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Municipal Mergers and Capitalization: Evaluating the Heisei Territorial Reform in Japan

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Abstract

Based on the theory of land price capitalization, we evaluate the effects of municipal mergers by exploiting extensive municipal mergers in the Heisei territorial reform and dataset of land prices compiled by the Japanese government. We allow the effects of municipal mergers to vary over time—both *ex-ante* (as anticipation effects) and *ex-post* (as realized effects). Through a series of estimation, we find a robust pattern of the effect of municipal mergers. The *ex-ante* or anticipation effect gradually grows to culminate in the year just before the actual amalgamation. Once the merger is realized, the *ex-post* effect, starting with a smaller effect than the maximal anticipation effect in the previous year, gradually declines toward zero. This result then implies that the Heisei territorial reform yielded only temporal benefits that dissipated in the long run.

Keywords: capitalization, land prices, municipal amalgamation, municipal merger, Japan

JEL Classification: J45, J51, H72, H77

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1. Introduction

Most industrial countries have restructured their local government systems through a series of subnational government mergers. In doing so, policy makers typically anticipate that merging fiscal units would exploit economies of population scale in public service production[1], so that the local public sector could attain cost savings or efficiency gains (Blom-Hansen et al., 2016).[2] Taking advantage of the developments of municipal mergers in various countries, the literature has been examining whether municipal mergers reduce per capita local government expenditures. The results are mixed. For example, mergers are found to reduce per capita expenditure in Israel (Reingewertz, 2012) and Denmark (Hansen et al., 2014). Conversely, estimations show opposite effects for Japan (Hirota and Yunoue, 2013; Miyazaki, 2013), Switzerland (Lüchinger and Stutzer, 2002), and the states of Baden-Württemberg (Fritz, 2011) and Saxony (Roesel, 2017), Germany. Meanwhile, no significant effects were obtained for studies on Finland (Moisio and Uusitalo, 2013), the Netherlands (Allers and Geertsema, 2016), the state of Brandenburg in Germany (Blesse and Baskaran, 2016), and again Denmark (Blom-Hansen et al., 2016).[3]

In fact, it is not surprising that municipal mergers do not always lead to reductions in the per capita expenditure, *even when* economies of population scale *are* present. Following Duncombe and Yinger (1993), economies of population scale exist if per capita cost is decreasing for the local population, *provided that all other variables in the local government cost function are maintained constant*. Such "other" variables typically include the level of public service, levels of input prices (e.g., wages of local government employees), and local characteristics that affect the technology of public service delivery (e.g., surface area and demographics). Obviously, an amalgamation would change all these variables by construction. It alters not only the population of a given fiscal unit, but also its surface area and demographics. Additionally, if the wage systems of municipal employees were different before merging, they are typically adjusted in a new fiscal unit. Furthermore, mergers also change political landscape (Elklit and Kjaer, 2009; Kjaer et al., 2010; Hansen, 2013; Lassen and Serritzlew, 2011; Felix, 2017; Yamada, 2018),[4]

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1 Although the literature uses "economies of scale," it is obvious that it refers to "economies of *population* scale," as these concern changes in *per capita* expenditures. For the different dimensions of economies of scale in public-sector production, see Duncombe and Yinger (1993).

2 For example, such efficiency gains were taken for granted for the 2007 amalgamation reform in the Netherlands, although they were seldom mentioned explicitly (Allers and Geertsema, 2016). Similarly, the territorial reform by the German state of Brandenburg in the 2000s regarded municipal mergers as an instrument to achieve cost savings for small municipalities (Blesse and Baskaran, 2016).

3 However, most studies obtained negative effects on administrative expenses (Fritz, 2011; Blesse and Baskaran, 2014; Blom-Hansen et al., 2014; Allers and Geertsema, 2016).

4 Studies show that municipal mergers change electoral behavior (Elklit and Kjaer, 2009; Felix, 2017; Van Houwelingen, 2017), the perceived influence of specific council members (Kjaer et al., 2010), local political trust (Hansen, 2012), internal political efficacy or individual citizens' beliefs that they are

which would result in a different level and spatial distribution of public service (Steiner and Kaiser, 2017; Allers and Geertsema, 2016; Yamada, 2018).[5] In addition, enlarged fiscal units may be assigned new expenditure functions by the upper-level authorities (Allers and Geertsema, 2016) and/or voluntarily expand their scope of public services, yielding "zoo effects" (Oates, 1988). Finally, merging with other municipalities may provide opportunities to adopt new practices that would improve or deteriorate cost efficiency (Hansen et al., 2014). Contrary to the claim made in the literature, we cannot easily identify economies of population scale, let alone welfare gains, by examining how per capita local government expenditure changes after amalgamations. While it is important to examine such changes *per se*, existing reduced-form studies on per capita expenditure hardly offer a clear economic interpretation of merger effects (Weese, 2015).

In this study, instead of using per capita expenditure, we utilize land prices as the outcome variable to evaluate the effects of municipal mergers, basing our analysis on the theory of capitalization (Bransington, 1997; Hu and Yinger, 2008; Duncombe et al., 2016).[6] While we also rely on reduced-form estimation, our estimates with land prices allow us to conduct a welfare evaluation in contrast to those with per capita expenditure that do not. In particular—as we will show with the capitalization framework—our reduced-form regression yields a sufficient statistic à la Chetty (2009) that corresponds to the net marginal benefits of municipal merger.

The municipal mergers we investigate are those observed during the period of the *Heisei* territorial reform in Japan.[7] From the late 1990s to the 2000s, the Japanese government

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competent to understand and take part in politics (Lassen and Serritzlew, 2011), and the spatial distribution of voting power within a region (Yamada, 2018).

[5] Indeed, Steiner and Kaiser (2016) provide evidence of improved public service delivery in Swiss municipalities, although Allers and Geertsema (2016) show no supporting evidence of improved public services in Dutch municipalities. In a study on Japanese municipalities, Yamada (2016) demonstrates that the level of public services is expected to decrease. A common pool effect may also contribute to changes in public services. As such, municipalities may opportunistically increase the level of public services by increasing spending through debt financing *before* amalgamation; by doing so, they can spread the burden of future debt payments onto the residents of the merging jurisdiction (Hinnerich, 2009; Jordahl and Liang, 2009; Blom-Hansen, 2010; Hansen, 2014; Saarimaa and Tukiainen, 2015).

[6] A number of studies have relied on the theory of capitalization it to assess a variety of policy issues, including local taxation (e.g., Oates, 1969), impact fees (e.g., Ihlanfeldt and Shaughnessy, 2004), public school services (e.g., Nguyen-Hoang and Yinger, 2011), infrastructure benefits (e.g., Haughwout, 2002), intergovernmental grants (e.g., Hilber et al., 2011), and debt policy (e.g., Banzhaf and Oates, 2008). However, only a few pioneering studies have applied it to the evaluation of local government mergers (Bransington, 1997; Hu and Yinger, 2008; Duncombe et al., 2016), focusing on the specific cases of U.S. school districts. Allers and Geertsema (2016) examine the merger effects on housing prices in the Netherlands. However, they did so to examine the effect of the mergers on public services, treating the price as a proxy for public service level.

[7] "Heisei" refers to the current imperial era (1989−) in Japan, after the Showa era (1926−1989). The considerable reduction in municipalities through amalgamations in the 2000s is often called the "Great Heisei Amalgamation" (*Heisei no daigappei*). There were also two waves of large municipal mergers in different imperial eras (i.e., the Meiji (1868−1912) and Showa eras). Weese et al. (2015) examine the welfare effects of the *Meiji* municipal amalgamations.

enthusiastically promoted municipal mergers by offering generous incentives for merged municipalities. As a result, it reduced the number of municipalities by 47%, from 3,229 at the end of FY1999 to 1,727 at the end of FY2010. To assess the effect, we utilize a large dataset of individual land parcels, provided by the Ministry of Land, Infrastructure, Transport and Tourism (MLIT). The dataset is the most comprehensive source of Japanese land prices in terms of year-to-year continuity, geographical coverage, and parcel specific information; it has been claimed to be unparalleled to any analogous dataset in other countries (Nakagawa et al., 2009). The combination of the large wave of municipal mergers in the 2000s and the availability of the large dataset for the prices of individual land parcels provides us an advantage to explore welfare effects of municipal mergers.

In addition, the Heisei reform gives us an opportunity to shed light on two sorts of interesting effects. First, we could examine potential anticipation effects of municipal mergers. If individuals are forward-looking with access to relevant information, they would change their behavior in response to the anticipation of a policy change in the future (Malani and Reif, 2015). The Heisei mergers constitute a good example to examine such an effect: many mergers in the reform period took years to complete the process; information on the process toward the mergers was concurrently made public; and local residents obviously paid attention to such information.

Second, we could also examine the effect of *rushing* into mergers. During the reform period, the central government offered generous incentives to encourage municipalities to merge. It initially planned to terminate the incentives at the end of FY2004 (i.e., March 31, 2005), but, in May of 2005, it postponed the termination one more year to the end of FY2005 (i.e., March 31, 2006). We may then regard the mergers realized in FY2004 as those successfully concluded as originally planned. In contrast, we could regard the mergers realized in FY2005 (i.e., after FY2004) as those that, while failing to realize themselves according to the original schedule, rushed to conclude themselves in time for the extended deadline. We could therefore capture the effect of this rushing by differentiating the effects between mergers in FY2004 and FY2005.

The remaining paper is organized as follows. Section 2 revisits the theory of capitalization and extends it to the case of municipal amalgamation, providing a clear interpretation of the effects of amalgamation on land prices. It also provides a corresponding regression model. Section 3 describes the institutional background, the sample we use, and the sources of our dataset. Section 4 provides the estimation results and discusses their relevance. Finally, Section 5 concludes the paper.

2. Model

2.1 Conceptual framework

We first discuss a framework to justify our application of the capitalization hypothesis to evaluate the effects of municipal mergers. In so doing, we extend the standard model of rent-capitalization to interpret the impacts of municipal mergers on land prices. Hu and Yinger (2008) develop such a model for the consolidation of U.S. school districts, building on the model by Yinger et al. (1988). However, since we employ data on individual land parcels to estimate merger effects, it would be useful to derive an equation that allows interpreting the effect of mergers on the prices of *individual* land parcels in a heterogeneous economy. For this purpose, we present a model that modifies the hedonic model by Rosen (1974) to allow for the existence of a local public sector as in Brueckner (1979, 1982), as well as the effects of municipal amalgamation, as in Hu and Yinger (2008).

In this formulation, there are different types of residents indexed with i. Type i individual consumes services from a fixed land parcel (or housing service) with multi-dimensional property $\boldsymbol{q}_i = \text{vec}[q_{1i}, q_{2i}, \ldots, q_{Bi}]$ for rental (hedonic) price $R(\boldsymbol{q}_i)$. The utility function is given by $U^i = U^i(x_i, \boldsymbol{q}_i; z, a)$ where x_i is the consumed level of a numéraire, z the level of local public service, and a denotes local characteristics. The individual provides a unit of labor in exchange for wage W_i, and pay tax T_i. The budget constraint is then given as $x_i + R(\boldsymbol{q}_i) = W_i - T_i$. Assuming that the local economy is small and open, we characterize the migration equilibrium for type i resident as

$$U^i(W_i - T_i - R(\boldsymbol{q}_i), \boldsymbol{q}_i, z, a) = \psi_i, \tag{1}$$

where ψ_i is the exogenous equilibrium utility level for the type i individual. We then totally differentiate Eq. (1) by changing all variables *except* $\boldsymbol{q}_i$ and obtain

$$dR = \frac{U_z^i}{U_x^i}dz + dW_i - dT_i + \frac{U_a^i}{U_x^i}da. \tag{2}$$

The standard model of capitalization suggests that changes in public service z, local characteristics a, and/or local population n would affect resident preferences, firms' technology, and local government revenues (Brueckner, 1979, 1982; Roback, 1982). Since such changes affect individual wages W_i and taxes T_i, we can express them as being affected by the three factors above (i.e., $W_i = w^i(z, a, n)$ and $T_i = t^i(z, a, n)$).

We now consider the annexation of a municipality by another municipality. This annexation necessarily changes population n and local characteristics (typically, surface areas) a of the annexing (amalgamated) municipality. It may also change political landscape, yielding a change in public service level z. Indexing the amalgamation (annexation) by M, we may therefore express

$z = z(M)$, $a = a(M)$, and $n = n(M)$. For exposition, we assume that M is continuous and $z(\cdot)$, $a(\cdot)$, and $n(\cdot)$ are differentiable, following Hu and Yinger (2008).[8]

This then allows us to express individual wages and taxes as $W^i = \omega^i(M) \equiv w^i(z(M), a(M), n(M))$, and $T^i = \tau^i(M) \equiv t^i(z(M), a(M), n(M))$, and the effects of mergers on the rental prices of land parcels as

$$\frac{dR_i}{dM} = \frac{U_z^i}{U_x^i}\frac{dz}{dM} + \frac{dW_i - dT_i}{dM} + \frac{U_a^i}{U_x^i}\frac{da}{dM}. \tag{3}$$

This equation shows that the response of the rent for individual land parcels to municipal amalgamation equals the combined value of marginal benefits of public service, an increase in net-of-tax wages ($W-T$), and marginal benefits of local characteristics caused by municipal amalgamation. In other words, the changes in the rent of a given land parcel caused by municipal amalgamation equal changes in the benefits enjoyed by the consumer that resides on that parcel.

Note that we can derive the land rent function or bit-rent function from Eq. (1) as

$$R_i = R(\boldsymbol{q}_i, W_i - T_i,\ z,\ a, \psi_i). \tag{4}$$

Substituting $z = z(M)$, $a = a(M)$, $n = n(M)$, $W^i = W^i(\cdot)$, and $T^i = T^i(\cdot)$ into Eq. (4) yields an reduced form:

$$\begin{aligned} R_i &= \lambda^i(M, \boldsymbol{q}_i, \psi_i) \\ &\equiv r^i\{\boldsymbol{q}_i, W^i(z(M), a(M), n(M)) - T^i(z(M), a(M), n(M)),\ z(M),\ a(M)), \psi_i\}. \end{aligned} \tag{5}$$

This shows that, provided that post-merger changes in z, a, and n originate *solely* from the merger, we can interpret the coefficient of M from the estimation of $\lambda^i(\cdot)$ as the marginal welfare increase of the individual dweller of the land parcel.

2.2 Regression models and estimation

We next specify an empirical equivalent of Eq. (5) to estimate the effects of municipal mergers on land rent given as an empirical equivalent of Eq. (3). We choose a linear regression model that takes advantage of a panel data for a large number of land parcels over relevant years. The specification is given as

$$\ln V_{it} = g(\boldsymbol{M}_{it}) + \sum_b \gamma_b \cdot q_{it}^b + \sum_j \eta_j \cdot w_{mt}^j + c_i + \epsilon_{it}, \tag{6}$$

where V_{it} is the price of land parcel i in year t; $g(\boldsymbol{M}_{it})$ represents various effects of municipal mergers with $\boldsymbol{M}_{it}$ is a vector of binary variables that should capture potential patterns of the merger effects; q_{it}^b's are characteristics specific to land parcel i in year t for **q** in Eq. (5); $w_{m,t}^j$'s are the j^{th} characteristic of municipality m where parcel i is located (or more wider areas where

[8] Our usage of "*M*" is analogous to the usage of "consolidation" variable "*C*" by Hu and Yinger (2008).

municipality m is located); c_i is the unobserved heterogeneity among individual parcels; and ϵ_{it} is idiosyncratic error. The Greek letters are the parameters to be estimated. We will elaborate on the elements of Eq. (6), especially those of $g(\boldsymbol{M}_{it})$ and $w_{m,t}^{j}$'s as follows.

2.3.1 Effects of municipal mergers

While the annexation that induces the change in rent in Eq. (3) is infinitesimal, the actual municipal merger is not. As such, $g(\boldsymbol{M}_{it})$ is an average increment in rent to be interpreted as net benefits and caused by a discrete change of municipal amalgamation. We consider three patterns of the realization of the merger effects.

First, the simplest pattern is the following constant effect:

$$g(\boldsymbol{M}_{it}) \equiv \beta_0 \cdot M_{it} \tag{7a}$$

where M_{it} is a binary variable that indicates if parcel i in year t is located in a municipality that has experienced mergers during the 2000s. In other words, M_{it} takes unity if parcel i in year t is located in municipality m for $t \geq t_m$, where t_m indicates the year when a municipality m started as a new merged fiscal unit. We call this "*constant ex-post effect*."

Secondly, while Eq. (7a) assumes that the effect of a merger is constant at β_0 since t_m, the effects may vary over the years. To capture such a pattern, we may then consider the following specification with lags:

$$g(\boldsymbol{M}_{it}) \equiv \beta_0 \cdot M_{it}^0 + \sum_{s=1}^{S} \beta_s \cdot M_{it-s}^0 \tag{7b}$$

where S is some finite number that may depend on the length of panel data and the timing of the treatment. Note that M_{it}^0 is different from M_{it} in that it only takes unity if land parcel i in year t is located in a municipality that completes its merger in the same year t. As such, unlike M_{it}, it takes zero if land parcel i in year t is located in a merged municipality after (and before) the year of its merger t_m (i.e., $t \neq t_m$). Then, β_k for $k \geq 0$ indicates the effect of merger in the k^{th} year since t_m. We then call β_ks for $k > 0$ "*variable ex-post effects*."

Third, if individuals are forward-looking with access to relevant information, they would change their behavior in response to the anticipation of a policy change in the future (Malani and Reif, 2015). Indeed, it is reasonable to assume that this anticipation applies to our case: almost all mergers in the Heisei reform took years to complete the process; information on the process toward the mergers was concurrently made public; and local residents apparently paid attention to the merger process. If this is the case, the effects of a merger may be realized even before the policy is adopted. We could then take advantage of a "quasi-myopic" model by Malani and Reif

(2015), which estimates anticipation effects in a non-parametric manner by including leads of M_{it}^0. We may consider two versions of the quasi-myopic model. The first is given as follows:

$$g(\boldsymbol{M}_{it}) \equiv \sum_{u=1}^{U} \beta_{-u} \cdot M_{it+u}^0 + \beta_0 \cdot M_{it} \tag{7c}$$

where U is some finite number that may again depend on the length of panel data and the timing of the treatment. This is a combination of constant *ex-post* effects and anticipation effects, β_ks for $k < 0$, or "*variable ex-ante effects*."

The other is the most general form, and given as

$$g(\boldsymbol{M}_{it}) \equiv \sum_{u=1}^{U} \beta_{-u} \cdot M_{it+u}^0 + \beta_0 \cdot M_{it}^0 + \sum_{s=1}^{S} \beta_s \cdot M_{it-s}^0 \tag{7d}$$

which also allows the *ex-post* effect to vary along the years in addition to the anticipation effects.

The estimates β_ks from (7a)–(7d) constitute the basis of our evaluation of the Japanese jurisdictional reform in the 2000s. As we briefly discussed in Introduction and will elaborate later, we also examine their variants, which reflect the timing of municipal mergers (FY2004 or FY2005), and the relative size of municipalities that participated in an amalgamation. We will obtain these estimates by performing a set of ordinary least squares (OLS) estimations on Eq. (6), with two sets of variance-covariance estimators clustered among land parcels within a municipal boundary before or after amalgamation. We may also interpret these estimates for relevant coefficients on $\boldsymbol{M}_{it}$ as the difference-in-differences (DD) estimator, since we utilize the fixed-effect model specification to obtain the within estimates in order to control for unobserved heterogeneity c_i.

2.3.2 Selection and municipality-wide covariates

While the central government provided municipalities with generous incentives to merge, the decisions to merge were made by municipalities themselves. Since the choices were voluntary, the selection of the treatment (mergers), and therefore its endogeneity, may be an issue. Notice that, unlike typical empirical studies on municipal mergers whose unit of observations is the municipality, our unit of observation is land parcel which obviously does not decide to merge themselves. In this sense, the conventional selection argument may not directly apply here. Nonetheless, there may be an unobserved municipality-wide factor ξ_{mt} that not only affects the prices of land parcels in a given municipality m but also the municipality's decision to participate in a merger. If such a factor is constant over the years ($\xi_{mt} = \xi_t$), our estimation that allows for the fixed effects automatically accommodates this issue. If it is time-variant, however, it may not always be the case.

A conventional diagnosis to detect the selection may be the examination of changes in outcomes before a treatment (Angrist and Pischke, 2008, Ch. 5). In our context, land prices may start to change before municipal mergers, when municipalities decide to merge in response to a change in factors that are correlated with land prices. However, it is also plausible in our case that the land prices change as a result of anticipation of a municipal merger (Malani and Reif, 2015). Since the anticipation of mergers was quite plausible in the Heisei reform as we discussed in Introduction, the existence of the pre-trend does not necessarily indicate the existence of the selection. However, arguing only the plausible role of anticipation obviously dose not exclude the possibility of the selection either.

Our basic strategy is to rely on "the selection on observables" by including a large number of relevant covariates and as many as available. Taking advantage of the large size of our cross-section units and the rich data sources, we avail ourselves of a large number of covariates for q_{it}^{b} and w_{mt}^{j} in Eq. (6). If this large set of covariates includes factors that reasonably explain ξ_{mt}, we could then condition the decision to merge on the covariates so that we would attain the selection on observables to alleviate the issue. In particular, the elements of $\sum_j \eta_j \cdot w_{mt}^{j}$ are decomposed of three groups of covariates as

$$\sum_{j} \eta_j \cdot w_{mt}^{j} \equiv \sum_{d} \theta_d \cdot x_{mt}^{d}$$
$$+\sum_{p}\sum_{t} \phi_{pt} \cdot \iota_p(i \text{ is located in prefecture } p) \cdot \iota_t(i \text{ is observed in year } t) \quad (8)$$
$$+\sum_{f} \lambda_f \cdot t \cdot \iota_f(i \text{ is located in municipality } f \text{ when } t = 2000).$$

The Greek letters are parameters to be estimated. Let us elaborate on the three groups in turn. First, x_{mt}^{d}'s are municipality-level characteristics that may affect ξ_{mt}. We also consider them to be factors that influence municipal decisions to merge. According to the empirical literature on the Heisei territorial reform, small municipalities with poor fiscal conditions were found to be likely to merge. In particular, Hirota and Yunoue (2017) utilize the following characteristics as factors that should affect municipal decisions to merge: population, surface area, elderly-population ratio, younger-population ratio, industry ratios by primary and tertiary employment, general grants, specific grants, and local government debt. Except the data that are not available every year (i.e., industry ratios by primary and tertiary employment), we will utilize these covariates as municipality-wide characteristics x_{mt}^{d}s that should affect the decision to merge.[9]

[9] Note that we effectively takes care of the effects of surface area, since it allows for the fixed effects along with the merger dummies. As such, we do not include surface area as a covariate in our regression model.

Note also that some of these covariates are related to the intervening variables we will elaborate on the next subsection. These characteristics may fail to sufficiently account for the variations in ξ_{mt}. As such, we shall also include the following two sets of effects in Eq. (6) as the second and third groups of covariates.

Second, we include year-prefecture effects ϕ_{pt} that express region (prefecture)-wide aggregate shocks that vary over years. Prefectures, as the upper tier of local government in Japan, contain municipalities under their jurisdiction as the lowest tier of local government. The land of Japan is divided into 47 prefectures. Prefectural policy, which should be common to all municipalities in a given prefecture, constitutes a region-wide aggregate shock to land prices. Moreover, the spatial boundaries of labor and land markets may typically be wider than municipal boundaries and as wide as prefectural boundaries. These may also affect the municipal decision to merge. We capture such effects by including interactions between (i) a binary variable $\iota_p(\cdot)$, which indicate takes unity if land parcel i is located in prefecture p, and (ii) another binary variable $\iota_t(\cdot)$, which indicates if land parcel i is observed in year t for $p = 1, \ldots, 47$ and $t = 1995, \ldots, 2015$. Given the structure of our data, the number of the interactions (or their coefficients ϕ_{pt}'s) is 987, barring those dropped due to multicollinearity. [10]

Third, we consider a set of municipality-specific time trends $\sum_f \lambda_f \cdot t \cdot \iota_f(\cdot)$, where $\iota_f(\cdot)$ is a binary variable that indicates if land parcel i is located in municipality f in year $t = 2000$. This allows the price of land parcels in different municipalities to follow different paths. Note that f refers to municipality as of before its merger if applicable. The number of time trend parameters λ_f then is more than 3,000. Including different time paths on the municipality basis, we are hoping that they capture the parts of the variations in ξ_{mt} (or ξ_{ft} to be exact) that the other

[10] Another motivation to include ϕ_{py} is to allow for cross-sectional error dependence (CSD). A typical way to handle CSD is to specify the error term as a spatial autoregressive process with a single autoregressive parameter and a spatial weights matrix **W**. However, since our panel of land parcel data is *unbalanced*, defining **W** is not straightforward. Another subclass of the CSD is what Bailey et al. (2015) call "factor model," which characterizes CSD in terms of common temporal factors $\boldsymbol{f}_t = [f_t, \ldots, f_t]'$ whose marginal effects, that is, factor loadings, $\boldsymbol{\psi}_i = [\psi_1, \ldots, \psi_N]'$ differ across cross-sectional units (Pesaran, 2006; Bailey et al., 2015). The total effect of those factors is given as an additive term $\boldsymbol{\psi}_i \cdot \boldsymbol{f}_t$ in the regression model, taking different values over cross-sectional unit i and time period t. When applied to the current analysis, the factor model captures cases where common temporal shocks affect all land parcels with differentiated factor loadings. To allow for these effects, Pesaran (2006) proposes the common correlated effects (CCE) estimator. However, since the CCE estimator requires a sufficiently long panel of data (Bailey et al., 2015), we cannot utilize the estimator, as our sample is very short ($T = 20$) compared with the number of cross-sectional units (N is more than 40,000). In addition, land parcels may be too "small" as a spatial unit of factor loadings. As such, we may instead assume that factor loadings take a common value for a group of land parcels within a given wider area. We could then allow for this type of unobserved temporal shock by augmenting the regression model with the interactions of year and wider areas dummies (or prefectures in our case). Although this relaxed assumption may not be a perfect substitute to the original factor model, it may still be a viable alternative to control for temporal shocks if land parcels (and municipalities) are too small a spatial unit to differentiate factor loadings.

covariates may fail to capture. In addition, DD estimation with this type of trend is likely to be more robust and convincing when the pretreatment data establish a clear trend that can be extrapolated into the post-treatment periods (Angrist and Pischke, 2008, Ch. 5). This should be the case with our data, as the land price on average exhibits different paths after the mid-2000 when a large number of municipal mergers occurred.

2.3 Intervening variables and municipality-level covariates

Our theoretical discussion treated population (n), local characteristics ($\boldsymbol{a}$), public service (z), wages (W), and local taxes (T) as *intervening variables* through which the effects of municipal mergers on land price are realized, by expressing them as $n = n(M)$, $\boldsymbol{a} = \boldsymbol{a}(M)$, $z = z(M)$, $W_i = \omega_i(M) \equiv W_i[n(M), \boldsymbol{a}(M), z(M)]$, and $T_i = \tau_i(M) \equiv T_i[n(M), \boldsymbol{a}(M), z(M)]$. If we are loyal to Eq. (5), we should exclude these intervening variables from covariates x_{it}^d's in Eq. (6). This would be a valid practice if their changes are caused *solely* by municipal mergers, which typically applies to the case of municipal surface area (as an important element of local characteristics $\boldsymbol{a}$). However, with the surface area as an exception, the other factors would change even in the absence of municipal mergers, albeit less drastically compared with those in the presence of municipal mergers. For example, local population and its demographics (another important element in $\boldsymbol{a}$) would not be constant over time even without amalgamation, say, due to different birth rates or different migration patterns among localities. The same applies to wages and taxes.

This then suggests that, for merged municipalities after their mergers, we should control the effects on land prices with respect to the part of the changes in the intervening variables that are not caused by municipal amalgamations. Since it is difficult to obtain such counterfactual values, we cope with this difficulty by comparing the results of different models, which includes or excludes covariates that may surrogate the intervening variables. We do this by assuming that n, a, and z are appropriately surrogated by total municipal population, ratio of population aged 14 or younger, and ratio of population aged 65 years or older.[11] To anticipate the results, changing the set of the control variables will not yield noticeable differences of the estimates. We therefore deem the raised issue to be of less concern, and include these variables as x_{it}^d in Eq. (6) as they also function as covariates that affect municipal decisions to amalgamate, as we have argued above.

[11] We proxy z with total population and other demographics. The literature indeed suggests that regional demographics, along with population size, are likely to be key determinants of the level of public services z in local politics (Poterba, 1997, 1998; Harris et al., 2001). This should be the case for Japan, since its municipalities provide a number of age-related services and personal transfers (Hayashi, 2010).

However, we cannot perform analogous experiments with the other two intervening variables, that is, wages W_i and taxes T_i. While they are specific to resident type (or land parcel) with subscript i in the theoretical formation, corresponding data are not available at the level of land parcel, let alone their counterfactual values. They thus count as missing variables in the regression model. However, the regression model controls for $\mathbf{q}$, the characteristics of individual land parcels including their size and types of structures built on them, to the extent that the size and type of housing are related to wages and taxes of the residents. We may hope that the bias caused by missing variables for W_i and T_i would be minimal, since our model also allows for unobserved heterogeneity and municipality-specific common trends.

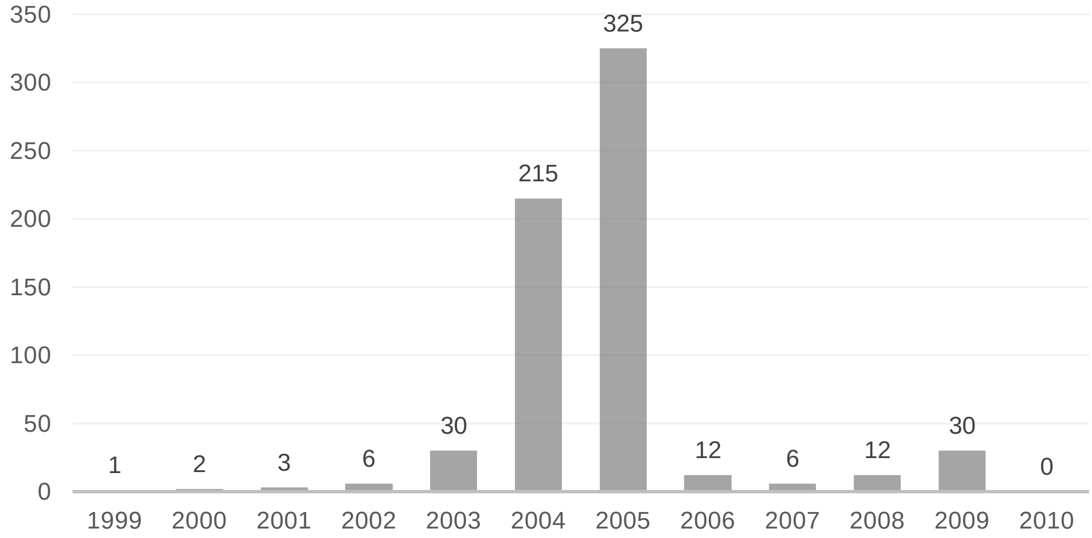
3. Institutional background, sample and data

3.1 Municipal mergers in the 2000s

After reviewing the roles of different levels of governments in the late 1990s, the central government in Japan enacted the Omnibus Law of Decentralization (OLD) in 1999, which defined the environment for municipal mergers in the 2000s to take place. The legislation emphasized the role of municipalities in providing public services and, particularly, the decentralization of expenditure functions to lower levels of government. A central government consultative body, the Nishio commission, noted that a majority of municipalities were too small to perform such decentralized functions. In 1999, the central government also revised the Special Mergers Law (SML-1999), originally enacted in 1965, to make the older version conform to the provisions of the OLD. The SML-1999 allowed the central government to advance municipal mergers with additional measures that provide generous fiscal and administrative incentives.[12] As a result, the Heisei territorial reform resulted in a massive wave of municipal mergers, which reduced the number of municipalities by 47%, from 3,229 at the end of FY1999 to 1,727 at the end of FY2010. **Figure 1** shows the numbers of municipal mergers from FY 1999 to FY 2010. Most of the mergers cluster in FY2004 and FY2005. The number peaked in FY2005, as the SML-1999, which provided the generous incentives, was replaced in FY2006 by a new SML (SML-2006), which substantially reduced the incentives. The phase of merger promotion defined by the SML-2006 ended at the end of FY2009.

[12] The incentives included the permission to issue local bonds to finance capital expenditures required for mergers with a generous subsidization (up to 70% of the costs of investment) through a system of central transfers. The law also allowed merged municipalities to retain council members in the participating municipalities before their merger for several years with a possible extension of their terms of office.

Figure 1. The number of municipal mergers: 2000–2010



Source: Ministry of Internal Affairs and Communications (2017).

In our empirical exercise, we focus on municipal mergers that were realized in FY2004 and FY2005. We obtain information on the municipal mergers the 2000s from the Ministry of Internal Affairs and Communications (2017). Obviously, it took time to complete the process toward municipal mergers. The SML-1999 required municipalities to set up an amalgamation committee where interested parties coordinate their effort toward finalizing their decisions to merge. It even took time to establish this committee since it also required agreements among concerned parties, based on the initiatives of municipal government or their residents. The committee was to set the procedures as well as the terms of conditions for merger, including the name of merged municipality, the schedule toward the merger, the location of municipal offices and council, and the coordination of differences in administrative systems. On average, it took 595 days from the formation of the committee to the completion of a given municipal merger, with a minimum of 112 and a maximum of 1,491 days (Nakazawa 2016). If we count the days from the start of anticipating a merger to its realization, they would span longer than those. At the longest, we might as well trace the start back to the mid-1990s where the movement toward the decentralization reform started to be realized as the OLD and the SLM-1999.

3.2 Land price (V)

We utilize the price of fixed property V rather than its rental price R, assuming that $V = R/r$, where r is the discount rate, holds. Note also that, while the use of housing prices is typical in non-Japanese studies on capitalization, we use *land*, not *housing* prices. This is mainly because, while there are no reliable data available for housing prices[13], the Japanese government provides a comprehensive dataset for land prices, drawing from two analogous surveys on land parcels—they are the Chika-Koji[14] system (CKS) and the Prefectural Land Price Survey (PLPS), both of which provide annual data for the prices (value of land parcels in JPY/m^2) of “representative” land parcels in a given area within a municipality, along with detailed data on their characteristics (to be delineated below). Following typical empirical studies on capitalization in Japan[15], we also utilize the CKS and the PLPS, focusing on residential land parcels.

CKS and PLPS prices are appraisal-based, not market-based. At least two (one) certified real estate appraisers assess the value of a given parcel for the CKS (PLPS). When making their assessments, they refer to market prices of comparable land parcels transacted within their assigned areas. While Yamazaki (2001) regards CKS prices as “the most reliable benchmarks for ordinary transaction prices,[16]” Shimizu and Nishimura (2006) caution that CKS prices may substantially depart from market prices when land markets experience large structural changes.[17] This is because the appraisers refer to “ordinary” transaction prices when making assessments and, as such, they tend to exclude the transactions that deviate from past trends. The analogous assessments should apply to PLPS prices. In addition, Shimizu and Nishimura (2006) argue that CKS prices have one- or two-year lags in reflecting market prices.

Nonetheless, a number of studies on the Japanese real estate market utilize CKS, as well as PLPS prices.[18] In addition to being the most comprehensive data sources available, the literature offers several justifications for using appraisal prices. First, discrepancies against market prices are of less concern when cross-sectional variations are important, since they would not affect the

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[13] Literature shows that, once houses are built, their values net of land prices are estimated at almost nil, sometimes even negative, in many transactions on the Japanese real estate market.

[14] Literally, “chika” means land prices and “koji” means “making public” or “public notification.” Different English translations are provided for Chika Koji, which may confuse non-Japanese readers. MLIT’s official website used “land price public notice system” previously, but has now re-translated it as “Land Market Value Publication.” Different researchers use different translations, such as “Published Land Prices” in Shimizu and Nishimura (2006) and Nakagawa et al. (2009), “officially assessed land-price” in Seya and Tsutsumi (2012) and Seya et al. (2016), “official land prices” in Nakanishi (2017) and Tanaka and Managi (2016), and “Public Notice of Land Prices” in Ishikawa and Fukushige (2012) and Matsui and Fukushige (2012). We retain the original name, Chika Koji.

[15] Recent examples in English include Nakagawa et al. (2009), Ishikawa and Fukushige (2012), Matsui and Fukushige (2012), Seya and Tsutsumi (2012), Seya et al. (2016), Nakanishi (2017), and Tanaka and Managi (2016). If we also include studies written in Japanese, their number increases significantly.

[16] Since the appraisers refer to “ordinary” transaction prices, the prices should be free from the potential bias caused by erratic market transactions (Nakanishi 2017). Such erratic cases include forced prices resulting from buying or selling sprees (Seya and Tsutsumi 2012), and a downward bias for sale prices resulting from seller financing (Pollakowski 1995) or non-arm’s length transactions (Ma and Swinton 2012).

[17] Saderion et al. (1994) discuss the adequacy of land price data assessed by professional appraisers, while Ma and Swinton (2012) caution against the use of appraisal prices for lands whose use is changing, but acknowledge the merits of appraisal prices against market prices.

[18] See references in footnote 15.

relationship of prices across subnational regions (Nakagawa et al., 2009). Second, as a simple corollary of the argument by Shimizu and Nishimura (2006), the bias should be small when the market structures are stable. While appraisals may lose important information in the late 1980s and early 1990s when the markets experience structural changes, they yield good land value estimates that are free of significant bias in the 2000s (which is our focus), when the markets were rather stable.

Finally, and perhaps most importantly, appraisal prices are the only option for panel analysis. As we compare land prices before and after municipal mergers, we require the price of a given location to exist in more than two periods before and after the incident. This data requirement is rarely satisfied with land transaction data due to the "thinness" of market transactions (Saderion et al., 1994): almost no parcels are traded every year. Conversely, the CKS and PLPS reassess almost all parcels assessed in previous years, allowing us to construct a panel of land parcel prices. Additionally, the panel structure allows us to mitigate possible problems resulting from discrepancies between appraisal and market prices. For example, Shimizu and Nishimura (2006) argue that the errors accumulate over time, implying that they are serially correlated. We could mitigate possible adverse effects of serial correlation by allowing for unobserved heterogeneity.

3.3 Sample

Our sample consists of a panel of residential land parcel prices in municipalities that completed mergers in FY2004 or FY2005 and those in municipalities that had never experienced mergers from FY1995 to FY2015, drawing the data both from the CKS and PLPS. The surveys replace parcels when they are divided into new parcels, merged into new ones, or when their land usage changes. Therefore, the structure of our panel data is necessarily unbalanced. The timing of land-price appraisals is January 1 for the CKS and July 1 for the PLPS. Since our time frame is the fiscal year (FY) (April to March), we combine PLPS data recorded on July 1 and CKS data recorded on January 1 to form a cross-section for a given fiscal year. Some land parcels are surveyed by both the CKS and PLPS. As most of these overlapping parcels exhibit different prices on July 1 (the PLPS) and January 1 (the CKS) within a given FY, we regard them as separate observations. Since the effect of the different timings of the two surveys may yield differences that would be time-variant and region-specific, we replace $I(\cdot, \cdot)$ with an interaction of the PLPS dummy and $I(\cdot, \cdot)$ in Eq. (6).

Table 1 lists the number of residential land parcels used in the estimation every five years over the period, by survey (CKS, PLPS, or both) and municipal type (city, town/village, or both) in addition to the number of parcels that were surveyed by the CKS and the PLPS. Cities have more residential parcels than towns and villages (TAV). While the total number of residential parcels does not change significantly (ranging from around 30,000 to 41,000), the number of parcels in TAVs decreased sharply starting in FY2005, suggesting that a large number of TAV parcels experienced municipal mergers. This also implies that more of the TAV observations in our sample would be essential in estimating the impacts of municipal mergers. This justifies our addition of PLPS observations to the CKS ones, since PLPS surveys relatively more TAV parcels than CKS.

Table 1. Number of residential parcels

Fiscal years	CKS + PLPS	CKS		PLPS		Surveyed both by the CKS and PLPS	
		cities	TAVs	cities	TAVs	cities	TAVs
1995	43,271	17,933	4,229	11,688	9,421	2061	186
1996	43,417	18,062	4,203	11,749	9,403	2163	193
1997	43,878	18,322	4,213	11,945	9,398	2205	193
1998	44,152	18,420	4,263	12,040	9,429	2219	191
1999	42,654	18,570	4,306	10,974	8,804	2219	194
2000	42,429	18,580	4,310	10,819	8,720	2210	194
2001	42,749	18,906	4,295	10,856	8,692	2246	185
2002	42,967	19,161	4,275	10,872	8,659	2263	185
2003	43,013	19,176	4,275	10,895	8,667	2251	186
2004	42,558	19,582	3,443	12,816	6,717	2300	169
2005	41,889	20,633	2,396	14,435	4,425	2346	122
2006	40,080	19,751	2,319	13,810	4,200	2253	114
2007	38,674	19,139	2,273	13,291	3,971	2299	114
2008	37,551	18,538	2,237	12,941	3,835	2313	114
2009	36,702	18,254	2,206	12,530	3,712	2321	110
2010	35,050	16,916	2,109	12,374	3,651	2317	112
2011	34,839	16,960	2,042	12,281	3,556	2316	109
2012	34,657	16,934	2,022	12,162	3,539	2247	106
2013	32,365	14,939	1,947	11,989	3,490	2231	105
2014	32,193	14,963	1,938	11,831	3,461	2248	107
2015	33,693	16,454	1,968	11,820	3,451	2266	106

Notes: (i) TAVs refer to "towns and villages." (ii) The table only list the numbers every five years starting 1995, and skip listing them in years between.
Sources: The Chika-Koji system, and the Prefectural Land Price Survey

Table 2 then shows the shares of land parcels located in municipalities that merged in FY2004 and FY2005, as well as both by years. Note that, since there are a small number of annual replacements in the sample, the shares are not always identical after the year of municipal merger. The table shows that 17.0–17.8% of the parcels in our sample experienced municipal amalgamation in FY2004, whereas 25.2–26.1% of the parcels in FY2005. In total, 42.2–43.9% of the parcels in the sample are in the treatment group for the DD estimation.

Table 2. Shares of land parcels in merged municipalities

Fiscal year	Total	In municipalities merged in FY2004	In municipalities merged in FY2005
1995	0.414	0.168	0.246
1996	0.413	0.168	0.246
1997	0.415	0.168	0.247
1998	0.416	0.168	0.247
1999	0.413	0.168	0.245
2000	0.412	0.168	0.244
2001	0.410	0.167	0.243
2002	0.409	0.166	0.242
2003	0.409	0.166	0.242
2004	0.409	0.167	0.243
2005	0.407	0.164	0.243
2006	0.404	0.164	0.240
2007	0.396	0.159	0.237
2008	0.397	0.160	0.237
2009	0.395	0.160	0.235
2010	0.396	0.160	0.236
2011	0.397	0.160	0.237
2012	0.395	0.159	0.236
2013	0.405	0.163	0.242
2014	0.404	0.162	0.242
2015	0.394	0.158	0.236

Sources: The Chika-Koji system, and the Prefectural Land Price Survey

3.4 Characteristics of individual land parcels

The CKS and PLPS also provide detailed data for characteristics of individual land parcels. We utilize them for $\mathbf{q}_{it}$ in Eq. (6), the details of which are as follows:

- *Parcel structure*: shape (almost square, almost trapezoid, almost rectangular, square, trapezoid, rectangular, and irregular), frontage-depth ratio, and size.
- *Infrastructure availability*: distance from the nearest station (subway or railway), availability of gas, water, and sewage facilities.
- *Characteristics of front road*: road width, paved or not, administration type (12 types, including municipal, prefectural, national, and private), orientation of the land parcel (north, northeast, east, southeast, south, southwest, west, and northwest).

- *Non-front roads*: lateral, rear, three-sided, four-sided, orientation of the land parcel (north, northeast, east, southeast, south, southwest, west, and northwest).
- *Buildings on the parcel*: structure (steel-reinforced concrete, reinforced concrete, steel, lightweight steel, block, or wooden), number of floors, and number of basement floors.
- *Land regulation*: zoning (26 types), building-to-land ratio, and floor-area ratio.

Most of the variables above are binary, except frontage-depth ratio, parcel size, distance from the nearest station, number of floors, number of basement floors, building-to-land ratio, and floor-area ratio.

3.5 Municipal characteristics

As mentioned in Section 2, covariates x_{mt}^{d} for Eq. (6) may include (a) municipal population, (b) elderly-population ratio (≥ 65), (c) younger-population ratio (< 14), (d) general grants, (e) specific grants, and (f) local government debt. In addition, we may consider as additional fiscal variables (g) fees and charges and (h) local bond issuance. Despite the discussion on personalized local taxes T_i, they may include (i) (aggregate) municipal tax revenues as well. The fiscal factors (d)–(i) obviously show different aspects of municipal budget. We obtain the data for them from the Ministry of Internal Affairs and Communications (various years). When used in regression, the fiscal variables are all measured in per capita terms with municipal population, and then we take their logarithm.

The types of municipalities may also be important, as the expenditure assignments of municipalities differ by their types. There are seven types: designated cities, core cities, special cities, cities, Tokyo metropolitan special wards, towns, and villages. For example, designated cities have expenditure functions comparable to those of prefectures, parts of which also apply to core and special cities. Meanwhile, TAVs have a smaller number of functions than cities do, especially in local social policy. Note that allowing for unobserved heterogeneity in Eq. (6) does not account for these differences, since municipal mergers often changed the municipal type. For example, land parcels in a village experience a change in the municipal type when it merges with others to form a new city. We also include in x_{mt}^{d} binary variables that indicate the type of municipality where land parcel i was located.[19]

[19] When creating the binary variables, we categorize (ordinal) cities into middle- (population > 100,000) and small-sized (population < 100,000), following the categorization by the Ministry of Internal Affairs and Communication. Since none of Tokyo's metropolitan special wards have experienced municipal mergers in the 2000s, we consider seven binary variables for municipal type, among which one variable is excluded from the estimation due to collinearity.

4. Results

We obtain the effects of municipal mergers on land price by estimating Eq. (6) with alternative specifications of the merger effects (7a) –(7c). Since we focus on the municipal mergers realized in FY2004 and FY2005 with the data from FY1995 to FY2015, we obtain as many as 20 estimates for the merger effects as specified in Eq. (7d). In addition, our regression models contain quite a large number of covariates. As we shall discuss later, we also estimate variants of the base-line model in Eq. (6), which multiplies the number of estimated coefficients. It is therefore impractical to list all the results for every coefficient estimate in standard regression tables. Rather than listing them all, we shall graphically present only the estimates for the merger effects as in Eq. (7a)–(7d) in charts along with their confidence intervals. The charts contain two sorts of 95% confidence intervals. One is based on the clustered standard errors among the land parcels within a municipality *before* amalgamation if applicable (as of 1996), shown with long dashed lines; the other is within a municipality *after* amalgamation if applicable (as of 2015), shown with long dashed dotted lines. As the charts below will show, these two are virtually identical.

In passing, recall that we mentioned we would experiment the results with different sets of municipality-level covariates to allow for the difficulties caused by the intervening variables. We have examined the following four patterns:

- Set A: general grants, categorical grants, fees and charges, local bond issuance, and local debt outstanding;
- Set B: items in Set A plus total municipal population, ratio of population aged 14 or younger, and ratio of population aged 65 years or older;
- Set C: items in Set B plus local tax revenues; and
- Set D: none of the above.

The choice among the four sets does not yield any noticeable differences among the estimates.[20] As such, we shall rely on the results from Set B in the following exposition.

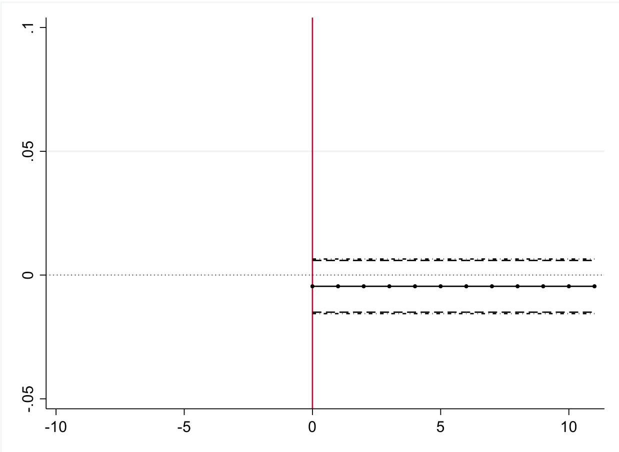
4.1 Baseline cases and the effect of rushing

Panels (a), (b), (c), and (d) in **Figure 2** show the results for Eq. (6) with (7a), (7b), (7c), and (7d), respectively.[21] The four panels show the following. First, when we specify the effect of mergers as *ex-post* constant, the point estimate is negative and insignificant at the 95% level seen in Panel (a). We would have then concluded that the effect is nil. Second, however, if we specify the effect variable *ex-post*, the point estimates now decline along the timeline as Panel (b) shows.

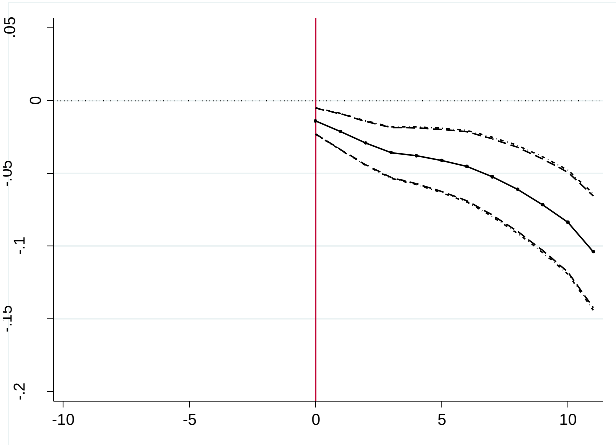
[20] Details will be provided by authors on request.
[21] Without losing important information, we relegate detailed results to Table A1 in Appendix.

In addition, the confidence intervals indicate that the negative effects are all statistically significant. However, allowing for the *ex-ante* anticipation effects reveals that these effects may be misleading.

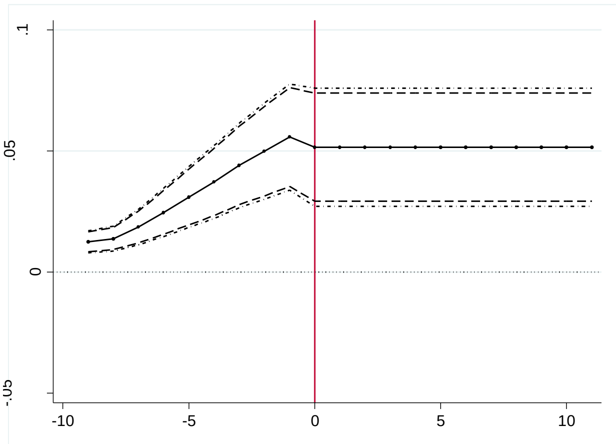
Figure 2. Baseline models



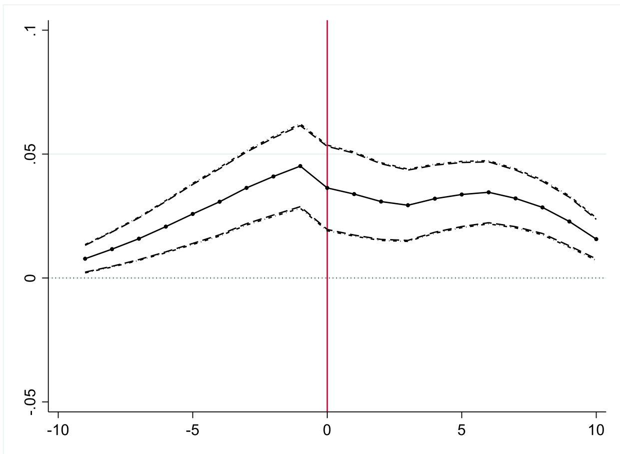
(a) Constant *ex-post* effects



(b) Variable *ex-post* effects



(c) Variable *ex-ante* and constant *ex-post* effects



(d) Variable *ex-ante* and *ex-post* effects

Note: (a) The horizontal axis measures relative years since the occurrence of municipal mergers; (b) The vertical axis measures ×100 percentage increase in land price when compared with those without mergers in the same year; (c) The dashed lines indicate 95% confidence intervals; (d) Long dashed lines refer to confidence intervals based on the clustering in a municipality before amalgamation (as of FY1995), while long dashed dotted lines refer to those based on clustering in a municipality after amalgamation (as of FY2015).

Results in Panels (c) and (d) allow for the anticipation effects. They show that all the effects are positive and statistically significant. In both panels, the *ex-ante* anticipation effects follow a similar pattern, gradually growing toward the year of actual merger. This suggests that market anticipation about possible gains from a municipal merger became gradually larger as the time approached its realization. Panel (c) shows constant positive effects that are statistically significant. However, Panel (d), which allows for the variable *ex-post* effects, reveals that, just after the merger is realized, the positive effect starts to fade away, perhaps toward zero if the time horizon is long enough. Note that we could conformably reject (7a)–(7c) against (7d) to select (7d) as the surviving model between the four.[22] This then may imply that the actual turns of events after the merger slowly betrayed the anticipation—the municipal mergers during the 2000s did not capitalize into land prices in the long run. In other words, the actual gains from the Heisei territorial reform were temporary and its welfare effects were nil in the long run.

In what follows, we extend our analysis above to estimate models that allow for the timing of mergers and the size of merging municipalities. Given that we select the model with the merger effect (7d), we use it as the baseline model from which we develop the following analysis.

4.2 The effect of rushing

Recall that FY2005 was the last fiscal year when the generous fiscal incentives for municipal mergers were available. Initially, the central government had planned to terminate the incentives at the end of FY2004 (i.e., March 31, 2005). In May, 2005, however, it effectively postponed the termination one more year to the end of FY2005 (i.e., March 31, 2006). The center allowed municipalities to receive the incentives if they filed their planned mergers for central approval before March 31, 2015, *and* implemented them within a year from the days of their filing. Indeed, as many as 185 mergers were realized only in the last month of FY2005 (i.e., March, 2006), which amounted to one-third of the total number of mergers during the 2000s.

We could then regard municipalities that completed their mergers in FY2005 as those that failed to conclude mergers before the original due date but, thanks to its extension, *rushed* to conclude otherwise unsuccessful mergers. To account for such an effect of possible rushing, we may allow coefficients β_k in (7d) to take on different values depending on whether the municipal mergers are realized in FY2004 or FY2005. We therefore modify (7d) as

$$g(\boldsymbol{M}_{it}) \equiv \sum_{y=2004}^{2005} \left(\sum_{u=1}^{U_y} \beta_{-u}^{y} \cdot M_{it+u}^{y} + \beta_0^{y} \cdot M_{it}^{y} + \sum_{s=1}^{S_y} \beta_s^{y} \cdot M_{it-s}^{y} \right) \tag{7e}$$

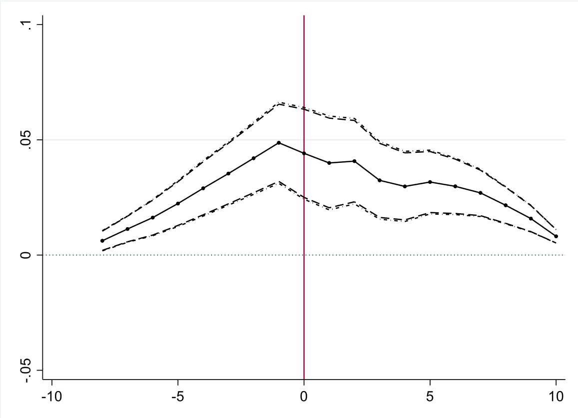
where M_{it}^{y} takes unity for land parcel *i* if it is located in a municipality that completed its merger in fiscal year *y* and observed when $t = y$, and zero otherwise. Given our data structure, the numbers of leads and lags are such that $U_{2004} = 8$, $S_{2004} = 10$, and $U_{2005} = S_{2005} = 9$. From this parameterization, we then obtain two different sequences of β_k^{y}'s for the mergers in FY2004 (y = 2004) and FY2005 (y = 2005). The model with (7b) is a multiple-treatment model that allows

[22] Details will be provided by authors on request.

for two different timing of mergers, FY2004 and FY2005. The two treatment groups consist of a set of municipalities that amalgamated in FY2004 and that in FY2005, and the control group is the set of municipalities that had never amalgamated.

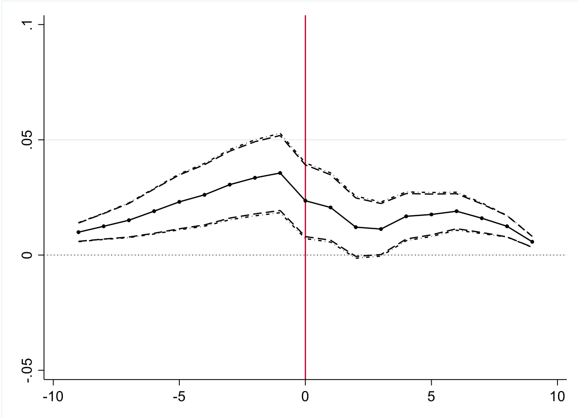
Figure 3. Different timing of mergers: FY2004 and FY2005

×100 percentage increase in the average price of land parcels by municipal mergers in FY2004



(a)-FY2004

×100 percentage increase in the average price of land parcels by municipal mergers in FY2005



(b)-FY2005

Note: (a) The horizontal axis measures relative years since the occurrence of municipal mergers; (b) The vertical axis measures ×100 percentage increase in land price; (c) The dashed lines indicate 95% confidence intervals; (d) Long dashed lines refer to confidence intervals based on the clustering in a municipality before amalgamation (as of FY1995), while long dashed dotted lines refer to those based on clustering in a municipality after amalgamation (as of FY2015).

Panels (a) and (b) in **Figure 3** show the effects of mergers in FY2004 and FY2005, respectively.[23] The estimates in the two panels follow the same pattern we described for Panel (d) in **Figure 2**, implying again that the effects are temporary. More revealing is the fact that, in both cases, the effects culminate just one year before the year of actual amalgamation, despite the fact that the timing of the amalgamation is different, that is, FY2004 and FY2005. Eyeballing the two panels however suggests that the point estimates for the effects of municipal mergers in FY2004 are larger than those in FY2005 for each time point along the horizontal line. Indeed, if we formally test the null hypothesis that the effects of the 2004 and 2005 mergers are identical ($\beta_k^{2004} = \beta_k^{2005}$ for all k), we can emphatically reject the hypothesis with virtual zero p values.[24] In addition, while all the coefficients for the *ex-ante* anticipation effects are statistically significant

[23] Without losing important information, we relegate detailed results to Table A2 in Appendix.

[24] Since we are using two estimates for the variance–covariance matrix, the test statistics are also two-fold, one based on clustering among land parcels within municipalities in FY1995 and the other based on clustering within municipalities that existed in FY2015. Both cases result in virtually zero p values. Note that when we construct the test statistics, we do not put the restriction on the coefficients that do not find their matches, that is, β_{10} for FY2004 and β_{-9} for FY2005.

for both cases, the coefficients for the *ex-post* effects are not necessarily so. While they are all statistically significant for FY2004 mergers, some of the *ex-post* effects for FY2005 mergers are not so, albeit marginally. This implies that the effect of FY2005 mergers is less positive throughout, and that the anticipation was dashed more easily in municipalities that rushed into merging.

4.3. Effects of size among participating municipalities

In the estimations above, we assume that the trajectory of the *ex-ante* and *ex-post* effects is common within a given boundary of merged municipalities. However, the merger effects may depend on the initial (i.e., pre-merger) size of municipalities that participate in a given merger. For example, if economies of scale exist, the effect may be larger for land parcels located in smaller participants. In contrast, it may also be possible for the effects to be larger in larger participants, since their residents could exert more political power with a larger share of voting power in a newly formed fiscal unit.

To examine such effects that depend on pre-merger size, we allow the coefficients in (7e) to take on different values depending on whether the relevant merger participants are "larger" or "smaller." Our criterion for a merger participant to be "larger" is whether it has the largest population among the participants in a given amalgamation. To identify such effects, we modify (7e) as

$$\begin{aligned} g(\boldsymbol{M}_{it}) \equiv \iota(\text{larger}) \cdot \sum_{y=2004}^{2005} \left(\sum_{u=1}^{U_y} \bar{\beta}_{-u}^{y} \cdot M_{it+u}^{y} + \bar{\beta}_{0}^{y} \cdot M_{it}^{y} + \sum_{s=1}^{S_y} \bar{\beta}_{s}^{y} \cdot M_{it-s}^{y} \right) \\ +\big(1-\iota(\text{larger})\big) \cdot \sum_{y=2004}^{2005} \left(\sum_{u=1}^{U_y} \tilde{\beta}_{-u}^{y} \cdot M_{it+u}^{y} + \tilde{\beta}_{0}^{y} \cdot M_{it}^{y} + \sum_{s=1}^{S_y} \tilde{\beta}_{s}^{y} \cdot M_{it-s}^{y} \right) \end{aligned} \tag{7f}$$

where $\iota(\text{larger})$ is a binary variable that takes unity if land parcel *i* is located in the municipality that had the largest population among the participants in a given merger, and zero otherwise. The bars and tilde above the coefficients refer to the effects for "larger" and "smaller" municipalities, respectively.

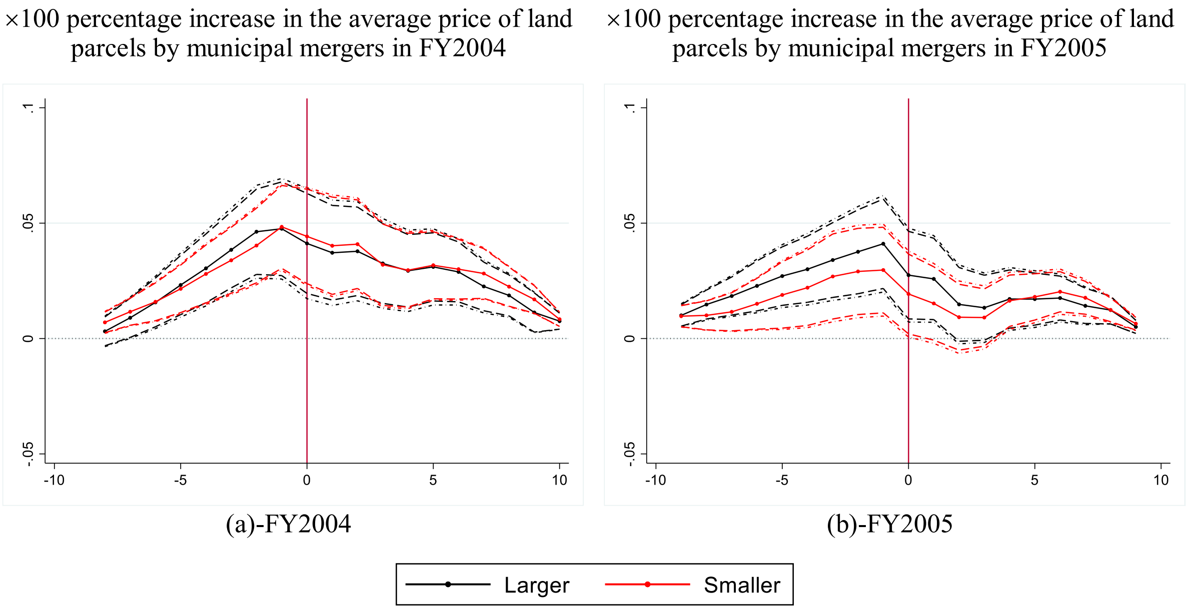
Two panels in **Figure 4** shows the estimates of $\bar{\beta}_k^y$'s for larger municipalities in black lines and $\tilde{\beta}_k^y$'s for smaller municipalities in red lines. Panels (a) and (b) exhibit them for FY2004 and FY2005 mergers, respectively.[25] The four sets of the estimates follow the pattern similar to what we have observed in **Figures 2** and **3**: the *ex-ante* anticipation effect gradually increases to peak in the year just before the year of amalgamation; and right after that year, the *ex-post* effect starts

[25] Without losing important information, we relegate detailed results to Table A3 in Appendix.

to fade away toward zero. As such, the effects are again temporary for all the four cases. In addition, the point estimates for FY2004 mergers are larger than those for FY2005 mergers within a class of municipality size (i.e., "larger" or "smaller"). In other words, the patterns still remain robust here.

The difference between smaller and larger participants is more conspicuous for FY2005 mergers. While the black and read lines are located closer in Panel (a), they are hardly so in Panel (b), where the red line is located below the black line for a majority of the periods. In particular, there are more numbers of statistically *in*significant estimates for smaller participants than for larger participants in Panel (b). Following the procedure analogous to that for the hypothesis tests in section 4.2, we test the null hypothesis that coefficients are identical between larger and smaller participants among the mergers in a given fiscal year ($\bar{\beta}_k^y = \tilde{\beta}_k^y$ for all k, given $y = 2004$ or 2005). The test rejects the hypothesis for FY2004 mergers emphatically with the p-values being virtually zero, and also does so for FY2005 mergers at the 0.01 level, with p values being a little under 0.01 (0.007 and 0.009). We may therefore interpret from these results that smaller participants fare worse than larger participants when they both rush into merging as they did in FY2005 mergers.

Figure 4. Large and small, with or without more than average population



Note: (a) The horizontal axis measures relative years since the occurrence of municipal mergers; (b) The vertical axis measures ×100 percentage increase in land price; (c) The dashed lines refer to 95% confidence intervals; (d) Long dashed lines refer to confidence intervals based on the clustering in a municipality before amalgamation (as of FY1995), while long dashed dotted lines refer to those based on clustering in a municipality after amalgamation (as of FY2015). (e) Black lines refer to the effects for "larger" municipalities, whereas black lines refer to those for "non-larger" municipalities.

5. Concluding remarks

We evaluated the effects of municipal mergers using the theory of land price capitalization. Our estimation exploited the extensive municipal mergers in the Heisei territorial reform and dataset of land prices compiled by the Japanese government. We allowed the effects of municipal mergers to vary over time—both *ex-ante* (as anticipation effects) and *ex-post* (as realized effects). We also allowed the effects to vary when municipalities rush to amalgamate, as in the mergers realized in FY2005. We also investigated if the relative population size among participating municipalities in a given merger affects its outcome. Throughout the series of estimation, we found a robust common pattern: the anticipation effect gradually starts to grow over several years and peaks in one year just before the actual amalgamation. Once the merger is realized, the *ex-post* effect, starting with a smaller effect than the maximal anticipation effect in the previous year, gradually declines towards zero. This would then imply that the positive effects of the Heisei territorial reform were *temporary*, and almost half of the effects are explained by the anticipation effects. From the analyses that differentiate the effects of FY2005 mergers, we also found that rushing into mergers would make the temporal effects smaller, and even further so among merger participants with smaller pre-merger population. In sum, the Heisei territorial reform yielded only temporal benefits that would soon be dissipated.

As always being the case, our estimates are not perfect. In particular, as argued in the text, fiscal variables, as well as other municipal characteristics, may potentially be an issue in our estimation and, generally, in estimations that rely on the capitalization hypothesis. Since the framework formulates the land (or housing) price equation as a reduced form from a system of equations, it is important to detect how "intervening" variables are affected by policy in question, that is, municipal mergers in our case. However, it is difficult to do so, since the observed data for the intervening variables also include variations not caused by the policy change in question. To allow for this, we examined how the results change with several combinations of the intervening variables to show some boundaries of the effects. While our handling of the issue may not be perfect, the estimates exhibited only small differences, giving our conclusions some credibility.

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Table A1. Estimation Results

	Eq. (7a)		Eq. (7b)		Eq. (7c)		Eq. (7d)	
	Only constant *ex-post* effect		Only variable *ex-post* effects		*Ex-ante* variable and *ex-post* constant effects		Variable *ex-ante* and *ex-post* effects	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
9 years before	–	–	–	–	0.012^{***}	(0.002)	0.008^{***}	(0.003)
8 years before	–	–	–	–	0.014^{***}	(0.002)	0.012^{***}	(0.004)
7 years before	–	–	–	–	0.019^{***}	(0.003)	0.016^{***}	(0.004)
6 years before	–	–	–	–	0.025^{***}	(0.005)	0.021^{***}	(0.005)
5 years before	–	–	–	–	0.031^{***}	(0.006)	0.026^{***}	(0.006)
4 years before	–	–	–	–	0.037^{***}	(0.007)	0.031^{***}	(0.007)
3 years before	–	–	–	–	0.044^{***}	(0.008)	0.036^{***}	(0.007)
2 years before	–	–	–	–	0.050^{***}	(0.009)	0.041^{***}	(0.008)
1 year before	–	–	–	–	0.056^{***}	(0.010)	0.045^{***}	(0.008)
The year of merger	–0.005	(0.005)	-0.014^{***}	(0.005)	0.052^{***}	(0.011)	0.036^{***}	(0.009)
1 year after			-0.021^{***}	(0.006)			0.034^{***}	(0.008)
2 years after			-0.029^{***}	(0.008)			0.031^{***}	(0.008)
3 years after			-0.036^{***}	(0.009)			0.029^{***}	(0.007)
4 years after			-0.038^{***}	(0.010)			0.032^{***}	(0.007)
5 years after			-0.041^{***}	(0.011)			0.034^{***}	(0.007)
6 years after			-0.045^{***}	(0.012)			0.035^{***}	(0.006)
7 years after			-0.052^{***}	(0.013)			0.032^{***}	(0.006)
8 years after			-0.061^{***}	(0.015)			0.028^{***}	(0.005)
9 years after			-0.072^{***}	(0.016)			0.023^{***}	(0.005)
10 years after			-0.084^{***}	(0.017)			0.016^{***}	(0.004)
11 years after			-0.100^{***}	(0.020)			–	–
Characteristics of land parcels	YES		YES		YES		YES	
Municipal-level characteristics	YES		YES		YES		YES	
Prefecture-year effects	YES		YES		YES		YES	
Municipal-level time trends	YES		YES		YES		YES	
R^2	0.881		0.881		0.881		0.881	
Sample size	828,781		828,781		828,781		828,781	
Average N	39,528		39,528		39,528		39,528	
Average T	12.596		12.596		12.596		12.596	

Note: While we obtained standard errors ("S.E."s) based on the clustering in a municipality before amalgamation (as of FY1995) and those based on clustering in a municipality after amalgamation (as of FY2015) in the figures in the text, we only list the formers in the table since their differences are negligible.

Table A2. Estimation Results for Eq. (7e)

	Merged in FY2004		Merged in FY2004	
	Coef.	S.E.	Coef.	S.E.
9 years before	–	–	0.010***	(0.002)
8 years before	0.006***	(0.002)	0.012***	(0.003)
7 years before	0.011***	(0.003)	0.015***	(0.004)
6 years before	0.016***	(0.004)	0.019***	(0.005)
5 years before	0.022***	(0.005)	0.023***	(0.006)
4 years before	0.029***	(0.006)	0.026***	(0.007)
3 years before	0.035***	(0.007)	0.031***	(0.007)
2 years before	0.042***	(0.008)	0.034***	(0.008)
1 year before	0.049***	(0.009)	0.036***	(0.008)
The year of merger	0.044***	(0.010)	0.024***	(0.008)
1 year after	0.040***	(0.010)	0.021***	(0.007)
2 years after	0.041***	(0.009)	0.012*	(0.007)
3 years after	0.032***	(0.008)	0.011**	(0.006)
4 years after	0.030***	(0.007)	0.017***	(0.005)
5 years after	0.032***	(0.007)	0.018***	(0.005)
6 years after	0.030***	(0.006)	0.019***	(0.004)
7 years after	0.027***	(0.005)	0.016***	(0.003)
8 years after	0.022***	(0.004)	0.012***	(0.002)
9 years after	0.016***	(0.003)	0.006***	(0.001)
10 years after	0.008***	(0.002)	–	–
Characteristics of land parcels	YES			
Municipal-level characteristics	YES			
Prefecture-year effects	YES			
Municipal-level time trends	YES			
R^2	0.881			
Sample size	828,781			
Average N	39,528			
Average T	12.596			

Note: While we obtained standard errors ("S.E."s) based on the clustering in a municipality before amalgamation (as of FY1995) and those based on clustering in a municipality after amalgamation (as of FY2015) in the figures in the text, we only list the formers in the table since their differences are negligible.

Table A3. Estimation Results for Eq. (7f)

	Merged in FY2004				Merged in FY2005			
	Larger		Smaller		Larger		Smaller	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
9 years before					0.010^{***}	(0.002)	0.010^{***}	(0.002)
8 years before	0.003	(0.003)	0.007^{***}	(0.002)	0.015^{***}	(0.003)	0.010^{***}	(0.003)
7 years before	0.009^{**}	(0.004)	0.012^{***}	(0.003)	0.018^{***}	(0.004)	0.012^{***}	(0.004)
6 years before	0.016^{***}	(0.005)	0.016^{***}	(0.004)	0.023^{***}	(0.006)	0.015^{***}	(0.006)
5 years before	0.023^{***}	(0.007)	0.022^{***}	(0.005)	0.027^{***}	(0.007)	0.019^{**}	(0.007)
4 years before	0.030^{***}	(0.008)	0.028^{***}	(0.006)	0.030^{***}	(0.007)	0.022^{***}	(0.008)
3 years before	0.038^{***}	(0.008)	0.034^{***}	(0.007)	0.034^{***}	(0.008)	0.027^{***}	(0.009)
2 years before	0.046^{***}	(0.009)	0.040^{***}	(0.008)	0.038^{***}	(0.009)	0.029^{***}	(0.010)
1 year before	0.048^{***}	(0.010)	0.048^{***}	(0.009)	0.041^{***}	(0.010)	0.030^{***}	(0.010)
The year of merger	0.041^{***}	(0.011)	0.044^{***}	(0.010)	0.027^{***}	(0.010)	0.019^{**}	(0.009)
1 year after	0.037^{***}	(0.010)	0.040^{***}	(0.011)	0.026^{***}	(0.009)	0.015^{*}	(0.008)
2 years after	0.038^{***}	(0.010)	0.041^{***}	(0.010)	0.015^{*}	(0.008)	0.009	(0.007)
3 years after	0.033^{***}	(0.009)	0.032^{***}	(0.009)	0.013^{*}	(0.007)	0.009	(0.006)
4 years after	0.029^{***}	(0.008)	0.030^{***}	(0.008)	0.017^{***}	(0.006)	0.016^{***}	(0.006)
5 years after	0.031^{***}	(0.008)	0.032^{***}	(0.007)	0.017^{***}	(0.006)	0.018^{***}	(0.005)
6 years after	0.029^{***}	(0.007)	0.030^{***}	(0.007)	0.018^{***}	(0.005)	0.020^{***}	(0.004)
7 years after	0.023^{***}	(0.005)	0.028^{***}	(0.006)	0.014^{***}	(0.004)	0.018^{***}	(0.004)
8 years after	0.019^{***}	(0.005)	0.022^{***}	(0.004)	0.012^{***}	(0.003)	0.012^{***}	(0.003)
9 years after	0.011^{***}	(0.004)	0.017^{***}	(0.003)	0.005^{***}	(0.001)	0.006^{***}	(0.001)
10 years after	0.008^{***}	(0.002)	0.008^{***}	(0.002)	–	–	–	–
Characteristics of land parcels	YES							
Municipal-level characteristics	YES							
Prefecture-year effects	YES							
Municipal-level time trends	YES							
R^2	0.881							
Sample size	828,781							
Average N	39,528							
Average T	12.596							

Note: While we obtained standard errors ("S.E."s) based on the clustering in a municipality before amalgamation (as of FY1995) and those based on clustering in a municipality after amalgamation (as of FY2015) in the figures in the text, we only list the formers in the table since their differences are negligible.