no significant difference between sectors[10]. The manufacturing and financial sectors do not concentrate in specific cities in the treated (and control) group.

This paper roughly calculates cost using the results above. This paper multiplies the average number of workers in the agricultural sector in 2005 by 11% and multiplies the average number of workers in the wholesale, retail trade and service sector in 2005 by 5%, then aggregating these two numbers. This aggregate is the number of workers increased by the place-based job creation programs. Dividing 2.5 million dollars by this increased number of workers equals approximately 4857 dollars. This amount may be an overestimation of the programs, as cities simultaneously use municipal budgets to promote local job creation and many cities also use subsidies provided by other ministries. In fact, when they apply this program, cities are recommended to design the program in relation to their industry promotion policy and related policies for local regeneration by other ministries. Therefore, they could use their budget for their industry promotion policy, and they could, in most cases, get subsidies from other ministries for related local regeneration policies.

Table 3 shows the estimation results with respect to mobility. This paper examines whether migration occurs as a result of increasing jobs in the treatment group. The first and second

[9] If this paper regresses the logged number of workers in the wholesale, retail trade and service sector by an unemployment rate in the base year, the coefficient is significantly positive: 0.24 (without control variables) and 0.26 (controlled for a lag share of manufacturing and a trend in population density), although the correlation between them is 0.395. The results are available upon request.

[10] The distributions are available upon request.

rows are the estimation results for population and number of households, respectively. In Column (1) and Column (2), the results indicate that the program increases the population and number of households in the treatment group. Furthermore, to examine whether these increases are a result of migration, the third and fourth rows indicate the estimation results for population inflow and population outflow, respectively. The results for population inflow are significant, while the results for population outflow are insignificant. The place-based job creation program induces population inflow, moreover, net inflow.

Table 4 displays the estimation results using Model (2). Columns (1) through (3) show the results regarding whether cities with an elderly population (older than 65 years old) of more than 32% generate fewer jobs. Column (1) is the result without control variables, Column (2) is the result with control variables for a share of manufacturing and the trend in population density over the period of 2000-2005[11], and Column (3) is the result with a control variable for the unemployment rate in the base year, in addition to control variables for a share of manufacturing and the trend in population density. The results in the first, third and fourth rows (results for workers who live in the cities in the treatment group, workers in the agriculture sector and workers in the wholesale, retail trade and service sector, respectively) indicate that the effect of this place-based job creation program decreases in cities with larger aging populations. Remarkably, comparing the first row and second row, i.e., the results for workers who *live in* the targeted cities and the results for workers who *work in* the targeted cities, respectively, this program's effect on local areas, including its effect on commuting workers, in cities with large aging populations is not smaller than the effect in cities with smaller aging populations. The cities with large aging populations have less consumer demand and a smaller labor supply. The results in Column (1) through Column (3) strongly support the latter factor rather than the former factor. It can be interpreted that establishments prefer to find their workers from thick labor market even if there is unemployment in their located areas with a large aging population.

Columns (4) through (6) display results that use the share of working age population, instead of the aging population. If there are fewer residents of working age, the programs' effect decreases, as shown in the results for workers who live in the targeted cities (first row) and workers

[11] There are no lag variables in this estimation.

work in the targeted cities (second row). In contrast to the results about the aging population, the programs' effect on the local area decreases, even if commuting workers are included. This decrease indicates that a small working age population has negative impacts, such as a lack of energy among locals. With regard to workers in the agricultural sector, the small working age population does not matter compared with the large aging population; the large aging population decreases the program's effect. The place-based job creation programs discussed in this manuscript affect the agricultural sector, regardless of the working age population.

Columns (7) through (9) indicate the results for population density. This paper predicts that the program affects cities with lower population densities less than those with higher population densities. However, all of the results are insignificant. Although this paper changes the cutoff point for population density, it does not show significant results. Kazekami (2016) argue that the multiplier effect of job creation (the magnitude of additional job creation when a new job is created) is smaller than that of the U.S. The insignificant results might be related to the treated regions being located mostly rural areas that do not benefit from the agglomeration economy. Through Column (1) to Column (9), the estimation coefficients of the cross-terms between the time dummy and treatment group indicator are significantly positive.

Furthermore, this paper examines the incidence of this place-based job creation program using a regression discontinuity design. Figure 4a indicates the log number of employment in 2005, the period before the start of the programs, and there is no discontinuity. In contrast, Figure 4b indicates the log number of employment in 2010, and there is discontinuity at the cutoff point. Figure 4c shows that the cities below the cutoff point slightly increased in employment from 2005 to 2010, compared to cities immediately above the cutoff point. The estimation result by a regression discontinuity design shows that the change in log number of employment from 2005 to 2010 in the cities that are above the threshold is approximately 5% smaller than the change in the cities that are below the threshold, in other words, cities that are eligible for this program. The magnitude is almost the same as the main results in Table 2. If half and double the bandwidth of the optimal level are used, the changes in log number of employment in the cities that are above the threshold are significantly smaller than the cities that are below the threshold. Therefore, although this paper compares the treatment cities with similar cities regarding job vacancies per job seeker, this place-based job creation program increases employment in the treated cities.

4. Optimal use of the programs

4.1 Theoretical model

In this section, this paper demonstrates the optimal use of this program's subsidy when the market failure exists due to hiring costs, showing that the cities with improved establishment of leasehold rights should have higher subsidies from these programs.

The size of the local labor force is N. Workers are homogeneous, migrate freely and search for jobs. Jobs are filled probabilistically via a constant returns to scale matching function M (U, V), which takes the number of unemployment *U*, and job vacancies, *V*, as arguments. $\theta \equiv \frac{V}{U}$ indicates market tightness. By the constant returns to scale assumption, the job filling rate is $q(\theta) \equiv \frac{M(U,V)}{V} = M(\theta, 1)$, which is assumed to obey $q'(\theta) < 0$, $\lim_{\theta \to 0} q(\theta) = \infty$, and $\lim_{\theta \to \infty} q(\theta) = 0$. Likewise, the job finding rate is $\theta q(\theta) \equiv \frac{M(U,V)}{U}$, which obeys $\lim_{\theta \to 0} \theta q(\theta) = 0$ and $\lim_{\theta \to \infty} \theta q(\theta) = \infty$.

The steady state value of searching for a job is given by the following equation:

$$rJ^U = b + \theta q(\theta)(J^E - J^U) \quad (1)$$

where *r* is the interest rate. The flow utility of unemployment *b* captures the generosity of the local safety net and the value of leisure. The term rJ^E indicates the steady state value of employment as follows:

$$rJ^E = w + s(J^U - J^E) \quad (2)$$

where *w* represents the wage, and *s* represents an exogenous separation probability. Assuming the standard general equilibrium of a fixed workforce that freely moves and obtains everywhere equal flow utility *z*, the restriction in an interior equilibrium is as follows:

$$rJ^U = z \quad (3)$$

Firms may post vacancies that entail flow cost *k*. Following Pissarides (1999, 2009) and Kline and Moretti (2013), this study assumes that the firm must pay a fixed hiring cost *H* before hiring a worker with whom it is matched. Note that *k* and *H* are distinguished by the fact that the vacancy costs are already sunk by the time the firm is matched with the worker, while the hiring costs are not. The value J^V of posting an unfilled vacancy is as follows:

$$rJ^V = -k + q(\theta)(J^F - J^V - H) \quad (4)$$

The value of J^F of a filled vacancy is as follows:

$$rJ^F = p - w - Aging + s(J^v - J^F) \quad (5)$$

where *p* is the productivity of the match, which we assume is city-specific and common to all matches in the city. *Aging* is the cost of filling vacancy by the owner of a firm. (1) Aging owners exert less effort because they are considering closing their establishments alongside offering jobs. (2) In the agricultural sector, aging landowners also decrease the value of a filled vacancy in the total local labor market. If aging landowners stop cultivating crops and do not lease/sell their land or hire young workers, the latent vacancy is not filled. The constraining factors for other

agricultural firms could be indirectly interpreted through Equation (5). *Aging* is city-specific and common to all matches in the city because the municipality operates the support systems that find successors or improve cropland leasing[12]. Aging owners are one of major problems in rural areas in Japan. This study is interested how the diversity of political treatment of the aging owner affects the optimal use of the place-based job creation program's subsidy.

The free entry of firms drives the value of an unfilled vacancy to zero:

$$\mathrm{r}J^V = 0 \tag{6}$$

In a steady state, there will be no migration, and the local unemployment rate, $\mathrm{u} \equiv \frac{U}{N}$ will be determined by the usual function of inflow and outflow rates:

$$\mathrm{u} = \frac{s}{s+\theta q(\theta)} \tag{7}$$

Finally, this study assumes that wages are set via Nash bargaining over the match surplus:

$$(1-\beta)(J^E - J^U) = \beta(J^F - J^V - H) \tag{8}$$

where β is the worker's share of the match surplus.

[12] The regulation of leasing is determined by the nation; however, allowances are determined at the municipal level, and improving the establishment of leasing rights varies across cities.

The equilibrium from (4), (5) and (6) is as follows:

$$\mathrm{q}(\theta) = \frac{k(r+s)}{p - w - Aging - (r+s)H} \tag{9}$$

Plunging (3) into (2) produces the following equation:

$$J^E = \frac{w + \frac{s}{r}z}{r+s} \tag{10}$$

Plunging (10) into (1) and free mobility is then rewritten as follows:

$$\theta = \frac{z-b}{q(\theta)\frac{w-z}{r+s}} \tag{11}$$

The following equation comes from (3), (5), (8) and (10):

$$w = \beta[p - Aging - (r+s)H] + (1-\beta)z \tag{12}$$

Equation (12) indicates that aging owners decrease the wage level.

Plunging (9) into (11) using (12) produces the following equation:

$$\theta = (z - b)\frac{1-\beta}{k\beta} \tag{13}$$

From (11) and (12), the following equation is then arranged[13]:

$$\frac{r+s}{q(\theta)} = \frac{1-\beta}{k}[p - Aging - (r+s)H - b] - \beta\theta \tag{14}$$

Using (13) and (14), this paper produces the following equation[14]:

$$k\theta = (z - b)\frac{1-\beta}{\beta} \tag{15}$$

Considering efficiency, the total surplus is maximized. The surplus, *S*, consists of the output of productive matches net of the steady state costs of hiring replacements plus the leisure associated with unemployment minus the flow cost of maintaining unfilled vacancies. This must then be netted out relative to the outside option, which offers workers utility level *z*.

$$S = [(p - sH)(1 - u) + (b - k\theta)u - z]N \tag{16}$$

[13] See Appendix 1 for details.

[14] See Appendix 1 for details.

The following equation assumes discount rates of zero to compare steady states without considering the convergent paths between steady states:

$$\max_{\theta} S \text{ s.t. } u = \frac{s}{s+\theta q(\theta)}$$

The first order condition of this problem is as follows:

$$(\mathrm{p} - \mathrm{b} - \mathrm{sH})\frac{\partial u}{\partial \theta} + ku + k\theta\frac{\partial u}{\partial \theta} = 0 \tag{17}$$

This equation governs the efficiency of the job creation process, which determines the local unemployment rate.

$$\frac{\partial u}{\partial \theta} = -\frac{1-\alpha}{s}q(\theta)u^2$$

,

where $\alpha \equiv -\frac{q'(\theta)}{q(\theta)}$ is the negative elasticity of the job filling rate. With this convention, (17) can be rewritten as follows:

$$\frac{s}{q(\theta)} = (p-b)\frac{1-\alpha}{k} - \alpha\theta$$

For this equation to coincide with (14), when agents have discount rates of zero, we use the following equation:

$$\frac{1-\beta}{k}(p - Aging - b - sH) - \beta\theta = (p-b)\frac{1-\alpha}{k} - \alpha\theta \tag{18}$$

Rearranging (18), this study writes the optimal hiring cost as follows:

$$H^* = \frac{(\alpha - \beta)(p - b + k\theta) - Aging(1 - \beta)}{s(1 - \beta)} \tag{19}$$

Using (15), this optimal hiring cost is rewritten as follows:

$$H^* = \frac{\alpha - \beta}{s}\left(\frac{p}{1 - \beta} - \frac{b}{1 - \beta} + \frac{z - b}{\beta} - Aging\right)$$

$$= \frac{\alpha - \beta}{s}\frac{1}{(1 - \beta)\beta}\{\beta(p - Aging) + (1 - \beta)z + \beta^2 Aging - b\}$$

$$= \frac{1}{s\beta}\frac{\alpha - \beta}{1 - \alpha}(w + \beta^2 Aging - b) \tag{20}$$

This equation indicates that optimal hiring costs are proportional to the local aging owners' level; especially cities with a large worker's share of the match surplus have high proportions. Following the finding of the previous studies (Higuchi, 2001; Nakata, 2007; Azetsu and Fukushige, 2009) that the hiring costs are high, this paper considers hiring costs to take the value $\bar{H} > H^*$. Therefore, the subsidies are efficient in this case. The optimal subsidy is as follows:

$$\beta^* = \bar{H} - \frac{1}{s\beta}\frac{\alpha - \beta}{1 - \alpha}(w + \beta^2 Aging - b)$$

Aging owners decrease this subsidy. The support systems for finding successors or establishing leasing rights decrease "*Aging*" (i.e., the owner's effort/cost to fill a vacancy). This means that the cities with establishing sufficient number of leasing rights or systems for finding successors can use the subsidy efficiently. Providing the subsidy to cities that have the support systems to find successors or improve the establishment of leasing rights is the optimal use of this program's subsidy.

4.2 Empirical approach and data

In this section, this paper examines whether the programs' effects are greater in the cities with the improved establishment of cropland leasehold rights. This paper divides the sample into the cities with major improvements to the establishment of leasehold rights and the cities with minimal improvements to the establishment of leasehold rights. This paper investigates the aging landowner's problem that constrains young workers and other agricultural firms rather than the support system to find a successor because the place-based job creating programs that this paper examines mainly affect the agricultural sector, as shown in the results in Section 3.3. This paper uses the following a criterion to divide the sample:

L= the area of the land with established leasehold rights / the number of potential entrepreneurs

If L is greater than 7.94, this paper determines that cities are presenting sufficient improvement in the establishment of leasehold rights to newcomers that start working in the agricultural sector; otherwise, the improvement is not sufficient. The value of 7.94 is the mean.

This paper compares the program effects, α_3, between the sample with L=0 and the sample with L=1 using Model (1) from Section 3.1. This paper obtains data about the area of land with established leasehold rights from the land administration information collection and analysis survey in 2007. This survey collects all information about the establishment of leasehold rights on cultivated land. The Ministry of Agriculture, Forestry and Fisheries gathers these data. This paper

aggregates the areas of the land with established leasehold rights via the Agricultural Land Act and the Agricultural Management Framework Reinforcement Act. This paper uses unemployment as the number of potential entrepreneurs.

4.3 Estimation results

Column (1) in Table 5 shows the estimation results without controls. Column (2) shows the estimation results with control variables for a lag share of employment in manufacturing and a trend in population density, and Column (3) shows the results with control variables for the log of cropland area, lag shares of manufacturing and a trend in population density. The upper line by each outcome indicates the result for cities with over than mean criterion areas, as mentioned in the previous section: the area of the land with established leasehold rights per potential entrepreneur. The results indicate that the effect of the place-based job creation programs is significantly positive and greater, especially in the agricultural sector, in cities that improved the establishment of leasehold rights than in comparative samples. While many cities in the treatment group try to make local brands using local specialty agricultural products with the program's subsidy, the program's effect in the cities that improved the establishment of leasehold rights is also greater in the wholesale, retail trade and services sector, including the retail trade of food goods in addition to aggregate workers (workers who live in/work in the targeted cities, including workers in the agricultural sector and the wholesale, retail trade and service sector). The results for the cities with few established leasehold rights show smaller (insignificant) coefficients than do those with well-established leasehold rights. Section 4.1 theoretically demonstrates that the optimal use of this program's subsidy is providing the cities that improve the establishment of leasehold rights, and the empirical results indicate that this program is actually more efficient in those cities.

5 Program rationale

5.1 Empirical model and data

Although the program is efficient, is not it a zero-sum game? In this section, this paper considers whether the increase in consumer demand and the employment growth in the treated cities do not take a consumer demand away from neighboring cities. This paper analyzes whether the logged number of workers who live in neighboring cities and the logged number of workers who work in neighboring cities decreases through place-based job creation programs. This paper also conducts the same analysis for sales, the number of employed workers and the number of establishments associated with general retail and food and beverage retail, as the cities primarily use this program in the tourism and agricultural sectors. If tourism were to increase in the treatment group, the sale of souvenirs, food, sweets and drinks could increase. In turn, if local brand goods that used local specialty agricultural products were sold well, the sale of souvenirs, food, and sweets could increase. The term "neighbor" is used if more than 10% of workers who live in a city *c* commute to any targeted cities in 2005[15]. This paper uses following model:

$$Y_{it} = \gamma_0 + \gamma_1 d_i + \gamma_2 N_i + \gamma_3 d_i N_i + \gamma_4 X_{it} + \varepsilon_{it} \quad (1)$$

where $Y_{it}$is the logged number of workers who live in the targeted cities and the logged number of workers who work in the targeted cities, the log of sales, employed workers, and establishments in the general retail trade sector and in the food and beverage retail trade sector in city *i* at time *t*. N_i is a dummy variable that equals 1 if cities are neighboring cities of the treatment or control group. This paper estimates (1) cities in the treatment group versus neighboring cities of the treatment group, (2) cities in the control group versus neighboring cities of the control group, and (3) neighboring cities of the treatment group versus neighboring cities of the control group. With respect to (1), if the program has a positive effect on the treatment group, it could be possible that

[15] Some American previous papers use concentric zone to determine a term neighbor. However, a frequency of coming and going depends on public transportations in Japan. Therefore, I use the definition in this manuscript.

we observe negative effect on neighboring cities of the treatment group even though outcomes of the neighboring cities do not change. As for (2), this paper confirms no effect on the neighboring cities of the control group. Then, as for (3), this paper predict a negative effect on neighboring cities of treatment group comparing the neighboring cities of control group if the increase in consumer demand and the employment growth in the treatment group take a consumer demand away from neighboring cities. This paper also estimates the sample, including cities in the treatment group, cities in the control group, neighboring cities of the treatment group and neighboring cities of the control group. Therefore, this paper modifies Model (1) as follows:

$$Y_{it} = \delta_0 + \delta_1 d_i + \delta_2 TC_i + \delta_3 Ntre_i + \delta_4 Ncon_i + \delta_5 d_i TC_i + \delta_6 d_i Ntre_i + \delta_7 d_i Ncon_i + \delta_8 X_{it} + \varepsilon_{it} \quad (2)$$

where TC_i is a treatment group dummy that equals 1 if city i is a treatment group and otherwise equals zero. $Ntre_i$ equals 1 if city i is a neighboring city of the treatment group, and $Ncon_i$ equals 1 if city i is a neighboring city of the control group. Therefore, the control group is the baseline. Data are obtained from the census of commerce. The Ministry of Economy, Trade and Industry conducts this survey, which covers all wholesale and retail trade stores. This paper also obtains data about the number of workers who live in the targeted cities and the number of workers who work in the targeted cities from the population census, as mentioned in Section 3.2. This paper estimates Equation (1) and Equation (2) using a block bootstrap. A prefecture level is used as a block.

5.2 Estimation results

Table 6 shows the estimation results for Model (1). Column (1) shows the estimation results of neighboring cities of the treatment group versus cities in the treatment group without controls, and Column (2) shows the estimation results with control variables. The first and second rows show results using population census data. The number of resident workers and commuting workers in neighboring cities of the treatment group does not decrease due to the place-based job

creation programs. The third row and below show results using census of commerce. The program decreases the number of workers and establishments in the general retail trade sector in neighboring cities of the treatment group. However, as mentioned in earlier, if the program has a positive effect on the treatment group, we observe negative effect on neighboring cities of the treatment group compared with the treatment group even though outcomes of the neighboring cities do not change. Table A1 (see Appendix) shows the results for this program's effect using census of commerce to establish difference-in-differences between the treatment group and the control group. The program increases the sales and number of establishments associated with general retail trade in the treatment group.

Columns (3) and (4) are the uncontrolled and controlled results of neighboring cities of the control group versus cities in the control group. As expected, the programs do not decrease the resident workers, commuting workers, sales, workers and establishments in general retail trade and food and beverage retail trade in the neighboring cities of the control group. Although the program has a significantly negative effect on the sales in the food and beverage retail trade sector in neighboring cities of the control group, this effect is insignificant if controlled by an industrial structure and a trend in population density. Instead of comparing the neighboring cities with the treatment group, this paper compares the neighboring cities of the treatment group with the neighboring cities of control group because there is a possibility of bias in the column (1) and (2). The effects of the programs on neighboring cities of the treatment group and neighboring cities of the control group do not differ, as displayed in Column (5) and Column (6). Figure 5 indicates the coefficients of the cross term between the time dummy and the neighboring cities' dummy by definitions of neighboring cities. The term "neighbor" is additionally used if more than 15% and 12.5% of workers who live in a city *c* commute to any targeted cities in 2005. The magnitudes of coefficients decrease with the expansion of the range of neighboring cities, although coefficients are insignificant.

Table 7 provides the results for the treatment group, the control group, neighboring cities of the treatment group and neighboring cities of the control group. Columns (1) and (2) are the uncontrolled results and the results controlled by a lag share of manufacturing and a trend in population density. The first row indicates that the cross terms between a treatment group dummy and a time dummy are significant. The programs increase the number of workers who live in the

treatment group and who work in the treatment group and the general retail sales in the treatment group compared to those of the control group. However, there is no difference between neighboring cities of the treatment and the control group, neighboring of the control group and the control group in terms of the number of workers, sales and establishments, as shown in the second and third rows by each outcome. As shown in Table 6 and Table 7, this paper does not clearly observe the program's destruction of neighboring cities' demand.

6 Robustness check

Finally, as a robustness check, this paper uses the data collected before starting the program to estimate Model (1) in Section 3.1. This paper uses the population censuses of 1995, 2000 and 2005. The time dummy, d_i, equals 1 if the data are from 2005 and is otherwise zero. This paper confirms that the outcomes of treatment group do not differ from those of the control group after 2005, i.e., the placebo event year. Table 8 shows the estimation results. Column (1) shows the uncontrolled estimation results; Column (2) are the results controlled by a lag share of manufacturing and a trend in population density; Column (3) indicates the results controlled by a lag share of manufacturing, a trend in population density and the unemployment rate in the base year (1995). All results indicate an insignificant effect. The placebo event does not differently affect the workers who live in/work in the targeted cities or workers in the agricultural sector and the wholesale, retail and service sector as well as the manufacturing and financial sectors in the treatment group compared with workers in the control group. By the same token, the placebo event does not affect population and the number of household in the treatment group as shown in the lower part of Table 8.

7 Conclusion and discussion

This paper has three purposes. First, the empirical evaluations of job creation have rarely been conducted in Japan. In particular, government officials generally focus on how many workers are generated per dollar/yen spent. Therefore, this paper empirically and precisely analyzes the incidence of place-based job creation programs conducted by the Ministry of Health, Labour and Wealth. The results show that the program increases workers, especially workers commuting to the cities conducting the programs, and the program induces a net population inflow. Furthermore, the programs remarkably affect the agricultural sector and the wholesale, retail trade and service sector in which most cities conduct trial business or seminars via these programs. However, the program has a lesser effect in cities with large aging populations or small working age populations.

Second, this paper demonstrates the optimal use of this program's subsidy when market failure exists due to high hiring costs. This paper makes the model and examines it empirically. Few studies empirically investigate this issue, but the detailed data allow empirical demonstration in this paper. As a result, this paper shows that cities that improve established leasehold rights for cropland (in an effort to use aging land owners' land by putting young workers, who have the will to work in the agricultural sector, to work) should have higher subsidies. The empirical results indicate that this program is actually more efficient in those cities.

Third, this paper confirms whether the zero-sum game does occur; in other words, the program increases employment in the treatment group but decreases the labor demand in neighboring cities due to a decrease in consumer demand. Many previous studies overlook the total aggregate effect, especially by empirical demonstration; in addition, government official only calculates the number of workers generated in the treated cities, but considering the total effect is important for policy evaluation. Many cities conduct seminars on how to attract people to local specialty products. Therefore, this paper uses the sales data from census of commerce and the population census. As a result, this paper does not observe a clear decrease in sales or the number of workers in neighboring cities of the treatment group.

One limitation of this study is that the magnitude of positive incidence of this place-based job creation program includes some impacts that come from other subsidies because treatment cities are recommended to design the program in relation to their industry promotion policy and related policies for local regeneration by other ministries. To expand this paper, it must be determined whether the program reduces the number of unemployment benefits in the treated

cities. However, this program also induces the participation of potentially unemployment such as female workers (sometimes from farm families).

Appendix 1 (Appendix for the theoretical model in Section 4.1)

From (11) and (12), we produce the following equation:

$$\theta q(\theta) = \frac{(z-b)(r+s)}{\beta[p - Aging - (r+s)H - z]}$$

We then establish the following equation:

$$\frac{r+s}{q(\theta)} = \frac{1-\beta}{k}[p - Aging - (r+s)H - b] - \beta\theta \tag{14}$$

We plunge (13) into (14), as follows:

$$p - Aging - (r+s)H - z = \frac{k}{1-\beta}\frac{r+s}{q(\theta)}$$

This paper rewrites this equation as follows:

$$z - b = k\frac{\beta}{1-\beta}\theta$$

This paper thus produces the following equation:

$$k\theta = (z - b)\frac{1-\beta}{\beta} \tag{15}$$

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Table1A Descriptive statistics of pre treatment sample (levels in 2005)

	Treatment group				Control group			
Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Workers live in [a]	9.01	1.10	6.03	11.04	9.40	1.09	5.82	11.44
Workers work in [b]	8.94	1.12	6.13	11.17	9.33	1.13	5.83	11.46
Population	9.74	1.13	6.56	11.80	10.11	1.10	6.08	12.05
Household	8.69	1.16	5.51	10.90	9.05	1.09	5.22	10.97
Population inflow	6.27	1.26	3.00	8.88	6.57	1.20	2.40	8.60
Population outflow	6.50	1.17	3.37	8.84	6.77	1.13	2.71	8.73
Workers in Agriculture	6.82	1.20	0.00	9.13	7.25	1.09	3.22	8.88
Wholesale, retail trade and service	8.23	1.20	5.23	10.41	8.61	1.16	4.73	10.57
Manufacturing	6.83	1.34	2.20	9.23	7.38	1.65	0.00	10.43
Financial	4.67	1.41	1.10	7.23	5.14	1.30	1.39	7.40
Observations	167				81			

All variables are logarithms.

Observations of workers in a financial sector are 165 in the treatment group and 79 in the control group.

a: Workers who live in the treatment/control cities

b: Workers who work in the treatment/control cities

Table1B Descriptive statistics –differences between groups

Variable	Treatment group Mean	Std. Dev.	Control group Mean	Std. Dev.	p–value of difference [a]
level in 2005					
Manufacturing share	13.16	6.20	15.92	7.80	0.003
Government service share	4.57	2.22	4.49	2.43	0.777
Unemployment rate	6.09	2.40	5.43	1.90	0.032
Aging population share	28.76	6.54	27.96	5.26	0.336
Working age population share	57.89	4.77	58.71	3.91	0.181
Population density [b]	314.81	613.89	223.02	396.94	0.221
trend over the 2000–2005 period					
Manufacturing share	–9.89	20.53	–11.92	12.32	0.412
Government service share	1.54	12.20	1.53	9.07	0.995
Unemployment rate	53.41	37.37	49.27	35.65	0.407
Aging population share	12.86	4.66	13.32	4.99	0.479
Working age population share	–2.91	2.26	–2.85	1.64	0.845
Population density	–17.41	26.20	–27.73	33.34	0.008
Observations	167		81		

a: p–value of difference between the treatment and the control groups

b: Population per square kilometers

Table2 Incidence of the programs

Dependent variable	(1) Coef.	(1) Robust Std. Err.	(2) Coef.	(2) Robust Std. Err.	(3) Coef.	(3) Robust Std. Err.
Workers live in [a]	0.062 **	0.031	0.055 *	0.030	0.045 +	0.027
Workers work in [b]	0.062 **	0.029	0.056 **	0.028	0.046 *	0.025
Workers in Agriculture	0.119 **	0.055	0.113 **	0.053	0.112 **	0.052
Wholesale, retail trade and service	0.055 *	0.029	0.050 *	0.028	0.038	0.026
Manufacturing	0.061	0.047	0.042	0.046	0.032	0.045
Financial	0.002	0.035	0.005	0.040	0.007	0.042
Control variables						
Manufacturing share (lag)	No		Yes		Yes	
Trend in population density	No		Yes		Yes	
Unemployment rate [c]	No		No		Yes	
Observations	742		742		742	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively. (+ indicates significant at 10.5% level.)
Observations of workers in a financial sector is 733
Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row.
Dependent variables are logarithms. Standard errors are clustered by city.
a: Workers who live in the treatment/control cities
b: Workers who work in the treatment/control cities
c: in the base year

Table3 Incidence of the programs about population, the number of household and the mobility

	(1)		(2)		(3)	
Dependent variable	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Population	0.062 *	0.034	0.056 *	0.032	0.045	0.029
Household	0.065 **	0.031	0.060 **	0.029	0.049 *	0.026
Population inflow	0.064 **	0.030	0.062 **	0.028	0.048 **	0.024
Population outflow	0.051	0.035	0.049	0.033	0.036	0.028
Control variables						
Manufacturing share (lag)	No		Yes		Yes	
Trend in population density	No		Yes		Yes	
Unemployment rate [a]	No		No		Yes	
Observations	742		742		742	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively.
Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row.
Dependent variables are logarithms. Standard errors are clustered by city.
a: in the base year

Table4 Incidence of the program-effect on aging population, working age population and population density

Factor (F)	Aging population						Working age population					
	(1)		(2)		(3)		(4)		(5)		(6)	
Dependent variable	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Workers live in [a]												
dTC	0.055 **	0.027	0.060 **	0.028	0.058 **	0.027	0.090 **	0.044	0.092 **	0.044	0.087 **	0.040
dTCF	−0.083 *	0.045	−0.092 *	0.053	−0.092 *	0.055	−0.100 *	0.053	−0.093 *	0.055	−0.086 +	0.052
Workers work in [b]												
dTC	0.053 **	0.025	0.058 **	0.025	0.056 **	0.024	0.089 **	0.040	0.091 **	0.039	0.086 **	0.036
dTCF	−0.061	0.047	−0.069	0.051	−0.069	0.053	−0.096 *	0.050	−0.090 *	0.050	−0.083 *	0.048
Agriculture												
dTC	0.129 **	0.059	0.133 **	0.059	0.134 **	0.059	0.160 *	0.087	0.162 *	0.086	0.163 *	0.086
dTCF	−0.243 *	0.135	−0.251 *	0.139	−0.251 *	0.139	−0.135	0.102	−0.129	0.102	−0.130	0.102
Wholesale, retail trade and service												
dTC	0.047 **	0.023	0.050 **	0.024	0.048 **	0.023	0.071 *	0.037	0.072 **	0.038	0.067 *	0.035
dTCF	−0.084 *	0.047	−0.090 *	0.052	−0.090 +	0.056	−0.082 *	0.049	−0.077	0.051	−0.069	0.049
Control variables												
Manufacturing share	No		Yes		Yes		No		Yes		Yes	
Trend in population density	No		Yes		Yes		No		Yes		Yes	
Unemployment rate [c]	No		No		Yes		No		No		Yes	
Observations	743		743		743		743		743		743	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively. (+ indicates significant at 11% level.)

Each entry gives the difference-in-differences estimate (dTC) and the difference-in-difference-in-differences (dTCF) of the program on the outcome presented in each row.

Dependent variables are logarithms. Standard errors are clustered by city.

a: Workers who live in the treatment/control cities

b: Workers who work in the treatment/control cities

c: in the base year

Table4 Incidence of the program-effect on aging population, working age population and population density (Continued)

Factor (F)	Population density					
	(7)		(8)		(9)	
Dependent variable	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Workers live in [a]						
dTC	0.044 *	0.024	0.052 *	0.027	0.050 *	0.028
dTCF	−0.015	0.039	−0.028	0.040	−0.027	0.042
Workers work in [b]						
dTC	0.047 **	0.023	0.054 **	0.025	0.052 **	0.026
dTCF	−0.024	0.036	−0.035	0.036	−0.034	0.037
Agriculture						
dTC	0.104 *	0.058	0.111 *	0.058	0.111 *	0.058
dTCF	−0.028	0.087	−0.039	0.085	−0.039	0.085
Wholesale, retail trade and service						
dTC	0.036 *	0.021	0.042 *	0.025	0.040	0.027
dTCF	−0.014	0.032	−0.023	0.035	−0.022	0.038
Control variables						
Manufacturing share	No		Yes		Yes	
Trend in population density	No		Yes		Yes	
Unemployment rate [c]	No		No		Yes	
Observations	743		743		743	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively. (+ indicates significant at 11% level.)

Each entry gives the difference-in-differences estimate (dTC) and the difference-in-difference-in-differences (dTCF) of the program on the outcome presented in each row.

Dependent variables are logarithms. Standard errors are clustered by city.

a: Workers who live in the treatment/control cities

b: Workers who work in the treatment/control cities

c: in the base year

Table 5 Optimal use of the subsidy

		(1)		(2)		(3)	
		Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Criterion1: the area of the land with established leasehold rights per a potential entrepreneur							
Workers in Agriculture	>7.94	0.206 ***	0.054	0.212 ***	0.051	0.104 *	0.054
	<	0.088	0.104	0.114	0.108	0.033	0.070
Workers live in [a]	>	0.157 ***	0.048	0.169 ***	0.049	0.057	0.049
	<	−0.020	0.055	0.008	0.063	−0.031	0.049
Workers work in [b]	>	0.137 ***	0.048	0.147 ***	0.049	0.030	0.051
	<	−0.006	0.057	0.022	0.064	−0.017	0.053
Workers in wholesale, retail trade and service	>	0.178 ***	0.053	0.188 ***	0.052	0.070	0.053
	<	−0.059	0.060	−0.026	0.069	−0.064	0.058
Criterion2: the area of the land with established leasehold rights per an area of the deserted cultivated land							
Workers in Agriculture	>0.042	0.086 *	0.045	0.092 *	0.046	0.089 *	0.045
	<	0.167	0.153	0.201	0.151	0.050	0.089
Workers live in [a]	>	0.022	0.024	0.024	0.025	0.021	0.024
	<	0.092	0.064	0.143	0.086	0.085	0.059
Workers work in [b]	>	0.014	0.022	0.016	0.022	0.013	0.022
	<	0.108	0.062	0.159 *	0.083	0.103 *	0.061
Workers in wholesale, retail trade and service	>	0.019	0.021	0.021	0.023	0.018	0.022
	<	0.064	0.048	0.120	0.080	0.069	0.055
Control variables							
Manufacturing share (lag)		No		Yes		Yes	
Government service share (lag)		No		Yes		Yes	
Cropland area (lag)		No		No		Yes	
Observations							
Criterion1	>7.94	371		371		371	
	<	372		371		368	
Criterion2	>0.042	531		531		531	
	<	173		172		169	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively.

Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row. Upper rows (>7.94 or >0.042) are effects in the cities with well- established leasehold rights and lower rows (<) are effect in the cities with few established leasehold rights.

Dependent variables are logarithms.

a: Workers who live in the treatment/control cities

b: Workers who work in the treatment/control cities

Table 6 The effect on the neighbor cities

	Treatment vs their neighbor				Control vs their neighbor				Neighbor of treatment vs neighbor of control			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.
Workers live in [a]	−0.018	0.032	−0.007	0.029	−0.027 *	0.016	−0.017	0.027	0.018	0.041	0.024	0.040
Workers work in [b]	−0.016	0.032	−0.006	0.035	−0.016	0.019	−0.007	0.028	0.009	0.036	0.015	0.041
General retail trade												
Sales	−0.056	0.181	−0.060	0.168	−0.057	0.110	−0.023	0.129	0.114	0.190	0.079	0.189
Workers	−0.057 **	0.027	−0.052 *	0.031	−0.050	0.056	−0.030	0.070	0.014	0.066	0.007	0.059
Establishments	−0.058 **	0.026	−0.054 **	0.025	−0.054	0.043	−0.037	0.054	0.026	0.043	0.022	0.048
Food and beverage retail trade												
Sales	−0.060	0.071	−0.056	0.071	−0.215 *	0.119	−0.183	0.118	0.130	0.154	0.121	0.135
Workers	−0.042	0.060	−0.037	0.061	−0.037	0.058	−0.018	0.064	−0.013	0.078	−0.020	0.080
Establishments	−0.038	0.029	−0.033	0.029	−0.049	0.050	−0.033	0.047	0.020	0.036	0.018	0.035
Control variables												
Manufacturing share (lag)	No		Yes		No		Yes		No		Yes	
Trend in population density	No		Yes		No		Yes		No		Yes	
Observations [c]	731		731		474		474		177		177	
Observations [d]	587		587		327		327		176		176	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively.
Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row.
Dependent variables are logarithms.
Estimations use a block bootstrap. A prefecture level is used as a block.
a: Workers who live in the treatment/control cities
b: Workers who work in the treatment/control cities
c: Observations for workers who live/ work in the targeted cities using population census
d: Observations for workers and establishments in a retail trade sector using census of commerce. Observations for sales in a general retail trade sector are 538, 538, 313, 313, 161 and 161 for column (1)-(6), respectively. Observations for sales in a food and beverage retail trade sector are 583, 583, 326, 326, 176 and 176 for column (1)-(6), respectively.

Table 7 The effect on the neighbor cities-treatment vs control vs neighbor of treatment vs neighbor of control

	(1)		(2)	
	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.
Workers live in				
Dtc	0.053 ***	0.018	0.054 **	0.024
Dntre	0.016	0.033	0.030	0.039
Dncon	−0.014	0.017	−0.015	0.025
Workers work in				
Dtc	0.053 ***	0.018	0.055 ***	0.021
Dntre	0.020	0.035	0.033	0.040
Dncon	−0.002	0.018	−0.003	0.025
General retail trade				
Sales				
Dtc	0.122 *	0.072	0.118 +	0.073
Dntre	0.053	0.177	0.043	0.139
Dncon	−0.038	0.109	−0.024	0.112
Workers				
Dtc	0.034	0.025	0.030	0.029
Dntre	−0.014	0.018	−0.013	0.037
Dncon	−0.029	0.049	−0.025	0.058
Establishments				
Dtc	0.041 *	0.023	0.038	0.026
Dntre	−0.007	0.017	−0.007	0.029
Dncon	−0.035	0.041	−0.033	0.050
Food and beverage retail trade				
Sales				
Dtc	−0.038	0.067	−0.032	0.053
Dntre	−0.059	0.074	−0.050	0.073
Dncon	−0.215 *	0.123	−0.204 *	0.119
Workers				
Dtc	0.000	0.034	−0.003	0.035
Dntre	−0.024	0.060	−0.024	0.061
Dncon	−0.023	0.068	−0.020	0.068
Establishments				
Dtc	0.020	0.025	0.016	0.027
Dntre	−0.007	0.025	−0.006	0.031
Dncon	−0.035	0.037	−0.033	0.042
Control variables				
Manufacturing share (lag)	No		Yes	
Trend in population density	No		Yes	
Observations [a]	974		974	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively.

Each entry gives the difference-in-differences estimate (dTC; time dummy × treatment dummy, dNtre; time dummy × neighbor of treatment group dummy, dNcon; time dummy × neighbor of control group dummy) of the program on the outcome presented in each row.

Dependent variables are logarithms.

Estimations use a block bootstrap. A prefecture level is used as a block.

a: Observations for sales in a general retail trade sector are 838, observations for sales in a food and beverage retail trade sector are 895, observations for workers and establishments in both sectors are 900 for column (1) and (2), respectively.

Table 8 Placebo test

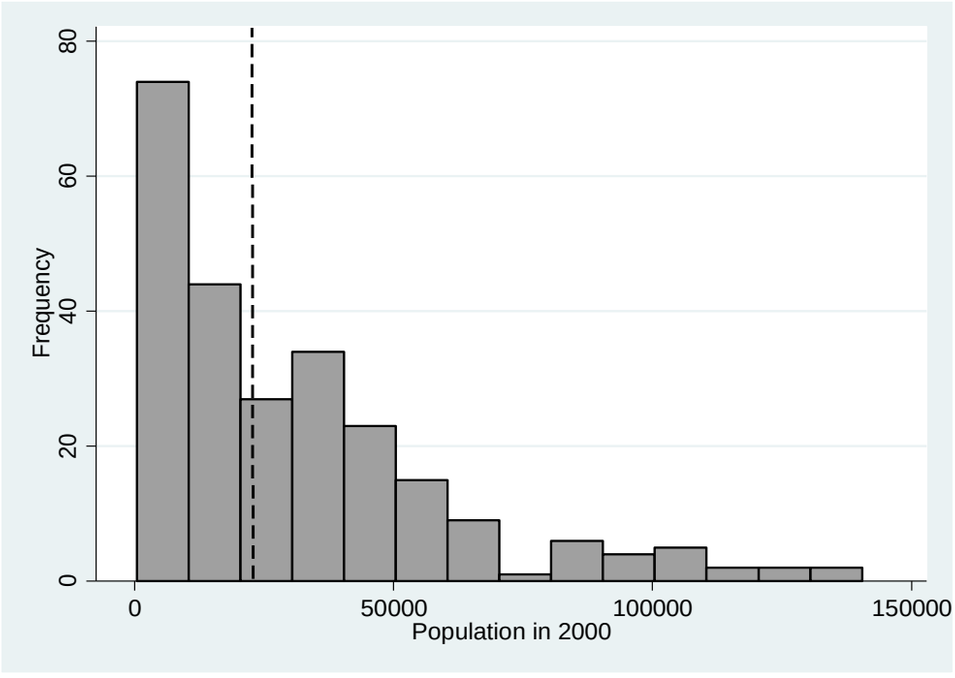
	(1)		(2)		(3)	
Dependent variable	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Workers live in [a]	−0.004	0.032	0.010	0.032	0.018	0.033
Workers work in [b]	0.001	0.031	0.013	0.031	0.021	0.032
Workers in Agriculture	0.018	0.044	0.029	0.043	0.029	0.043
Wholesale, retail trade and service	−0.006	0.029	0.005	0.030	0.014	0.031
Manufacturing	−0.016	0.043	0.033	0.040	0.041	0.042
Financial	−0.026	0.073	−0.008	0.072	0.011	0.067
Population	0.002	0.030	0.014	0.030	0.023	0.031
household	0.006	0.030	0.013	0.030	0.022	0.031
Control variables						
Manufacturing share (lag)	No		Yes		Yes	
Trend in population density	No		Yes		Yes	
Unemployment rate [c]	No		No		Yes	
Observations	741		741		741	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively.
Observations of workers in a financial sector is 735
Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row.
Dependent variables are logarithms. Standard errors are clustered by city.
a: Workers who live in the treatment/control cities
b: Workers who work in the treatment/control cities
c: in the base year

Appendix Table A1 Incidence of the program using cnesus of commerce

	(1)		(2)	
	Observed Coef.	Bootstrap Std. Err.	Observed Coef.	Bootstrap Std. Err.
General retail trade				
Sales	0.122 *	0.073	0.116 *	0.064
Workers	0.035	0.024	0.030	0.023
Establishments	0.042 *	0.024	0.038 *	0.022
Food and beverage retail trade				
Sales	−0.034	0.065	−0.030	0.075
Workers	0.004	0.033	−0.001	0.033
Establishments	0.022	0.023	0.018	0.027
Control variables				
Manufacturing share (lag)	No		Yes	
Trend in population density	No		Yes	
Observations [a]	736		736	

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively.
Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row.
Dependent variables are logarithms. Standard errors are clustered by city.
a: Observations for sales in a general retail trade sector are 688 for column (1) and (2). Observations for sales in a food and beverage retail trade sector are 731 for column (1) and (2).



Dash line is median (21927).
Width of bins is 10000.

Figure1 The distribution of population in 2000

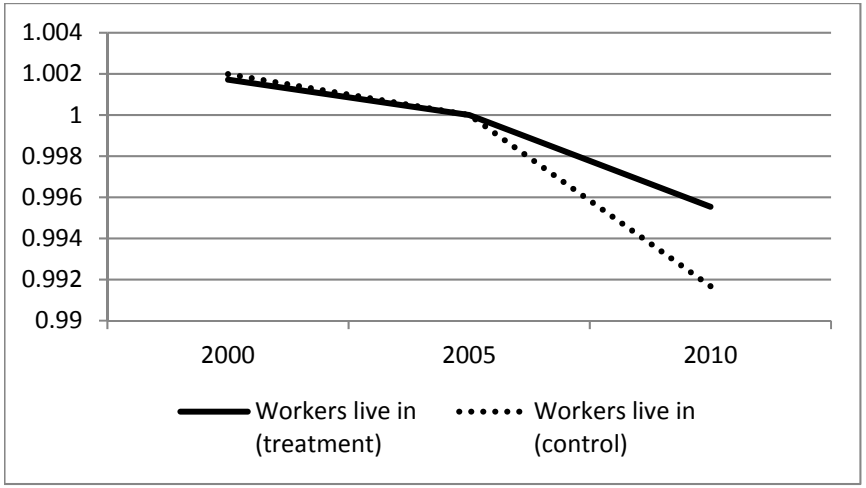


Figure 2a Workers who live in the treatment/ control group

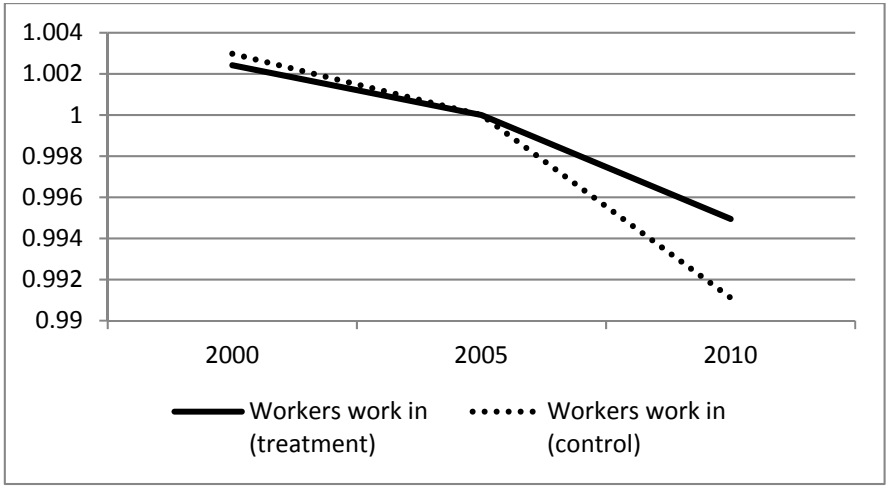


Figure 2b Workers who work in the treatment/control group

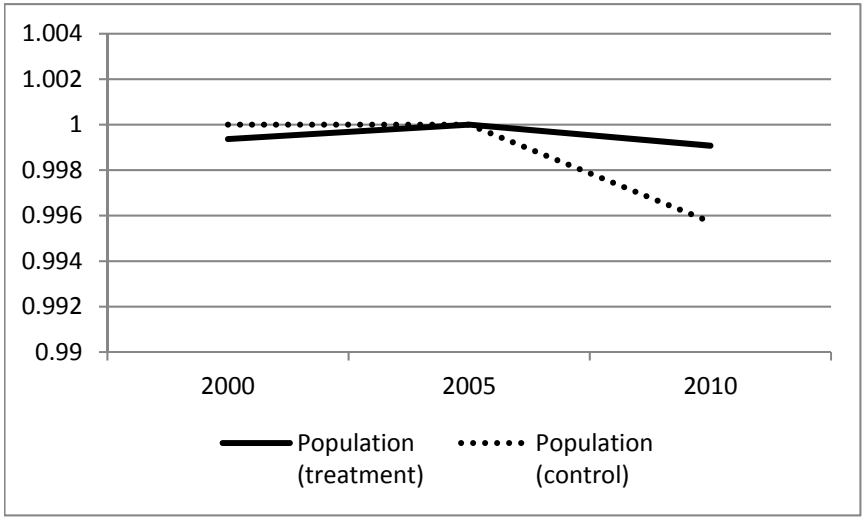


Figure 2c Population

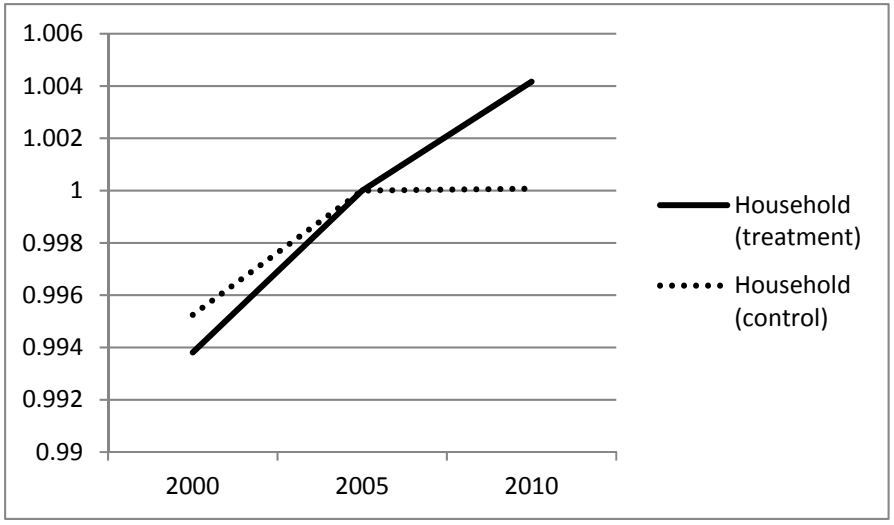


Figure 2d Household

*Vertical axis is normalized.

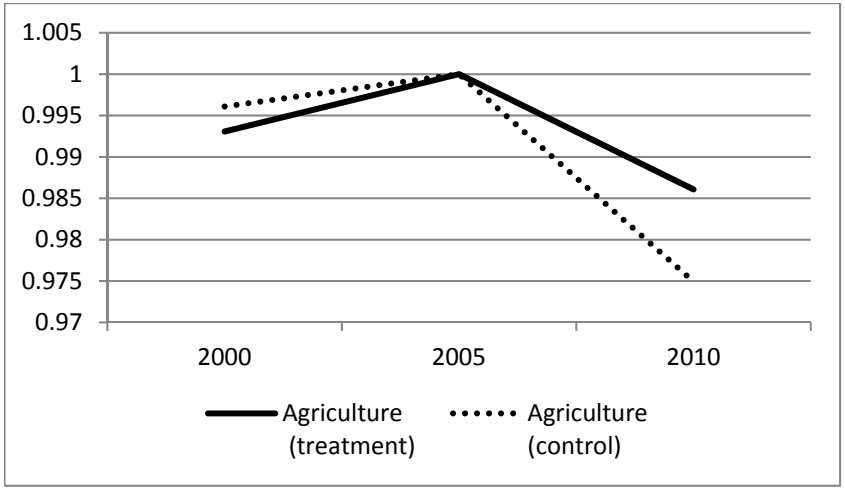


Figure2e Agriculture

Figure2f Wholesale, retail and service

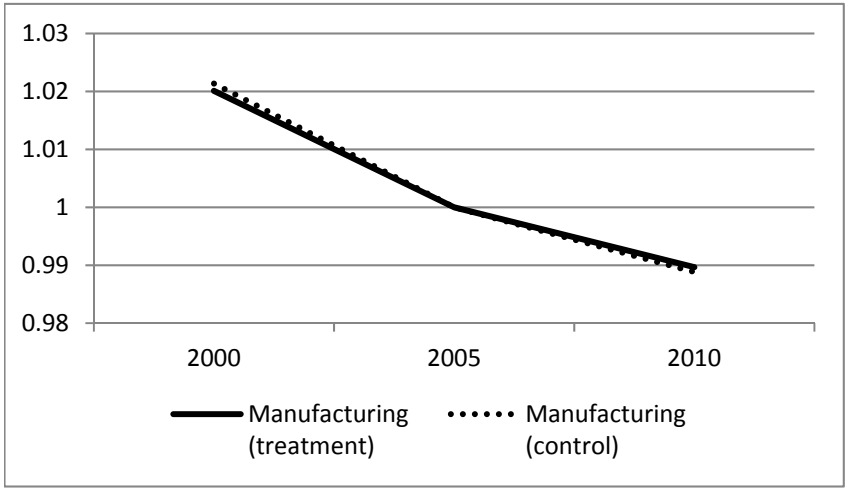


Figure2g Manufacturing

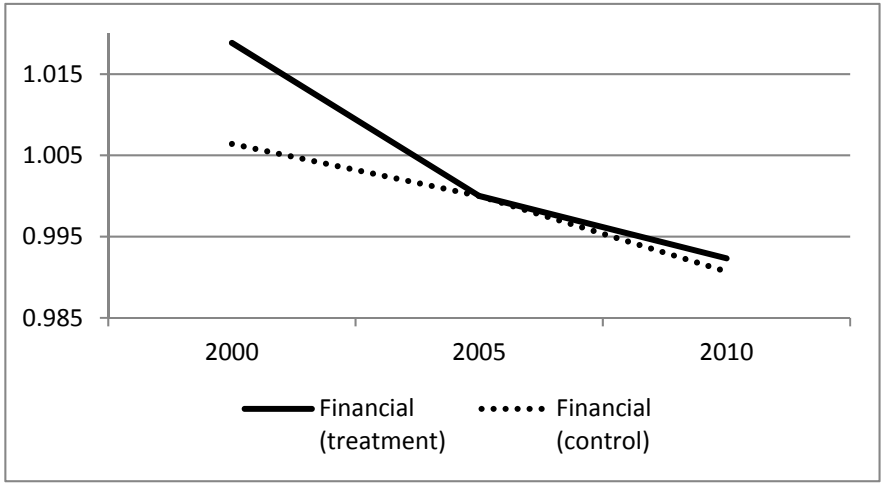


Figure2h Financial

*Vertical axis is normalized.

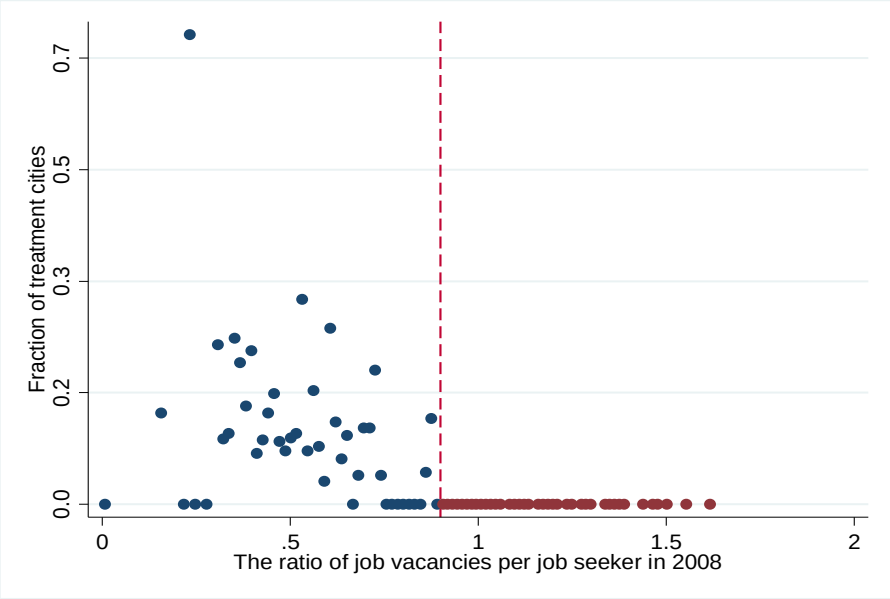


Figure3 Fraction of cities conducted the place-based job creation program

Figure4a log number of employment in 2005

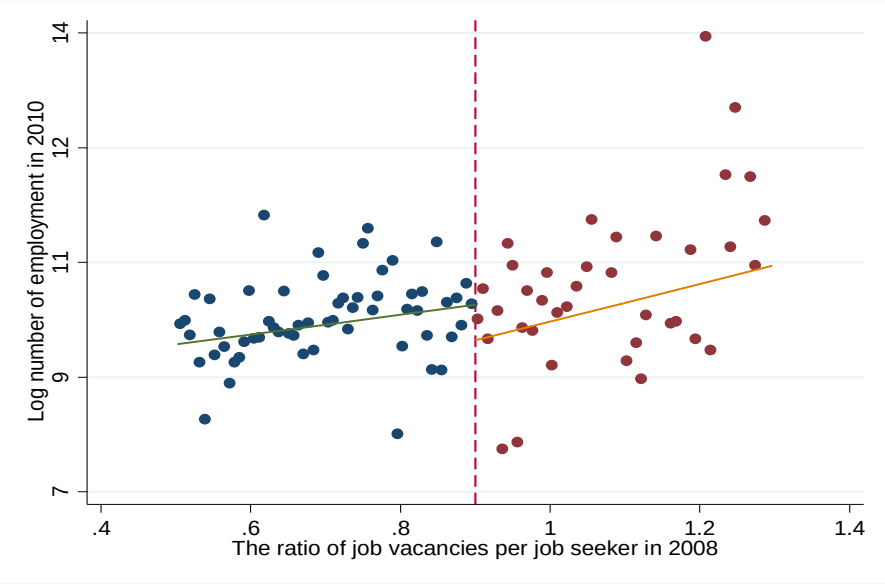


Figure4b log number of employment in 2010

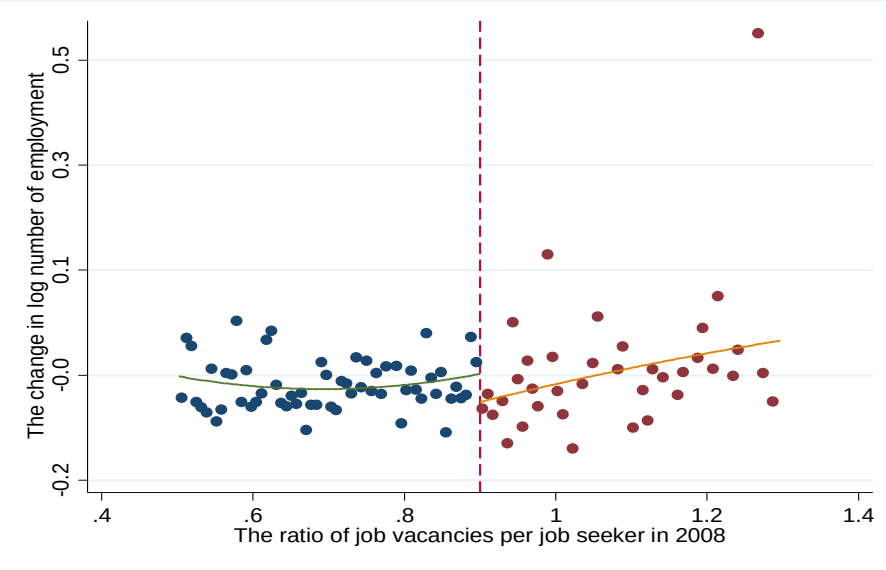


Figure4c the change in log number of employment from 2005 to 2010

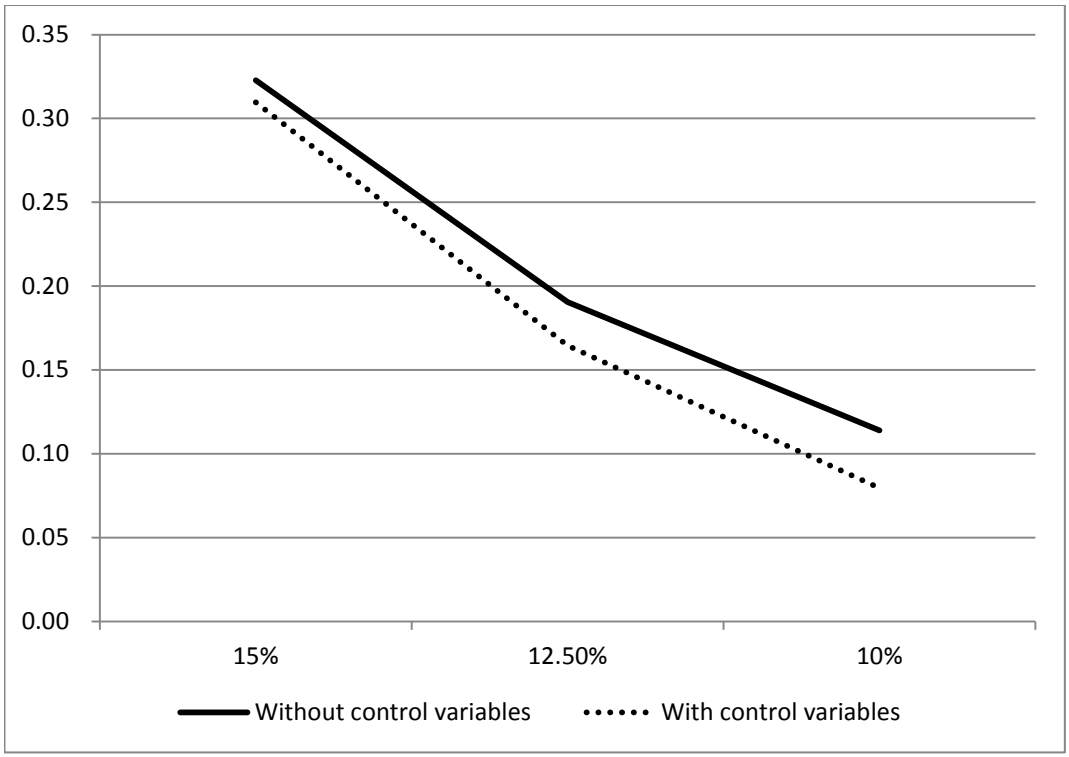


Figure indicates the coefficients of the cross term between the time dummy and the neighboring cities' dummy by definitions of neighboring cities.
Outcomes are log of sales in general retail trade when this paper compares neighboring cities of the treatment group versus neighboring cities of the control group.
10, 12.5 and 15% indicate that the term of "neighbor" is used if more than 10%, 12.5% and 15% of workers who live in a city c commute to any targeted cities in 2005.

Figure 5 Effects on neighboring cities by definitions of neighboring cities