Local Employment Multiplier: Evidence from Relocation of Public-Sector Entities in South Korea*

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Abstract

We exploit a series of public-sector entity relocations in South Korea as an exogenous source of variation in public sector employment to estimate the local employment multiplier. We find that the introduction of one public sector employment position increases private sector employment by one unit, primarily driven by the service sector. Consistent with existing literature, we document that the effect of public employment on private employment is highly localized. In addition to changes in private employment, we also discover that the relocations led to a positive net influx of residents into the treated neighborhoods; this effect is also localized. Lastly, by estimating the local employment multiplier for each relocation site, we document the heterogeneity of the local employment multiplier and provide suggestive evidence that this heterogeneity is shaped by the local economic environment's capacity to accommodate additional general equilibrium responses.

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1 Introduction

The public sector constituted, on average, about 41% of GDP across OECD countries in 2019 (OECD, 2021). Undoubtedly, it generates many socioeconomic benefits as well as costs. There is a longstanding debate on whether public investments and projects serve as expansionary policy tools to stimulate overall economic activity (Blinder and Solow, 1973; Friedman, 1978; Buiter, 1977; Aschauer, 1989). An increase in public-sector employment may have positive effects on private employment. However, it may also have negative effects on the economy by crowding out private employment. Furthermore, increased public-sector employment may lead to changes in industry composition, thus sparking structural transformation. Reflecting the important roles the public sector plays in shaping the distribution of economic activity and economic development, the labor and urban economics literature have estimated local employment multipliers from different types of public shocks (Auricchio et al., 2019, 2020; Becker et al., 2021; Jofre-Monseny et al., 2020; Faggio, 2019; Faggio et al., 2022). Previous literature has focused on a single event of a public-sector shock to identify local employment multipliers. There is still room for improving identification and exploring economic factors and forces that interact with the effects of public employment on private employment.

In this paper, we estimate the local employment multiplier of public employment, leveraging both temporal and spatial variation in a series of public entities relocating across South Korea away from Seoul, its capital city. To enable causal analysis, we provide evidence supporting the quasi-random nature of the aforementioned variation. First, we employ an event study model and semi-parametrically estimate the changes in private employment before and after the relocations that took place in various years spanning from 2011 to 2017. Refining the model, we further explore whether the effect of the public employment shock permeated across space. Second, we use a treatment intensity framework that extends the standard difference-in-difference approach to estimate the local employment multiplier. Lastly, we exploit the unique opportunity that our empirical setting provides to estimate the local employment multiplier specific to each relocation and explore the factors that explain the heterogeneity of the local employment multiplier.

The empirical setting of this paper is South Korea. The primary reason for focusing on South Korea to estimate the local employment multiplier and its heterogeneity is the series of relocations of public entities that occurred as part of the national government's efforts to achieve equitable and balanced growth across the nation. Two features make South Korea particularly suitable for this study. First, we demonstrate that pre-relocation (pre-treatment) baseline characteristics lack the power to predict the destination neighborhoods to which public entities moved. Therefore, we argue that the treated neighborhoods and the control neighborhoods were similar prior to any relocations. We provide additional evidence to support our identification strategy. Second, there are several dimensions that differentiate each relocation from one another, including the timing of relocation, destination neighborhood, size of relocation, and the types of relocation. We leverage these dimensions to understand how they shaped the heterogeneity of the local employment multiplier.

Based on our event study estimation results, we draw the following conclusions: First, before the relocation, the differences in total private employment between the treated neighborhoods and their neighboring areas (i.e., the control group) were economically negligible and not statistically significantly different from zero at the 5 percent significance level. Second, private employment in the treated neighborhoods began to differ from that in the untreated neighborhoods starting from the first year of treatment, and this divergence continued after the first wave of relocations. This pattern may capture both the dynamic effect of relocation that manifests over time and the effect of subsequent relocations after the initial one.

For both the manufacturing and services sectors, the estimates of event study coefficients before the relocation show no pre-trend: employment levels in both sectors in the neighborhoods of the treated and control groups are not statistically different from each other. On one hand, this pattern continues after the treatment for the manufacturing sector. While the share of employment in the manufacturing sector is smaller on average compared to that in the service sector, the manufacturing sector constitutes an economically significant part of the local labor market in both the treated and control neighborhoods. Therefore, this result is not mechanically driven by neighborhoods starting off with little employment in the manufacturing sector, making it challenging to observe any changes. On the other hand, there is a clear structural break in employment in the service sector upon treatment. The estimated event study coefficients for the service sector almost completely explain the changes in total employment.

We employ a complementary difference-in-difference estimator to estimate the average effect of the shock, summarizing the estimated event study coefficients after the relocation: the relocation of public entities resulted in an increase in private employment by 1,330. The results for the service sector are both qualitatively and quantitatively similar to those of total private employment. This similarity is expected, given the event study results where we demonstrated that the changes in the service sector explain most of the changes in total private employment. However, for the manufacturing sector, the overall results indicate that the relocation did not significantly change employment in an economic or statistical sense.

To interpret these results as causal evidence of an increase in private employment due to a public sector shock, one of the necessary assumptions is that there are no external spatial spillovers. Our analysis aligns with existing literature and confirms that spillover effects diminish rapidly across space. Based on this evidence, we conclude that the relocation of public entities had a positive causal effect on private employment, and this effect is highly localized. Lastly, we utilize the treatment intensity model to directly estimate the average local employment multiplier. We find that private employment increased by 0.99 for each unit increase in public employment from 2010 to 2018. This result indicates an almost one-to-one correspondence between public employment and private employment.

Furthermore, we examine how people's migration decisions responded to the public sector shocks introduced across the country. We observe that there was a net inflow of migrants into the treated neighborhoods. On one hand, the relocations had the most significant impact on internal migration within the same metropolitan cities, particularly funneling people into these treated neighborhoods. On the other hand, individuals from various cities, including the SMA, moved to the treated neighborhoods more than they did to nearby areas. These findings on spatial mobility suggest that the treated neighborhoods became more desirable places to live, potentially due to improved quality of life, higher amenities, and increased employment and income opportunities. We provide additional evidence to support that the estimated local employment multiplier is not solely driven by the relocation of public employees due to the treatment and their household members.

For each destination neighborhood separately, we estimate local employment multipliers. Our results yield the following implications: Local employment multipliers are positive for relocations that occurred in locations with existing infrastructure networks and relatively well-functioning housing and labor markets, as well as in areas where the relocation was accompanied by significant housing supply shocks and infrastructure construction (e.g., the creation of Innovative Cities). Together with our findings on migratory responses, it suggests that the extent of the local employment multiplier may depend on how well the local economic environment can accommodate additional general equilibrium responses, such as migration, endogenous amenities, agglomeration, and job opportunities. In summary, we provide suggestive evidence that local employment multipliers are heterogeneous, and their distribution is shaped by the local economic environment.

We provide a series of robustness checks. First, we offer evidence supporting the quasi-random nature of the public employment shocks we exploit to identify the local employment multiplier in this paper. We demonstrate that the selection of destination neighborhoods to which public entities relocated cannot be predicted by pre-treatment neighborhood characteristics. Second, we estimate the relocation effect on private employment and the local employment multiplier under alternative sample restrictions. Our conclusion is that the main results are not driven by sample selection. Third, we address a potential inference issue arising from the small number of treated observations (Ferman and Pinto, 2019) by implementing a variant of Fisher's permutation or randomization (Fisher, 1935). Fourth, we check the robustness of our results to potential bias due to the staggered introduction of shocks, following recent advancements in the two-way fixed effects literature (Goodman-Bacon, 2018; de Chaisemartin and D'Haultfœuille, 2020). Lastly, we conduct a pair of placebo tests. The first placebo test uses pre-treatment changes in private employment as the dependent variable in the treatment intensity model. In the second placebo test, inspired by Greenstone et al. (2010), we estimate the effect of relocation on private employment using the runner-up neighborhoods as treated and their nearby areas as control. These runner-up neighborhoods were finalists as potential relocation sites but were not ultimately chosen. The results of both placebo tests reinforce our main findings.

We contribute to the labor, public, and urban economics literature by estimating employment multiplier effects in four ways. First, we provide a new estimate based on a quasi-natural experiment in South Korea. Most previous research has focused on a single relocation event (or a one-time public-sector shock) in countries like England(Faggio and Overman, 2014; Faggio, 2019), Italy (Auricchio et al., 2019), and Germany (Becker et al., 2021; Faggio et al., 2022), among others. Compared to the previous literature, the case of South Korea—the empirical setting of this paper—provides a rare opportunity to estimate employment multipliers from increases in public-sector employment with rich spatial and temporal variation. Second, we document the heterogeneity of the local employment multiplier and discuss some of the economic factors that shape this heterogeneity. To the best of our knowledge, our paper is the first to discuss this heterogeneity within a single empirical context and explore its potential local determinants. Third, we find that the local employment multiplier is highly localized, and the effect of an increase in public employment does not seem to spill over across space; if it does, the rate of its spatial decay is high. This result aligns with previous literature estimating the spatial spillovers of local productivity and amenities (Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008; Rosenthal and Strange, 2008; Andersson et al., 2009; Ahlfeldt et al., 2015). Together with Faggio (2019), this paper is one of the first to study the spatial spillover effects of public sector employment. Lastly, our paper contributes to the discussion on place-based policies (Glaeser and Maré, 2001; Neumark and Kolko, 2010; Greenstone et al., 2010; Busso et al., 2013; Moretti and Wilson, 2017; Gaubert et al., 2020) by studying the local economic forces that interact with the effect of public employment on private employment.

The rest of this paper is organized as follows: Section 2 provides key details of the institutional background and an episode of the relocation of public entities in South Korea, the empirical setting of this paper, and explains the primary data sources. In Section 3, we present the empirical strategy to estimate the causal effect of relocating public entities on private employment and the identifying assumptions. In Sections 4 and 5, we discuss our main results and robustness checks. Section 6 concludes.

2 Institutional Background and Data

To estimate the local employment multiplier of the public sector, we constructed a geocoded annual panel dataset comprising 1,069 neighborhoods from 2006 to 2018. This dataset captures local employment separately for the public sector and the private sectors (i.e., manufacturing and service) along with local characteristics. In this section, we provide details about the institutional background of the empirical setting of this paper, South Korea, and the series of relocations of public-sector entities. These entities were originally concentrated in the Seoul Metropolitan Area (SMA) but began moving away from the SMA starting in 2011. Subsequently, we discuss the data sources, sample selection, and provide descriptive statistics.

2.1 Institutional Background

In 2003, the national government of South Korea announced its preliminary plan to disperse public-sector entities, which had previously been heavily concentrated in the SMA.[1] The primary

[1] The SMA includes the metropolitan city of Seoul and two adjacent municipalities, namely the province of Gyeonggi and the metropolitan city of Incheon. In South Korea, metropolitan cities and provinces serve as second-tier governing bodies, each further subdivided into districts and counties (referred to as 'si,' 'gun,' and 'gu' in Korean), and then further divided into neighborhoods ('eup,' 'myeon,' and 'dong' in Korean). The most granular administrative

motivation behind this policy agenda was to promote equitable growth across the nation, and in 2004, the national government laid the legal groundwork by enacting the Special Act on Balanced National Development. In addition to the relocation of public entities, this Special Act also outlined development plans for 10 Innovative Cities in each of the 10 metropolitan cities and provinces, excluding the SMA, Daejeon, and the province of South Chungcheong. In the following year, the national government proposed a list of 345 agencies and entities for relocation and established a committee at the Ministry of Construction and Transportation to assess the feasibility and prospects of their relocation. By June 2005, the committee had narrowed down the list to include 175 public-sector entities for relocation. While the details of the selection process and the reasons for selecting each entity are classified, the Special Act on Balanced National Development guided the selection process. For instance, the committee may have excluded public entities if the costs of their relocation significantly outweighed the expected benefits, if their nature closely resembled that of the private sector, or if they had an intrinsic and historical association with their original locations in the SMA.

Destinations for the selected public entities were determined in two steps. After selecting the public entities for relocation, the committee further specified the metropolitan cities and provinces where the selected entities would be relocated, taking into account regional economic conditions, existing and planned infrastructure, and the potential economic impacts of relocation. Subsequently, the metropolitan cities and provinces had their independent committees to determine host neighborhoods for these public entities. Similarly, the metropolitan cities and provinces had the authority to select sites for constructing the Innovative Cities, often resulting in the creation of new neighborhoods by splitting existing ones. To guide their selection process and to align with the goal of achieving balanced growth nationwide, the national government provided a general rubric for local governments to follow when assessing potential sites. This rubric emphasized growth potentials for the broader regions as well as host neighborhoods, along with considerations of developmental feasibility and suitability. The first wave of relocation took place in 2011. The relocation, along with the creation of Innovative Cities across the nation, constituted major national projects that had cost approximately USD 10 billion by 2015, according to the White Paper on the Relocation of Public Entities and the Creation of Innovative Cities. By the end of 2018, 128 public entities had completed their relocation, introducing over 52,808 public-sector employees to neighborhoods in different parts of the nation."

The aforementioned relocation of public entities differs in four important dimensions. The first two dimensions are its timing and destination locations. We leverage this spatial and temporal variation to identify the effect of public-sector relocation on private employment, thereby estimating the local employment multiplier. As explained above, the destination locations are selected after a rigorous review process. Predicting the exact timing of when relocation takes place is challenging, as relocating public entities consider a multitude of factors, including local infrastructure and the

unit, the neighborhood, serves as the spatial unit for this paper. In total, there are 3,357 neighborhoods.There are 3,357 neighborhoods in total.

extent of relocation itself.

The third dimension arises from the different types of relocation. We categorize the relocation of public entities into three types. First, public entities were relocated to neighborhoods that already had supporting infrastructure and relatively well-functioning central business districts and were not part of new Innovative Cities; we refer to this case as "Relocation to Old Towns." Second, public entities moved into neighborhoods located in one of the new Innovative Cities: "Relocation to New Towns." For these first two types of relocation, the selected neighborhoods received clusters of public entities. The last type, which we call "Single Entity Relocation," includes cases in which an individual public entity moved to a destination neighborhood alone, not as a cluster of multiple entities. Compared to the first two types, the last type of relocation resulted in a relatively small injection of public employment into the host neighborhoods. The fourth dimension is captured by the number of public jobs introduced to host neighborhoods and the local economic characteristics of these neighborhoods.

2.2 Data

Data Sources

The main outcome variable of interest in this paper is local private employment. We construct annual panel data of neighborhoods from 2006 to 2018 based on the administrative records of the Census on Establishments, maintained by Statistics Korea. The Census on Establishments surveys the universe of establishments in South Korea and collects establishment-level information on the number of employees as well as the industry code. Based on this information, we count the number of employees in the private sector by neighborhoods in each year. We also construct the same dataset focusing on establishments in the manufacturing and service sectors separately.

There are two challenges associated with building this dataset. First, the industry classification changed multiple times during the sample period. We ensure that we use a consistent definition of industries by tracing the changes and creating a mapping across the sample period. Second, some neighborhoods experienced changes in their boundaries by splitting into multiple neighborhoods or merging with other neighborhoods. To address this issue, we consider each group of neighborhoods that split and the neighborhoods that merged as a single neighborhood throughout our analysis.

The spatial and temporal variation in the relocation of public entities may help us identify the effect of public-sector employment on the changes in private employment. However, this is not enough to estimate the value of the local employment multiplier, which has a unit equal to the number of private-sector employment per 1 public-sector employment. For this, we need to know the number of public employment that each relocation brought to its host neighborhood. We access an online platform run by the Ministry of Economy and Finance, called All Public Information in One (ALIO), from which we collect information on the number of employees in public entities. However, a public entity can have multiple offices, so the total number of employees in ALIO may not reflect the actual number of workers in a relocated headquarters. Thus, we collect information on the actual number of workers who relocated for each public entity by manually requesting this information from each entity. We further cross-validate and improve the data collected from ALIO with the responses from all the entities that relocated. Finally, by geocoding the location of public entities, we create an annual panel dataset of neighborhoods with the number of public employment from 2006 to 2018 and merge this dataset with the data on private employment.

Lastly, we augment the dataset with local characteristics at the neighborhood level (Korean Statistical Services) and measure the distance between the centroid of each neighborhood and the nearest neighborhood to which a public entity relocated. We primarily use the local characteristics to control for differences in baseline characteristics when estimating the effects of public-sector relocation on private employment. To support the robustness of our results, we test whether baseline characteristics determine where public entities moved. Given the granularity of our spatial unit, we investigate potential spatial spillover effects. To do so, we need to know how close neighborhoods are located to destination neighborhoods. We compute centroid-to-centroid bilateral distances between all pairs of neighborhoods using the Statistical Geographic Information System (SGIS). In particular, we create a set of dummy variables indicating whether a neighborhood is located within a certain distance from neighborhoods that received public entities.

Sample Selection

In line with the objective of this paper, we follow a set of rules for selecting the sample of neighborhoods for our analysis. First, we exclude neighborhoods located in the SMA, where the public entities had been previously located prior to relocation. Some of these neighborhoods experienced the loss of public employment; therefore, including them in our analysis may bias the estimated effect of public employment on private employment.[2] Second, we focus on the neighborhoods that are located within 30 kilometers from the nearest neighborhoods that received public entities. Assuming no spatial spillover arising from the relocation, the neighborhoods near the treated neighborhoods where public entities moved would be similar enough to the treated areas and serve as a control group. The excluded neighborhoods based on this criterion are often remote and may differ substantially in their baseline characteristics. For this reason, it is not reasonable to assume that their outcomes would have been similar to those of the treated neighborhoods in the absence of the relocations. We believe that neighborhoods that are located too distant away from the treated areas are unfit for our analysis. Third, we exclude the selected destination neighborhoods and their neighboring areas if the relocation coincided in the area with other developmental plans that are likely to have confounding effects on private employment, as including these neighborhoods in the analysis would posit the effect of these other development strategies as part of the effect of the public employment.

Ultimately, the goal of the sample selection criteria is to ensure that we create the best dataset

[2] Using these neighborhoods that lost public employment may shed light on potentially asymmetric effects of public employment on the private sector. The direction of bias resulting from adding these neighborhoods to the sample is theoretically ambiguous. The relocations out of these neighborhoods do not have much spatial and time-series variation because these neighborhoods are geographically clustered close to each other in the SMA. Hence, we exclude the neighborhoods located in the SMA.

of neighborhoods that allows us to cleanly identify the causal effect of the public employment shock on private employment (Greenstone et al., 2010; Neumark and Kolko, 2010; Busso et al., 2013). As a result, we focus on 19 neighborhoods (and their 1,050 nearby neighborhoods) to which 108 public entities relocated, introducing a total of 47,787 public employments.[3] The timing of relocation for these neighborhoods span from 2011 to 2017.[4]

Descriptive Statistics

We provide summary statistics of the baseline characteristics at the neighborhood level observed in 2010 in Table 1. This table aims to demonstrate whether the treated neighborhoods, to which public entities relocated from 2011 to 2017, are, on average, similar to or different from their nearby control neighborhoods in terms of the observed pre-treatment characteristics.

In 2010, the average population in the treated neighborhoods is higher than in the rest of the neighborhoods in the sample, with the population level monotonically decreasing as neighborhoods are located farther away from the treated neighborhoods. However, their land areas differ. We normalize the population level by land area and compute population density, which reveals a different pattern. In 2010, the treated neighborhoods had the lowest population density but were located adjacent to neighborhoods with the highest population density on average. Then, the population density decreases as the distance increases.

For all the other observable characteristics, such as sex ratio, average household size, share of the working-age population, industry composition, and growth of private employment from 2006 to 2010, the treated and other neighborhoods appear to be similar on average.[5] We compare the treated and the untreated neighborhoods more formally and report the difference of the mean values in Table A.2 in the appendix. While we find no statistically significant difference for most variables between the treated and the control neighborhoods, the treated neighborhoods had a larger population and household size, on average. In our main analysis, in order to account for the baseline differences, we include these baseline characteristics as control variables and refine our analysis using them to purge out their potential confounding effects over time from the estimated effect of the public-sector shocks on local private employment.

In Figure 1, we plot the average number of employments in the public sector (dashed line) and the private sector (dotted line) separately for the treated neighborhoods (black circle) and the control neighborhoods (gray diamond) annually from 2006 to 2011. Prior to 2011, before any relocations took place, the difference in employment levels for both sectors between the treated

[3] The final sample captures over 90% of the relocations that took place between 2011 and 2017, measured in terms of the changes in public employment. In the appendix, Figure A.1 displays a map of the neighborhoods in South Korea and the spatial distribution of the treated and control neighborhoods as well as the neighborhoods that are excluded from our analysis.

[4] In case of multiple public entities relocating one neighborhood, we denote the year when the first entity relocated as the treatment year.

[5] In the appendix, Table A.1 provides the summary statistics of the same set of baseline characteristics for neighborhoods, starting with no sample restriction and gradually adding the sample restrictions discussed in the previous subsection.

and control groups is small and fairly consistent between 2006 and 2011. However, starting from 2011, the treated neighborhoods break away from the trend, and the gaps between the treated and the control for both sectors widen over time. These patterns serve as prima facie evidence of the positive effect of the relocation of public-sector jobs on its local private employment effect. It is the goal of this paper to investigate to what extent the increase in private employment is caused by the increase in public-sector employment. We discuss our empirical strategy in the subsequent section to tease out the causal effect of the public sector shocks on local private employment and estimate local employment multiplier.

3 Empirical Strategy

In this section, we present our empirical strategies to identify the effect of public employment on private employment. We leverage the temporal and cross-sectional variation in the relocation of public entities in South Korea and introduce a rich set of fixed effects and control variables to account for key confounding forces. We estimate the local employment multiplier of public employment by exploiting the treatment intensity, which is defined as the change in public employment normalized by the number of additional public employment due to relocation.

3.1 Two-Way Fixed Effects Event Study Model

Based on the temporal variation in relocation timing and the spatial variation in destination neighborhoods, we conduct an event study to semi-parametrically estimate the effect of relocation on private employment using the following specification:

$$Y_{i,t} = \phi_i + \psi_{c(i),t} + (X_i \times t)'\Gamma + \sum_{\tau=-5}^{5} \beta_\tau D_{i,t}^{(\tau)} + \epsilon_{i,t}, \tag{1}$$

where dependent variable $Y_{i,t}$ represents the number of private employment (total, manufacturing, and service) in neighborhood i in year t. Neighborhood fixed effects ϕ_i account for all time-invariant neighborhood-level characteristics. City-by-year fixed effects $\psi_{c(d),t}$ flexibly control for year-to-year changes in metropolitan city-level characteristics as well as national-level changes correlated with neighborhood-level employment and the relocation shock (e.g., local labor and housing market conditions). X_i is a vector of neighborhood-level pre-treatment baseline local characteristics observed in 2010; it is interacted with a linear year trend to partially control for the dynamic effects of the baseline characteristics. The pre-treatment baseline characteristics include population, population density, age composition, sex ratio, stock of housing per capita, and industry composition. Note that we do not include the contemporaneous values of these neighborhood level characteristics as they may also be endogenously affected by the public-employment shock. $\left\{D_{i,t}^{(\tau)}\right\}_{\tau=-5}^{5}$ is a set of dummy variables indicating the number of years τ before and after public entities relocated to neighborhood i relative to year t. Finally, $\epsilon_{i,t}$ captures stochastic error and all other determinants of private employment.

The event study coefficients $\{\beta_\tau\}_{\tau=-5}^{5}$ measure the changes in private employment τ years before and after the relocation of public entities. Because the dependent variable is measured at the end of each calendar year, the relocation shock is likely to affect the private employment in the year when the first relocation took place in each neighborhood: $\tau = 0$. Therefore, we impose $\beta_{-1} = 0$.

The identification assumption for a causal interpretation of $\{\beta_\tau\}_{\tau=-5}^{5}$ in Equation 1 is that, in the absence of the shock arising from the relocation of public entities, private employments would vary across neighborhoods within a city in a given year for reasons orthogonal to the relocation. This assumption may fail to hold for several reasons. For instance, the neighborhoods in the control group may differ significantly from those in the treatment group with respect to their baseline characteristics that may have put these neighborhoods on different trajectories (i.e., a violation of the parallel trend assumption). We have preemptively considered this problem and focused on the neighborhoods that (a) are within 30 kilometers from the nearest neighborhood where public entities relocated and (b) are not part of the Seoul Metropolitan area.

Moreover, another threat to identification is the presence of spatial spillover. A positive (negative) spillover effect across space would pressure down (up) and bias the estimate of the event-study coefficients after relocation took place. We address this concern by directly estimating the changes in private employment across space using the following specification:

$$Y_{i,t} = \phi_i + \psi_{c(i),t} + (X_i \times t)'\Gamma + \sum_{\tau=-5}^{5} \beta_\tau D_{i,t}^{(\tau)} + \sum_{r=5,10,15,20} \sum_{\tau=-5}^{5} \sigma_{r,\tau} N_i(r) \times D_{i,t}^{(\tau)} + \epsilon_{i,t}, \qquad (2)$$

where $N_i(r)$ is a dummy variable indicating whether neighborhood i is located within $(r-5, r]$ kilometers from the nearest neighborhood where public entities relocated, for $r \in \{5, 10, 15, 20\}$. Then, a set of coefficients$\{\sigma_{\tau,r}\}_{\tau=-5}^{5}$ measures the changes in private employment τ years before and after the relocation of public entities relative to the neighborhoods that are located more than 20 kilometers away from the neighborhoods to which public entities relocated. In this specification, the control group includes neighborhoods that are more than 20 kilometers away, but within 30 kilometers. Should there be spatial spillover effects, $\{\sigma_{\tau,r}\}_{\tau=-5}^{5}$ would be estimated away from zero for some r. The findings of the previous literature which estimates how fast productivity (and amenity) spillovers decay across space (Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008; Rosenthal and Strange, 2008; Andersson et al., 2009; Ahlfeldt et al., 2015; Faggio, 2019) suggest that spatial spillovers should, if they operate, decay completely across neighborhoods in our analysis because the geographical coverage of the treated and their nearby neighborhoods in our data is sufficiently large.

These strategies aim to identify the causal impact of relocating public entities on private employment without taking into account the variation in the size of relocation across destination neighborhoods. While a back-of-envelope calculation can be done, the results cannot be read directly as a local employment multiplier. Furthermore, because the event study dummy variables are defined in terms of the arrival of the first public entities and there are, in some cases, multiple entities moving into the same neighborhoods at later times, the dynamics that would be captured in the estimated event study coefficients may capture both the actual dynamic effect of the first relocation and the effect of the later arrivals. We aim to address these issues next.

3.2 Long-Difference Model (Treatment Intensity Model)

Next, we leverage the variation in the size of relocation to estimate the local employment multiplier. To do so, we employ an estimation strategy that extends a difference-in-difference approach with heterogeneous treatment effects, similar to the approach used in Faggio (2019). That is,

$$\Delta Y_{i,2018-2010} = \psi_{c(i)} + X_i'\Gamma + \delta \Delta PE_i + \sum_{r=5,10,15,20} \delta_r \Delta PE_{\mathcal{N}_i(r)} + \Delta Y_{i,2010-2006} + \eta_i, \tag{3}$$

where $\Delta Y_{i,2018-2010}$ corresponds to the change in private employment (total, manufacturing, and service) in neighborhood i from 2010 to 2018; city fixed effects $\psi_{c(i)}$ capture all city-level factors that explain the change in the outcome of interest; X_i is a vector of neighborhood-level pre-treatment baseline characteristics observed in 2010; ΔPE_i refers to the change in public employment in neighborhood i due to the relocation between 2011 and 2018; $\Delta PE_{\mathcal{N}_i(r)}$ is a variable interacting the change in public employment in the nearest treated neighborhood with an indicator variable denoting whether neighborhood i is located within $(r-5, r]$ kilometers away from the nearest treated neighborhood. For example, if neighborhood i is treated, then ΔPE_i takes a strictly positive value while $\Delta PE_{\mathcal{N}_i(r)}$ is zero $\forall\, r$. If neighborhood i is untreated and located within $(r'-5, r']$ kilometers away the nearest treated neighborhood, then $\Delta PE_{\mathcal{N}_i(r')}$ takes a strictly positive value while ΔPE_i and $\left\{\Delta PE_{\mathcal{N}_i(r)}\right\}_{r\neq r'}$ are equal to zero. In essence, $\left\{\Delta PE_{\mathcal{N}_i(r)}\right\}_{\forall r}$ captures potential spatial spillover effects.

Lastly, we include the pre-treatment changes in private sector employment between 2006 and 2010, $\Delta Y_{i,2010-2006}$. This additional variable parses out the effect of past growth and controls for the differences in private-sector employment growth between the treated and control neighborhoods. Furthermore, controlling for the change in private sector employment prior to relocation addresses a potential bias due to the anticipatory effect from the announcement of the destination neighborhoods for the selected public entities. The last term, η_i, captures stochastic error and all other determinants of the change in local private employment. Then, the coefficient δ corresponds to the local employment multiplier driven by the public-sector employment shock, i.e., the change in the number of private sector jobs for a unit increase in public sector employment.

4 Results

In this section, we present the results estimating the effect of public employment on private employment based on the event study framework (Section 3.1) and the local employment multiplier based on the treatment intensity model (Section 3.2). Then, we conduct additional exercises to study the migratory responses to the public employment shock, which may partly explain the changes in the private employment. Lastly, we estimate the local employment multiplier for each of the treated neighborhood and provide suggestive evidence on the factors that may the heterogeneity of the local employment multiplier.

4.1 Main Findings

We first estimate Equation 1 using the total number of private employment (in 1,000s) as the dependent variable. In the top panel of Figure 2, the estimated event study coefficients are plotted (hollow black circles). These coefficients reveal three clear patterns. Initially, the differences in total private employment between the treated and control neighborhoods were economically negligible and not statistically significant at the 5 percent level. Subsequently, the private employment in the treated neighborhoods began to diverge from the control neighborhoods from the first year of treatment, continuing to widen thereafter. This pattern likely captures both the dynamic effect of relocation over time and the impact of subsequent relocations. Thirdly, the standard errors increased significantly post-relocation. Although the point estimates are positive, the large standard errors and the confidence intervals that include zero during the first two years suggest caution. However, after two years, the event study coefficients are large and statistically significant at the 5 percent level. The large standard errors might reflect the initial years' variations in the size and type of public jobs each relocation episode brought, as detailed in Section 2.1), both of which we explore in the next subsection.

Then, we estimate Equation 1 ussing employment in the manufacturing and service sectors (measured in 1,000s) as separate dependent variables. The estimated event study coefficients are plotted with red diamonds for the manufacturing sector and blue x's for the service sector in the top panel of Figure 2. For both sectors, the coefficients prior to the relocation show no pre-trend: employment levels in these sectors in both treated and control neighborhoods were not statistically different. In the manufacturing sector, this pattern continued post-treatment. Despite the manufacturing sector's smaller average employment share compared to the service sector, it remains economically significant in both treated and control neighborhoods, ensuring the results are not driven by initial low employment levels. Conversely, a clear structural break is evident in the service-sector employment after relocation, with the coefficients for this sector almost entirely explaining the changes in total employment.

To interpret these results as causal evidence of an increase in private employment from a public sector shock, one of the assumptions we make is no presence of spatial spillovers of the public-employment shock. While we believe this assumption is reasonable based on the previous literature on the external economies of agglomeration across space, we directly test this assumption by estimating Equation 2 and summarize the estimation results in the middle and bottom panels of Figure 2. Since we find that the relocation did not affect employment in the manufacturing sector, we only look at the total private employment, the changes of which can be attributed to the changes in employment in the service sector. All the estimated coefficients plotted in both panels are from a single estimation and that the estimates are relative to the neighborhoods located 20 kilometers away but within 30 kilometers from the nearest neighborhoods to which public entities relocated.

In the middle panel, the estimated event study coefficients for the treated neighborhoods are plotted in black. The treated neighborhoods experienced an increase in the total employment in the private sector after the relocation. The estimated coefficients are almost identical to the estimates plotted in the top panel. However, the private employment in the neighboring areas located within 5 kilometers from the treated areas (indicated by blue x's) were not affected by the increase in the public employment in the nearby treated neighborhoods. Likewise, the results in the bottom panel show that the other neighborhoods in the sample also did not experience changes in their private employment. The results are in line with the literature and confirm that the spillovers decay fast across space; in our case, they do so completely within the treated neighborhoods. With this evidence, we conclude that the relocation of public entities had a positive causal effect on the private employment and that this effect was highly localized.[6]

We estimate a complementary difference-in-difference model to obtain the average treatment effect of relocation on private employment as follows:

$$Y_{i,t} = \beta D_i \times Post_t + \phi_i + \psi_{c(i),t} + (X_i \times t)'\Gamma + \epsilon_{i,t}, \tag{4}$$

where the set of event study dummy variables are replaced with a single dummy variable indicating post-treatment years and the treatment units. The estimated event study coefficients for the years prior to treatment support the parallel trend assumption for the identification using the difference-in-difference approach.[7] Panel A of 2 reports the estimation results using total private employment in Columns 1–3, service-sector employment in Columns 4–6, and manufacturing-sector employment in Columns 7–9. For each dependent variable, we begin by including the neighborhood fixed effects (Column 1, 4, and 7) and gradually introduce additional control variables and fixed effects across columns.

In Column 1, the estimated effect of public entities' relocation is 1.94, indicating roughly 1,940 additional jobs in the private sector. This coefficient is estimated conditional on neighborhood fixed effects ϕ_i, capturing constant effects of intrinsic neighborhood characteristics. However, these effects do not account for potential dynamic impacts of baseline characteristics. For example, if population growth was faster in treated areas than in the control group before the treatment, and this growth was unaffected by the treatment, the relocation's effect would be overestimated, assuming a positive correlation between population and employment. To address this, we included

[6]We consider an alternative specification in which we break out the control neighborhoods located within 5 kilometers into two groups: 0 to 2 kilometers and 2 to 5 kilometers. The estimation results are plotted in Figure A.2 in the appendix for the total employment (top panel), the service sector (middle), and the manufacturing sector (bottom). The results based on this alternative definition of the immediate neighboring areas shows no indication of spatial spillover across the neighborhood boundaries.

[7]We may flexibly account for the pre-trend by adding the event study dummy variables for event years -5 to -2. However, we expect that this refinement would not change the results much based on the event study results presented in Figure 2.

a vector of pre-treatment neighborhood characteristics interacted with a linear trend term, $(X_i \times t)$. With these additional controls, the coefficient in Column 2 drops to 1.29. Introducing city-by-year fixed effects to control for time-varying citywide shocks, the coefficient's significance level decreases to the 10 percent level. According to the fully saturated model in Column 3, public entities' relocation resulted in an approximate 1,330 job increase in the private sector. Results for the service sector mirror those of the total private employment, aligning with previous event study findings that changes in the service sector largely account for the overall employment changes. For the manufacturing sector, the relocation had no significant economic or statistical impact on employment across different specification.[8]

Finally, we focus on the findings from the treatment intensity model (Equation 3), which are summarized in Panel B of Table 2. Columns 1 to 3 present the estimation results using the change in total private employment from 2010 to 2018 as the dependent variable. In Column 1, we start by incorporating a set of pre-treatment baseline characteristics as control variables. The results indicate a positive effect, with a 0.95 increase in private employment for each additional public sector job, statistically significant at the 1 percent level. In Column 2, we add the interaction term $\Delta PE_{\mathcal{N}_i(r)}$ to account for potential spatial spillover effects of the shock. The coefficient estimate remains largely unchanged, aligning with our earlier findings of negligible spatial spillover effects. Column 3 represents our most refined specification, where we further adjust for changes in total private employment prior to the treatment, between 2006 and 2010. Here, the coefficient estimate slightly rises to 0.99, continuing to be statistically significant at the 1 percent level. This suggests that, from 2010 to 2018, private employment increased by approximately one employee for each additional public sector job created due to the relocation.

This result indicates that there was almost one-to-one correspondence between the public employment and the private employment. The results reported in the other columns together show that the increase in the total private employment is driven by the increase in employment in the service sector.[9] Our estimated employment multiplier in the South Korean context falls within the range of estimates found in existing literature, despite differences in empirical settings and sources of variation. In the context of the U.K., Faggio (2019) found that the introduction of 10 civil service workers in an area increased private sector employment by 11 workers. This increase was entirely driven by the service sector, with no statistically significant changes in the manufacturing sector. Similar to our findings, this effect is highly localized, showing no impact of public employment on private employment beyond the 0-3 km radius. The employment multipliers of public employment

[8]We introduced neighborhood fixed effects in various columns and included an interaction of baseline local characteristics with a linear time trend in the second and third model specifications. This approach accounts for differences across neighborhoods, such as in population and land area. Consequently, the observed results are not influenced by these factors. To ensure robustness against variations in neighborhood sizes, we replicated the analysis in Panel A of Table 2, this time using log transformed values of the dependent variable as detailed in Table A.3. This replication confirmed that the results are qualitatively consistent, reaffirming the reliability of our findings.

[9]By further disaggregating the service-sector employment by 14 different categories, we estimate local employment multiplier for each category. The three top categories with the highest values of employment multiplier are accommodation and food service activities (0.21), construction (0.16), and wholesale and retail trade (0.13). The estimation results are available upon request.

in Spain (Jofre-Monseny et al., 2020), stand at 0.8 for the non-tradable sector and -0.4 for the tradable sector. These estimates are comparable to the multipliers of 1.05 and 0.5 for the non-tradable sector and -0.19 and –0.4 for the tradable sectors estimated in Germany and the U.K. (Becker et al., 2021; Faggio and Overman, 2014).

4.2 Additional Results and Discussion

Migratory Responses

So far, our findings indicate that an increase in public employment has also boosted private employment, particularly in the service sector. Additionally, the areas receiving these public entities might experience population growth due to migration, for two main reasons. Firstly, the relocation of public organizations could directly bring both the workers from these entities and their family members to the destination areas. However, considering the granularity of our spatial analysis unit, it's important to note that an increase in either private or public employment within a neighborhood does not necessarily translate into a higher resident population in that same neighborhood. This is due to the ease with which workers can commute between different neighborhoods.[10] Secondly, a general equilibrium response might occur, manifesting as spatial sorting of residents through various channels. This could include enhancements in residential amenities, such as safety, infrastructure, and the provision of service goods, stemming from the influx of skilled workers (e.g., government officials) in these neighborhoods. Such improvements could attract more residents to these areas, further influencing the demographic composition (Ahlfeldt et al., 2015; Diamond, 2016; Kim, 2023b).[11]

To examine the impact of the relocation of public entities on people's migration decisions, we utilized the comprehensive database of resident registration records. By law, individuals are required to update their residency registration within 14 days of relocating. Each record in this database includes details about the origin and destination neighborhoods, the date of the move, and demographic information of the individuals. We calculated the total number of people moving into (inflows) and out of (outflows) each neighborhood from 2010 to 2018. By subtracting the outflows from the inflows, we determined the total net inflows for each neighborhood. This net inflow figure is used as the dependent variable in our treatment intensity model. The estimation results derived from this model are detailed in Table A.4 in the appendix.

The estimated coefficient for treated neighborhoods, as shown in Column 1, indicates that the net inflow of migrants increased by 3.47 individuals for each public-sector employee relocated. This increase is statistically significant at the 5 percent level. Notably, the migration effect is highly localized; adjacent neighborhoods did not experience statistically significant changes in net migration inflows. To further explore these effects, we adjusted the outcome variable to focus

[10] There is no publicly available data sources that capture commuting patterns at the neighborhood level in South Korea. Across districts, about 1/3 of people work in districts (a unit aggregating neighborhoods) where they do not reside (Kim, 2023b).

[11] These public entities that moved do not provision *local* good and services as they are part of the national government and their objectives concern the national interests, not the locals.

on various age intervals and re-estimated the model, presenting these findings in Columns 2 to 13. The migration effects across different age groups are uniformly positive and remain highly localized. However, the magnitude of these effects varies by age. Specifically, the net in-migration per public sector employment ranges from 0.21 individuals in the 20–25 age group to 0.48 individuals in the 30–35 age group, then progressively decreases to 0.05 for the 65–70 age group. While it's acknowledged that individuals aged 25 to 40 typically exhibit higher mobility, the results aren't merely attributable to this age-composition effect. This is because our estimates are derived by comparing migration rates across neighborhoods, *conditional on* each age interval. In summary, these findings collectively suggest that neighborhoods receiving additional public employment due to relocation have attracted more migrants, particularly those in their prime working years.

In Table 3, we delve into the migratory responses by examining the origins of the migrants. By comparing the estimates in the first row across different columns, it becomes evident that the net influx of migrants into the treated neighborhoods was primarily driven by residents relocating within the same metropolitan cities and provinces where the public entities were moved to. Given that all the public entities were relocated from the Seoul Metropolitan Area (SMA), this finding suggests that the migration effect is not solely a result of the dispersion of public employment from the SMA. This is partly reflected in the positive net migration from the SMA, as shown in Column 2. Equally notable is the estimated impact on migrants originating from outside the metropolitan city/province of the treatment location, but not from the SMA, as reported in Column 3. The estimated coefficient here is larger than that in Column 2, although not different statistically significantly from each other. Migration between metropolitan cities and provinces typically involves longer distances than a within-city moving. The estimate of 0.74 for treated neighborhoods (Row 1), in contrast to the estimates for adjacent neighborhoods (Rows 2–4) which are not statistically different from zero. The results indirectly suggest that the treated areas are seen as relatively more desirable relocation destinations. This could be due to perceived improvements in living quality, higher value of amenities, and increased opportunities for employment and income in these areas.

Finally, we seek to answer the following question: To what extent do the family members of relocated employees contribute to the increase in private-sector employment? A major limitation of the resident registry records is their lack of personal information, providing only the sex and age of residents. This restricts our ability to identify the specific households that relocated public employees belong to. To overcome this, we turn to the 2015 Population Census of South Korea. This census provides data on each household, including the district of residence five years prior, the current residential district, and household composition. This information will enable us to approximate the migration patterns of households potentially linked to the relocated public sector employees and assess their impact on private-sector employment growth.[12] For each household members, each record contains information on the demographic characteristics, employment status, occupation, and industry. We focus on the households whose current residential *districts* as of 2015 experienced the arrival of public employees due to the relations between 2010 and 2015.[13]

[12]The most granular spatial unit in the Census is district, a group of neighborhoods.

[13]We restrict the sample to households with at least one person in the labor force and consider only individuals

The results of an accounting exercise, summarized in Table 4, suggest that the family members of relocated public employees only partially account for the employment multiplier estimated in this paper. Table 4 reports the number of public and private employees, the number of their family members over the age of 15, and the number of family members who were employed (or seeking employment) in the service or manufacturing sector, categorized by their residence in 2010. Panel A indicates that 15,801 public employees migrated from the Seoul Metropolitan Area (SMA) to the treated districts. Accompanying them were 8,220 household members, translating to approximately 5.2 family members migrating for every 10 public employees due to the relocation. Of these, 2,654 household members are employed in the service sector. A simple calibration yields an employment multiplier of 0.17, which is significantly lower than our estimated local employment multiplier. We also calculate these figures for public employees whose previous residences were outside the SMA.[14] On average, each of these public employees had 1.28 household members. Among these, 0.48 household members were working in the service sector. This proportion is more than three times larger compared to that of public employees who moved due to relocations. To provide a comprehensive view, Panel B of the report details similar statistics for private employees and the employment patterns of their household members.

Heterogeneous Local Employment Multiplier

We estimated local employment multiplier based on the empirical strategies robust to treatment heterogeneity; therefore, our estimate corresponds to a weighted average of heterogeneous local employment multiplier. We investigate whether local employment multiplier differs by relocation type: i.e., single-entity relocation (SR), relocation to an old town (OT), and relocation to a new town (NT). To do so, we allow δ in Equation 3 to be specific to each relocation type $k \in \{new, old, single\}$ and report the estimation results in Table A.5 in the appendix. According to the estimation results based on our preferred specification reported in Column 3, the local employment multiplier is positive and statistically significantly different from zero for OT and NT, while it is not different from zero for SR in which case only a single public entity relocated. Further, the magnitude of the estimated local employment for NT is 1.06, which is about 2.7 times larger than the local employment multiplier for OT.

The results offer several suggestive insights. First, local employment multipliers are positively associated with relocations to areas that either already have established infrastructure networks and well-functioning housing and labor markets, or where the relocation coincides with significant housing supply shocks and infrastructure development, such as the creation of Innovative Cities. Second, we observe that the estimated local employment multiplier effect is particularly larger for NT. This, coupled with our findings on migratory responses, suggests that the magnitude of the local employment multiplier is dependent on the extent to which the local economic environment

older than 15.

[14]These public employees include those who migrated from another district outside of the SMA into the treated districts and whose who had lived and continued living in the treated districts. Thus, they were not subject to the relocations.

can accommodate additional general equilibrium responses. These responses include migratory patterns, endogenous amenities, agglomeration effects, and job opportunities. Lastly, the scale of relocation may also contribute to the heterogeneity in local employment multipliers.[15]

5 Robustness Checks and Internal Validity

To ensure the robustness of our findings, we have conducted several analyses that affirm the internal consistency of our results. Firstly, we provide evidence supporting the quasi-random nature of the public employment shocks utilized in this study. This is demonstrated by showing that the selection of destination neighborhoods for public entity relocation cannot be predicted based on pre-treatment neighborhood characteristics. Furthermore, we reassess the effect of relocation on private employment and the local employment multiplier using alternative sample restrictions.

Secondly, to address potential inference issues due to the limited number of treated observations, as highlighted by (Ferman and Pinto, 2019), we implement a variant of Fisher's permutation or randomization test (Fisher, 1935). Thirdly we test the robustness of our results against potential biases arising from the staggered introduction of shocks, in line with recent advancements in the two-way fixed effects literature (Goodman-Bacon, 2018; de Chaisemartin and D'Haultfœuille, 2020). Lastly, we conduct two placebo tests. The first uses pre-treatment changes in private employment as the dependent variable in our treatment intensity model. The second estimates the local employment multiplier for 'runner-up' neighborhoods, following the approach of Greenstone et al. (2010).

Determinants of Relocation Sites and Sample Selection

The key identification assumption in our paper is that, had the treatment (i.e., the relocation of public entities) not occurred, the level of private employment, or its changes, would have been similar between the treated neighborhoods and nearby areas. This assumption is conditional on the national government's selection of cities for relocating the public entities. To test this assumption, we assess the predictive power of pre-treatment baseline local characteristics for determining which neighborhoods received public entities. We employ a linear probability model, formulated as follows:

$$Treated_i = \phi + X_i'\Omega + \mu_i, \tag{5}$$

$Treated_i$ is a dummy variable indicating the destination neighborhoods to which public entities relocated; X_i is a vector of pre-treatment baseline characteristics observed in 2010 capturing the

[15] To provide further, yet suggestive evidence in support of these implications, we pool samples by each relocation site and its nearby neighborhoods and separately estimate the local employment multiplier for each destination neighborhood using its corresponding neighboring areas as control group. In the appendix, Figure A.3, we plot the estimated local employment multipliers against the size of public employment shock (top left), distance to Seoul (top right), population density in 2010 (bottom left), and unemployment rate in 2010 (bottom right). The results suggest that local employment multiplier is positive associated with the size of public employment shock and the baseline unemployment rate and negatively associated with how distant the treated neighborhoods are from Seoul.

local demographic composition and the housing and labor market conditions.

In the appendix, Table A.6 reports the estimated coefficients of Ω under different sample restrictions. Notably, the only coefficient that is statistically significant at the 5 percent level is for population density, and this significance is observed when no sample selection criteria are applied. This implies that, ceteris paribus, a neighborhood's likelihood of being chosen for the relocation of public employees decreases with its population density. However, once neighborhoods in the Seoul Metropolitan Area (SMA) are excluded (as shown in Columns 2 to 4), this coefficient is no longer statistically significant. Furthermore, we report the F-statistics to test the null hypothesis that all coefficients are jointly zero. Across all columns, we find that the null hypothesis cannot be rejected. These findings suggest that pre-treatment local characteristics do not effectively predict which neighborhoods would ultimately receive treatment. Overall, the results indicate that the spatial variation in the public employment shocks, which form the basis of our analysis, is likely quasi-random.

We show that the main results of this paper are robust to varying sample selection criteria. For the main analysis, we applied a set of sample selection criteria to come up with a dataset of the treated neighborhoods and arguably the *best* control neighborhoods. As a result, the number of neighborhoods in the final data set is 1,069, only about a third of the total number of neighborhoods in South Korea. One may worry that the results we find may be driven by the sample selection criteria. To address this concern, in Figure A.4 and Table A.7, we reproduce our main results without imposing any sample restriction and then gradually add restrictions: non-SMA and closer proximity to the treated neighborhoods. The estimated event study coefficients, the difference-in-difference estimates, and the estimates of local employment multiplier change little, notwithstanding the changes in the sample.

Statistical Inference

Inference from our empirical approach in which we cluster the errors at the neighborhood level relies on the asymptotic approximations assuming increasing number of samples within each cluster and/or growing number of clusters (Ferman and Pinto, 2019). This assumption does not hold in our case because only 19 neighborhoods are treated (out of 1,069 neighborhoods). To address this inference issue, we implement a variant of Fisher's permutation or randomization (Fisher, 1935), similarly done in Buchmueller et al. (2011) and Cunningham and Shah (2017). We estimate the event study coefficients in Equation 1, the difference-in-difference coefficient in Equation 4, and the employment multiplier in Equation 3 an additional 100,000 times while randomly shuffling the treatment history (i.e., year and intensity of each relocation) across 1,069 neighborhoods each time with replacement. Then, we treat the resulting 100,000 placebo estimates of a coefficient of interest as the sampling distribution for the coefficient and construct conservative confidence intervals based on the placebo distribution.

Panel A of Figure 3 plots the estimates of the event study coefficients in black circle and the dark (lighter and thinner) error bars correspond to the top and bottom 5th (1st) of the placebo distribution of each event study estimate. For the total and service-sector private employment, the coefficient estimates overlap with the placebo-based confidence intervals for years leading up to the relocation, but start to differ from the intervals thereafter. For years after 2 years since the relocation, the estimated event study coefficients are statistically significantly different from the placebo estimates. For the manufacturing-sector employment, all the coefficient estimates are within the placebo-based confidence intervals. In Panel B and C, we show that the estimated effect of relocation on the total private and service-sector employment and their local employment multipliers reported in Columns 3 and 6 of Table 2 are statistically significantly different from the placebo estimates. Our estimates for the manufacturing sector reported in Column 9 of Table 2 are well within the 95% and 99% confidence intervals of the placebo estimates.

Staggered Introduction of Treatment

Estimating of Equation 1 and its complimentary difference-in-difference estimate by OLS may not identify the average treatment effect of the relocation on the private employment in this empirical setting as the timings of policy implementation across neighborhoods are *staggered* over different years.[16] We follow the estimation approach in Kim (2023a), which refines the estimation method of Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) robust to potential biases from staggered introduction of treatment and permits the time fixed effects to vary by groups of neighborhoods. Figure A.5 in the appendix replicates Figure 2. All the event study coefficients before treatment are estimated close to zero and statistically indistinguishable from zero. After the relocation, the treated neighborhoods experienced increase in the private-sector employment, the magnitude of which also increased over time. This change is completely explained by the service sector. The results are similar to Panel A of Figure 2, although the point estimates after treatment are larger in Figure A.5.

Placebo Tests

We conduct two different placebo tests. First, we turn to a placebo test based on the treatment intensity model and check whether the treatment affected the outcomes variables before treatment. Under the assumption that there was no anticipatory effects, private sector employment should not have changed during the pre-treatment period. Especially for the neighborhoods that experienced the public-sector shock later in the sample period, this assumption is likely to hold. To do so, we estimate Eq. 3 using the change in the private employment from 2006 to 2010, during which no relocation took place. In addition to being a placebo test, this analysis also directly probes for any anticipatory effects. In Table 5, Panel A reports the estimation results for the changes in the total private-sector employment in Column 1, the service-sector employment in Column 2, and the

[16]There has been recent advancement in the literature on two-way fixed effects difference-indifference estimators, which address the identification concerns with a staggered treatment setup (see Goodman-Bacon (2018), de Chaisemartin and D'Haultfœuille (2020), Sant'Anna and Zhao (2020), Callaway and Sant'Anna (2021), and Borusyak et al. (2021).

manufacturing-sector employment in Column 3 based on our most preferred specification with the full set of fixed effects as in Column 3, 6, and 9 in Table 2.

The estimated coefficient for total private employment in Column 1 is negative, but statistically significantly not different from zero. This result implies that the private employment in the destination neighborhoods did not differ from the rest of the neighborhoods in our sample before treatment. Also, we reject the null hypothesis that there may have been an anticipatory effect. Both of these implications further support our identifying assumptions for our main analysis. The results for the service-sector employment in Column 2 are similar to those in Column 1. In Column 3, the estimated effect of relocation on the manufacturing sector is slightly negative and statistically different from zero at the 5 percent significance level. We believe that this estimate is driven somewhat mechanically because relocating public entities require land areas. This may have crowded out manufacturing employment in the destination neighborhoods prior to the relocation. However, this is not evidence undermining the validity of our main results for two reasons. Firstly, the effect size is economically small. Secondly and more importantly, we are interested in the changes in private employment as relocation takes place and injects public jobs into the destination areas, which we achieve by estimating Eq. 3 while controlling for the changes in employment during the pre-treatment period.

As a second placebo test, we estimate changes in private employment in the runner-up neighborhoods à la Greenstone et al. (2010) based on the event study design. Except for a few destination neighborhoods, we were able to identify runner-up neighborhoods which barely did not get selected. We construct an additional data set of these runner-up neighborhoods and their nearby neighborhoods located within 30 kilometers. Then, we estimate Eq. 4 and summarize the estimation results in Panel B of Table 5.[17] The estimate of 0.23 in Column 1 is both statistically not different from zero and economically small in terms of magnitude, compared to the estimate of 1.33 in Column 3 in Panel A of Table 2. Similarly, the estimated changes in the service sector (Column 2) and in the manufacturing sector (Column 3) are economically small and statistically not different from zero. Overall, these runner-up neighborhoods seem to have experience, if at all, small increase in private employment driven by the manufacturing sector more than the service sector.

6 Conclusion

In this paper, we leverage an episode of the relocation of public entities in South Korea taking place between 2011 and 2017 to different areas of the country to estimate local employment multiplier of public employment. We provide evidence supporting that the variation in the destination neighborhoods to which public entities relocated is quasi-random to identify the causal estimates of the effect of local public employment shocks arising from the relocation on the private employment.

[17] In the appendix, Figure A.6 plots the estimated event study coefficients. Across all event times before and after the relocation, none of the estimated coefficients are statistically significantly different from zero at the 5 percent significance level for the total private employment, the service-sector employment, and the manufacturing-sector employment.

Based on the event study and treatment intensity model, we show that an increase in public employment in turn increased private-sector employment, particularly the service sector. According to our estimate, an additional public-sector job raises the private-sector employment by about 1 unit. In addition, we document some suggestive evidence on the general equilibrium effects of these shocks on the migratory responses. Due to the limitations of the data, we are not able to disentangle specific general equilibrium forces. We argue that an increase in the public employment is likely to improve the residential amenities, job opportunities, and earning potentials based on our results showing positive net inflows of migrants into the neighborhoods which experienced an increase in their public employment. In addition, we unpack the average local employment multiplier and show that different types of relocation, the size of the public employment shock, and accessibility shape the heterogeneity of local employment multiplier. Ultimately, our results help policymakers to better design a place-based policy to enhance local economies by using public employment as an engine.

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Figures and Tables

	(1) Treated	(2) 0-5km	(3) 5-10km	(4) 10-15km	(5) 15-20km	(6) 20-30km
Total Population	17,486	15,204	15,841	12,969	12,537	10,471
Population Density	3,169	10,801	6,213	6,024	5,468	3,871
Female Share	0.5	0.502	0.503	0.505	0.505	0.513
Avg. Household Size	2.825	2.683	2.72	2.641	2.609	2.507
Elderly Share (65+)	0.179	0.125	0.152	0.183	0.202	0.248
Share of Working Pop. (15-65)	0.658	0.724	0.693	0.672	0.657	0.622
Share of Female Workers	0.438	0.465	0.434	0.444	0.428	0.45
Share or Service Workers	0.724	0.829	0.773	0.735	0.705	0.733
Δ Private Employment (2010-2006)	0.141	0.128	0.158	0.13	0.131	0.115
Observations	19	158	223	276	304	402

Table 1: Summary Statistics by Proximity to Treated Neighborhoods

Notes: This table reports the average values of local characteristics observed in 2010 for the treated neighborhoods where public employment increased due to the relocations (Column 1) and the comparison neighborhoods located adjacent to the treated neighborhoods by proximity to the treated neighborhoods (Columns 2 to 6).

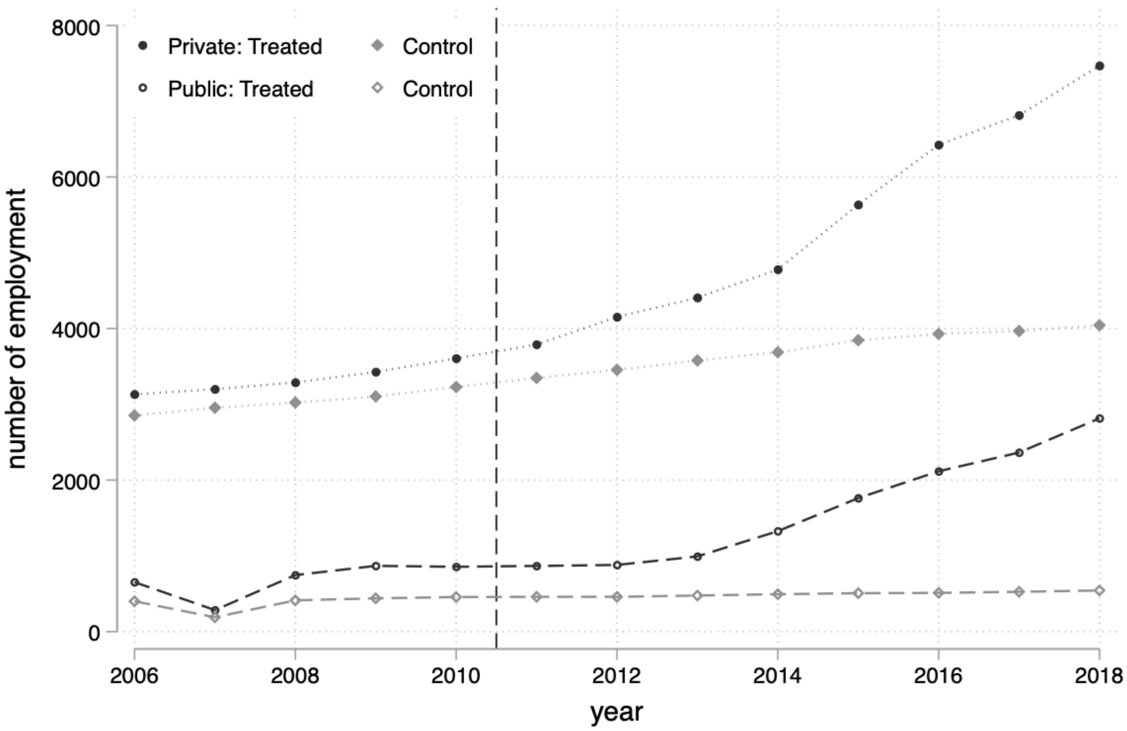


Figure 1: Public vs. Private Employment over Time (Treated vs. Control)

Notes: This figure plots the average number of private employment (solid marker; dotted line) and public employment (hollow marker; dash line) for the treated neighborhoods (in black circle) and the control neighborhoods (gray diamond) from 2006 to 2018. The vertical dash line indicates the years before and after any relocations of public entities.

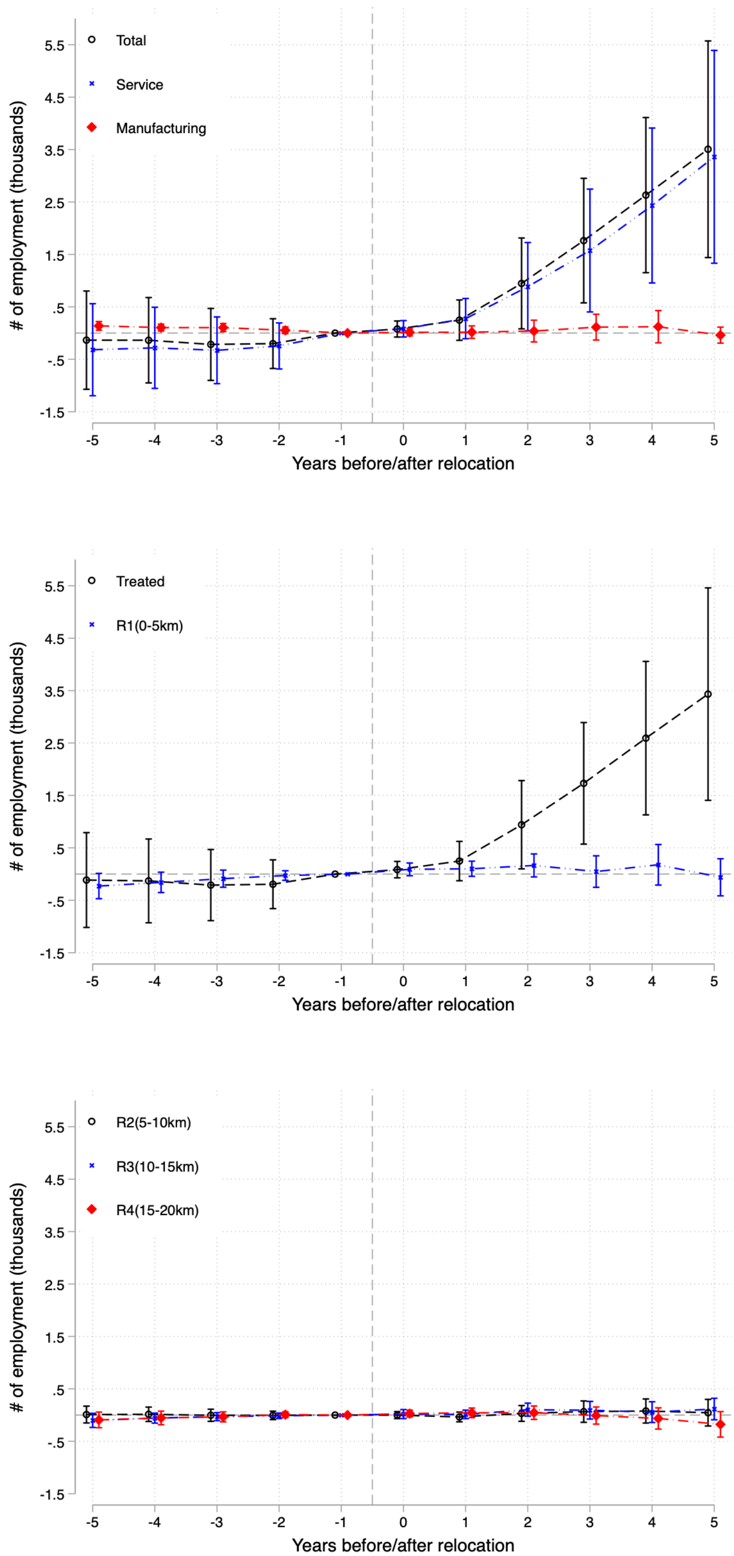


Figure 2: Event Study Estimates of Changes in Private Employment

Notes: This figure summarizes the event study results based on Equation 1 (top) and Equation 2 (middle and bottom). The top panel plots the estimated event study coefficients for private employment (total in black circle; service sector in blue cross; manufacturing in red diamond). The middle and bottom panels plot the estimation results for private employment (total) for the treated neighborhoods (black circle in the middle panel) and the nearby neighborhoods: R1 in the middle panel denotes a set of the neighborhoods located within 5 kilometers from the treated neighborhoods; R2, R3, and R4 in the bottom panel denote the neighborhoods located 5 to 10 kilometers, 10 to 15 kilometers, and 15 to 20 kilometers away from the treated neighborhoods, respectively. Error bars indicate the 95% confidence intervals, constructed based on the heteroskedasticity-robust standard errors clustered at the neighborhood level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Total			Service			Manufacturing	
				Panel A: Difference-in-Difference Estimates					
$Treated \times Post$	1.94**	1.29*	1.33*	1.84**	1.37*	1.41**	0.09	−0.07	−0.07
	(0.95)	(0.72)	(0.74)	(0.90)	(0.71)	(0.72)	(0.13)	(0.08)	(0.08)
ϕ_i	Y	Y	Y	Y	Y	Y	Y	Y	Y
$X_i \times t$		Y	Y		Y	Y		Y	Y
$\psi_{c(i),t}$			Y			Y			Y
Observations	13,897	13,897	13,897	13,897	13,897	13,897	13,897	13,897	13,897
				Panel B: Employment Multiplier Estimates					
$Treated$	0.94***	0.98***	0.99***	0.92***	0.95***	0.96***	−0.02	−0.01	−0.01
	(0.17)	(0.17)	(0.16)	(0.17)	(0.17)	(0.16)	(0.02)	(0.02)	(0.02)
X_i	Y	Y	Y	Y	Y	Y	Y	Y	Y
$\Delta PE_{N_i(r)}, \phi_{c(i)}$		Y	Y		Y	Y		Y	Y
$\Delta Y_{i,2010-2006}$			Y			Y			Y
Observations	1,069	1,069	1,069	1,069	1,069	1,069	1,069	1,069	1,069

Table 2: The Relocation Effects on Private Employment

Notes: Panel A reports the estimation results based on Equation 4, a difference-in-difference version of Equation 1. The estimates capture the average change in the number of employment measured in 1,000s (total in Columns 1 to 3, service sector in Columns 4 to 6, and manufacturing sector in Columns 7 to 9) after the relocations. The left columns include the neighborhood fixed effects; the middle columns controls for a linear time trend for each of the baseline characteristics observed in 2010; the right columns include the city-by-year fixed effects. Panel B reports the estimation results based on Equation 3 using the change in private-sector employment between 2010 and 2018 as dependent variable (total in Columns 1 to 3, service sector in Columns 4 to 6, and manufacturing sector in Columns 7 to 9). The left columns include a set of neighborhood characteristics observed in 2010; the middle columns control for the spatial spillovers of the public employment shocks and the city fixed effects; the right columns control for the employment growth rate between 2006 and 2010. The unit of estimates in Panel B is the change in private-sector employment per one additional unit of public-sector employment. Heteroskedasticity-robust standard errors are (clustered at the neighborhood level for Panel A and) reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

	(1)	(2)	(3)
	Net Inflow of Migrants from:		
	Same City	Different Cities	
		non-SMA	SMA
Treated	2.08***	0.74**	0.65***
	(0.40)	(0.32)	(0.04)
$0 - 5$km	0.02	−0.03	0.00
	(0.15)	(0.03)	(0.02)
$5 - 10$km	−0.08	0.00	0.00
	(0.06)	(0.02)	(0.01)
$10 - 15$km	−0.03	0.00	0.01
	(0.05)	(0.01)	(0.01)
$15 - 20$km	−0.02	−0.02*	0.00
	(0.06)	(0.01)	(0.01)
Observations	1,011	1,011	1,011

Table 3: Migratory Responses by the Origins of Migrants

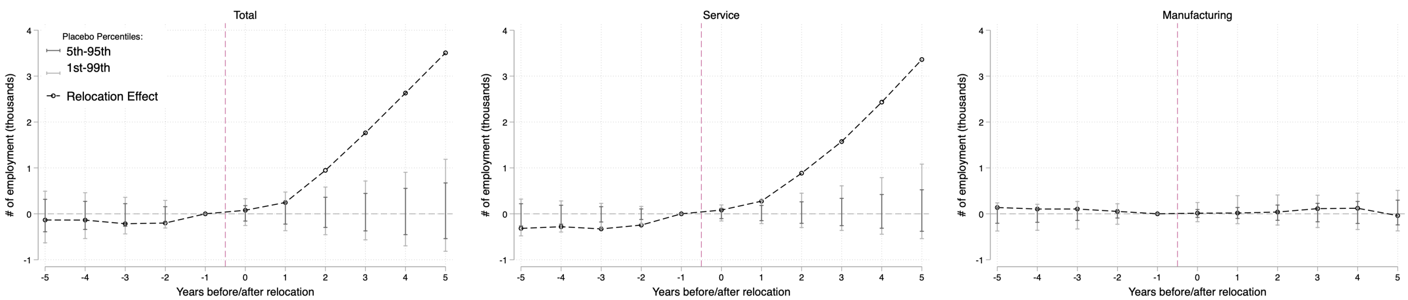
Notes: This table estimates the effects of public-sector employment on migratory responses based on Equation 3. In Column 1, the dependent variable is the net inflow of migrants from the same city where each neighborhood is part of; the dependent variable is the net inflow of migrants from non-Seoul Metropolitan Area (SMA) cities in Column 2 and from SMA in Column 3. Across columns, the city fixed effects and a set of neighborhood-level control variables are included. Heteroskedasticity-robust standard errors are reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

		(1)	(2)	(3)	(4)
		Residence in 2010:			
		SMA		non-SMA	
		#	per employee	#	per employee
		A. Public Employees			
# Employees		15,801		120,591	
# Household Members	Total	8,220	0.52	154,068	1.28
	Service	2,654	0.17	55,190	0.46
	Manufacturing	309	0.03	11,184	0.09
		B. Private Employees			
# Employees		7,698		186,216	
# Household Members	Total	6,394	0.83	21,437	1.16
	Service	2,780	0.36	74,554	0.40
	Manufacturing	845	0.11	20,000	0.11

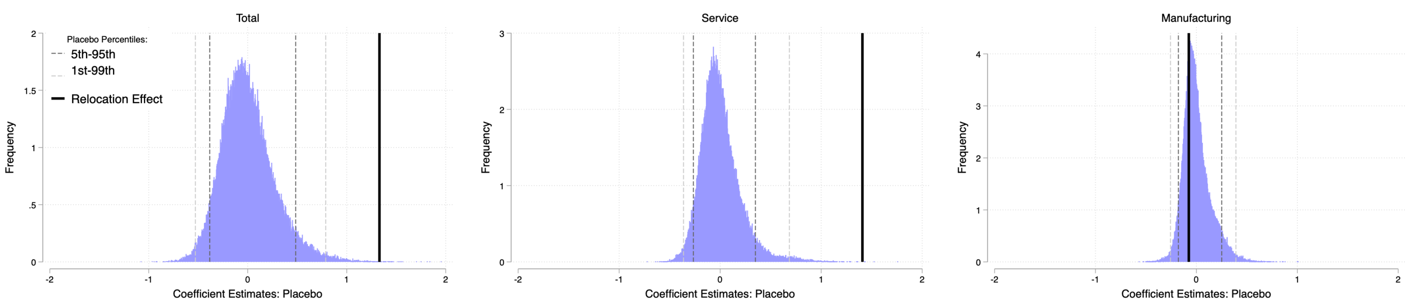
Table 4: The Distribution of Private and Public Employees and their Household Members

Notes: Based on the 2015 Population Census of South Korea, this table tabulates the number of public employees (Panel A) and private employees (Panel B) and their family members (in the labor force and older than 15 years of age) living in the district which experienced the arrival of additional public employees between 2010 and 2015 due to the relocation of public entities by their residence in 2010. The total number of households members includes both employed and unemployed. The next two rows correspond to the number of these household members working in the service and manufacturing sectors. All the individuals counted under *SMA* migrated sometime in between 2010 and 2015 to one of the districts where the public entities relocated outside of the Seoul Metropolitan Area (SMA). The individuals counted under *non-SMA* includes both those who migrated into these districts and those who had already lived there and continued their residency through 2015.

(a) Event Study Coefficients



(b) Difference-in-Difference Results



(c) Local Employment Multiplier

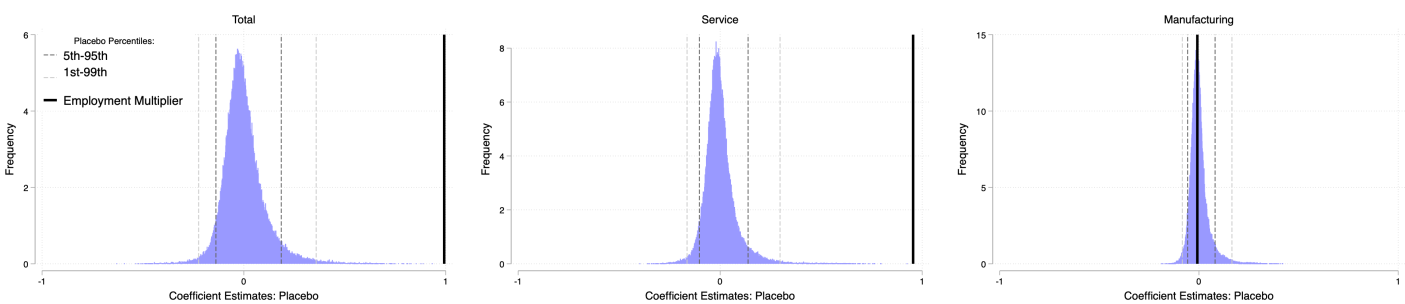


Figure 3: Placebo-Based Inference Robust to Small Number of Treated Neighborhoods

Notes: This figure plots the results implementing a variant of Fisher's permutation or randomization test to address an inference issue arising from the small number of treated neighborhoods. All of the panels display the 95% and 99% confidence intervals of the placebo estimates as well as the estimates of the event study coefficients (Panel A), the relocation effect (Panel B), and the local employment multiplier (Panel C) for the total private employment (left), the service sector (middle), and the manufacturing sector (right).

	(1) Total	(2) Service	(3) Manufacturing
Panel A: Changes between 2006 and 2010			
Treated	-0.05	0.00	-0.03^{**}
	(0.04)	(0.03)	(0.02)
Observations	1,069	1,069	1,069
Panel B: Using Runner-up Neighborhoods as Treated			
$Treated \times Post$	0.23	0.19	0.27
	(0.24)	(0.13)	(0.17)
Observations	9,503	9,503	9,503

Table 5: Two Placebo Tests

Notes: This table summarizes the results of two placebo results. In Panel A, we estimate Equation 3 using the change in the private-sector employment as dependent variable between 2006 and 2010 (total in Column 1, service in Column 2, and manufacturing in Column 3). In Panel B, we use the runner-up neighborhoods, which barely missed the selection, and their nearby neighborhoods and estimate Equation 4. In the appendix, Figure A.6 plots the event study coefficients estimating Equation 1. Heteroskedasticity-robust standard errors are (clustered at the neighborhood level for Panel B and) reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

Appendix: Additional Figures and Tables

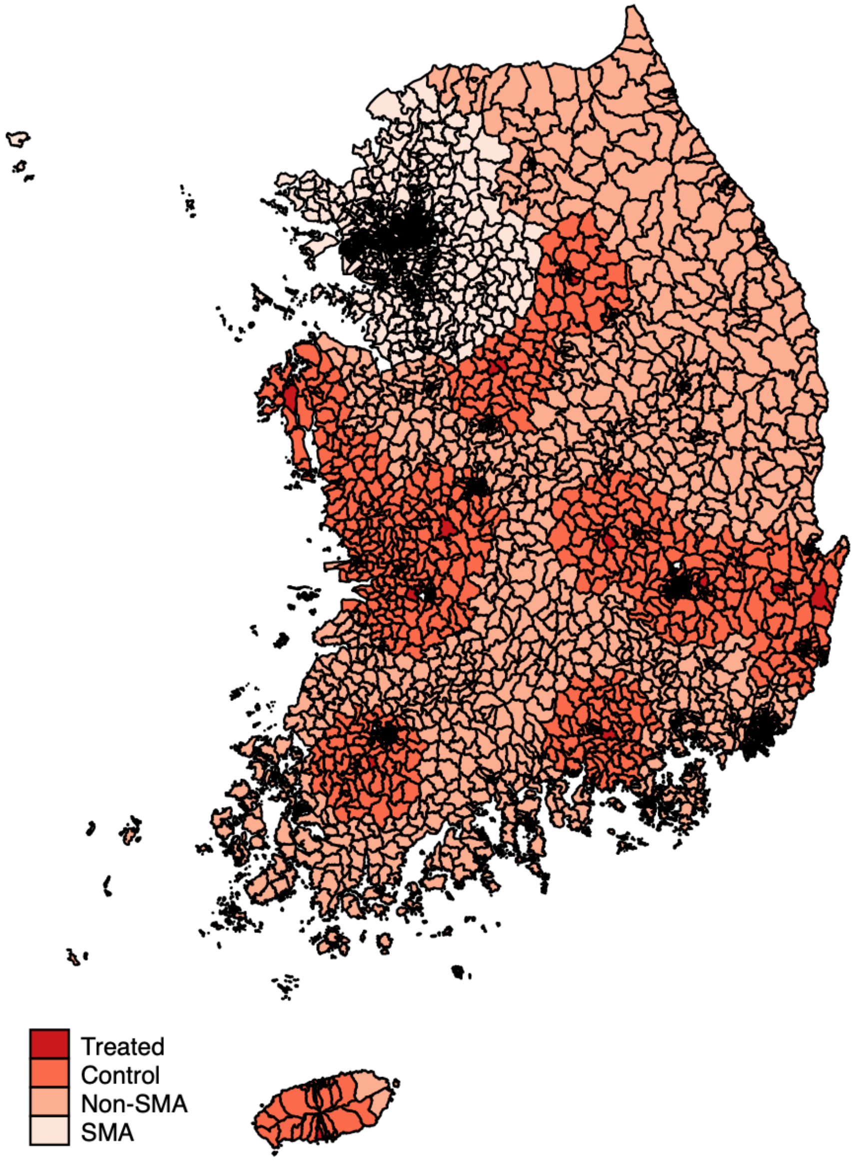


Figure A.1: Map of Treated vs. Control vs. Excluded Neighborhoods in South Korea

Notes: This map displays the neighborhood boundaries and the groups of neighborhoods in different shades: the treated neighborhoods (darkest), the control neighborhoods under all the sample selection criteria (darker), all the neighborhoods located outside the Seoul Metropolitan Area (SMA) not part of our main analysis (lighter), and the SMA neighborhoods (lightest; located in the northeast of South Korea).

	(1) No Sample Restriction	(2) Non-SMA	(3) within 30km	(4) Final Sample
Total Population	14,241	10,571	12,903	12,153
Population Density	8,001	4,194	5,772	5,018
Female Share	0.508	0.511	0.507	0.508
Avg. Household Size	2.606	2.508	2.597	2.593
Elderly Share (65+)	0.197	0.238	0.201	0.208
Share of Working Pop. (15-65)	0.661	0.632	0.661	0.653
Share of Female Workers	0.447	0.449	0.44	0.442
Share or Service Workers	0.765	0.749	0.739	0.738
Δ Employment (2010-2006)	0.142	0.132	0.147	0.132
Observations	3,357	2,315	1,456	1,069

Table A.1: Summary Statistics under Different Sample Restrictions

Notes: This table reports the average values of local characteristics observed in 2010 for all the neighborhoods of South Korea without any sample restriction (Column 1), the neighborhoods outside of the Seoul Metropolitan Area (SMA) (Column 2), the neighborhoods within 30 kilometers away from the treated neighborhoods (Column 3), and the final sample used for the main analysis (Column 4) which further excludes the treated neighborhoods and their nearby areas if they coincided with other developmental plans, not part of the national policies.

	(1) Treated	(2) Control	(3) Difference
Total Population	17,486	12,057	$5,429^{**}$
Population Density	3,169	5,051	-1,882
Female Share	0.5	0.508	-0.008
Avg. Household Size	2.825	2.589	0.236^{***}
Elderly Share (65+)	0.179	0.209	-0.03
Share of Working Pop. (15-65)	0.658	0.653	0.005
Share of Female Workers	0.438	0.442	-0.005
Share or Service Workers	0.724	0.738	-0.014
Δ Employment (2010-2006)	0.141	0.131	0.009
Observations	19	1,050	1,069

Table A.2: Summary Statistics: Treated vs. Control Neighborhoods

Notes: This table compares the average values of the baseline local characteristics observed in 2010 between the treated (Column 1) and control (Column 2) neighborhoods. Column 3 reports the difference: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

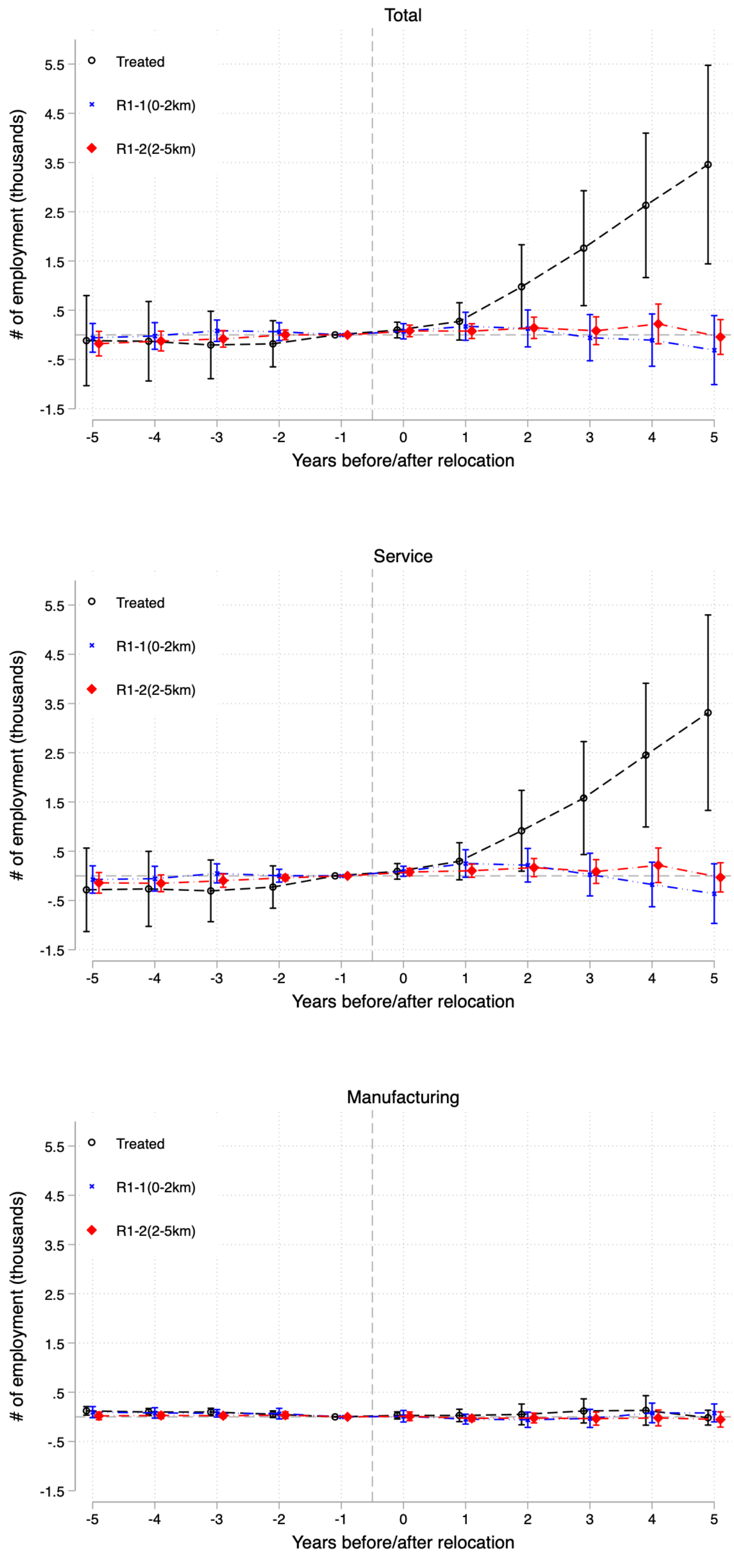


Figure A.2: Event Study Results Splitting Ring 1 (0-5km) into 0-2km and 2-5km

Notes: This figure plots the event study coefficients based on Equation 2 for the treated and the immediately adjacent neighborhoods located within 5 kilometers while further splitting them into 0-2 kilometer and 2-5 kilometer groups. The dependent variables are the total private employment (top), service (middle), and manufacturing (bottom). Error bars indicate the 95% confidence intervals, constructed based on the heteroskedasticity-robust standard errors clustered at the neighborhood level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Total			Service			Manufacturing	
$Treated \times Post$	0.25**	0.18**	0.17**	0.41**	0.33**	0.33**	0.05	-0.03	-0.05
	(0.09)	(0.03)	(0.08)	(0.15)	(0.05)	(0.12)	(0.07)	(0.03)	(0.06)
ϕ_i	Y	Y	Y	Y	Y	Y	Y	Y	Y
$X_i \times t$		Y	Y		Y	Y		Y	Y
$\psi_{c(i),t}$			Y			Y			Y
Observations	13,897	13,897	13,897	13,897	13,897	13,897	13,897	13,897	13,897

Table A.3: Difference-in-Difference Estimates based on Log Transformed Values of Private Employment

Notes: This table reports the estimation results using the log transformed values of the private employment (total in Columns 1 to 3, service in Columns 4 to 6, and manufacturing in Columns 7 to 9) based on Equation 4, a difference-in-difference version of Equation 1. The left columns include the neighborhood fixed effects; the middle columns controls for a linear time trend for each of the baseline characteristics observed in 2010; the right columns include the city-by-year fixed effects. Heteroskedasticity-robust standard errors are clustered at the neighborhood level and reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Net Inflow	By Age Group:											
	Total	≤ 20	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70+
Treated	3.47**	0.92**	0.21**	0.33**	0.48**	0.42**	0.32**	0.25**	0.20**	0.15**	0.08**	0.05**	0.07**
	(0.35)	(0.10)	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
0–5km	-0.01	0.01	0.01	-0.00	-0.01	0.00	0.01	-0.00	-0.01	−0.01*	-0.01	-0.00	-0.00
	(0.15)	(0.05)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
5–10km	-0.07	-0.00	-0.01	−0.02*	-0.01	-0.00	-0.00	-0.01	-0.01	-0.00	-0.00	-0.00	−0.01**
	(0.07)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
10–15km	-0.03	-0.02	0.00	0.00	-0.00	-0.01	-0.01	-0.00	0.00	0.00	-0.00	-0.00	0.00
	(0.07)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
15–20km	-0.03	-0.01	-0.00	-0.00	-0.00	-0.01	-0.00	-0.00	0.00	-0.00	-0.00	-0.00	-0.00
	(0.05)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	1,030	1,030	1,030	1,030	1,030	1,030	1,030	1,030	1,030	1,030	1,030	1,030	1,030

Table A.4: Migratory Responses by Age Group

Notes: This table estimates the effects of public-sector employment on migratory responses based on Equation 3. In Column 1, the dependent variable is the net inflow of migrants. In the other columns, the dependent variables are age-specific. Across columns, the city fixed effects and a set of neighborhood-level control variables are included. Heteroskedasticity-robust standard errors are reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

	(1)	(2)	(3)
Relocation Type	Employment Multipliers		
Single-Entity Relocation (SR)	-0.23	-0.14	-0.12
	(0.15)	(0.11)	(0.12)
Relocation to Old Town (OT)	0.34	0.28	0.39**
	(0.25)	(0.21)	(0.18)
Relocation to New Town (NT)	1.01***	1.06***	1.06***
	(0.20)	(0.20)	(0.19)
X_i	Y	Y	Y
$\Delta PE_{N_i(r)}, \phi_{c(i)}$		Y	Y
$\Delta Y_{i,2010-2006}$			Y
Observations	1,069	1,069	1,069

Table A.5: Local Employment Multiplier by Relocation Type

Notes: This table reports the estimation results based on a modified version of Equation 3 in which local employment multiplier varies by relocation type. Column 1 includes a set of neighborhood characteristics observed in 2010; column 2 controls for the spatial spillovers of the public employment shocks and the city fixed effects; Column 3 controls for the employment growth rate between 2006 and 2010. Heteroskedasticity-robust standard errors are reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

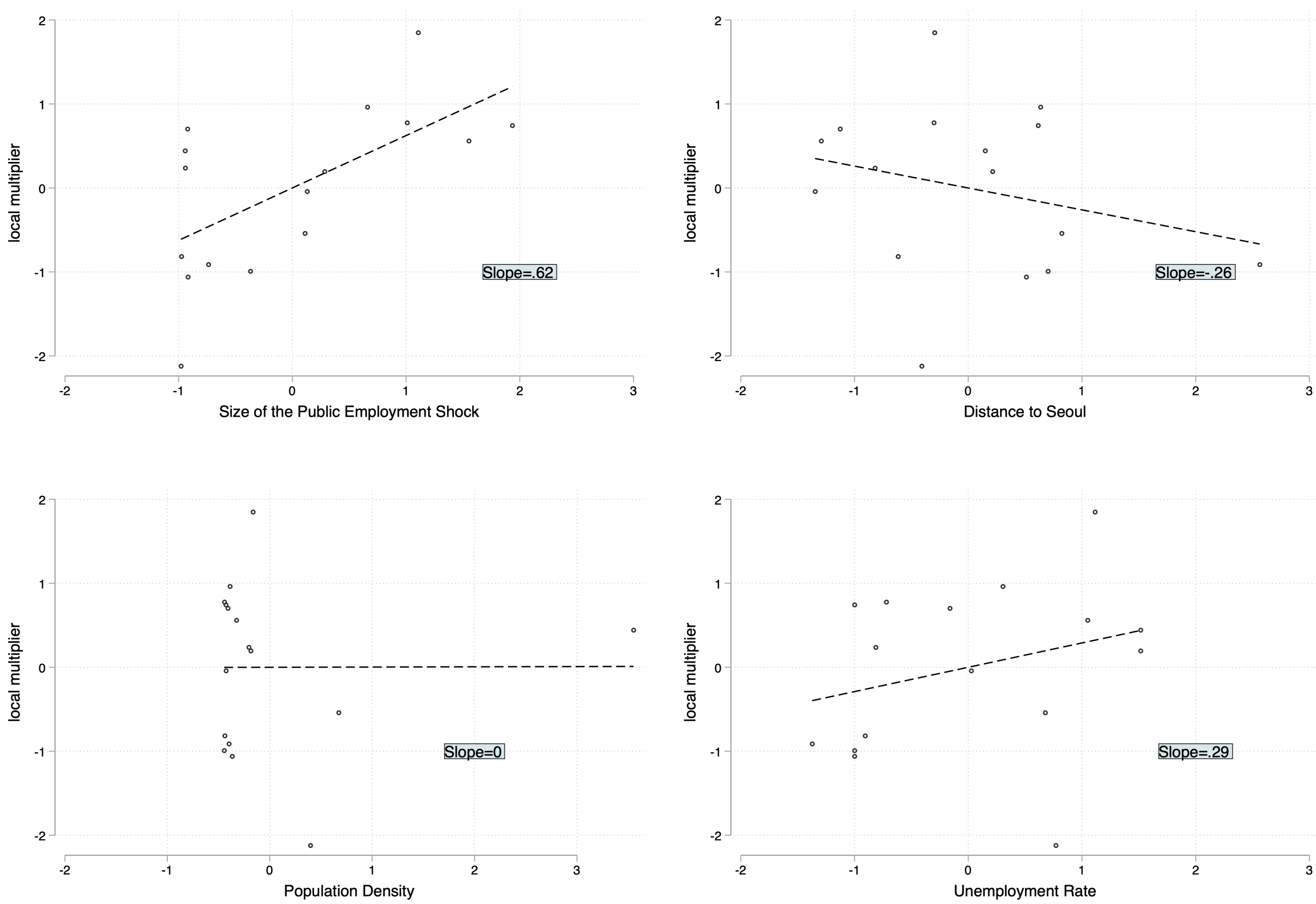


Figure A.3: Local Employment Multiplier vs. Local

Notes: This figure plots the local employment multipliers estimated by pooling each treated neighborhoods and its neighboring areas at a time against the size of the public employment shock (upper-left), distance to Seoul (upper-right), population density measured in 2010 (bottom-left), and umployment rate measured in 2010 (bottom-right). The data points are normalized to zero.

	(1) No Sample Restriction	(2) Excluding SMA	(3) Within 30km	(4) Final Sample
Total Population	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Population Density	-0.03^{**}	-0.04	-0.05	-0.05
	(0.02)	(0.05)	(0.06)	(0.08)
Female Share	-0.05	-0.07	-0.17	-0.23
	(0.07)	(0.09)	(0.17)	(0.23)
Avg. Household Size	0.01	0.02	0.03	0.04
	(0.01)	(0.02)	(0.03)	(0.03)
Housing Stock	-0.01	-0.03	-0.05	-0.07
	(0.02)	(0.03)	(0.06)	(0.07)
Elderly Share (65+)	0.04	0.11	0.15	0.12
	(0.08)	(0.12)	(0.19)	(0.25)
Share of Working Pop. (15-65)	0.02	0.05	0.05	-0.03
	(0.06)	(0.10)	(0.15)	(0.20)
Share of Female Workers	0.01	0.01	0.01	0.02
	(0.01)	(0.02)	(0.03)	(0.04)
Share of Service Workers	-0.00	-0.00	0.00	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)
Unemployment Rate	-0.18	-0.25	-0.65	-0.84
	(0.17)	(0.27)	(0.46)	(0.55)
F Statistic	1.08	1.38	1.12	1.33
R^2	0.08	0.10	0.12	0.13
Observations	3,357	2,315	1,456	1,069

Table A.6: Determinants of Relocation Neighborhood Selection

Notes: This table reports the results estimating Equation 5 to investigate whether pre-treatment characteristics observed in 2010 predict the neighborhoods to which public entities relocated under different sample selection criteria: all the neighborhoods of South Korea without any sample restriction (Column 1), the neighborhoods outside of the Seoul Metropolitan Area (SMA) (Column 2), the neighborhoods within 30 kilometers away from the treated neighborhoods (Column 3), and the final sample used for the main analysis (Column 4) which further excludes the treated neighborhoods and their nearby areas if they coincided with other developmental plans, not part of the national policies.

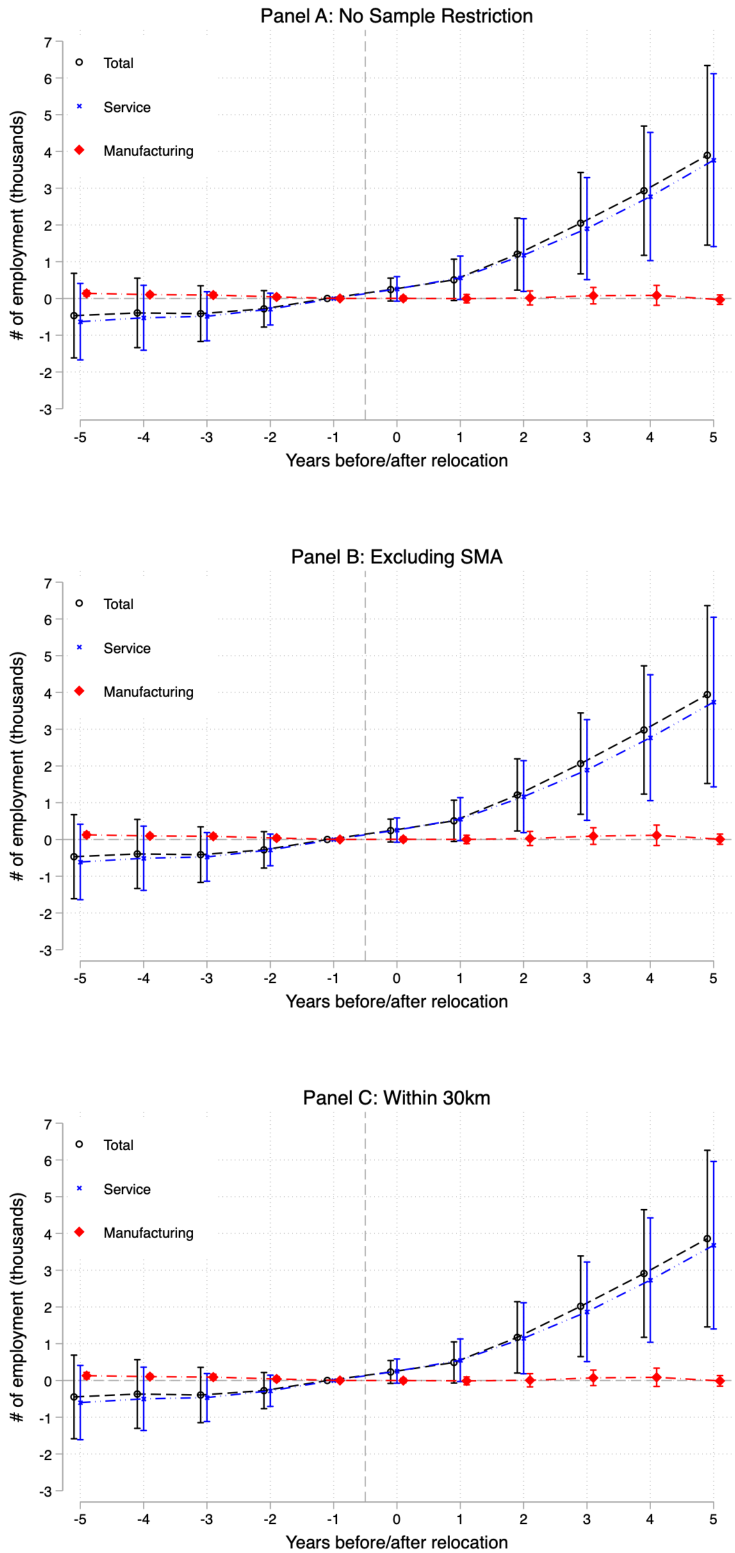


Figure A.4: Robustness to Sample Selection: Event Study Results

Notes: This figure summarizes the event study results based on Equation 1 under alternative sample selection criteria: all the neighborhoods of South Korea without any sample restriction (top), the neighborhoods outside of the Seoul Metropolitan Area (SMA) (middle), and the neighborhoods within 30 kilometers away from the treated neighborhoods (bottom). Error bars indicate the 95% confidence intervals, constructed based on the heteroskedasticity-robust standard errors clustered at the neighborhood level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	No Sample Restriction			Excluding SMA			Within 30km		
	Total	Service	Manufacturing	Total	Service	Manufacturing	Total	Service	Manufacturing
				Panel A: Difference-in-Difference Estimates					
Treated	1.86*	1.94**	-0.09	1.87*	1.91**	-0.07	1.81*	1.88**	-0.09
	(0.96)	(0.92)	(0.07)	(0.96)	(0.91)	(0.08)	(0.95)	(0.89)	(0.07)
Observations	43,641	43,641	43,641	30,095	30,095	30,095	18,928	18,928	18,928
				Panel B: Employment Multiplier Estimates					
$Treated \times Post$	0.97**	0.90**	0.00	1.00*	0.93**	0.01	0.98**	0.92**	-0.00
	(0.16)	(0.15)	(0.02)	(0.16)	(0.15)	(0.02)	(0.16)	(0.15)	(0.02)
Observations	3,357	3,357	3,357	2,315	2,315	2,315	1,456	1,456	1,456

Table A.7: Robustness to Sample Selection: DID and Long Difference Estimates

Notes: This table summarizes the results estimating Equation 4 in Panel A and Equation 3 in Panel B under alternative sample selection criteria: all the neighborhoods of South Korea without any sample restriction (Columns 1-3), the neighborhoods outside of the Seoul Metropolitan Area (SMA) (Columns 4-6), and the neighborhoods within 30 kilometers away from the treated neighborhoods (Columns 7-9). Heteroskedasticity-robust standard errors are (clustered at the neighborhood level for Panel A and) reported in parentheses: * Significant at the 10 percent level, ** Significant at the 5 percent level, and *** Significant at the 1 percent level.

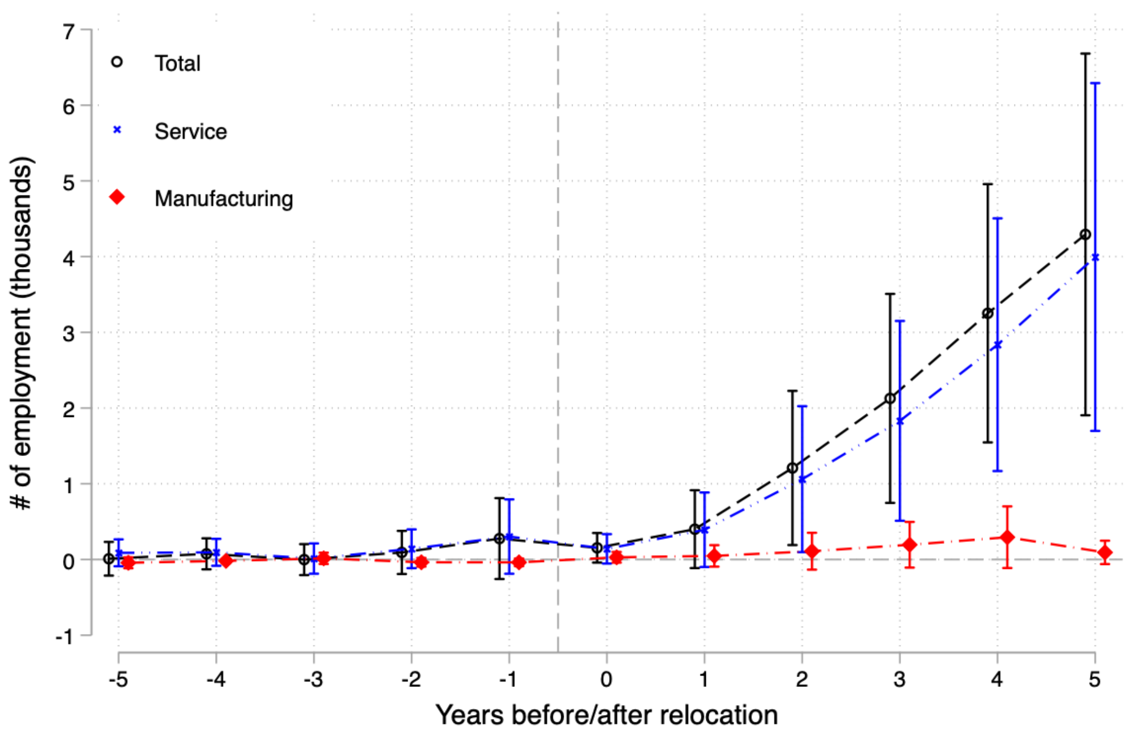


Figure A.5: Event Study Results Robust to Heterogeneous Treatment Effect

Notes: This figure plots the event-study coefficients estimated based on Eq. 1 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021) for the total private employment in hollow circle, service in x's, and manufacturing in diamond. Error bars show 95% confidence intervals, constructed based on the bootstrapped heteroskedasticity-robust standard errors clustered at the neighborhood level.

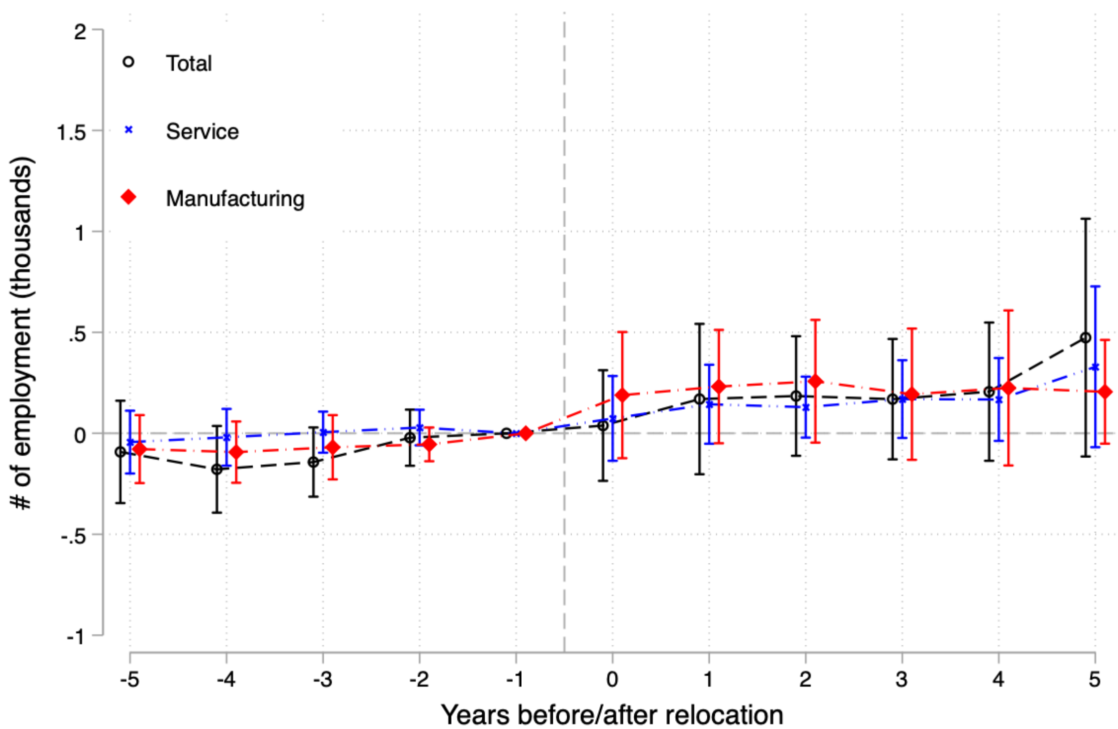


Figure A.6: Event Study Results using Runner-Up Neighborhoods as Treated

Notes: This figure plots the event study coefficients estimating Equation 1 based on the runner-up neighborhoods, which barely missed the selection, and their nearby neighborhoods for the total private employment in hollow circle, service in x's, and manufacturing in diamond. Error bars indicate the 95% confidence intervals, constructed based on the heteroskedasticity-robust standard errors clustered at the neighborhood level.