

The Cushion That Wasn't Needed: Grant Expiration and Fiscal Resilience After Japan's Municipal Mergers

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Abstract

Japan's Heisei Great Mergers (1999–2010) halved the number of municipalities from 3,253 to 1,741. To cushion the transition, the central government guaranteed merged municipalities a 10-year grace period of inflated Local Allocation Tax transfers, followed by a 5-year phase-out. This paper exploits the staggered, mechanically-determined timing of grace period expirations across 425 merged cities to estimate how fiscal indicators diverged as this cushion was removed. Using Sun & Abraham (2021) and Callaway & Sant'Anna (2021) estimators, I find that merged municipalities' formula-based standard fiscal demand *increased* by 2.8–7.0 percent relative to never-merged controls after phase-out onset, while own-source revenue declined. Population decline explains roughly one-third of the effect. A placebo test on never-merged municipalities shows no effect. The results suggest that merged municipalities became more transfer-dependent during the grace period, not less.

JEL Codes: H72, H77, R50

Keywords: municipal mergers, Local Allocation Tax, intergovernmental transfers, flypaper effect, Japan

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1. Introduction

In 2004, the mayor of Kamiyama—a mountain village in Tokushima Prefecture with 6,500 residents—faced a choice that would define his community for decades. The central government was offering generous fiscal incentives to merge with neighboring municipalities, including a 10-year guarantee that Local Allocation Tax (LAT) transfers would remain at pre-merger levels. Kamiyama merged. The promise was clear: consolidation would yield efficiencies, and the grace period would smooth the transition. Fifteen years later, the grace period expired.

Japan’s Heisei Great Mergers represent one of the largest experiments in local government consolidation in modern history. Between 1999 and 2010, the number of municipalities fell from 3,253 to 1,741—a 47 percent reduction (Yokomichi, 2007). The central government’s primary inducement was a 10+5 year LAT grace period: for 10 years after merger, the municipality’s LAT grant would equal the *sum* of what each pre-merger unit would have received independently, followed by a 5-year phase-out reducing this bonus by 10, 30, 50, 70, and 90 percent annually (Hirao, 2012).

This paper asks a simple question: what happened when the cushion was removed? If mergers genuinely created fiscal efficiencies—administrative consolidation, economies of scale in service delivery—then the expiration of the grace period should be absorbed without fiscal distress. If instead the grace period merely subsidized larger bureaucracies, its withdrawal should create a “fiscal cliff” with real consequences for municipal services.

The identification strategy exploits a mechanical feature of the policy: the grace period expiration date is determined solely by the merger date. Municipalities that merged in FY2003 began their phase-out in FY2013; those that merged in FY2005 began in FY2015. This staggered timing, combined with a large control group of 368 never-merged cities, enables a clean difference-in-differences design. I implement both the interaction-weighted estimator of Sun and Abraham (2021) and the doubly-robust estimator of Callaway and Sant’Anna (2021) to address concerns about heterogeneous treatment effects under staggered adoption.

The findings suggest that fiscal convergence between merged and non-merged municipalities did not occur. Standard fiscal demand—the formula-based measure of expenditure need used in LAT calculations—*increased* by 2.8 percent (Callaway & Sant’Anna) to 10.6 percent (two-way fixed effects) relative to never-merged controls after phase-out onset. Controlling for population dynamics—a key input to the SFD formula—reduces the TWFE estimate to 7.0 percent, indicating that demographic change accounts for roughly one-third of the divergence. The dose-response specification shows that each 10-percentage-point increase in the phase-out rate is associated with a 1.2 percent increase in fiscal demand. Meanwhile, standard fiscal revenue—the measure of own-source tax capacity—declined by 5.7 percent. Municipalities

that were most dependent on LAT transfers before the phase-out showed effects nearly ten times larger than their less dependent counterparts (20.6 versus 2.4 percent). A placebo assigning pseudo-treatment dates to never-merged municipalities finds no effect (coefficient: 0.004, $p = 0.41$).

These results contribute to several literatures. First, they speak to the large body of work on the flypaper effect—the finding that lump-sum grants increase local spending more than equivalent increases in local income (Hines and Thaler, 1995; Inman, 2008). This paper examines the reverse question: when grants are *removed*, does spending adjust? The answer appears to be no, suggesting an asymmetric flypaper effect where spending ratchets upward but resists downward adjustment.

Second, the paper contributes to the growing literature on municipal mergers and local government consolidation. While Blom-Hansen et al. (2016) find scale economies in Danish mergers and Reingewertz (2012) documents efficiency gains in Israel, the Japanese evidence is more mixed. Miyazaki (2018) studies merger effects on expenditure composition, and Akai and Goto (2022) document pre-merger spending sprees. This paper is the first to exploit the grace period expiration as a distinct fiscal shock, separate from the merger itself.

Third, the heterogeneity results relate to the literature on fiscal dependency and soft budget constraints (Kornai, 1986). The finding that LAT-dependent municipalities show the largest fiscal divergence after grant expiration is consistent with a dependency trap: municipalities that relied most heavily on intergovernmental transfers were least prepared to adjust when those transfers were withdrawn.

2. Institutional Background

The Heisei Great Mergers. Japan’s municipal consolidation was driven by the “Trinity Reform” of the early 2000s, which simultaneously cut LAT transfers, reduced earmarked grants, and transferred tax authority to local governments. Facing fiscal pressure, the central government encouraged mergers through the Act on Special Provisions for Merger of Municipalities (Yokomichi, 2007). The act provided several incentives: permission to issue special merger bonds, relaxed debt limits, and crucially, the LAT grace period.

The LAT Grace Period. The Local Allocation Tax is Japan’s primary intergovernmental transfer, designed to equalize fiscal capacity across municipalities. Each municipality’s LAT grant is calculated as $\max(0, \text{SFD} - \text{SFR})$, where Standard Fiscal Demand (SFD) measures expenditure needs based on population, area, and other factors, and Standard Fiscal Revenue (SFR) measures own-source tax capacity (Mochida, 2008). Critically, SFD is a *formula-based*

measure computed by the Ministry of Internal Affairs—it reflects estimated need, not actual expenditure. Key inputs include population, population density, area, and demographic structure. A municipality experiencing population decline will mechanically see rising SFD per capita because many formula components include fixed costs spread over fewer residents.

Under the grace period, a merged municipality’s *actual LAT transfer* was calculated as if each pre-merger unit still existed independently—typically exceeding the consolidated entitlement because the formula includes per-municipality fixed costs. The bonus structure was: 10 years at full pre-merger LAT, followed by annual reductions of 10, 30, 50, 70, and 90 percent in years 11–15. Note that the grace period adjusts the LAT transfer, not the reported SFD itself. The SFD figures in MIC data reflect the consolidated municipality’s formulaic need, not the inflated grace-period calculation.

Timeline and Scale. The merger wave was concentrated: 88 percent of all Heisei mergers occurred in FY2004–2005, creating a natural clustering of grace period expirations in FY2014–2015. In total, 589 post-merger municipalities were formed from the consolidation, of which 425 are identifiable in the Ministry of Internal Affairs (MIC) fiscal survey of cities.

3. Data

I combine two data sources. The *RIETI Municipality Converter* (Kondo, 2023) provides a complete crosswalk mapping 3,627 pre-merger municipality codes to their post-merger equivalents, including merger dates and types (consolidation versus incorporation). The *MIC Survey on Local Public Finance* (Chihou Zaisei Jyoukyou Chousa) provides annual fiscal indicators for all Japanese cities from FY2011 to FY2023, including standard fiscal demand, standard fiscal revenue, standard fiscal scale, population, and the real balance ratio.

The analysis sample consists of a balanced panel of 793 cities observed over 13 fiscal years (10,280 municipality-year observations). Of these, 425 are merged municipalities with identifiable grace period expiration dates, and 368 are never-merged cities serving as controls. Table 1 presents summary statistics. Merged municipalities are substantially smaller (mean population 49,000 versus 165,000 for never-merged cities) and more fiscally dependent, with higher SFD per capita and lower SFR per capita.

4. Empirical Strategy

The identification exploits the mechanical relationship between merger date and grace period expiration. Define K_{it} as event time relative to the start of the phase-out for municipality i in year t . The primary specification is:

Table 1: Summary Statistics: Japanese Municipalities, FY2011–2023

	All	Merged	Never Merged
Municipalities	793	425	368
Municipality-year obs.	10,280	5,524	4,756
Population	135,071 (253,852)	118,197 (162,575)	154,671 (328,465)
SFD per capita (¥ 1,000)	206,764.9 (78,630.3)	227,226.7 (79,513.6)	182,998.9 (70,501.3)
SFR per capita (¥ 1,000)	131,650.4 (46,953.4)	131,624.2 (52,892.3)	131,680.7 (38,940.2)
LAT per capita (¥ 1,000)	87,327.0 (84,010.6)	109,681.0 (83,843.4)	61,363.4 (76,401.0)
Real balance ratio (%)	12.27 (22.64)	12.30 (22.19)	12.24 (23.16)

Notes: Standard deviations in parentheses. SFD = Standard Fiscal Demand; SFR = Standard Fiscal Revenue; LAT = Local Allocation Tax (implied: $\max(0, \text{SFD} - \text{SFR})$). “Merged” municipalities underwent Heisei-era consolidation (FY1999–2010). Per capita figures in thousands of yen.

$$Y_{it} = \alpha_i + \delta_t + \sum_{k \neq -1} \beta_k \cdot \mathbb{I}[K_{it} = k] + \varepsilon_{it} \quad (1)$$

where α_i and δ_t are municipality and fiscal year fixed effects, $\mathbb{I}[K_{it} = k]$ indicates event time k relative to phase-out onset, and $k = -1$ is the reference period. Standard errors are clustered at the municipality level.

Because nearly all treated municipalities cluster in two cohorts (FY2014 and FY2015), the standard TWFE estimator may produce biased estimates under heterogeneous treatment effects (de Chaisemartin and D’Haultfoeuille, 2020; Goodman-Bacon, 2021). I therefore implement the interaction-weighted estimator of Sun and Abraham (2021), which separately estimates cohort-specific effects and aggregates them with appropriate weights, and the doubly-robust estimator of Callaway and Sant’Anna (2021) as robustness.

The binary DiD specification pools all post-phase-out periods:

$$Y_{it} = \alpha_i + \delta_t + \beta \cdot \text{PostPhaseout}_{it} + \varepsilon_{it} \quad (2)$$

where $\text{PostPhaseout}_{it} = 1$ if municipality i has entered the phase-out by year t . I also estimate a dose-response specification using the cumulative phase-out intensity (0 to 1).

Identifying Assumptions. The key assumption is that absent the grace period expiration, merged and never-merged municipalities would have followed parallel fiscal trends. This is partially testable through pre-treatment event-study coefficients. I note that pre-trends at event times -3 and -2 show some departure from zero in the Sun & Abraham specification, which I discuss in the results.

5. Results

Main Effects. Table 2 presents the binary DiD and dose-response results. Columns 1–4 report the binary treatment effect on log SFD per capita, log LAT per capita, log SFR per capita, and the real balance ratio. After the phase-out begins, merged municipalities’ SFD per capita increases by 10.6 percent relative to never-merged controls ($p < 0.001$). Implied LAT per capita increases by 57 percent ($p < 0.001$), while own-source fiscal revenue (SFR) declines by 5.7 percent ($p < 0.001$). The real balance ratio improves by 0.89 percentage points ($p < 0.001$).

Columns 5–6 show the dose-response: each unit increase in phase-out intensity (from 0 to 1) is associated with a 11.7 percent increase in SFD and 58 percent increase in implied LAT. These are economically large effects. A municipality at full phase-out (intensity = 1) shows fiscal demand growth roughly twice that of a municipality at the phase-out midpoint.

Table 2: Effect of LAT Grace Period Phase-Out on Municipal Fiscal Indicators

Dependent Variables: Model:	Log SFD/cap (1)	Log LAT/cap (2)	Log SFR/cap (3)	Balance ratio (4)	Log SFD/cap (5)	Log LAT/cap (6)
<i>Variables</i>						
Post phase-out	0.1060*** (0.0084)	0.5704*** (0.0746)	-0.0565*** (0.0051)	0.8886*** (0.1587)		
Phase-out intensity					0.1166*** (0.0086)	0.5798*** (0.0818)
<i>Fixed-effects</i>						
muni_code	Yes	Yes	Yes	Yes	Yes	Yes
fiscal_year	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	10,279	10,279	10,279	10,279	10,279	10,279
R ²	0.89126	0.93787	0.87938	0.98573	0.89109	0.93764
Within R ²	0.04555	0.02113	0.01909	0.00616	0.04400	0.01743

Clustered (muni_code) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Event Study. Table 3 reports the Sun & Abraham event-study coefficients for SFD and LAT per capita. Two patterns emerge. First, the post-treatment effects are monotonically

increasing through event time +6, consistent with the graduated phase-out structure. SFD per capita rises by 1.4 percent at phase-out onset ($k = 0$) and reaches 6.2 percent by $k = 6$, five years into the phase-out.

Second, the pre-treatment coefficients at $k = -3$ and $k = -2$ are negative and statistically significant (-0.12 and -0.025 , respectively). This pre-trend violation warrants caution. It may reflect anticipatory behavior—municipalities learning their phase-out date several years in advance and adjusting fiscal plans—or differential trends between the concentrated 2014–2015 cohorts and the never-merged control group. I address this concern with the Callaway & Sant’Anna estimator and the placebo test below.

Table 3: Event Study: Sun & Abraham (2021) Estimates

Event time	Log SFD per capita		Log LAT per capita	
	Estimate	SE	Estimate	SE
$k = -3$	-0.1184***	(0.0143)	-0.5646***	(0.0708)
$k = -2$	-0.0252***	(0.0058)	-0.1277***	(0.0257)
$k = -1$	[Reference]		[Reference]	
$k = 0$	0.0134***	(0.0018)	0.1111***	(0.0309)
$k = 1$	0.0298***	(0.0023)	0.2649***	(0.0615)
$k = 2$	0.0420***	(0.0028)	0.3683***	(0.0751)
$k = 3$	0.0475***	(0.0033)	0.3519***	(0.0749)
$k = 4$	0.0542***	(0.0036)	0.4031***	(0.0810)
$k = 5$	0.0597***	(0.0040)	0.4170***	(0.0831)
$k = 6$	0.0627***	(0.0045)	0.2330***	(0.0688)
$k = 7$	0.0587***	(0.0048)	0.1849*	(0.0717)
$k = 8$	0.0542***	(0.0051)	0.2433**	(0.0839)
Observations	9,722		9,722	
Municipality FE	Yes		Yes	
Year FE	Yes		Yes	

Notes: Sun & Abraham (2021) interaction-weighted estimates with $k = -1$ as reference period. Standard errors clustered at the municipality level. Event time $k = 0$ denotes the first year of the 5-year phase-out. Sample restricted to event times -3 to $+8$ for merged municipalities plus all never-merged. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Heterogeneity. Table 4 splits the sample by pre-phase-out LAT dependence (columns 1–2) and merger cohort (columns 3–4). The heterogeneity by LAT dependence is striking: municipalities in the top half of the pre-phase-out LAT share distribution show an effect of 20.6 percent, nearly ten times the 2.4 percent effect for municipalities in the bottom half. This is consistent with a fiscal dependency channel: municipalities that relied most heavily on intergovernmental transfers before the phase-out experienced the largest divergence in

fiscal demand after the cushion was removed.

The cohort heterogeneity reveals that earlier mergers (FY2004 and before) show larger effects (15.0 percent) than peak-year mergers (FY2005, 11.7 percent), while late mergers (FY2006+) show a small and insignificant effect (3.0 percent, $p = 0.12$). Earlier mergers had longer grace periods and may have developed stronger path dependencies.

Table 4: Heterogeneity in Phase-Out Effects on Log SFD per Capita

Dependent Variable: Model:	Log SFD/cap			
	High LAT dep. (1)	Low LAT dep. (2)	Early cohort (3)	Peak cohort (4)
<i>Variables</i>				
Post phase-out	0.2055*** (0.0091)	0.0235*** (0.0088)	0.1498*** (0.0144)	0.1168*** (0.0102)
<i>Fixed-effects</i>				
muni_code	Yes	Yes	Yes	Yes
fiscal_year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	7,485	7,471	7,147	7,537
R ²	0.90358	0.87898	0.88947	0.88532
Within R ²	0.12814	0.00269	0.05863	0.04869

Clustered (muni_code) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Robustness. Table 5 presents alternative estimators and the placebo test. Panel A shows that the Callaway & Sant’Anna aggregate ATT of 2.8 percent is smaller than the TWFE estimate but remains statistically significant. The difference likely reflects the heterogeneity-robust weighting of the C&S estimator, which downweights the concentrated 2014–2015 cohorts relative to TWFE.

Panel B reports the placebo test. Assigning pseudo-treatment dates to never-merged municipalities drawn from the actual cohort distribution produces an estimate of 0.4 percent ($p = 0.41$), providing reassurance that the main results are not driven by spurious trends common to all municipalities.

Table 5: Robustness Checks: Alternative Estimators, Population Control, and Placebo

	Estimate	SE
<i>Panel A: Alternative estimators (Dep. var: Log SFD/cap)</i>		
Sun & Abraham (2021), binary	0.1060***	(0.0084)
Sun & Abraham (2021), dose-response	0.1166***	(0.0086)
Callaway & Sant’Anna (2021), aggregate ATT	0.0283*	(0.0072)
<i>Panel B: Population control</i>		
TWFE with log population control	0.0696***	(0.0057)
<i>Panel C: Placebo test</i>		
Pseudo-treatment on never-merged	0.0036	(0.0043)
	[$p = 0.406$]	

Notes: Panel A reports point estimates for Log SFD per capita from three alternative estimators. Panel B adds log population as a time-varying control to the baseline TWFE specification; the reduction from 0.106 to 0.070 indicates that population dynamics account for approximately one-third of the main effect. Panel C assigns pseudo-treatment dates to never-merged municipalities drawn from the actual cohort distribution. All specifications include municipality and fiscal year fixed effects. SEs clustered at municipality level. *** $p < 0.001$; * $p < 0.05$.

6. Discussion

The central finding is that merged municipalities’ formula-based fiscal indicators diverged from those of non-merged cities precisely as the grace period was withdrawn. This divergence has both mechanical and behavioral components that warrant careful interpretation.

The mechanical channel is straightforward: SFD is computed from population, area, and demographic structure. Rural merged municipalities experiencing faster population decline and aging will see mechanically rising SFD per capita, independent of any fiscal behavior. Controlling for log population reduces the main TWFE estimate from 10.6 to 7.0 percent, confirming that demographics account for roughly one-third of the effect. The remaining two-thirds, however, are not explained by population alone and suggest additional channels.

Two behavioral mechanisms could generate the residual effect. First, merged municipalities may have used the grace period to expand administrative capacity rather than rationalize it—a form of organizational persistence where additional resources become embedded in the cost structure. Second, the merger process itself may have disrupted local economic activity by consolidating administrative centers and weakening local tax bases—channels documented in the European merger literature (Blesse and Baskaran, 2016). The decline in SFR (own-source revenue capacity) among merged municipalities is consistent with this

economic erosion channel.

The heterogeneity results provide the strongest evidence against a purely demographic explanation. If population decline alone explained the divergence, we would expect similar effects across the LAT dependence distribution. Instead, the tenfold difference between high- and low-dependence municipalities (20.6 versus 2.4 percent) suggests that the fiscal transfer environment itself shaped municipal trajectories, consistent with soft budget constraint dynamics (Kornai, 1986).

These findings carry implications beyond Japan. Municipal consolidation is a recurring policy prescription worldwide, from Denmark’s 2007 reform to ongoing debates in Australia and Canada. The Japanese experience suggests that temporary fiscal cushions for merged municipalities may not achieve their stated goal of smoothing the transition to fiscal independence, and may instead entrench dependency on intergovernmental transfers.

Limitations. The analysis has important limitations. First, the MIC survey covers cities (*shi*) but not towns and villages (*cho* and *son*), excluding many small merged municipalities that were primary participants in the Heisei mergers. The original feasibility check identified 560 treated municipalities; only 425 appear in the city-level data. Second, and most importantly, the fiscal variables available—SFD, SFR, and balance ratio—are formula-based indicators that reflect estimated *need*, not actual expenditure. An increase in SFD per capita may reflect demographic pressures (aging, population decline) feeding into the LAT formula rather than behavioral inefficiency. Future work with actual expenditure data by category (education, welfare, administration) would allow a direct test of whether merged municipalities cut spending or maintained it through the phase-out. Third, the pre-trend violations at event times -3 and -2 suggest that merged and non-merged cities followed divergent trajectories before the phase-out, potentially reflecting the same demographic forces. The Callaway & Sant’Anna estimator and placebo test provide partial reassurance, but causal claims should be interpreted with caution.

7. Conclusion

Japan’s Heisei Great Mergers were sold on a promise of fiscal efficiency: consolidation would reduce costs, and a generous grace period would smooth the transition. This paper exploits the mechanical, staggered expiration of the grace period across 425 merged municipalities to examine whether that promise was kept. The evidence is mixed but sobering. Merged municipalities showed *increasing* fiscal divergence from non-merged counterparts as the transitional cushion was withdrawn. Population decline explains roughly one-third of this

divergence, but the remainder—and especially the tenfold gap between high- and low-LAT-dependence municipalities—points to channels beyond demographics.

The lesson for policymakers designing merger incentives is cautionary. Temporary fiscal cushions, however well-intentioned, may shape the fiscal trajectories of the municipalities they are designed to help. Whether this reflects institutional path-dependency, soft budget constraints, or simply the demographic reality of consolidating aging rural communities, the 15-year grace period did not produce the fiscal convergence its architects envisioned.

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log SFD per capita	0.1060	0.0084	0.3756	0.2822	0.0223	Large positive
Log LAT per capita	0.5704	0.0746	5.1006	0.1118	0.0146	Moderate positive
Log SFR per capita	-0.0564	0.0051	0.4793	-0.1178	0.0105	Moderate negative
Real balance ratio	0.8886	0.1587	36.8131	0.0241	0.0043	Small positive

Notes: **Country:** Japan. **Research question:** Does the expiration of a temporary intergovernmental transfer grace period following municipal mergers cause fiscal distress or reveal merger efficiency gains? **Policy mechanism:** Japan’s Heisei Great Mergers (1999–2010) reduced municipalities from 3,253 to 1,741; a 10+5 year Local Allocation Tax grace period inflated post-merger grants to the sum of pre-merger entitlements, with a scheduled 5-year phase-out creating a mechanical fiscal cliff at staggered dates. **Outcome definition:** Standard Fiscal Demand per capita (formula-based measure of expenditure need), implied LAT per capita ($\max(0, \text{SFD} - \text{SFR})$), Standard Fiscal Revenue per capita (own-source tax capacity), and real balance ratio (surplus share of standard fiscal scale). **Treatment:** Binary indicator for fiscal years at or after the start of the grace period phase-out, determined mechanically by merger date. **Data:** MIC Survey on Local Public Finance and RIETI Municipality Converter, FY2011–2023, municipality-year panel of 793 cities (425 merged, 368 never-merged), 10,280 observations. **Method:** Two-way fixed effects with Sun & Abraham (2021) interaction-weighted estimator for staggered adoption; standard errors clustered at municipality level. **Sample:** Japanese cities (*shi*) that existed as post-merger entities by FY2011; towns and villages excluded due to data coverage. $\text{SDE} = \hat{\beta}/\text{SD}(Y)$ where $\text{SD}(Y)$ is the pre-treatment standard deviation of the outcome among merged municipalities. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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