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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 has been 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 explores the cities that the program most affects. Those with large aging populations and those with small working age population decrease the effects of the program. Third, this paper assesses the rationale of this program and does not observe a strong reduction in sales, workers or establishments in the neighboring cities of the treated city.

Keywords: place-based policy, job creation, unemployment, 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 on differences in local labor market outcomes across cities and regions. Neumark and Simpson (2015) argue that agglomeration economy, imperfect labor mobility or spatial mismatches and network effects justify place-based policies. Most previous studies have emphasized assessing the subsidy's impact on job creation, and most academic research and official government documents have evaluated how the policies affect local employment, usually with the goal of computing the number of jobs created per dollar/yen spent. However, this emphasis has been insufficient.

Kline and Moretti (2014a) argue that the job growth resulting from place-based policies induces migration into the treated community and increases living costs, thus, the benefit from a subsidy turns to land owners under perfect mobility. Solely evaluating the number of created jobs does not tell us if a place-based policy improves opportunities for employment, the income or welfare of disadvantaged residents, or if the landowners, migrants and workers who commute to the treated city benefit from the policy. From this perspective, Busson, Gregory and Kline (2013) analyze the federal urban empowerment zone program, noting 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. Few theoretical and empirical papers examine this issue. Therefore, this paper evaluates precisely the incidence of the place-based job creation program in Japan, including residents' mobility. This paper compares the program's effects on both residents' workers and workers included commuting workers, and it examines the change in the number of residents and households.

Moreover, this paper considers the optimal use of this program's subsidy. Few papers have empirically studied the optimal use of place-based policies, but it is important to consider optimal use because the budgets of policy programs are always limited. Kline and Moretti (2013), using a theoretical model, show that the targeting of less-productive areas implicit in place-based subsidies are efficient when the hiring costs are excessive. They find that the subsidy is optimal even when labor is perfectly mobile across cities. Briant, Lafourcade and Schmutz (2015) demonstrate that spatially integrated neighborhoods increase the effect of the second wave of Zones Franches Urbaines (ZFU) by creating a global index of spatial isolation using the numbers of rivers, railroads and roads that separated the ZFU, the smallest distances between ZFU border and highway junctions, train or metro stations, the distances to all municipalities within the metropolitan area and other variables. ZFU targets municipalities facing acute unemployment, and it offers relief on property taxes, corporate income taxes and wage taxes. Furthermore, they conduct a separate analysis for each dimension of spatial isolation and find that severance inhibits the effects of ZFU, and accessibility and centrality increase the impact of the ZFU program. Neumark and Grijalva (2013) assess many hiring credits and clarify that hiring credits based on investment, credits that are paid based on growth in part-time employment, refundable

hiring credits, credits that allow for the recapture of payments if the required goals are not met and credits targeting the unemployment occurring during the Great Recession succeeded in boosting employment.

The place-based job creation programs that this paper analyzes are conducted in rural areas (I will explain in detail later). These areas have an aging population problem. Therefore, this paper especially focuses on the population size (aging population size, working age population size and population density) among regional characteristics and explores which cities that the program most affects. The aging population problem is one of the political, economic and social issues in Japan, and other countries, such as China and India, as well as other developed countries, are going to face this problem in the near future following Japan.

Although this program is efficient and used optimally, we must consider whether the program is a zero-sum game. In other words, this paper assesses the rationale for this program. If consumption demand increases in the treated cities because the program that this paper analyzes offers how-to seminars for local establishments to help them attract consumers to their local goods, in many cases using local specialty agricultural products, 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, then 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 place-based policies increase the productivity of firms in the treated zones and contribute to economic growth at the national level, not only at the local level. Freedman (2012) estimates the effects of new market tax credits using regression discontinuity design, and the study shows that the spillover effect on neighboring areas is small. Neumark and Kolko (2010) examine the California enterprise zone program using geographic mapping

methods. They compare the 1000 ft distance from the outer boundary of the treated areas and the areas designated as the enterprise zone in later years, and these areas were closer than the outer boundary ring. Neumark and Kolko (2010) argue that, if there is a positive spillover, the estimation results of the latter must be larger than the former, and if there is a negative spillover, the results must be reversed. However, the estimation results of the outer boundary ring of the enterprise zone and the areas designated in later years are similar; thus, they conclude that there is no spillover effect. Ham, Swenson, İmrohoroğlu and Song (2011) find positive impacts of state enterprise zones, federal empowerment zones and federal enterprise community programs on local labor markets using the difference in difference estimation and the impact of insignificant spillover on neighboring census tracts.

In contrast, Chirinko and Wilson (2008) shows that state investment tax credits in the U.S. increases investment in the treated states, but the neighboring states' capital prices increase (and investments decrease). Wilson (2009) shows that R&D tax credits increase the state's R&D, but the decreases in R&D in other states cancel out this increase. He demonstrates that the state's magnitude of elasticity is the same with that of other states. Goolsbee and Maydew (2000) also show that reducing the payroll tax weight increases the employment in the same state but decreases the employment in other states. Roughly speaking, the studies that have considered the effects on other states' prices have shown negative externalities. Freedman (2012) notes that prior evidence of spatial spillovers associated with geographically targeted incentive programs has been mixed.

These mixed results might have arisen from reallocation in the case of negative effects and positive externality effects for positive effects, as well as the literature examining different policy programs and using different methods. Neumark and Simpson (2015) point out the

distributional effect of place-based policies because skilled workers are highly mobile, they migrate to the treated cities, and low-income residents who are targeted leave the treated areas, in addition to the program having the greatest impact on the new businesses in the enterprise zones or relocation of businesses into the zones. Hanson and Rohlin (2013) argue that the effect of enterprise zone programs arises from relocation because the program negatively affects the number of establishments and the employment in neighboring areas. Givord, Rathelot and Sillard (2013) explore negative spillovers on nearby areas. They examine the second wave of ZFU in 2004 and find that the ZFU boosted the number of establishments in the treated areas by approximately 5-7% via births and relocations, but the relative impact on relocations is much greater. Givord et al (2013) examines the 300-m rings around the treatment group based on similar rings around the control group. They find that the spillover effects on economic activity have more or less the same magnitude as the direct effects. They argue that the direct impact of ZFU is compensated for by losses in the neighboring territories.

In addition to reallocation issues, if place-based policies positively/negatively affect neighboring areas, the estimation results based on comparing between treatment groups and neighboring areas as a control group have estimation bias. In either case, few empirical studies have addressed the rationales for place-based policies, and this paper examines whether the gains in the treated cities are cancelled out by losses in the neighboring cities and whether the total gain at the national level is zero.

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 could be increased if the gain were 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 regard 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 the academic literature include the analysis of who gains from the program, the exploration of the regional features of optimal use and the examination of the program's rationale. As a 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 has been a lack of data. The place-based job creation programs that this paper analyzes have the following advantages. First, this paper has the advantage of being able to provide data. Some policy programs do not publish detailed data. Moreover, Neumark and Kolko (2010) see as a problem the difference between the actual geographic location of place-based policies' treated areas and the geographic segmentation recorded in the data source. However, this paper obtains the exact same segmentation as the data source with the segmentation to designate the treated areas. Furthermore, some policy programs are place-based, but all prefectures are targeted.

The second advantage is the ability to estimate a pure effect. 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 only to create jobs. The third advantage that it is a financial program. A major place-based policy for job creation is the Japanese policy of designated structural reform districts; many studies have discussed this policy. However, a designated structural reform district is exempted from regulations and does

not reap financial benefits, such as a tax reduction[1] or subsidy. Since 2016, the government has reformed the designated structural reform districts and designed new special zones with reduced taxes. Prior to the effect of these new special zones being known, the results presented in this manuscript will provide some milestones regarding the efficacy of the place-based policy.

To evaluate the incidence of the place-based job creation program, this paper mentions a control group. Neumark and Kolko (2010) carefully discuss how to create a control group in their estimation of California's enterprise zone program, and Neumark and Simpson (2015) report how to identify a control group. They argue that some previous studies' broad control groups make no sense, e.g., estimating the effects of enterprise zones in a number of states by comparing them with areas in states outside the enterprise zones. In addition, they argue that matching census tracts with enterprise zones that use propensity score matching does not account for the unobservable sources of job growth differences.

The first approach to creating a control group is to designate a narrow buffer immediately outside the treated areas. Billings (2009) and Neumark and Kolko (2010) use this approach. However, the place-based job creation program that this paper examines includes seminars on how to attract people to buy local products from the treated area, making it possible to push neighboring cities' consumer demand far from the neighboring cities' markets. Therefore, this paper does not use this approach. The second approach, as in Busso and Kline (2007)[2] and Neumark and Kolko (2010), is using areas added later or earlier to enterprise zones/treated areas. Busson, Gregory and Kline (2013) use rejected and future applicants to the empowerment program as a control group. Neumark and Simpson (2015) argue that this second approach "can be more reliable than using close areas as controls because it has been demonstrated through the policy process that the areas in the control groups that were included in the zone (treated areas)

at some point during the sample period were appropriate for enterprise zone (policy) designation" (the author has added the words in parentheses).

The third approach, this paper adds, is to use a regression discontinuity design. Areas that are immediately above or below a threshold of a regression discontinuity design can avoid arbitrariness, such as areas that local authorities have strong motivations or abilities to improve their municipal areas being designated as treated areas; and these areas are randomly assigned. This paper uses the second approach in the difference in difference estimation and the third approach in the regression discontinuity design. This paper uses rejected and future applicants to the Japanese place-based program as a control group following Busson, Gregory and Kline (2013). This paper uses areas just above the threshold of national average job vacancies per job seeker, which is a criterion that is required to apply this Japanese program in the regression discontinuity designs.

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. This paper clarifies who and what (residents or others, people's mobility and working sectors) gain from the programs. This paper mainly uses the difference in difference estimation, and it uses a regression discontinuity design in part. Second, this paper explores the regional features of optimal use. 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. Section 3 presents the empirical approach for examining the incidence of the programs and demonstrating the regional features of optimal use.

Section 3 also explains the data, displays the fundamental evidence using figures, and explains the estimation results. Section 4 analyzes the rationale of the programs. Section 5 presents the robustness check of the program incidence results from Section 3. Section 6 presents the conclusions and discussion.

2. The place-based job creation program

This place-based job creation program[3] seeks to support to the municipal job creation policy in areas with few job opportunities. Local authorities design the job creation plan and compete for money subsidized by the Ministry of Health, Labour and Welfare. They are recommended to design the plan in relation to their municipal industry promotion policy and related policies for local regeneration by other ministries. Local authorities can apply for this subsidy at the city (municipal) level, but only cities that have lower levels of job vacancies per job seeker that are less than the national average level can use this subsidy. Cities compete for money, but in reality, if the job creation plan of an applying city does not meet a minimal criterion that the Ministry of Health, Labour and Welfare determines, and the applying city cannot revise the plan to a suitable level, the applying city is rejected, but few cities are rejected. This Japanese program might be similar to the Patti Territorial program in Italy in 1997 in terms of the development of the program plan by the local authorities or local entities concerned. In

contrast, this Japanese program differs from Law 488 in Italy, which ranks firms' plans and funds them in the order of their ranking. The Japanese program does not fund using a rank order in reality, and local authorities are required to meet a minimal criterion even if the ministry announces competition.

The subsidy amounts to a maximum of two million dollars per year[4]. The amounts depend on the job creation plan, but the cities are required to pay less than 15 thousand dollars for each newly created job. Therefore, the amounts are associated with the population size as an expectation of the number of newly created jobs being higher in larger cities with much unemployment and many establishments. Local authorities use this subsidy for job training for local unemployment, seminars about increasing business for local establishments and activities to help find jobs and fill vacancies. Local authorities collaborate with job placement offices and discuss job vacancies with participants in job training. Local authorities also hold joint job festivals and joint job interviews for local job seekers and establishments. Local authorities cannot use this program's subsidies for physical capital investment or infrastructure investment as in other countries' place based programs, e.g., Law 488 in Italy or the European Regional Development Fund.

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. Many cities conduct this program in the agricultural and tourism sectors. The Ministry of Health, Labour and Welfare stops municipal

plans if a targeted city cannot reach 50% of the expected outcome or 90% of all expected subset goals and so on. Neumark and Grijalva (2013) argue that allowing regions to claw back credits if the job creation goals are not met appears to make these credits more effective, as mentioned earlier. This Japanese place-based job creation program started in 2007, and the government decided on a new policy for local regeneration at that time.

3. Incidence of the program and optimal use

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 first uses the difference-in-differences method. Bell, Blundell and Reenen (1999) note that the treatment and control groups should react similarly to exogenous macro-shocks; therefore, some of the previous studies have selected time periods that present similar macro-environments with the periods before and after an event change, and they 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 it cannot obtain the average population effect. Instead of this method, Busson, Gregory and Kline (2013) resolved this issue by selecting a control group. They make regions that were previously

rejected but allowed into designated empowerment zones in subsequent rounds as 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. There is no information about why the cities in the control group did not use the program in earlier years. The control group might have rejected or did not apply this subsidy because it was unwilling to apply for the program, or it did not have information about the program. This paper checked whether the level of job vacancy per job seeker of the control group is higher than the eligible level to apply for this subsidy, but most of the cities in a control group were also eligible to apply for this program's subsidy 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 as outcomes: (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. This paper also estimates the effect by sector, including: (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 in wholesale, retail trade and service sector because many treated cities conduct trial businesses, 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 of the 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 are no data about wages or housing costs at the city level, especially in the 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 decrease in Japan. This paper observes sales volume, instead of wages, in Section 4. d_i is the time dummy, which equals zero before the period from 2007 to 2009 and one after 2010. TC_i is the treatment group dummy, which equals one if city i is the treatment group and is otherwise zero. α_3 is the estimated effect of the place-based job creation program. X_{it} are control variables, such as the lag share of manufacturing among the total number of workers, the change in population density over the 2000-2005 period and the unemployment rate in the base year (2000).

Second, to explore the regional feature for optimal use, 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) one if the proportion of people older than 65 years old is more than 32% of the total population in 2000 and zero otherwise; (2) one if the working age population is less than 58.5% of the total population in 2000 and zero otherwise; and (3) one 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[5]. A city with a large aging population has less consumer demand. A large aging population also indicates a smaller labor supply. This paper examines whether the effect of this program is less in cities with large aging populations. The aim of using the working age population rather than the labor force participation rate is roughly to capture the negative effect of having too few young residents (e.g., only one class in each grade at school, resulting in less competition, fewer peer effects, less information about career options from friends, less influence and fewer opportunities for younger generations to play important roles in the community). This paper also predicts that the programs will affect cities with lower population densities less because lower population density does not 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 models (1) and (2) are clustered by city[6].

Third, in addition to the above estimations, this paper examines the effects of this job creation program using a regression discontinuity design. Instead of using the above control group, this paper compares the treated cities with cities that have job vacancies per job seeker that are only slightly above the national average. As shown in Section 2, only cities that have less than the national average levels of job vacancies per job seeker can use this job creation program's subsidy. The cities that have slightly more than the national average levels of job

vacancies per job seeker are similar cities to the treated cities and are randomly assigned. The programs started in multiple years (2007 to 2009), but this paper uses the data from a single year, 2008, for a single threshold. Additionally, the common data on job vacancies per job seeker are used in the multiple cities administered by the same job placement agency. These factors are why this paper does not use a regression discontinuity design for the main analysis.

3.2 Data

This paper obtains data from population censuses in 2000, 2005 and 2010. Japan's population census is 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 places 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 for the difference in difference estimation. These cities include few cities with enormous populations; thus, this paper excludes cities with populations greater than the 90th percentile. After excluding enormous cities greater than 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. Table 1A shows descriptive statistics of the pretreatment sample, and Table 1B provides p-values for the t-test of the null hypothesis that average pretreatment levels and the 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 trends in population density. This paper excludes these differences using control variables. As for population inflow and outflow, this paper uses data from Basic Resident Registration, for which mayors are responsible.

Figure 2 shows the changes 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 magnitude of the decrease is the same between the treatment and control groups from 2000 to 2005. However, after starting the program, the treatment group's decreasing trend is slowed, while the control group's trend becomes more rapid. Figure 2b shows the normalized logged numbers of workers who *work in* the treatment/control group. We find the same evidence here. The two groups' decreasing trends are almost the same from 2000 to 2005, and the decrease is greater 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 groups' 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 recorded a decrease in total population for the first time since the Second World War. The normalized logged number of households increases in 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 numbers of workers by sector. In the control group, the agricultural sector and the wholesale, retail trade and service sector show more rapid downward trends after 2005. The manufacturing and financial sectors show almost similar trends in the treatment and control groups after starting the program. There are no data about wages or housing costs at the city level in the treatment and control groups. However, instead of

wages, this paper discusses sales volume in Section 4. In addition, the housing costs might not increase dramatically because 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[7].

3.3 Results

Table 2 shows the estimation results of α_3, the coefficient of the cross-term between the 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 among 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),[8] in addition to control variables for a lag share of manufacturing and a trend in population density. All of the 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, indicating 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 undertake job training through this program.

The third row and below shows the 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 might 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 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), respectively, which are smaller (but positive) effects than those in the agricultural sector. If the model controls for the unemployment rate in the base year, the effect is insignificant, but the results in a subsequent table (Table 4) are significant when the cities are controlled for the aging rate or the working age population size. The employment in the agricultural sector increases through the rising sale of agricultural specialties via this program's promotion or via rising demand for a supply of new local brands. Sometimes, female farmers start to work for a new local brand. The employment in the wholesale, retail trade and service sectors increases through job training in concierge services or information technology skills. In contrast, the program does

not affect the manufacturing and financial sectors. The coefficients are insignificant from Column (1) to Column (3). The program does not affect these sectors as predicted. Additionally, these results indicate that the program does not induce a transfer from the manufacturing or financial sectors to the agricultural or wholesale, retail trade and service sectors.

This paper roughly calculates cost using the results above. It 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%, followed by aggregation of 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 might be an overestimation of the programs because 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 their programs in relation to their industry promotion policies and related policies for local regeneration by other ministries. Therefore, they could use their budgets for their industry promotion policy, and they could, in most cases, obtain subsidies from other ministries for related local regeneration policies. Note, however, that the cities in the control group also generally use their municipal budgets to promote local job creation, and they obtain subsidies provided by other ministries.

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

Next, this paper considers which cities the program most affects. 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[9], 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 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. Table 5 displays the estimation results by a regression discontinuity design. The first line is the result when the bandwidth is optimal, and the second and third lines are half and double the bandwidth of the optimal level, respectively. 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. The second and third lines also indicate that 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. Program rationale

4.1 Empirical model and data

Although the program is efficient, is 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 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 decrease through place-based job creation programs using the difference in difference method. 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 because 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 use local specialty agricultural products 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 city *c* commute to any targeted cities in 2005[10]. Neumark and Kolko (2010) and Givord et al (2013) use a ring around a targeted area as a neighboring area. However, mountains and rivers are often obstacles to traffic, and public transportation is more common rather than driving in Japan. Thus, commuting zones are not concentric circles in Japan. This paper uses the 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 one if the 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 regard to (1), if the program has a positive effect on the treatment group, it is possible that we observe a negative effect on neighboring cities of the treatment group, although the outcomes of the neighboring cities do not change. Regarding (2), this paper confirms no effect on the neighboring cities of the control group. Then, regarding (3), this paper predicts a negative effect on neighboring cities of the treatment group, compared to the neighboring cities of the control group, if the increase in consumer demand and the employment growth in the treatment group take 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 one if city i is a treatment group and otherwise equals zero. $Ntre_i$ equals one if city i is a neighboring city of the treatment group, and $Ncon_i$ equals one 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 method. The prefecture level is used as a block.

4.2 Estimation results

Table 6 shows the estimation results for Model (1). Column (1) shows the estimation results for neighboring cities of the treatment group versus cities in the treatment group without controls, and Column (2) shows the estimation results with control variables[11]. 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 the results using the 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 earlier, if the program has a positive effect on the treatment group, we observe negative effects on neighboring cities of the treatment group compared with the treatment group, although the outcomes of the neighboring cities do not change. Table A1 (see Appendix) shows the results for this program's effects using the 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, respectively, 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 or

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 the industrial structure and the trend in population density are controlled for. 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 Columns (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 shown in Column (5) and Column (6). Figure 5 indicates the coefficients of the cross-terms between the time dummy and the neighboring cities' dummy by the definitions of the neighboring cities. The term "neighbor" is additionally used if more than 15% and 12.5% of workers who live in city *c* commute to any of the targeted cities in 2005. The magnitudes of coefficients decrease with the expansion of the ranges of neighboring cities, although the 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 for the lag share of manufacturing and the trend in population density, respectively. 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 control groups, neighboring cities 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 Tables 6

and 7, this paper does not clearly observe the program's destruction of neighboring cities' demand.

5 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 one if the data are from 2005 and is otherwise zero. This paper confirms that the outcomes of the 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 for the lag share of manufacturing and the trend in population density; Column (3) indicates the results controlled for the lag share of manufacturing, the trend in population density and the unemployment rate in the base year (1995). All of the 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, nor does it affect 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 the population or the number of households in the treatment group, as shown in the lower part of Table 8.

6 Conclusion and discussion

This paper has three purposes. First, 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. However, it is important to know the incidence in greater detail, e.g. whether residents in disadvantaged areas gain from programs or whether the place-based job creation program induces migration. 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 the number of 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 businesses or seminars via these programs.

Second, this paper demonstrates the optimal use of this program's subsidy by analyzing the cities that the program most affects. Few studies have empirically investigated this issue, but it is important to know the optimal use because a policy budget is generally limited. Addressing population issues (aging population rate, working age population size and population density) in regional features is especially important in Japan, which has an aging problem. However, other developed countries and Asian countries are also going to face an aging problem following Japan. As a result, the program has lesser effects in cities with large aging populations or small working age populations. These results might indicate that the sole improvement in job creation in the

disadvantaged areas is not sufficient. The firms prefer a thick labor market; thus, an agglomeration of people is needed, although the estimation results of population density are insignificant. A small working age population has negative impacts on young people and local economic activity. The policy of reversing the decrease in young people or improving local activity might be needed with a place-based job creation program.

Third, this paper confirms whether a 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 have overlooked the total aggregate effect, especially by empirical demonstration; in addition, government officials only calculate 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 purchase local specialty products. Therefore, this paper uses the sales data from a census of commerce and the population census. As a result, this paper does not observe a clear decrease in sales or in the number of workers in neighboring cities of the treatment group.

One limitation of this study is that the magnitude of the positive incidence of this place-based job creation program includes some impacts of other subsidies because the treatment cities are recommended to design the program in relation to their industry promotion policies and related policies for local regeneration by other ministries, although the cities in the control group also use these subsidies. To expand this paper, it must be determined whether the program reduces the unemployment benefits in the treated cities. However, this program also induces the participation of the potentially unemployed, such as female workers (sometimes from farming families).

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Endnotes

[1] The financial special zone, IT industry special zone and distribution special zone in Okinawa prefecture benefit from tax reductions, but the labor market conditions of Okinawa prefecture, such as its unemployment insurance system, differ from those of other prefectures because the U.S. military base covers a large area of land.

[2] 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 methods reported by Busson, Gregory and Kline (2013).

[3] *Chiiki koyo suishin jigyo* in Japanese

[4] If neighboring cities apply jointly, this subsidy can be up to three million dollars per year.

[5] According to predictions by the National Institute of Population and Social Security Research in January 2012, the aging rates 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).

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

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

[8] As shown in Table 1B, the unemployment rate of a treatment group in 2005 (before starting programs) was slightly higher than that of the control group, and the trends in unemployment rates were almost the same in both groups. The job creation program affects unemployment rate. Therefore, this paper controls for unemployment rate in the base year.

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

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

[11] The results with a control variable for the unemployment rate in the base year, in addition to control variables for a lag share of manufacturing and a trend in population density, indicate the same conclusion as Table 6 and are available upon request. s

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

	(1)		(2)		(3)	
Dependent variable	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	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

Table5 Regression discontinuity design

Bandwidth:	Coef.	Std. Err.
0.27	−0.048 ***	0.017
0.13	−0.080 ***	0.026
0.54	−0.032 *	0.017

***, ** and * indicate significant at the 1 % , 5% and 10% level, respectively. (+ indicates significant at 10.5% level.)
Dependent variables are the change in log number of employment from 2005 to 2010.

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

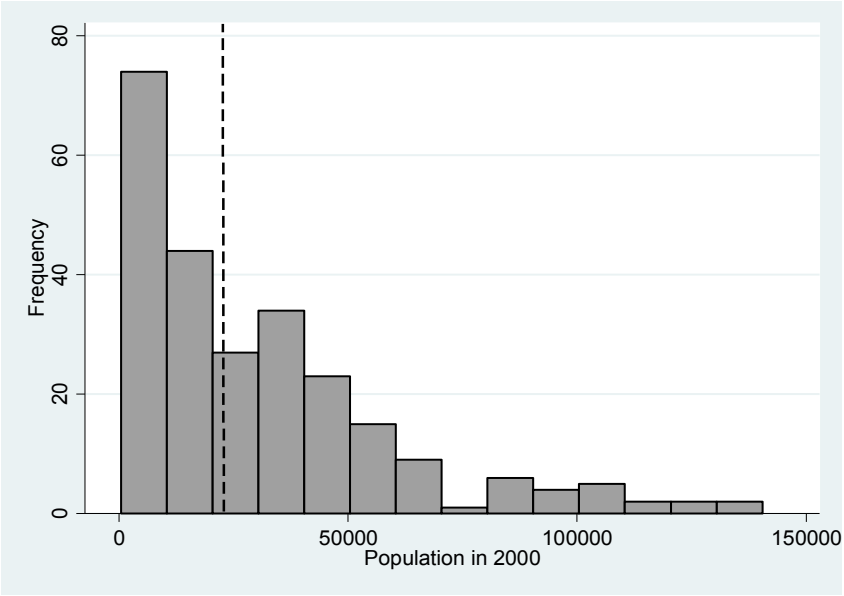
	(1)		(2)		(3)	
Dependent variable	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.	Coef.	Robust Std. Err.
Workers live in [a]	−0.004	0.032	0.010	0.032	0.018	0.033
Workers work in [b]	0.001	0.031	0.013	0.031	0.021	0.032
Workers in Agriculture	0.018	0.044	0.029	0.043	0.029	0.043
Wholesale, retail trade and 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

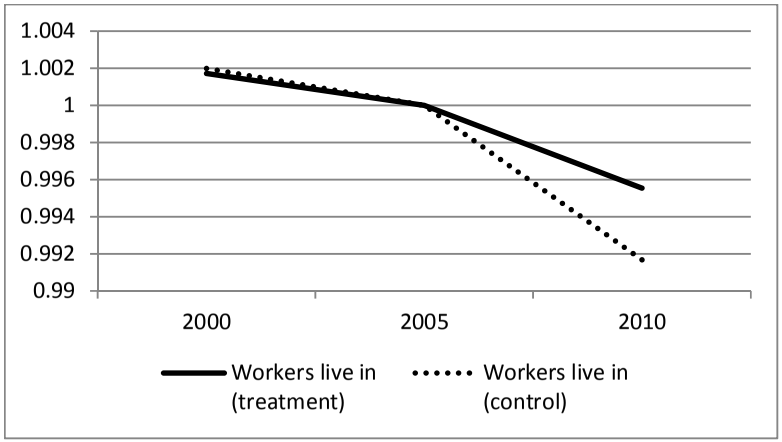


Figure 2a Workers who live in the treatment/ control group

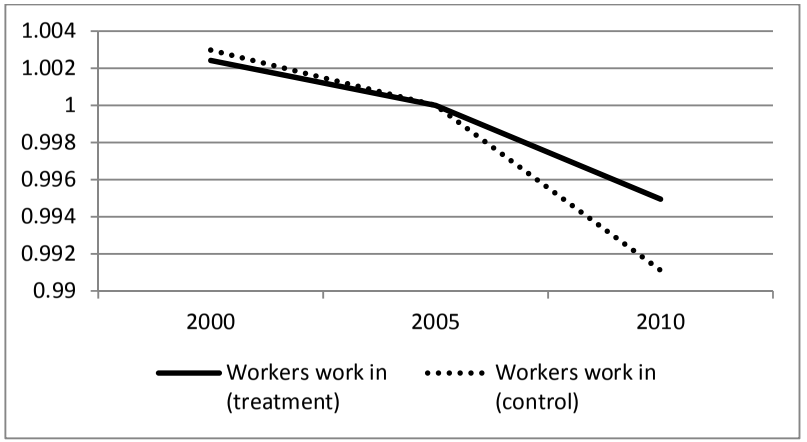


Figure 2b Workers who work in the treatment/control group

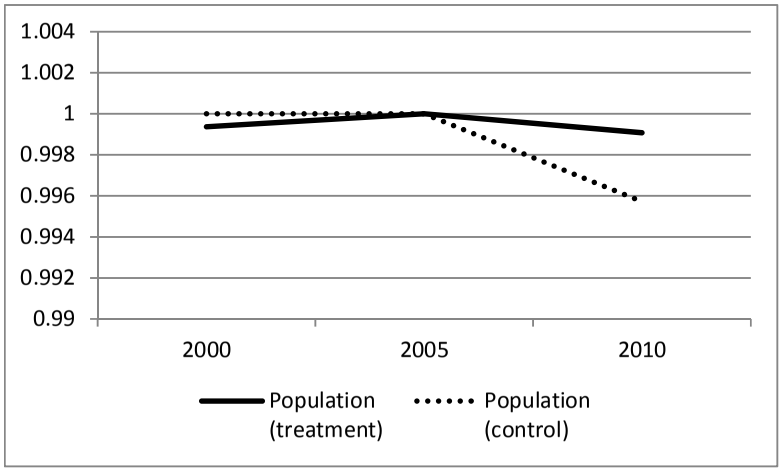


Figure 2c Population

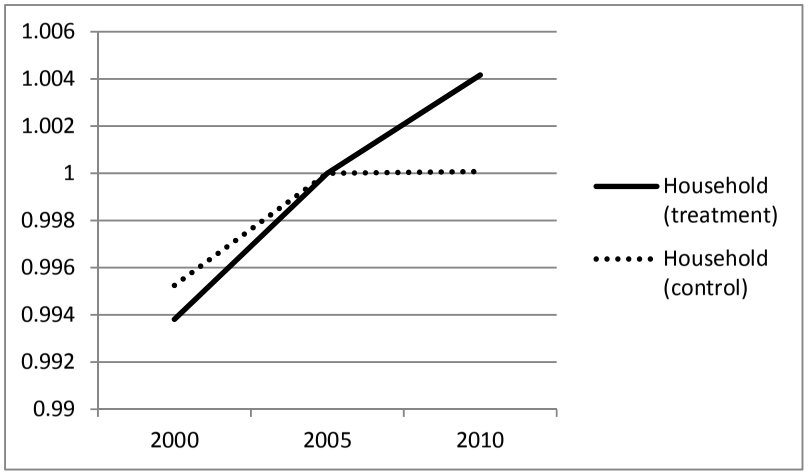


Figure 2d Household

*Vertical axis is normalized.

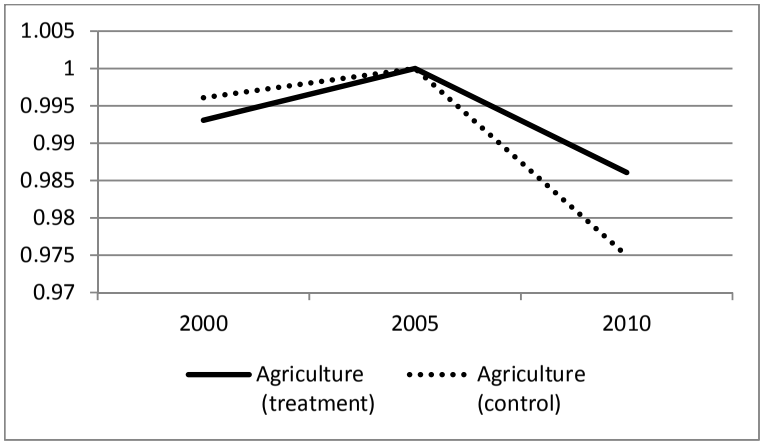


Figure2e Agriculture

Figure2f Wholesale, retail and service

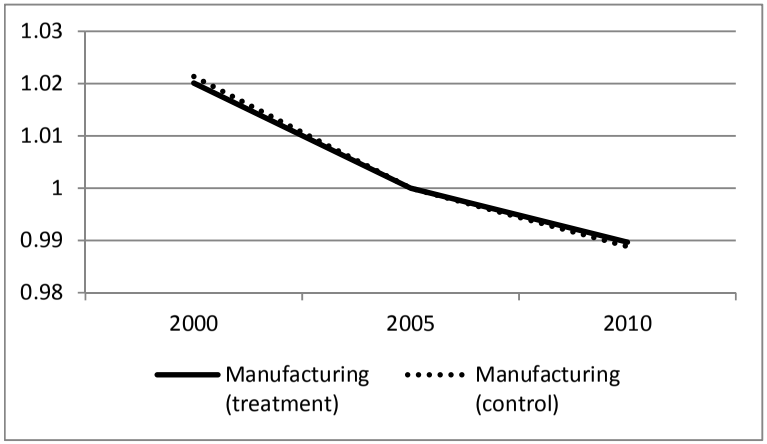


Figure2g Manufacturing

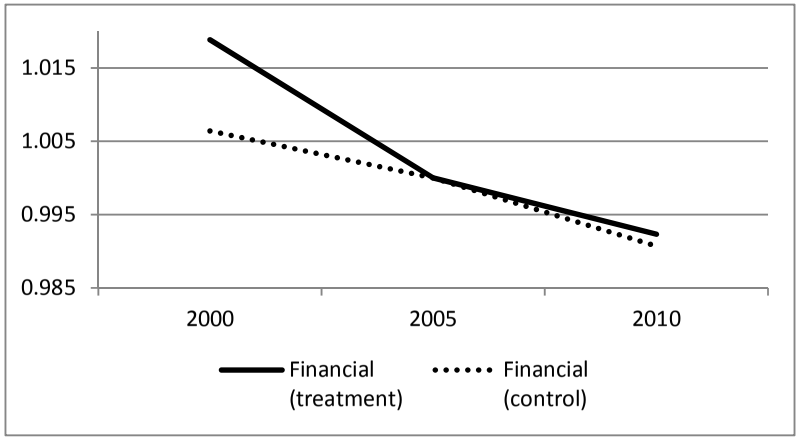


Figure2h Financial

*Vertical axis is normalized.

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

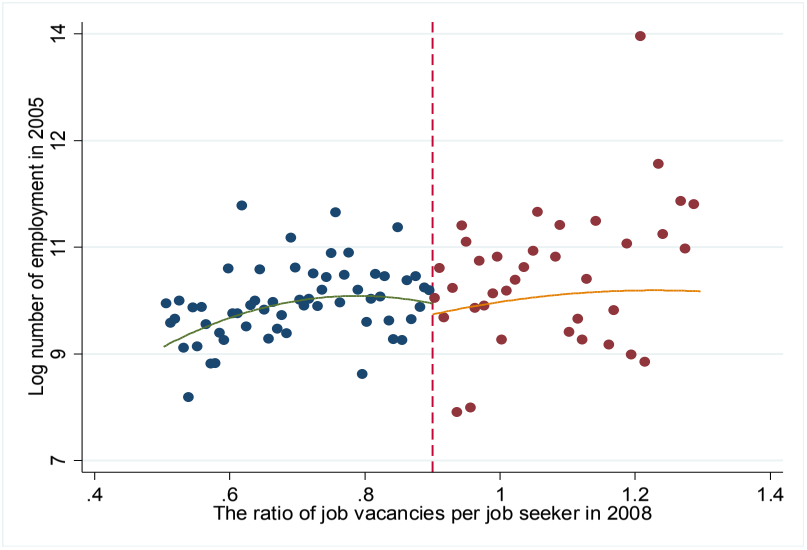


Figure4a log number of employment in 2005

Figure4b log number of employment in 2010

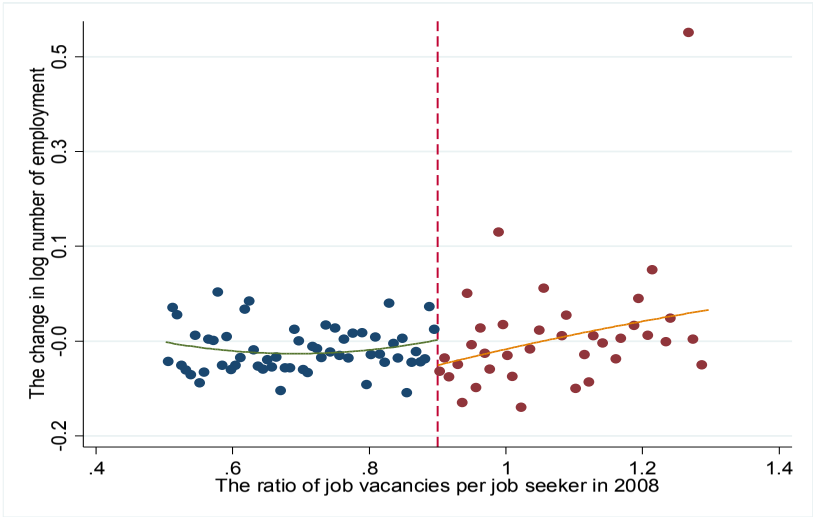


Figure4c the change in log number of employment from 2005 to 2010

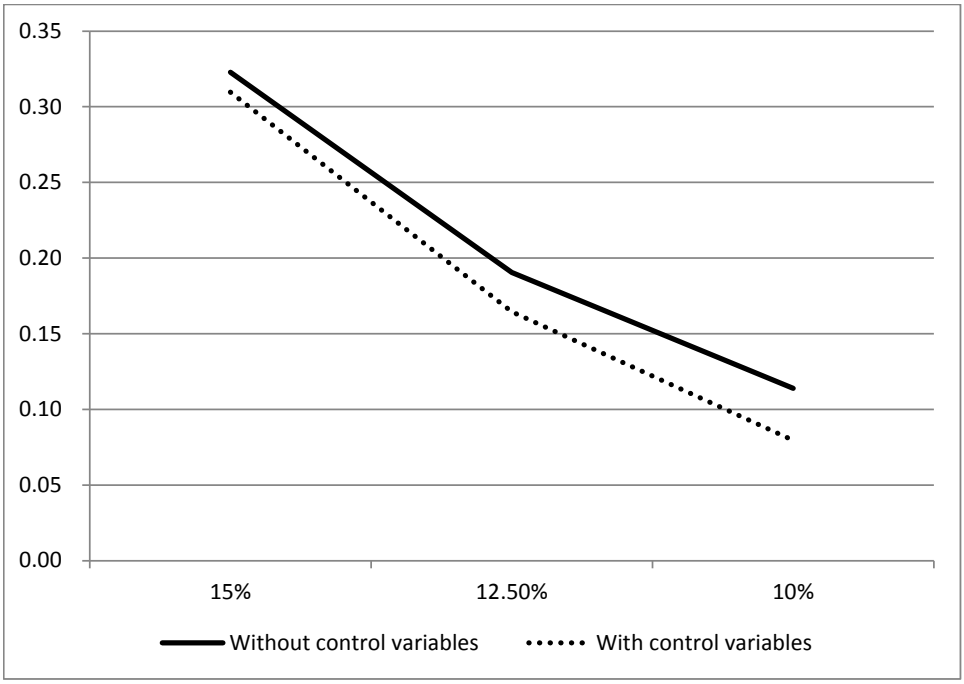


Figure indicates the coefficients of the cross term between the time dummy and the neighboring cities' dummy by definitions of neighboring cities.
Outcomes are log of sales in general retail trade when this paper compares neighboring cities of the treatment group versus neighboring cities of the control 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