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Evaluating place-based job creation programs in Japan

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Abstract

First, this paper empirically evaluates the incidence of the Japanese place-based job creation program, which has rarely been studied in Japan. The program increases employment, especially in the agricultural, retail trade, and service sectors, which most treated municipalities promote. Second, this paper explores the municipalities that the program affects most. Those with large aging populations and those with small working age populations decrease the effects of the program. Third, this paper assesses the externality effect of this program and does not observe a strong reduction in sales, workers, or establishments in the neighboring municipalities of a treated municipality.

Keywords: Place-based policy, Job creation, Unemployment, Externality effect
JEL Classification: J23, J68, R23, H22, H23

1 Introduction

Place-based policies are currently conducted in many countries such as European countries, the USA, and Asian countries. This paper analyzes the job creation program in Japan. This Japanese place-based job creation program is a bit unique. It is not a tax credit, but a subsidy for the individual job creation plan designed by a municipality. A detailed empirical evaluation of Japanese place-based policies for job creation has rarely been conducted prior to this study, due to a lack of data.

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 economies, imperfect labor mobility or spatial mismatches, and network effects justify place-based policies. Most previous studies have emphasized assessing a subsidy's impact on job creation, and most academic research and official government documents have evaluated how 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 treated communities and increases living costs; thus, the benefit from a subsidy turns to landowners under perfect mobility. Solely evaluating the number of created jobs does not tell us whether a place-based policy improves opportunities for employment, increases the income or welfare of disadvantaged

residents, or benefits the landowners, migrants, and workers who commute to the treated municipality. From this perspective, Busso et al. (2013) analyze the federal urban empowerment zone program and find that the empowerment zone program increases employment and wages without triggering corresponding increases in population and local costs 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 resident workers and workers included commuting workers, and it examines the change in the number of residents and households.

Moreover, this paper considers the efficient use of this program's subsidy. Few papers have empirically studied the efficiency of place-based policies, but it is important to consider efficient 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 is efficient when hiring costs are excessive. Briant et al. (2015) demonstrate that spatially integrated neighborhoods increase the effect of the second wave of Zones Franches Urbaines (ZFU). Neumark and Grijalva (2013) assess many hiring credits and clarify that credits that allow for the recapture of payments if required goals are not met succeed in boosting employment.

This paper focuses on population size (size of the aging population, size of the working age population, and population density) among regional characteristics and explores which municipalities are affected most by the program. The reason for this focus is that the place-based job creation programs that this paper analyzes are conducted in rural areas (as explained in detail later). These areas have an aging population problem. The aging population problem is one of the political, economic, and social issues in Japan and other countries, including China, India, and other developed countries, will face this problem soon.

Although this program is efficient and used optimally, we must consider whether the program is a zero-sum game. If consumption demand increases in the treated municipalities because the program that this paper analyzes offers how-to seminars for local establishments to help them attract consumers to their local goods using local specialty agricultural products in many cases, thus increasing the number of jobs created, but the consumption demand decreases in neighboring municipalities, thus resulting in a decrease in the number of jobs, then the gain in the treated municipalities is canceled out by the loss in the neighboring municipalities, and the total gain at the national level is zero.

Results in previous papers have been mixed. Bartik (1991) argues for a positive effect. Both Freedman (2012), who estimates the effects of new market tax credits, and Gobillon et al. (2012), who examines the French enterprise zone program that exempts wage tax if firms hire more than 20% local labor, find a small spillover effect; Neumark and Kolko (2010) and Ham et al. (2011) find no spillover effect; and Chirinko and Wilson (2008), Wilson (2009), and Goolsbee and Maydew (2000) show a negative effect on neighboring areas.

These mixed results could be attributed to reallocation in the case of negative effects and positive externality effects in the case of positive effects and the fact that the literature examines different policy programs and uses different methods. Neumark and Simpson (2015) note the distributional effect of place-based policies because skilled workers are highly mobile and migrate to the treated cities, while low-income residents

who are targeted leave the treated areas in addition to the greatest impact to be on businesses that are founded in enterprise zones or relocate into such zones. Hanson and Rohlin (2013) argue that the effect of enterprise zone programs arises from relocation. Givord et al. (2013) find that the second wave of ZFU in 2004 boosted the number of establishments in the treated areas by approximately 5–7% via births and relocations, but the relative impact on relocation is much greater and produces negative spillovers on nearby areas. Kline and Moretti (2014a) also discuss the related rationale of the agglomeration economy. 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 serious flaws in the agglomeration rationale for spatially progressive subsidies in US manufacturing. In addition to reallocation issues, if place-based policies positively/negatively affect neighboring areas, the estimation results based on comparisons between treatment groups and neighboring areas as a control group have estimation bias.

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

2 The place-based job creation program

This place-based job creation program[1] seeks to support 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.[2]

Local authorities can apply for this subsidy at the municipal level, but only municipalities that have levels of job vacancies per job seeker that are lower than the national average can use this subsidy. Municipalities compete for money, but in reality, if the job creation plan of an applying municipality does not meet a minimal criterion that the Ministry of Health, Labour and Welfare determines and the applying municipality cannot revise the plan to a suitable level, the applying municipality is rejected; few municipalities, however, 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 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.[3] This program lasts for 3 years. The amounts depend on the job creation plan, but the municipalities are required to pay less than 15,000 dollars for each newly created job. Therefore, amounts are associated with the population size, due to an expectation that the number of newly created jobs will be higher in larger municipalities with high unemployment and many establishments. In addition, designated municipalities 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 municipalities use local specialty agricultural products to create local brand goods) or the expansion of new business opportunities.

Local authorities use this subsidy for job training to address 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).

The contents of job training and seminars for local establishments differ among municipalities, depending on the local industrial structure. However, the guidebook of the Ministry of Health, Labour and Welfare suggests that it is efficient if the contents of job training, seminars for local establishments, and business activities that the treated municipalities conduct are all related. The business activities are consistent with the business areas that local establishments expect to expand, and job training is consistent with the skills that those business activities and expected expansion business areas require for workers. According to a public announcement, 81.4 and 66.6% of municipalities conducted activities in the agricultural and tourism sectors, respectively, in a program started in 2008. Many municipalities conduct this program in the agricultural and tourism sectors.

Regarding planned uses of the subsidy, the budget for salaries for persons who conduct the program (the municipalities are required to hire new workers) accounts for 40 to 60% of the total budget of this program, not including the subsidy for the business activities. The budget for seminars for local establishments accounts for 30 to 40% of the rest of the budget, the budget for job trainings accounts for 50 to 60%, and the budget for activities to help find jobs and fill vacancies accounts for approximately 10%. As for the budget for business activities, the budget for salaries for persons who conduct business activities (the municipalities are required to hire new workers) accounts for approximately 50%. The municipalities use the rest of the subsidy for raw material, monitors, trial marketing, and other uses.

In Fig. 1, red indicates municipalities that conducted this program from 2007 to 2009 and the blue is for 2010 to 2013.[4] The Ministry of Health, Labour and Welfare stops municipal plans if a targeted municipality cannot reach 50% of the expected outcome or 90% of all expected subset goals. Neumark and Grijalva (2013) argue that allowing regions to claw back credits when 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 efficient 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 uses the difference-

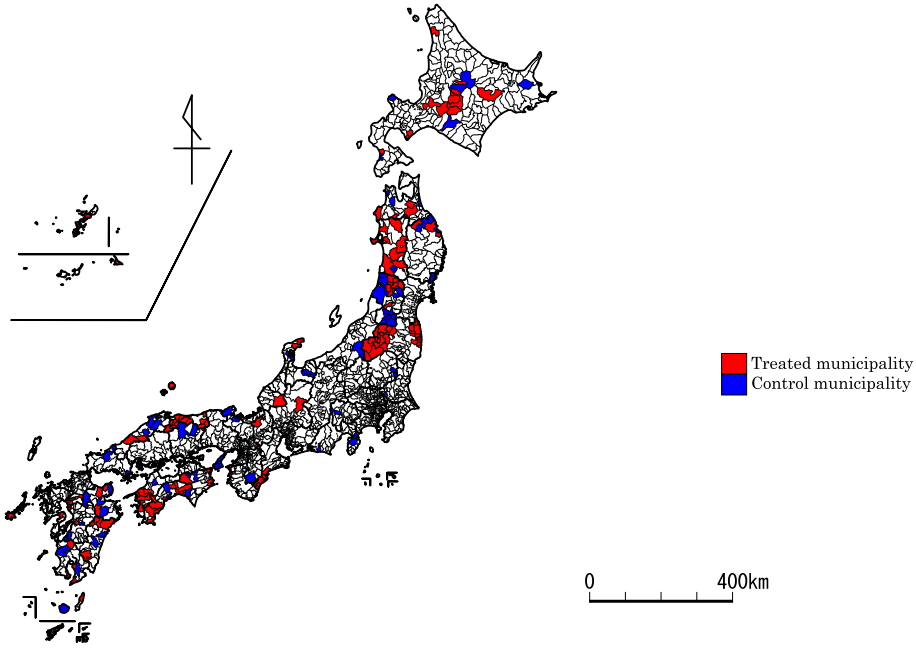

Fig. 1 Treated municipalities (*red*) and control municipalities (*blue*). The treatment municipalities started the Japanese place-based job creation program in 2007, 2008, or 2009. The control municipalities started the program in 2010, 2011, 2012, or 2013 and did not conduct the program from 2007 to 2009

in-differences method. 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 several 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.

One 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 municipalities' consumer demand far from the neighboring municipalities' markets. Therefore, this paper does not use this approach. The other approach, as in Busso and Kline (2007)[5] and Neumark and Kolko (2010), is to use areas added later or earlier to enterprise zones or treated areas. Busso et al. (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." This paper uses rejected and future applicants to the Japanese place-based program as a control group following Busso et al. (2013).

Therefore, the treatment group includes municipalities that started the program in 2007, 2008, or 2009. The control group includes municipalities 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 municipalities in the control group did not use the program in earlier years. The control group might have rejected or not applied for this subsidy because members were unwilling to apply for the program or did not have information about the program. This paper examined 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 municipalities in the control group were eligible to apply for this program's subsidy from 2007 to 2009. The empirical model is as follows:

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

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 in the wholesale, retail trade, and service sector because many treated municipalities 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 municipalities. 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 municipality level, especially in the small municipalities where the treatment or control groups are located, while Kline and Moretti (2014a) argue that housing cost could increase depending on the elasticity of the housing supply. However, the impact of housing costs is minimal in these areas due to the current population decrease in Japan. Some houses become unoccupied because of the decrease in the population. This paper observes sales volume, instead of wages, in Section 4. d_t is the time dummy, which equals 0 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 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 (i.e., 2000).

Second, to explore the feature of regions where this program works more efficiently, this paper estimates Eq. (1) by the sample group divided by (1) the proportion of people older than 65 years old, which is more than 32% of the total population in 2000 or otherwise; (2) the working age population, which is less than 58.5% of the total population in 2000 or otherwise; and (3) the population per square kilometer, which is more

than 176.65 and less than 439.825 or otherwise. Thirty-two percent is almost equivalent to the prediction for the national average aging rate in 2030.[6] A municipality 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 municipalities with large aging populations. The aim of using the working age population rather than the labor force participation rate is to roughly 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; and less influence and fewer opportunities for younger generations to play important roles in the community). This paper also predicts that the programs will affect municipalities with lower population densities less because municipalities with lower population density do not benefit from an agglomeration economy. An overly high population density also has disadvantages; therefore, this paper divides the sample by the third quartile of population density (when the population per square kilometer is more than 176.65 and less than 439.825) or otherwise. The estimations of model (1) are clustered by municipality.[7]

3.2 Data

This paper obtains data from population censuses in 2000, 2005, and 2010. Japan's population census is conducted almost every 5 years. It covers all Japanese territories and surveys numerous topics (e.g., work status, place of work, population by age, and number of households). Treatment (started the program in 2007, 2008, or 2009) and control (started the projects in 2010, 2011, 2012, or 2013 and did not take part in the program from 2007 to 2009) municipalities include few municipalities with enormous populations; thus, this paper excludes municipalities with populations greater than the 90th percentile. After excluding municipalities with populations greater than the 90th percentile, there are 167 municipalities in the treatment group and 81 municipalities in the control group. The population distribution in 2000 is shown in Fig. 2. Figure 1, mentioned earlier, indicates that the red area represents treated municipalities and the

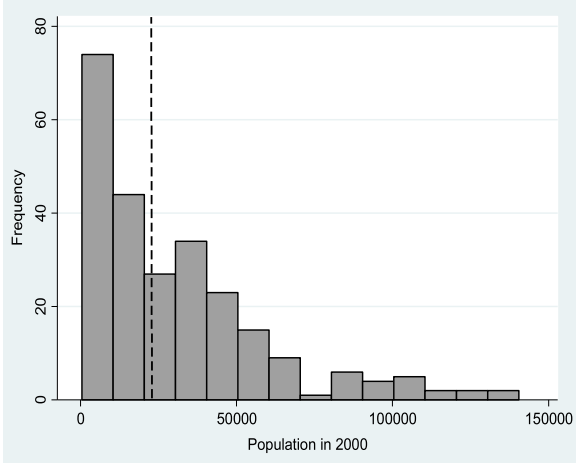

Fig. 2 The distribution of population in 2000. *Dash line* is median (21,927). Width of bins is 10,000. Data from Japan's population census in 2000

blue area represents control municipalities. Figure 3 indicates the number of programs launched by year. Table 1 shows descriptive statistics of the pretreatment sample, and Table 2 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 2 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. For population inflow and outflow, this paper uses data from the Basic Resident Registration, for which mayors are responsible.

Figure 4 shows the changes in outcomes for the treatment group and control group. The solid line indicates the treatment group, and dotted lines indicate its confidence intervals. The dash line indicates the control group, and gray dash lines indicate its confidence intervals. Figure 4a 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 1990 to 2005. The confidence intervals of treatment and control groups overlap. However, after starting the program, the treatment group's decreasing trend is slowed, while the control group's trend becomes more rapid. The confidence intervals of the two groups do not overlap. Figure 4b shows the normalized logged numbers of workers who *work in* the treatment/control group. We find the same evidence here. Figure 4c, d shows 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 level of normalized logged number of households of the control group is higher until 2005, and both groups increase during the years 1995 to 2005, but the number stagnates in the control group and increases slightly in the treatment group after 2005.

Figure 4e–h indicates 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, while the levels of the control group are higher in those sectors until 2005. The manufacturing and financial sectors show

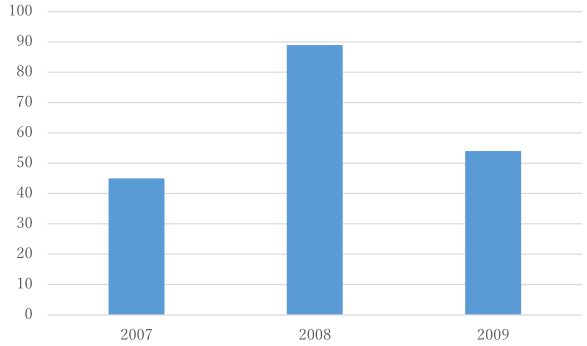

Fig. 3 The number of programs launched each year

Table 1 Descriptive statistics of pre-treatment sample (levels in 2005)

Variable	Treatment group				Control group			
	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
Households	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			

Means, standard deviations, maximum values, and minimum values using population census in 2005. 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 municipalities
[b]Workers who work in the treatment/control municipalities

similar trends in the treatment and control groups after starting the program. There are no data about wages or housing costs at the municipality level in the treatment and control groups. However, instead of wages, this paper discusses sales volume in Section 4. In addition, housing costs might not increase dramatically because the population decreases at the national level, and the treatment and control groups are in rural areas.

Table 2 Descriptive statistics—differences between groups

Variable	Treatment group		Control group		*p* value of difference[a]
	Mean	Std. dev.	Mean	Std. dev.	
Level in 2005					
Manufacturing share[b]	13.16	6.20	15.92	7.80	0.003
Government service share[c]	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[d]	314.81	613.89	223.02	396.94	0.221
Trend over the 2000–2005 period					
Manufacturing share[b]	−9.89	20.53	−11.92	12.32	0.412
Government service share[c]	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[d]	−17.41	26.20	−27.73	33.34	0.008
Observations	167		81		

Means, standard deviations, and *p* values using population census in 2000 and 2005
[a]*p* value of difference presents *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
[b]Manufacturing share is a share of manufacturing among the total number of workers
[c]Government service share is a share of government services among the total number of workers
[d]Population density is population per square kilometers

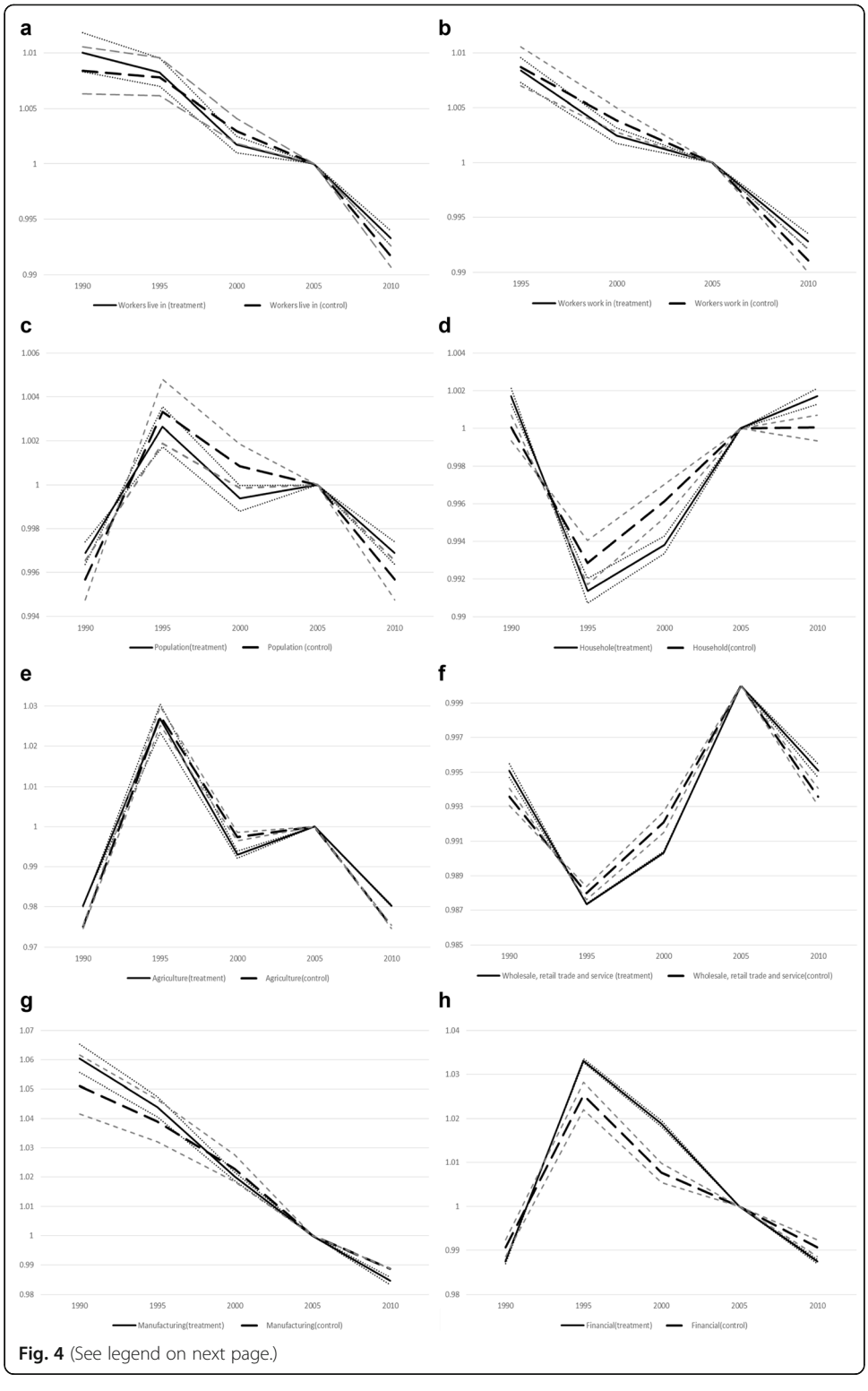

Fig. 4 (See legend on next page.)

(See figure on previous page.)
Fig. 4 **a** Workers who live in the treatment/control group. **b** Workers who work in the treatment/control group. **c** Population. **d** Households. Workers in **e** Agriculture. **f** Wholesale, retail trade, and service. **g** Manufacturing. **h** Financial. Depicts means of logged numbers of the listed variables in treatment and control municipalities. The means are normalized by the values in 2005. The *solid line* indicates the treatment group, and *dotted lines* indicate its confidence intervals. The *dash line* indicates the control group, and *gray dash lines* indicate its confidence intervals

3.3 Results

Table 3 shows that the coefficients of the cross-term between the time dummy and treatment group dummy are significantly positive in the first and second rows.[8] The place-based job creation program increases the number of local jobs by approximately 5%. The effect on workers who *work in* the treated municipalities is slightly greater than the effect on workers who *live in* the treated municipalities, 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 show the estimation results by sector. As mentioned in Section 2, most municipalities use this program in the agricultural and tourism sectors. 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 affects the agricultural sector most positively. The program increases the number of 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 it is

Table 3 Incidence of the programs

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

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 logged numbers of workers. Standard errors are clustered by municipality. Data from Japan's population census in 2000, 2005, and 2010

** and *indicate significance at the 5 and 10% level, respectively

[a]Workers who live in the treatment/control municipalities

[b]Workers who work in the treatment/control municipalities

[c]In the base year

significant in municipalities with a lower aging rate or larger working age population size, as shown later. 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. Employment in the wholesale, retail trade, and service sectors increases through job training in concierge services or information technology skills. By contrast, the program does not affect the manufacturing and financial sectors as predicted. The coefficients are insignificant from column (1) to column (3). 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 municipalities simultaneously use municipal budgets to promote local job creation, and many municipalities also use subsidies provided by other ministries. In fact, when they apply this program, municipalities are recommended to design their programs in relation to their industry promotion policies and related policies for local regeneration by other ministries. Therefore, they can use their budgets for their industry promotion policy, and they can, in most cases, obtain subsidies from other ministries for related local regeneration policies.[9] Note, however, that the municipalities in the control group also generally use their municipal budgets to promote local job creation, and they obtain subsidies provided by other ministries.

Table 4 shows estimation results regarding mobility. This paper examines whether migration occurs because of increasing jobs in the treatment group. In column (1) and column (2), the results indicate that the program increases the population and number of households in the treatment group. Furthermore, regarding whether these increases

Table 4 Incidence of the programs about population, the number of household, and the mobility

Dependent variable	(1)		(2)		(3)	
	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
Households	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	

Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row.
Dependent variables are logged population or numbers of households. Standard errors are clustered by municipality.
Data from Japan's population census in 2000, 2005, and 2010
** and *indicate significance at the 5 and 10% level, respectively
[a]In the base year

are the results of migration, the results for population inflow in the third row are significant, while the results for population outflow in the fourth row are insignificant. The place-based job creation program induces population inflow and, moreover, net inflow.

Next, this paper considers which municipalities the program affects most. Table 5 shows that the program has more effects in the municipalities with small aging populations or large working age populations. The upper part of Table 5 indicates that municipalities with an elderly population (older than 65 years old) of less than 32% generate more jobs for workers who live in the municipalities in the treatment group; workers who work in the municipalities in the treatment group; workers in the agriculture sector; and workers in the wholesale, retail trade, and service sector. Moreover, the coefficients of the cross-term between the time dummy and treatment group dummy in municipalities with large elderly populations are insignificant. Remarkably, the magnitude of coefficients is larger for workers in agricultural sectors in municipalities with smaller elderly populations than in all municipalities.

The middle part of Table 5 displays results for municipalities divided by the share of working-age population, instead of the aging population. The magnitudes of the programs' effects are significantly larger in municipalities with large working age populations than in that of the whole sample. If there are fewer residents of working age, the programs' effect is insignificant. Even if commuting workers are included, the result is insignificant. Thus, the insignificant result does not come from a shortage of labor supply. These results indicate that a small working age population has negative impacts, such as a lack of energy among locals. The lower part of Table 5 indicates the results from dividing municipalities by population density. This paper predicts that the program affects municipalities with lower population densities less than those with higher population densities. The results are almost the same as predicted.[10]

4 Externality of the program

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 municipalities do not take consumer demand away from neighboring municipalities. This paper analyzes whether the logged number of workers who live in neighboring municipalities and the logged number of workers who work in neighboring municipalities decrease through place-based job creation programs using the difference-in-difference method. This paper 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 municipalities 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 municipalities in 2005. 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

Table 5 The efficient use of programs

Dependent variable	Workers live in [a]				Workers work in [b]				Workers in agriculture				Workers in wholesale, retail trade, and service			
Municipalities divided by		Robust		Robust		Robust		Robust		Robust		Robust		Robust		Robust
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
	The proportion of people older than 65 years old is more than 32% of the total population in 2000 or otherwise															
Less than or equal to 32%	0.055**	0.027	0.047*	0.027	0.053**	0.025	0.045*	0.024	0.129**	0.058	0.128**	0.058	0.047**	0.023	0.039*	0.023
More than 32%	0.094	0.120	0.027	0.070	0.109	0.115	0.040	0.062	0.020	0.187	−0.044	0.157	0.082	0.125	0.007	0.072
	The working age population is less than 58.5% of the total population in 2000 or otherwise															
More than or equal to 58.5%	0.090**	0.044	0.096**	0.041	0.089**	0.040	0.094**	0.037	0.160*	0.087	0.166*	0.088	0.071*	0.037	0.074**	0.035
Less than 58.5%	0.055	0.069	0.001	0.045	0.055	0.067	0.008	0.043	0.098	0.087	0.055	0.080	0.052	0.070	0.003	0.044
	The population per square kilometer is more than 176.65 and less than 439.825 or otherwise															
The third quartile	0.073*	0.038	0.054	0.033	0.075**	0.036	0.056*	0.031	0.134**	0.064	0.129**	0.061	0.065*	0.036	0.044	0.032
Otherwise	0.029	0.034	0.024	0.033	0.023	0.030	0.020	0.029	0.076	0.075	0.074	0.075	0.023	0.027	0.020	0.026
Control variables																
Manufacturing share (lag)	No		Yes		No		Yes		No		Yes		No		Yes	
Trend in population density	No		Yes		No		Yes		No		Yes		No		Yes	
Unemployment rate [c]	No		Yes		No		Yes		No		Yes		No		Yes	

** and *indicate significance at the 5 and 10% level, respectively

Observations are 653, 89, 482, 260, 556, and 186 beginning at the first row. Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row. Dependent variables are logged numbers of workers. Standard errors are clustered by municipality. Data from Japan's population census in 2000, 2005, and 2010

[a]Workers who live in the treatment/control municipalities

[b]Workers who work in the treatment/control municipalities

[c]In the base year

public transportation is more common than driving in Japan. Thus, commuting zones are not concentric circles in Japan. This paper uses the following model:

$$Y_{it} = \delta_0 + \delta_1 d_t + \delta_2 \mathrm{TC}_i + \delta_3 \mathrm{Ntre}_i + \delta_4 \mathrm{Ncon}_i + \delta_5 d_t \mathrm{TC}_i + \delta_6 d_t \mathrm{Ntre}_i + \delta_7 d_t \mathrm{Ncon}_i + \delta_8 X_{it} + \varepsilon_{it}, \quad (2)$$

where Y_{it} is the logged number of workers who live in the targeted municipalities and the logged number of workers who work in the targeted municipalities, 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. Ntre_i equals 1 if city i is a neighboring municipality of the treatment group, and Ncon_i equals 1 if city i is a neighboring municipality of the control group. Therefore, the control group is the baseline. This paper confirms the positive effects on the treatment municipalities and predicts a negative effect on neighboring municipalities of the treatment group compared with the control municipalities if the program takes the consumption of neighboring municipalities away. Moreover, the coefficient of neighboring municipalities of the control group is insignificant in theory.

Data are obtained from the census of commerce in 2002, 2007, and 2012.[11] 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 municipalities and the number of workers who work in the targeted municipalities from the population census, as mentioned in Section 3.2. This paper estimates Eq. (2) using a block bootstrap method. The prefecture level is used as a block.

4.2 Estimation results

The first row in Table 6 indicates that the cross-terms between a treatment group dummy and a time dummy are significantly positive. The programs increase the number of workers who live in the treatment group, the number of workers who work in the treatment group, and the general retail sales in the treatment group, compared the control group. However, there is no difference between neighboring municipalities of the treatment and control groups, neighboring municipalities 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. This paper does not clearly observe the program's destruction of neighboring municipalities' demand.

5 Robustness check

Finally, as a robustness check, this paper uses 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_t, equals 1 if the data are from 2005 and is otherwise 0. 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 7 shows the estimation results. All the results indicate an insignificant effect. The placebo event does not differently affect the workers who live in or work in the targeted municipalities 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

Table 6 The effect on the neighboring municipalities

	(1)		(2)	
	Observed Coef.	Bootstrap Std. err.	Observed Coef.	Bootstrap Std. err.
All sectors				
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 sector				
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 sector				
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

Table 6 The effect on the neighboring municipalities *(Continued)*

Control variables		
Manufacturing share (lag)	No	Yes
Trend in population density	No	Yes
Observations[a]	974	974

***, **, and * indicate significance 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. *dTC* time dummy × treatment dummy, *dNtre* time dummy × neighbor of treatment group dummy, *dNcon* time dummy × neighbor of control group dummy. Dependent variables are logged numbers of workers, sales, or numbers of establishments. Data from the population census in 2000, 2005, and 2010 for all sectors and from the census of commerce in 2002, 2007, and 2012 (to be more precise, the economic census in 2012. It covers the census of commerce) for a general retail trade sector and food and beverage retail trade sector. 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 columns (1) and (2), respectively

does not affect the population or the number of households in the treatment group, as shown in the lower part of Table 7.

6 Conclusion

This paper has three purposes. First, empirical evaluations of job creation have rarely been conducted in Japan. The results show that the program increases the number of workers, especially workers commuting to the municipalities 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 municipalities conduct trial businesses or seminars via these programs.

Table 7 Placebo test

Dependent variable	(1)		(2)		(3)	
		Robust		Robust		Robust
	Coef.	Std. err.	Coef.	Std. err.	Coef.	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
Households	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			

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 logged numbers of workers, population, or households. Standard errors are clustered by municipality. Data from population census in 1995, 2000, and 2005. This paper assumes that the placebo event occurs in 2005 and confirms that the outcomes of the treatment group do not differ from those of the control group after 2005
***, **, and *indicate significance at the 1, 5, and 10% level, respectively
[a]Workers who live in the treatment/control municipalities
[b]Workers who work in the treatment/control municipalities
[c]In the base year

Intuitively, the joining together of the local firms, municipal government, members conducting this program, job placement offices, and job seekers, all owing to the launch of this program, is a key factor in increasing employment.

Second, this paper demonstrates that the program has lesser effects in municipalities 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. A small working age population has negative impacts on young people and local economic activity. A 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 occurs. Government officials only calculate the number of workers generated in the treated municipalities, but considering the total effect is important for policy evaluation. Many municipalities conduct seminars on how to attract people to purchase local specialty products. However, this paper does not observe a clear decrease in sales or in the number of workers in neighboring municipalities 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 municipalities are recommended to design the program in relation to their industry promotion policies and related policies for local regeneration by other ministries, although the municipalities in the control group also use these subsidies. To expand this paper, it must be determined whether the program reduces unemployment benefits in the treated municipalities. However, this program also induces the participation of the potentially unemployed, such as female workers (sometimes from farming families).

Endnotes

[1]*Chiiki koyo suishin jigyo* in Japanese

[2]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. This paper cannot obtain the exact information. However, for example, city *A* conducts this program from the last half year in 2014 to 2016 (this paper does not cover the program from 2014). The amount of subsidy for city *A* was approximately 300,000 dollars for half a year in 2014 and 830,000 dollars in 2015. In 2014, city *A* had a budget for local industry promotion policies totaling 50,000 dollars. This budget was used for the subsidy for the cost of renovation or rent of persons who open new shops using a local vacant building and tax exemption for firms who open new establishments or increase the number of establishments.

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

[4]This paper explains that this paper excludes the few municipalities with enormous populations (greater than the 90th percentile) in Section 3.2. Figure 1 excludes those municipalities (not painted red or blue).

[5]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 Busso, Gregory, and Kline (2013).

[6]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).

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

[8]As shown in Table 2, 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 the unemployment rate. Therefore, this paper controls for unemployment rate in the base year.

[9]See note 2.

[10]Sample sizes of municipalities with less elderly people, large working age population, and high population density are larger than those of municipalities with more elderly people, a small working age population, and lower population density. Therefore, this paper calculates the effect size that is not influenced by the sample size. As a result, the program surely affects municipalities with fewer elderly people. Additionally, this paper runs another estimation for robustness. This paper adds a cross-term of the time dummy, treatment dummy, and features of regions (i.e., elderly, smaller working age population, or less population density dummy) in Eq. (1) and estimates the entire sample. As a result, the elderly dummy and the lower working age population dummy are significantly negative for workers although this estimation is constrained by some coefficients being equal.

[11]To be more precise, data in 2012 from the economic census in 2012. The economic census covers the census of commerce.

Table 8 The estimation result using alternative methods

Dependent variable	(1)		(2)	
	Coef.	Robust Std. err.	Coef.	Robust Std. err.
Workers live in[a]	0.148*	0.076	0.044	0.028
Workers work in[b]	0.156*	0.081	0.045*	0.026
Workers in				
Agriculture	0.240**	0.095	0.111**	0.052
Wholesale, retail trade, and service	0.136*	0.080	0.037	0.026
Manufacturing	0.158	0.096	0.030	0.046
Financial	0.138	0.109	0.008	0.043
Population	0.144*	0.076	0.044	0.030
Households	0.135*	0.075	0.048*	0.026
Observations	678		742	

Dependent variables are the change in logged number of workers/people/households. Column (1): this paper estimates the model using the selected control municipalities that are close enough to the treatment municipalities. Column (2): this paper applies the inverse of probability weight. The estimation models for the column (1) and (2) includes all control variables (the lag share of manufacturing, the trend in population density and the unemployment rate in the base year)
** and * indicate significance at the 5 and 10% level, respectively
[a]Workers who live in the treatment/control municipalities
[b]Workers who work in the treatment/control municipalities

Appendix 1

This paper estimates the Japanese place-based program using alternative methods for double robustness. First, this paper selects the control municipalities using propensity matching score to use only control municipalities that are close enough to the treatment municipalities. Then, this paper estimates Eq. (1) using difference in difference method. The results shown in column (1) in Table 8 indicate that the effects of the program on the treatment municipalities compared with the selected control group are greater than the effects when this study uses all the control municipalities. Additionally, the coefficients of the effects on the workers in the wholesale, retail trade, and service sector, and the population are significant even if this paper uses all control variables although they are insignificant in Tables 3 and 4.

Second, this paper estimates Eq. (1) applying the inverse of probability weight because the number of control group is smaller than that of treatment group. The results shown in column (2) in Table 8 indicate the similar results to Tables 3 and 4. The coefficients are very slightly smaller than the coefficients in Tables 3 and 4. Even if this paper apply the inverse of probability weight, this paper finds that the Japanese place-based job creation program increases the number of workers who *work in* the treated municipalities, workers in the agricultural sector, and the number of households.

Appendix 2

To analyze the zero-sum game, this paper also estimates model (2) in Section 4.1: (1) municipalities in the treatment group versus neighboring municipalities of the treatment group ($Ncon_i$ and TC_i equal 0 in this case); (2) municipalities in the control group versus neighboring municipalities of the control group ($Ntre_i$ and TC_i equal 0 in this case); and (3) neighboring municipalities of the treatment group versus neighboring municipalities of the control group ($Ncon_i$ and TC_i equal 0 and the baseline consists of neighboring municipalities of control municipalities in this case). Regarding (1), if the program has a positive effect on the treatment group, it is possible that we observe a negative effect on neighboring municipalities of the treatment group, although the outcomes of the neighboring municipalities do not change. Regarding (2), this paper confirms no effect on the neighboring municipalities of the control group. Then, regarding (3), this paper predicts a negative effect on neighboring municipalities of the treatment group, compared to the neighboring municipalities of the control group, if the increase in consumer demand and the employment growth in the treatment group take consumer demand away from neighboring municipalities.

As a result, column (1) and column (2) in Table 9 show that the number of resident workers and commuting workers in neighboring municipalities of the treatment group does not decrease due to place-based job creation programs. The program decreases the number of workers and establishments in the general retail trade sector in neighboring municipalities 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 municipalities of the treatment group compared with the treatment group, although the outcomes of the neighboring municipalities do not change. Table 10 shows the results for this program's effects using the census of commerce to establish difference-in-differences between

Table 9 The effect on the neighboring municipalities; separate samples

	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.
All sectors												
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 sector												
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 sector												
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

Table 9 The effect on the neighboring municipalities; separate samples *(Continued)*

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

Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row. Columns (1) and (2) indicate the effects of Japanese place-based program on the neighboring municipalities of the treatment group compared with the treatment group. Columns (3) and (4) indicate the effects of the program on the neighboring municipalities of control group compared with the control group. Columns (5) and (6) indicate the effects of the program on the neighboring municipalities of treatment group compared with the neighboring municipalities of the control group. Dependent variables are logged number of workers, sales, or numbers of establishments. Data from the population census in 2000, 2005, and 2010 for all sectors and from the census of commerce in 2002, 2007, and 2012 (to be more precise, the economic census in 2012. It covers the census of commerce) for a general retail trade sector and food and beverage retail trade sector. Estimations use a block bootstrap. A prefecture level is used as a block

** and * indicate significance at the 5 and 10% level, respectively

[a]Workers who live in the treatment/control municipalities [b]Workers who work in the treatment/control municipalities [c]Observations for workers who live/work in the targeted municipalities 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 columns (1)–(6), respectively. Observations for sales in a food and beverage retail trade sector are 583, 583, 326, 326, 176, and 176 for columns (1)–(6), respectively

Table 10 Incidence of the program using census 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	

Each entry gives the difference-in-differences estimate of the program on the outcome presented in each row. The effects of the Japanese place-based program on the treated municipalities compared with the control municipalities. Dependent variables are logged sales, numbers of workers, or numbers of establishments. Standard errors are clustered by municipality. Data from the census of commerce in 2002, 2007, and 2012 (to be more precise, the economic census in 2012. It covers the census of commerce) for a general retail trade sector and food and beverage retail trade sector

* indicates significance at the 10% level

[a]Observations for sales in a general retail trade sector are 688 for columns (1) and (2). Observations for sales in a food and beverage retail trade sector are 731 for columns (1) and (2)

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) in Table 9 shows neighboring municipalities of the control group versus municipalities in the control group. As expected, the programs do not

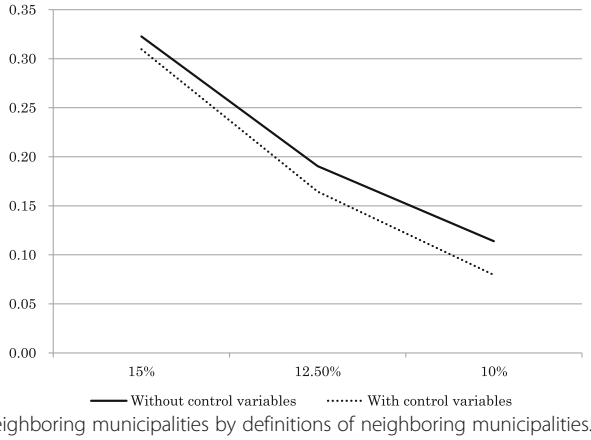

Fig. 5 Effects on neighboring municipalities by definitions of neighboring municipalities. Figure indicates the coefficients of the cross-term between the time dummy and the neighboring municipalities of the treatment group' dummy by definitions of neighboring municipalities. Outcomes are log of sales in general retail trade when this paper compares neighboring municipalities of the treatment group versus neighboring municipalities 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 municipalities in 2005. Data from the census of commerce in 2002, 2007, and 2012 (to be more precise, the economic census in 2012. It covers the census of commerce)

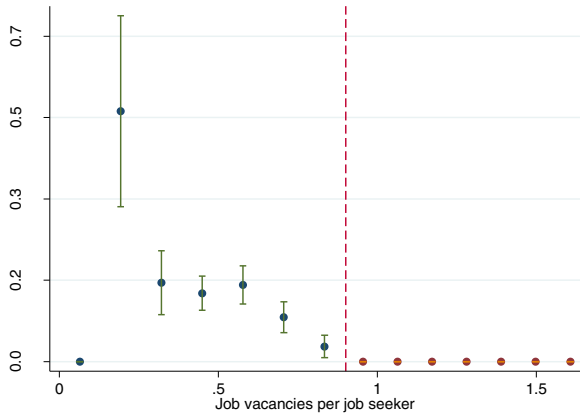

Fig. 6 The fraction of municipalities that conducted the place-based job creation program. *Blue* and *red points* are sample average within bin. *Lines passing through blue points* are the confidence interval. *Red vertical dash line* is the job vacancies per job seeker threshold to apply the program

decrease the resident workers, commuting workers, sales, workers, or establishments in general retail trade and food and beverage retail trade in the neighboring municipalities of the control group. Although the program has a significantly negative effect on resident workers and sales in the food and beverage retail trade sector in neighboring municipalities of the control group, this effect is insignificant if we control for the industrial structure and the trend in population density. Instead of comparing neighboring municipalities with the treatment group, this paper compares the neighboring municipalities of the treatment group with the neighboring municipalities of the control group because there is a possibility of bias in columns (1) and (2). The effects of the programs on neighboring municipalities of the treatment group and neighboring municipalities 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 municipalities' dummy by the definitions of the neighboring municipalities. 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 municipalities in 2005. The magnitudes of coefficients decrease with the expansion of the ranges of neighboring municipalities, although the coefficients are insignificant.

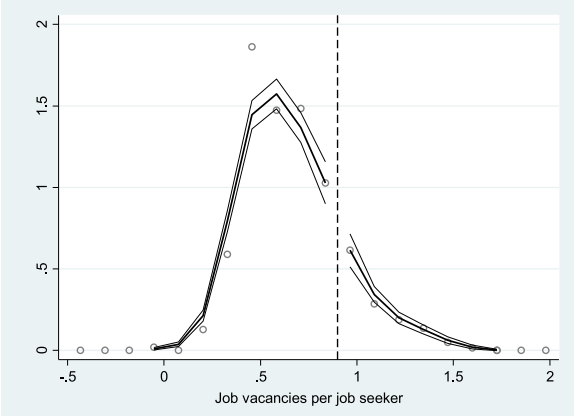

Fig. 7 McCrary test. The *vertical dash line* is the job vacancies per job seeker threshold to apply the program

Table 11 The result of MacCrary test

	Coef.	Std. err.
	−0.025	0.173
Observation	1602	

The null hypothesis of McCrary test is continuity. Data from regional data at the municipal level of job vacancies per job seeker provided by the Ministry of Health, Labour and Welfare
***, **, and * indicate significance at the 1, 5, and 10% level, respectively

Appendix 3 Online appendix

This paper examines the effects of this job creation program using a regression discontinuity design for robustness. Instead of using the control group described in Section 3.1, this paper compares the treated municipalities with municipalities that have job vacancies per job seeker that are only slightly above the national average. As shown in Section 2, only municipalities that have less than the national average level of job vacancies per job seeker can use this job creation program's subsidy. Municipalities that have slightly more than the national average level of job vacancies per job seeker are similar municipalities to the treated municipalities and are randomly assigned.

Figure 6 shows the fraction of municipalities that conducted this place-based job creation program around the job vacancies per job seeker threshold. Regional data at the municipal 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 municipalities. The fraction of municipalities 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. The programs started in multiple years (2007 to 2009), but this paper uses data from a single year, 2008, for a single threshold. Additionally, common data on job vacancies per job seeker are used in the multiple municipalities administered by the same job placement agency. These factors demonstrate why this paper does not use a regression discontinuity design for the main analysis.

This paper confirms the continuity assumption using the McCrary test before using regression discontinuity design. Figure 7 indicates that the density function

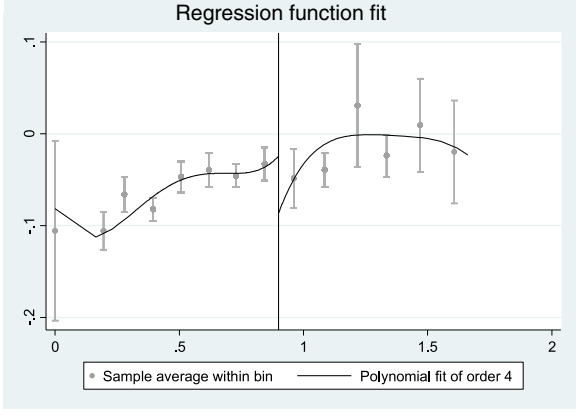

Fig. 8 Regression discontinuity design. The change in log number of employment from 2005 to 2010 in municipalities at the job vacancies per job seeker eligibility threshold. Sample includes 1602 municipalities. Bin size = 0.123

Table 12 The result using a regression discontinuity design

	Coef.	Std. err.
	−0.081***	0.023
Observation	1602	

***indicates significance at the 1% level
Each entry gives the regression discontinuity design estimate of the program on the change in log number of employment from 2005 to 2010. Data from Japan's population census. Regional data at the municipal level of job vacancies per job seeker are provided by the Ministry of Health, Labour and Welfare

of the job vacancies per job seeker is smooth. Table 11 also shows that a t test fails to reject the null hypothesis of continuity. As for results, Figure 8 shows that municipalities below the cutoff point increase in employment from 2005 to 2010, compared to municipalities immediately above the cutoff point. Table 12 displays estimation results by a regression discontinuity design. I selected the optimal bandwidth using the Calonico et al. (2016) framework. The change in log number of employment from 2005 to 2010 in municipalities that are above the threshold is approximately 8% smaller than the change in municipalities that are below the threshold (i.e., municipalities that are eligible for this program). The magnitude is slightly larger than the main results in Table 3. Therefore, although this paper compares the treatment of municipalities with similar municipalities regarding job vacancies per job seeker, this place-based job creation program increases employment in the treated municipalities.

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The IZA Journal of Labor Policy is committed to the IZA Guiding Principles of Research Integrity. The author declares that she has observed these principles.

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