CIRJE-F- 1250

Workload and Bureaucratic Disentitlement: Evidence from Public Assistance in Japan

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May 2025

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Abstract: This study examines whether variations in workload influence rationing behavior in welfare provision by Japanese local governments. Exploiting exogenous changes in welfare caseloads resulting from a large wave of municipal mergers, it analyzes how workload size affects the number of welfare applications, withdrawals, and rejections at the city level. Controlling for pre-existing caseloads, the analysis finds that an increase in caseworker staffing (i.e., a reduction in workload) leads to more applications and withdrawals, but does not affect rejections. Notably, the increase in applications exceeds the increase in withdrawals, resulting in a net rise in accepted applications. Moreover, the positive effects of increased caseworker capacity on applications and withdrawals are more pronounced under heavier workloads. These results suggest that Japanese welfare offices may rely on informal forms of rejection. Overall, the findings support the Type I error explanation of bureaucratic disentitlement—where eligible individuals are erroneously excluded—as emphasized in the literature, rather than the Type II error explanation associated with the "cursory assessment" hypothesis advanced by the Japanese government.

Keywords: bureaucratic disentitlement, welfare caseloads, caseworkers, workload, public assistance, rationing behavior

JEL Codes: H73, H75, H77

* This study was financially supported by Grant-in-Aid for Scientific Research (C) 21K01521, (B) 20330064, and (A) 15070000033. The author would like to thank Mototsugu Fukushige, Haruko Noguchi, Yasuyuki Sato and participants at various academic meetings for their valuable comments and feedback.

1. Introduction

Heavy workloads among welfare caseworkers pose significant challenges to the effective implementation of welfare programs[1]. Excessive caseloads can deter caseworkers from engaging in discretionary efforts beyond their prescribed responsibilities (Ridzi and London 2006) and may lead to staff burnout (Takeda et al., 2002), thereby reducing overall working efficacy (Lloyd et al., 2002). This strain may, in turn, contribute to higher staff turnover, further exacerbating the workload for remaining personnel (Smith, 2005). As a result, caseworkers may tend to overlook legitimate applications or deny ongoing claims, undermining the assistance that they are tasked with providing. For example, Moffitt (2003) documents that caseworkers in U.S. cities frequently discouraged qualified individuals from applying for welfare benefits. Meanwhile, Lens (2006) reports that nearly half the sanctions imposed by caseworkers under the Temporary Assistance for Needy Families (TANF) program in the U.S. were overturned on appeal. These findings suggest that caseworkers may occasionally use improper procedures to restrict access to benefits. This outcome exemplifies the concept of "bureaucratic disentitlement" (Lipsky, 1984; Brodkin, 1997) and constitutes a Type I error, in which eligible individuals are erroneously excluded from the assistance to which they are entitled. If heavy workloads are a source of bureaucratic disentitlement, as argued above, increasing caseworker staffing could help reduce workload pressure and thereby alleviate excessive rationing in welfare provision.

However, an opposing claim emerged in 2005 when the central government negotiated a cost-sharing scheme with local governments for Japan's Public Assistance (PA) program.[2] The central government, which sets the rules and benefit levels of the program, provides funding for local governments to implement the assistance. It argued that, as PA benefits require a labor-intensive means-testing procedure, caseworkers in understaffed local offices may conduct superficial and hasty assessments of applications, leading to an unnecessary increase in caseloads (Kimura, 2006). In contrast to the Type I error associated with bureaucratic disentitlement, this scenario—according to the central government—represents a Type II error, in which individuals who are not entitled to receive benefits are nonetheless approved. This argument may be called the "cursory assessment" hypothesis, which posits that increasing the number of caseworkers would reduce workloads, lessen the occurrence of cursory assessments, and ultimately bring the caseload to an appropriate level.

Accordingly, increasing the number of caseworkers may lead to fewer acceptances (as

[1] "Workload" is defined as the amount of work required for a single caseworker to complete their designated tasks (Strolin et al., 2007).

[2] Here, PA refers to the social assistance known in Japanese as *Seikatsu Hogo*, which literally means "the protection (*hogo*) of daily life (*seikatsu*)."

suggested by the cursory assessment hypothesis) or to more acceptances (as implied by the bureaucratic disentitlement hypothesis). In either case, it is imperative to examine whether understaffed welfare offices compromise the effective implementation of social programs. Despite the importance of this issue, however, only a few studies have quantitatively examined the impact of workload on program implementation.[3] Hainmueller et al. (2016), using data from a large-scale pilot conducted by Germany's employment agency, found that offices with lighter workloads increased monitoring, imposed more sanctions, enhanced job search efforts, and registered additional job vacancies. Schmieder and Trenklee (2020), using data from the Integrated Employment Biographies in Germany, similarly found that caseworker teams handling larger caseloads spent less time and fewer resources on individual cases. Meanwhile, in the Japanese context, Suzuki and Zhou (2007) addressed a related question by regressing caseload sizes on the number of caseloads per caseworker. However, their analysis was less direct in evaluating either the bureaucratic disentitlement or cursory assessment hypotheses, as it relied on the stock measures of caseloads rather than the flow indicators, such as the number of new applications accepted or rejected.[4] Moreover, their study did not address the issue of endogeneity. When local governments allocate additional caseworkers in response to increased caseloads, the number of caseworkers becomes endogenous. Naturally, empirical analysis must allow for this endogeneity.

This study examines the effect of workload on welfare rationing using municipal-level data from Japan. There are three key advantages to using Japanese data. First, exogenous variation in PA caseloads resulting from municipal boundary reforms in the mid-2000s can be exploited as an instrument to address the issue of endogeneity. Japan's local government system is two-tiered, with municipalities (cities, towns, and villages) forming the first tier and prefectures the second. Under national law, cities and prefectures are required to establish welfare offices to implement PA programs. In contrast, towns and villages (TVs) are not required to do so, as PA programs for their residents are typically administered by prefectural welfare offices (with some exceptions). Consequently, when a city merges with one or more TVs, it assumes administrative responsibility

[3] Numerous studies have investigated the factors influencing welfare caseloads; however, most have focused on aspects other than caseworkers' workload. Furthermore, most of these studies were conducted in the United States, with similar analyses conducted in Canada (Spindler and Gilbreath, 1979), Sweden (Gustafsson, 1984), Spain (Ayala and Pérez, 2005), and Japan (Suzuki and Zhou, 2007). With the exception of Brehm and Saving (1964), U.S. studies have been primarily motivated by the sharp increase in caseloads observed in the early 1990s. This was followed by an abrupt decline after 1994, which coincides with a series of welfare reforms at the state and federal levels. Thus, studies in the U.S. primarily investigate the effects of economic factors—such as income levels and unemployment—along with the effect of welfare program changes (Schiller and Brasher, 1993; Johnson et al., 1994; Schiller, 1999; Ziliak et al., 2000; Blank, 2001; Huang et al., 2004; Moffitt, 2003; Cadena et al., 2006; Danielson and Klerman, 2008). Moreover, researchers have examined additional influencing factors, including the size of at-risk populations (Conte et al., 1998), sluggish adjustments in welfare participation (Figlio and Ziliak, 1999; Ziliak et al., 2000), and regional labor market conditions (Lee et al., 2002; Lewis and Henry, 2004; Hill and Murray, 2008).

[4] Suzuki and Zhou (2007) argue that increased number of caseworkers implies greater resources to encourage clients to transition from reliance on welfare benefits to self-sufficiency, thereby reducing the number of welfare recipients.

for PA recipients in the former TV areas who were previously under prefectural jurisdiction, resulting in an exogenous increase in the recipient population in the merged city. This increase can be used as an instrument to estimate the effects of increased PA caseworker staffing.

Second, the institutional design of the Japanese program provides another advantage. Local governments in Japan implement PA program according to uniform rules set by the central government and cannot alter the level of assistance or other policy parameters within the system. Therefore, the endogeneity of such policy parameters (Mayer, 2000) is of less concern in the current study. Moreover, this uniformity allows us to utilize data from all cities without concern for the differences in assistance systems among subnational regions. This stands in contrast to studies on the analogous issues in the U.S., where assistance systems vary across states. To avoid complications arising from interstate differences, prior studies have used small samples that are limited to a single state (Grubb 1984; Lee et al. 2002; Kerman and Haider 2004; Hill and Murray, 2008).

Third, in contrast to studies that rely solely on PA caseload data, this study utilizes multiple output measures that capture welfare rationing, taking advantage of unpublished administrative data from the *Report on Social Welfare Administration and Services* compiled by the Japanese Ministry of Health, Labour and Welfare (MHLW). Specifically, these measures consist of (1) the number of applications for PA programs, (2) the number of application withdrawals, and (3) the number of application rejections. Welfare offices typically conduct intake interviews with potential applicants before applications are formally submitted. Anecdotal evidence—including reports by Japanese newspapers and journalists—suggests that these interviews are sometimes used to discourage potential recipients from applying. Thus, a larger number of applications can serve as a proxy for relaxed assistance rationing. Even after applications are formally submitted, welfare offices may still attempt to persuade some applicants to withdraw. In addition to outright rejections, these two forms of implicit rationing may plausibly be shaped by the availability of administrative resources at welfare offices.

The remainder of this paper is organized as follows. Section 2 outlines the institutional background of Japan's PA system and examines changes in PA caseloads in cities that merged with TVs, along with subsequent changes in the number of caseworkers. Section 3 presents the regression models used to estimate the effects of caseworker size on the rationing behavior of welfare offices and discusses the results. Section 4 concludes.

2. Effect of exogenous changes in PA caseloads on the number of caseworkers

2.1. Municipal mergers and changes in PA caseloads

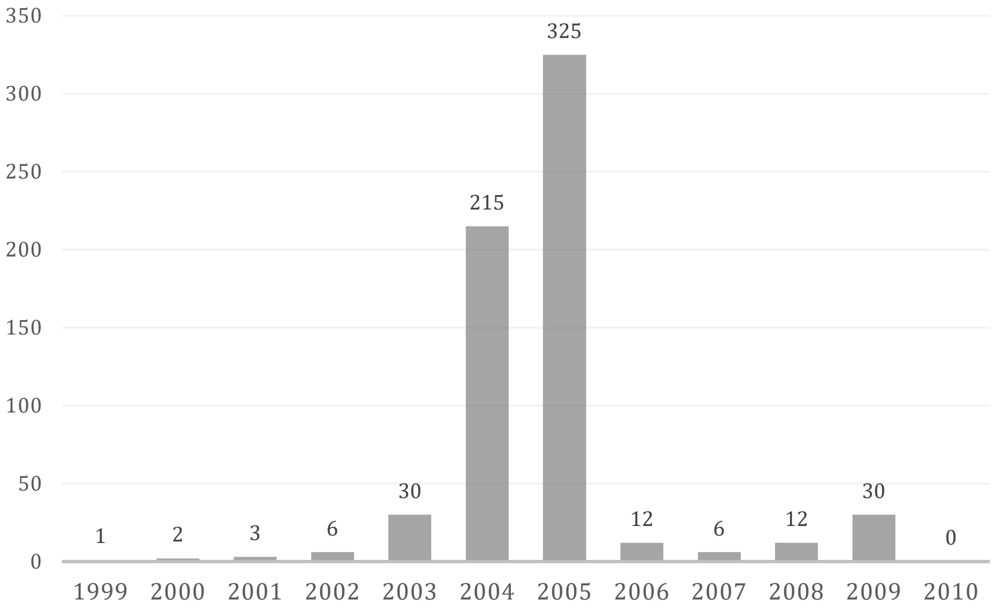
To obtain exogenous variation in PA caseloads, this study exploits the wave of Japanese municipal mergers in the mid-2000s. This wave was triggered by a policy shift in 1999, when legislation was enacted to promote fiscal decentralization and emphasize the role of municipalities in providing public services. Recognizing that many municipalities were too small to manage decentralized functions effectively, the central government encouraged mergers by offering generous fiscal and administrative incentives. As a result, numerous mergers occurred, reducing the number of municipalities by 47%, from 3,229 at the end of FY1999 to 1,727 at the end of FY2010. As illustrated in **Figure 1**, these mergers were most concentrated in FY2004 and FY2005, peaking in FY2005. Thereafter, incentives for mergers were significantly scaled back, and the campaign officially ended at the end of FY2009.

The key source of exogenous variation is found in cities that merged with TVs. These mergers increased the PA caseloads in the merged cities, as the original city's PA program was extended to include recipients in former TV areas who had previously been covered by the prefectural program. Since municipal decisions to merge were unrelated to the specific operations of welfare offices, the resulting changes in caseloads can be considered exogenous. Moreover, these mergers may have prompted increases in the number of PA caseworkers, as national law provides a guideline for local governments recommending a ratio of one caseworker per 80 recipients. However, because this ratio is not legally binding, municipal responses may have varied depending on local conditions.

Between 1999 and 2007, 341 out of 786 cities had merged with TVs. This study focuses on FY2005, which recorded the highest number of municipal mergers in a single fiscal year.[5] After excluding cities with data anomalies, the final sample includes 124 cities that merged with TVs between April 2005 and March 2006 (FY2005). **Figure 2** displays monthly PA caseloads for each of these cities from April 2004 to March 2007, revealing abrupt increases during the months in which mergers occurred. In each panel, the red vertical line indicates the month of the merger, and the horizontal axis represents the number of months since April 2004.

[5] In a preliminary analysis, an analogous estimation was conducted for FY2004 mergers with FY2004 and FY2005 data. A sample of 452 cities including 82 that merged with TVs in FY2004 (April 2004 to March 2005). The instruments in this analysis were weak, likely due to a small share of merged cities (0.181 = 81/452 for FY2004, compared with 0.254 = 126/496 for FY2005). Consequently, the instrumental variable (IV) estimates using FY2004/FY2005 data were statistically insignificant.

Figure 1. Number of municipal mergers (2000–2010)



Source: Ministry of Internal Affairs and Communications

Figure 2. Monthly caseloads of cities merged with TVs in FY2005

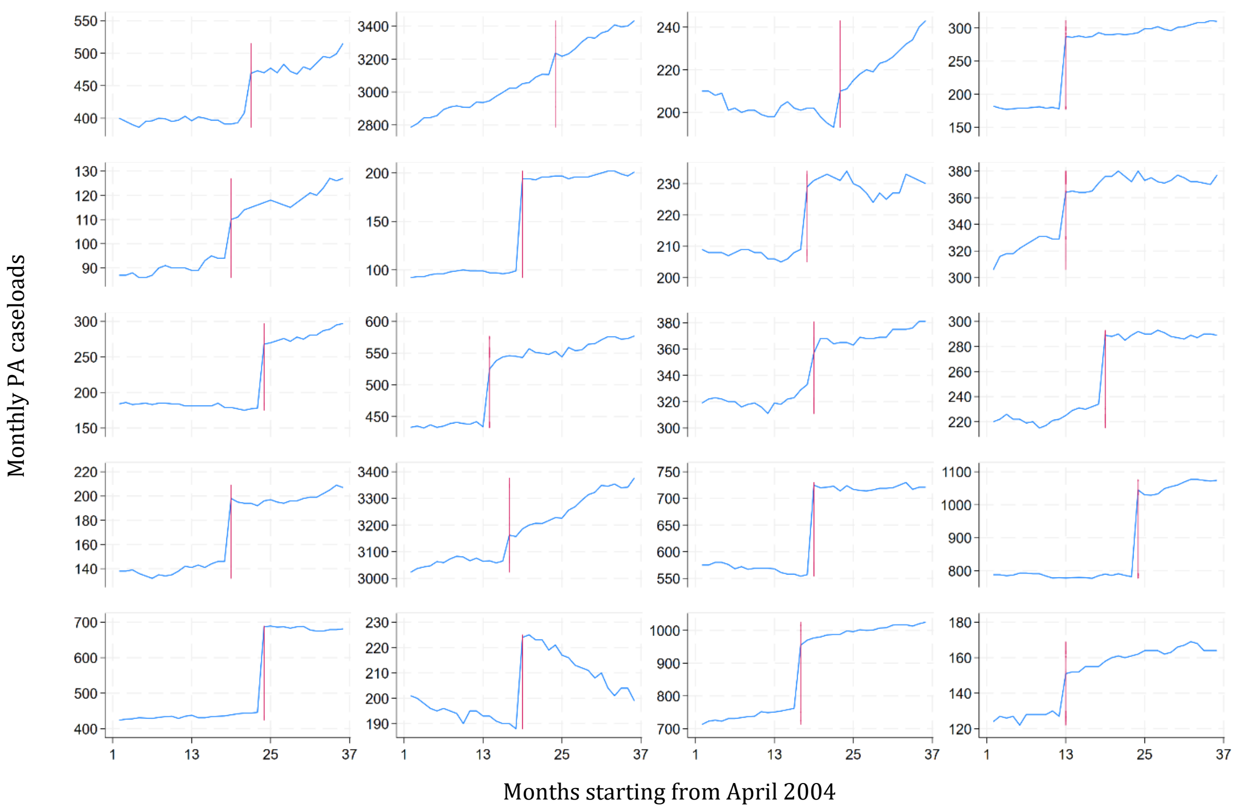


Figure 2. Monthly caseloads for cities merged with TVs in FY2005 (Continued)

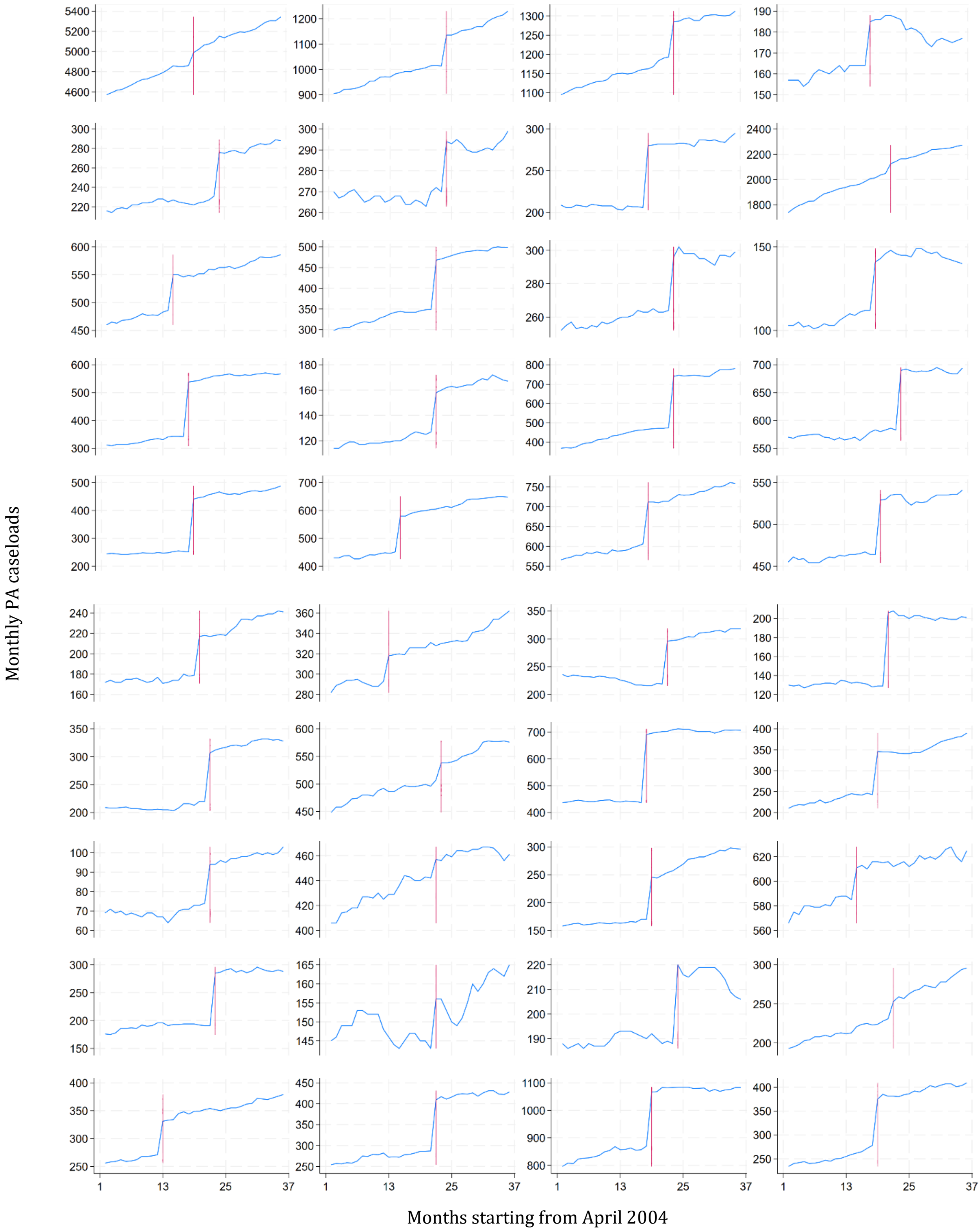


Figure 2. Monthly caseloads for cities merged with TVs in FY2005 (Continued)

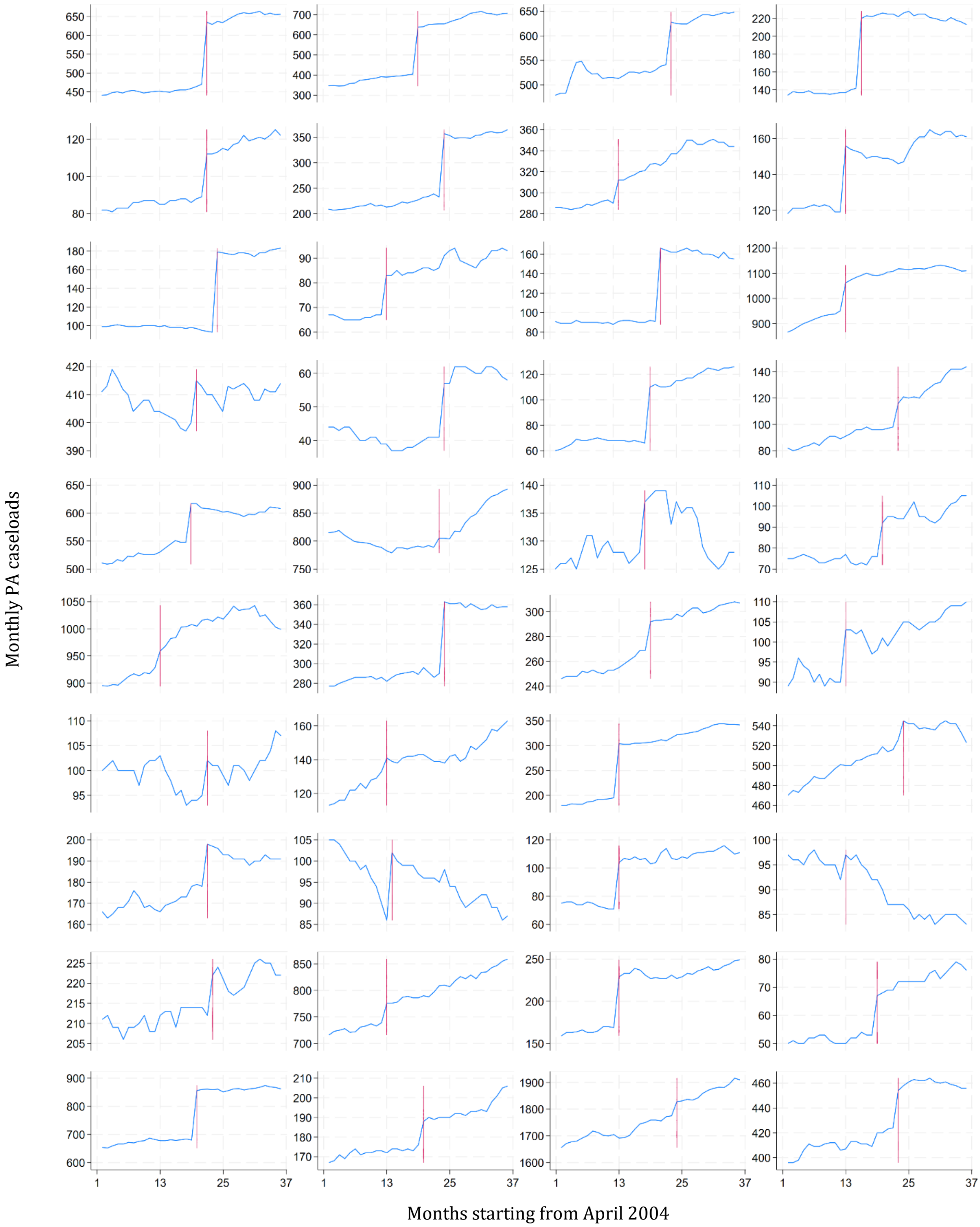
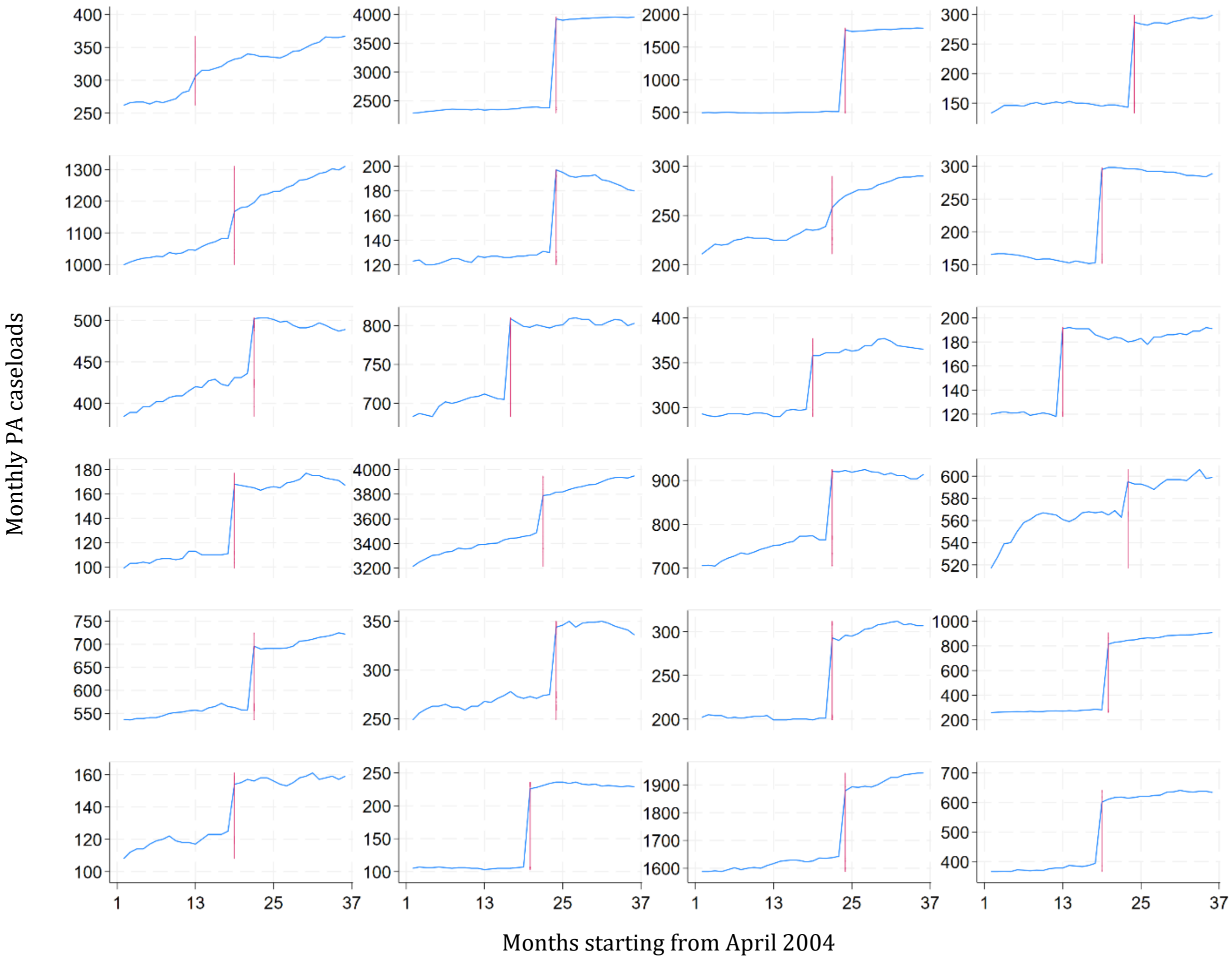


Figure 2. Monthly caseloads for cities merged with TVs in FY2005 (Continued)

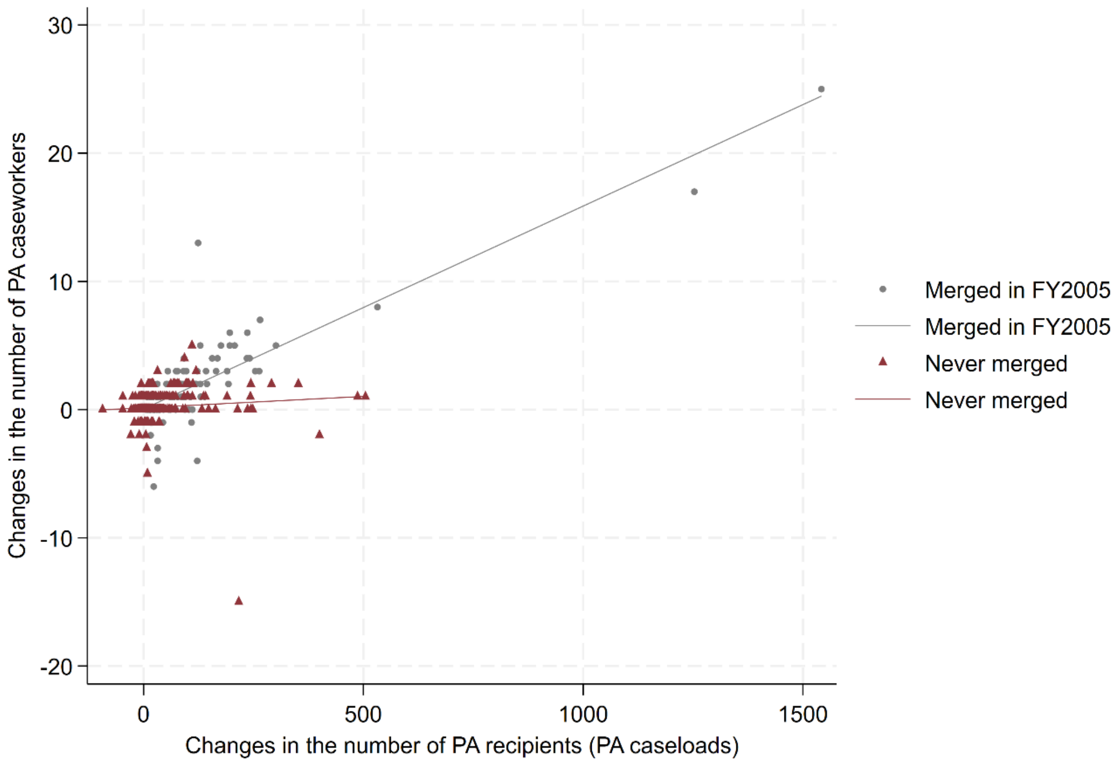


Notes: (1) The vertical axis indicates the number of monthly PA caseloads, while the horizontal axis shows the number of months since April 2004. (2) The red vertical line denotes the month in which municipal mergers occurred.
Source: Ministry of Health, Labour and Welfare. *Report on Social Welfare Administration and Services*.

2.2. Increased PA caseloads and resulting changes in PA caseworkers

The estimation uses the 124 cities listed in **Figure 2** as the treatment group, along with 370 cities that did not undergo mergers during the 2000s as the control group. **Figure 3** plots changes in PA caseloads (horizontal axis) against changes in the number of PA caseworkers (vertical axis). The dots and their fitted line represent cities that merged with TVs. For these merged cities, changes in PA caseloads are measured as the difference between the caseloads in the month of the merger and those in the preceding month. Because the number of PA caseworkers is recorded on the first day of each fiscal year (i.e., April 1), changes in the number of caseworkers are measured as the difference between April 1, 2005 (the beginning of FY2005) and April 1, 2006 (the beginning of FY2006). Despite some erratic observations indicating a decrease in the number of caseworkers, the scatter of dots suggests a positive correlation between the two variables. The triangles and their fitted line represent cities that did not undergo mergers during the 2000s. For these cities, changes in PA caseloads are calculated as the difference between the annual average caseloads in FY2005 and FY2006. The flatter slope of the fitted line for unmerged cities indicates a weaker correlation than that observed for the merged cities. Overall, the pattern in **Figure 3** supports using these changes in PA caseloads induced by mergers with TVs as a valid instrument for the estimation.

Figure 3. Correlation between changes in caseloads and caseworkers



Notes: Dots and their fitted line represent changes in cities that merged with TVs in FY2005; triangles and their fitted line represent cities that did not undergo mergers during the 2000s. For merged cities, changes in PA caseloads are calculated as the difference between the caseload in the month of the merger and that in the preceding month. The number of PA caseworkers is recorded on the first day of each fiscal year, with changes measured between the beginning of FY2005 and FY2006.

To assess whether the caseload changes induced by municipal mergers constitute a valid instrumental variable, the following analysis regresses changes in the number of caseworkers on changes in caseloads, using a combined sample of the two groups shown in **Figure 3**. The analysis begins with an undifferenced model that incorporates unobserved heterogeneity c_i^x:

$$x_{it} = \alpha \cdot z_{it-1} + \sum_k \gamma_k^x \cdot w_{k.it} + c_i^x + \mu_t^x + u_{it}^x$$

for t = FY2005 and FY2006. Taking the first difference yields the following regression model:

$$\Delta x_{it} = \alpha \cdot \Delta z_{it-1} + \sum_k \gamma_k^x \cdot \Delta w_{k.it} + \delta^x + \epsilon_{it}^x \tag{1}$$

for t = FY2006, where Δx_{it} denotes the annual changes in the number of caseworkers from the beginning of FY2005 to that of FY2006, and Δz_{it-1}represents the change in caseloads caused by municipal mergers in FY2005. This analysis does not use a panel of differenced data because the exogenous variation (Δz_{it-1}) is observed only in FY2005 and can plausibly affect changes in the number of PA caseworkers (Δx_{it}) only in the subsequent fiscal year, i.e., FY2006.

The variables in Eq. (1) are defined as follows. First, the value of Δz_{it-1} for a merged city corresponds to the change in caseloads around the timing of the merger, as indicated by the vertical line in each panel of **Figure 2**. Its magnitude is measured as the difference between the caseload in the month of the merger and that in the subsequent month. For unmerged cities, this variable is set to zero. Second, the covariates $w_{k.it}$ comprise the size of caseloads in the last month of the previous fiscal year, the share of the female population, the share of the elderly (aged 65 and over), total population size, the number of households, and the fiscal capacity index. The fiscal capacity index measures a municipality's revenue-generating capacity relative to its estimated fiscal needs, both calculated by the central government. These covariates are also used as control variables in the IV estimation presented in the following section. Third, $\delta^x \equiv \Delta\mu_t^x$ represents the differenced year effect, which is absorbed into the constant term in the cross-sectional regression using differenced data. Finally, $\epsilon^x \equiv \Delta u_{it}^x$ denotes the error term. Summary statistics and data sources are provided in the Appendix.

The ordinary least squares (OLS) estimates of Eq. (1) are presented in **Table 1**. First, a baseline specification of Eq. (1) excluding all covariates ($\gamma_k^x = 0$ for all k) is estimated. The second column of **Table 1** reports that the coefficient is 0.0156 and is statistically significant, indicating that, on average, one additional caseworker is assigned when the PA caseload increases by approximately 64 households (= 1/0.0156). This caseload threshold—the increase in caseloads required to induce a one-unit increase in the number of PA caseworkers—is hereafter referred to as the "PA household equivalent" (PAHE). Next, a specification of Eq. (1) that includes all covariates is estimated. The third column of **Table 1** indicates that the inclusion of covariates reduces the coefficient to 0.0139, which also remains statistically significant. This implies that one additional caseworker is assigned when the caseload increases by approximately 71 households (= 1/0.0139); that is, the PAHE is 71. The Japanese government recommends—but does not mandate—that local governments maintain a "standard" caseload of 80 PA recipient households per caseworker. The PAHE estimates reported in **Table 1**, both of which are below this benchmark, suggest that, on average, cities that merged with TVs in FY2005 increased their number of PA caseworkers beyond the recommended threshold. This finding is not surprising, however, given that the government's benchmark includes not only PA caseworkers but also other caseworkers assigned to different programs administered by welfare offices.

Table 1. Effect on changes in the number of PA caseworkers

Include covariates?	No	Yes
Effect of changes in PA caseloads due to mergers	0.0156*** (0.001)	0.0139*** (0.002)
F-value	405.0	63.9
R^2	0.588	0.599
Sample size	494	494
PA household equivalent (PAHE)	64	71

Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) "PA household equivalent (PAHE)" refers to the increase in the number of Public Assistance (PA) recipient households required to induce a one-unit increase in the number of PA caseworkers.

3. Effect of workload on assistance rationing

3.1. Outcome variables for assistance rationing

The analysis uses three city-level outcome variables that capture different stages of assistance rationing: (1) the number of applications for PA programs, (2) the number of application withdrawals, and (3) the number of application rejections. As discussed in the Introduction, the number of applications is included because welfare offices may use intake interviews to discourage potential recipients before they formally apply. Furthermore, even after applications are submitted, welfare offices may still attempt to persuade applicants to withdraw before they issue a formal rejection.

3.2. Regression model

To estimate the effects of caseworker size, the analysis begins with the following regression model, where y generically represents each of the three outcome variables:

$$y_{it} = \beta \cdot x_{it-1} + \sum_k \gamma_k^y \cdot w_{k.it} + c_i^y + \mu_t^y + u_{it}^y,$$

for $t =$ FY2005 and FY2006. The explanatory variables are as defined in the previous section, and the parameters and error terms are analogous to those in Eq. (1). Taking first differences yields the following model:

$$\Delta y_{it} = \beta \cdot \Delta x_{it-1} + \sum_k \gamma_k^y \cdot \Delta w_{k.it} + \delta^y + \epsilon_{it}^y \qquad (2)$$

for $t =$ FY2006, where δ^y and ϵ_{it}^y are defined analogously to those in Eq. (1). Again, only a cross-section of FY2006 data is used, as the exogenous variation in FY2005 affects changes in the number of caseworkers only in the subsequent fiscal year. The sample used to estimate Eq. (2) is identical to that used for Eq. (1).

The regression model in Eq. (2) is specified as follows. First, Δx_{it} measures changes in the number of PA caseworkers from the beginning of FY2005 to the beginning of FY2006. This variation is instrumented with $\Delta z_{i\mathrm{FY2005}}$, which captures changes in PA caseloads resulting from mergers with TVs in FY2005. This variable takes a value of zero for cities that did not undergo any mergers. Second, while $y_{i\mathrm{FY2005}}$ in Δy_{it} reflects the full-year value for cities without mergers, it is adjusted for cities that merged with TVs. Specifically, $y_{i\mathrm{FY2005}}$ is calculated as the average of monthly values prior to the merger, multiplied by 12 to construct an annual equivalent. This adjustment ensures consistency with $\Delta x_{i\mathrm{FY2005}}$, whose undifferenced values are measured at the beginning of FY2004 and FY2005.

Third, Eq. (2) includes the existing caseload size as a covariate to identify the effect of "workload," defined as the amount of work required for a single caseworker to complete their designated tasks. Since workload is difficult to measure precisely under this definition, caseloads per caseworker—or the "average workload" in a locality—are used as a proxy for workload. To account for the effect of average workload, the regression model includes the size of the PA caseload in the last month of the previous fiscal year as one of the covariates $w_{k,it}$, representing the existing caseload handled by welfare offices. Accordingly, provided that the estimation is properly conducted, changes in the number of caseworkers correspond to changes in average workload in the opposite direction. If $\beta > 0$, a higher (lower) workload leads to a lower (higher) output. Conversely, if $\beta < 0$, a higher (lower) workload results in a higher (lower) output.

Fourth, including the existing caseloads as a regressor introduces another endogeneity issue in Eq. (2). As the last month of fiscal year t is March of the calendar year $t+1$, we obtain $\Delta w_{1,i\mathrm{FY2006}} = z_{i\mathrm{March\ 2006}} - z_{i\mathrm{March\ 2005}}$ in Eq. (2). If $y_{i\mathrm{FY2005}}$ affects $z_{i\mathrm{March\ 2006}}$, then $\Delta y_{i\mathrm{FY2006}} = y_{i\mathrm{FY2006}} - y_{i\mathrm{FY2005}}$ also influences $\Delta w_{1,i\mathrm{FY2006}} = z_{i\mathrm{March\ 2006}} - z_{i\mathrm{March\ 2005}}$, raising concerns about endogeneity due to reverse causality. In addition to the instrument $\Delta z_{i\mathrm{FY2005}}$, which may also affect $\Delta w_{1,i\mathrm{FY2006}}$, the IV estimation requires an additional instrument for identification. To this end, the regression employs a twice-lagged value of $\Delta w_{1,i\mathrm{FY2006}}$ as an

Anderson–Hsiao-type instrument (Anderson & Hsiao, 1982): $\Delta w_{1,i\text{FY}2004} = z_{i\text{March 2004}} - z_{i\text{March 2003}}$. This variable is uncorrelated with both $y_{i\text{FY}2006}$ and $y_{i\text{FY}2005}$, provided that serial correlation in y_{it} is less than second order. Hansen's J-test is not applicable here because the IV regression is just identified with two instruments— $\Delta z_{i\text{FY}2005}$ and $\Delta w_{1,i\text{FY}2004}$ —for two endogenous regressors: $\Delta x_{i\text{FY}2006}$ and $\Delta w_{1,i\text{FY}2006}$.

Finally, Eq. (2) accounts for unobserved heterogeneity c_i^y, which reflects several important factors. For instance, caseworkers may adopt collective values shared within their organizations (Keiser & Soss, 1998). Community attitudes also play a crucial role, as they may discourage eligible individuals from applying for welfare or prompt caseworkers to adopt stricter stances when assessing eligibility (Grubb, 1984; Weissert, 1994). Since such shared values are likely to remain stable over short periods but vary across cities, the unobserved heterogeneity captures their influence, along with that of other time-invariant, city-specific factors.

3.3 Results

Table 2 presents the IV estimates of the effects of PA caseworker size and the existing size of PA caseloads on the three rationing outcomes: applications, withdrawals, and rejections. For comparison, the corresponding OLS estimates are also reported alongside their IV counterparts. Because the error term is likely to be nonspherical in the cross-section of differenced data, a heteroskedasticity-consistent covariance matrix estimator is used to compute standard errors. As indicated in the last four rows of the table, the instruments pass the weak instrument test proposed by Montiel Olea and Pflueger (2013) at the 5% significance level ($\alpha = 0.05$) and the desired threshold of $\tau = 0.10$.[6] Moreover, the table reveals substantial differences between the OLS and IV estimates, lending support to the use of the IV estimator.

The IV estimation reveals statistically significant effects of PA caseworker size, except in the case of rejections. The estimate for applications suggests that a one-unit increase in the number of caseworkers, holding the existing caseload size constant, leads to an annual increase of 24 applications. As discussed earlier, welfare offices may use intake interviews to discourage potential applicants from formally submitting their applications. An increase in caseworker staffing may enable caseworkers to devote more time to their designated tasks, thereby reducing reliance on such implicit rationing mechanisms.

The estimation also indicates that the number of caseworkers is significantly associated with the frequency of withdrawals but not with that of rejections. If a larger number of caseworkers leads to an increase in applications, a corresponding rise in withdrawals may follow,

[6] For this test, we used a Stata postestimation routine, weakivtest, developed by Pflueger and Wang (2015).

as the two are often positively correlated. Furthermore, according to the cursory assessment hypothesis, additional staff resources may allow welfare offices to devote more effort to evaluating applicants, potentially resulting in more formal rejections. However, the empirical findings reveal no significant effect on rejections. This result may reflect a tendency among welfare offices to avoid formal rejections by informally encouraging applicants to withdraw their applications prior to official processing. This pattern—characterized by an increase in applications and withdrawals without a corresponding rise in rejections—suggests that Japanese welfare offices tend to rely on informal mechanisms for case resolution. Nevertheless, the increase in withdrawals (six additional cases) is insufficient to offset the increase in applications (24 additional cases). Since an increase in caseworker staffing does not lead to more rejections, it ultimately contributes to a rise in accepted PA recipients.

The IV estimates for the existing PA caseload size align with the effects of caseworker size. Holding the number of caseworkers constant, an increase in the existing caseload size implies a heavier workload. The negative coefficients on applications and withdrawals associated with caseload size mirror the positive coefficients associated with caseworker size in both cases. Moreover, neither the existing caseload size nor the number of caseworkers exhibits a statistically significant effect on rejections.

Table 2. Effects on rationing behavior

		Applications		Withdrawals		Rejections	
		IV	OLS	IV	OLS	IV	OLS
Caseworker size		24.37^{***} (7.56)	4.71^{*} (2.56)	5.72^{***} (1.51)	1.39^{*} (0.83)	0.05 (0.69)	0.30^{**} (0.16)
Existing caseload size		-0.25^{**} (0.11)	0.01 (0.05)	-0.06^{***} (0.02)	−0.004 (0.02)	0.005 (0.01)	0.005 (0.003)
Sample size		494	494	494	494	494	494
#cities merged with TVs		124	124	124	124	124	124
Effective F ($\alpha = 0.05$)		19.44		19.44		19.44	
Critical values for $\alpha = 0.05$	$\tau = 0.10$	18.05		18.01		17.99	
	$\tau = 0.20$	11.97		11.94		11.93	
	$\tau = 0.30$	9.68		9.65		9.64	

Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) The results in the last four rows are based on the weak instrument test proposed by Montiel Olea and Pflueger (2013).

4. Effect of workload with different intensities

4.1. Heterogeneous effects and the standard workload threshold

The negative effect of increasing the number of caseworkers on assistance rationing is expected to be more pronounced as the workload borne by existing caseworkers increases. To examine this heterogeneity, the sample is divided according to workload intensity. As noted in Section 2, the Japanese government recommends a standard workload threshold of 80 recipient households per caseworker (RPC) for city welfare offices. Accordingly, the analysis below splits the sample into two groups: cities with $RPC > 80$ and those with $RPC \leq 80$.

4.2. Effects on the number of caseworkers

Additional regressions are conducted to assess the impact of exogenous changes in caseloads on the number of caseworkers, using two subsamples of cities. **Table 3** presents the estimation results. In cities with $RPC > 80$, the PAHE is estimated at 65 $(= 1/0.0154)$ without covariates and 54 $(= 1/0.0186)$ with covariates. In contrast, in cities with $RPC \leq 80$, the PAHE is 111 $(= 1/0.009)$ without covariates and 62 $(= 1/0.0112)$ with covariates. When covariates are included, increases in caseworker staffing are more responsive to increases in caseloads in cities with heavier existing workloads. Nonetheless, irrespective of workload intensity, the PAHE estimates suggest that cities that merged with TVs in FY2005 expanded their PA caseworker staffing beyond the central government's recommended threshold.

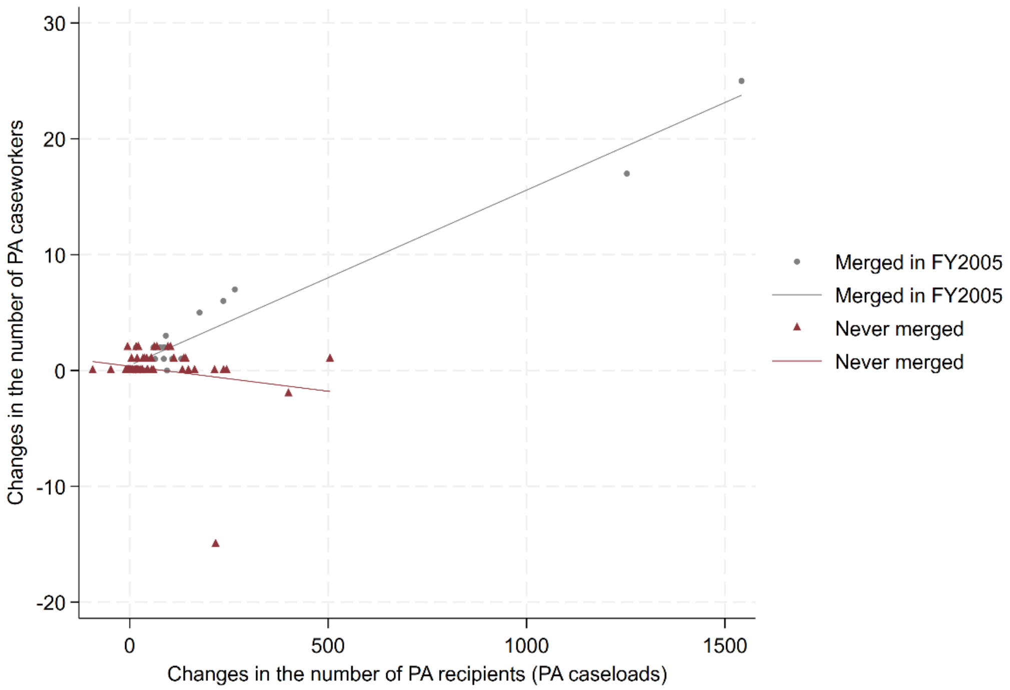
Table 3. Effect on the number of PA caseworkers under different workload intensity

	> 80		≤ 80	
Include covariates?	No	Yes	No	Yes
Changes in PA caseloads due to mergers	0.0154*** (0.001)	0.0186*** (0.003)	0.0161*** (0.002)	0.0090*** (0.002)
F-value	283.8	45.1	112.0	29.0
R^2	0.755	0.767	0.339	0.419
Sample size	87	87	407	407
PA household equivalent (PAHE)	65	54	62	111

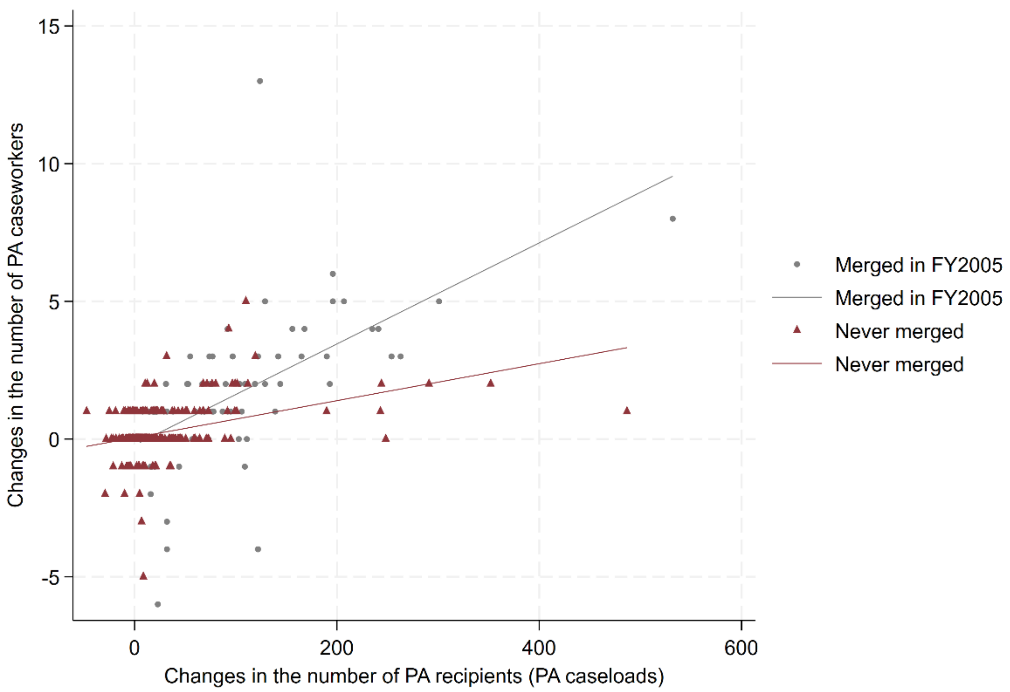
Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) "PA household equivalent (PAHE)" refers to the increase in the number of Public Assistance (PA) recipient households required to induce a one-unit increase in the number of PA caseworkers.

Figure 4. Correlation between changes in caseloads and caseworker number by workload intensity

(a) High workload Intensity: PA recipients per caseworker > 80



(b) Non-high workload Intensity: PA recipients per caseworker ≤ 80



Notes: Dots and their fitted line represent changes in cities that merged with TVs in FY2005; triangles and their fitted line represent cities that did not undergo mergers during the 2000s. For merged cities, changes in PA caseloads are calculated as the difference between the caseload in the month of the merger and that in the preceding month. The number of PA caseworkers is recorded on the first day of each fiscal year, with changes measured between the beginning of FY2005 and FY2006.

Analogous to Figure 3, **Figure 4** plots and fits changes in the numbers of caseloads and caseworker and fits regression lines for merged and unmerged cities, separately for the subsample with $RPC > 80$ (Panel A) and in that with $RPC \leq 80$ (Panel B). A visual inspection of the plotted markers in both panels again suggests a weaker correlation among unmerged cities compared with merged cities, further supporting the use of merger-induced caseload changes as a valid instrumental variable for each of the two subsamples.

4.3 Effects of workload with different intensity

Table 4 presents the estimation results for the effects of changes in the number of caseworkers on caseloads for each of the two subsamples. In the subsample with $RPC > 80$, the instruments pass the Montiel Olea–Pflueger test for weak instruments at a significance level of $\alpha = 0.05$, albeit under a relatively high desired threshold of $\tau = 0.20$. In contrast, in the subsample with $RPC \leq 80$, the test is passed under an even more stringent threshold of $\tau = 0.10$.

Table 4. Effects on rationing behavior under different workload intensities

		Applications		Withdrawals		Rejections	
PA recipient households per caseworker (RPC)		> 80	≤ 80	> 80	≤ 80	> 80	≤ 80
Number of caseworkers		24.65** (11.51)	19.86 (13.39)	6.03** (2.66)	4.82*** (1.52)	0.23 (0.65)	0.27 (1.53)
Existing caseload size		−0.26 (0.17)	−0.20 (0.20)	−0.06* (0.04)	−0.05*** (0.02)	0.005 (0.01)	0.01 (0.02)
Sample size		87	407	87	407	87	407
#cities merged with TVs		24	100	24	100	24	100
Effective F ($\alpha = 0.05$)		13.07	14.32	13.07	14.32	13.07	14.31
Critical values for $\alpha = 0.05$	$\tau = 0.10$	15.99	10.83	15.85	9.56	15.77	9.73
	$\tau = 0.20$	10.70	7.54	10.62	6.80	10.57	6.90
	$\tau = 0.30$	8.70	6.29	8.64	5.74	8.60	5.80

Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) The results in the last four rows are based on the weak instrument test proposed by Montiel Olea and Pflueger (2013).

The estimated coefficients from the $RPC > 80$ subsample closely resemble those from the full sample (i.e., the IV estimates in **Table 2**). However, in this subsample, the existing caseload size has no significant effect on applications and is only marginally significant for withdrawals. In the $RPC \leq 80$ subsample, neither the number of caseworkers nor the existing caseload size has a statistically significant effect on applications, which is consistent with expectations for welfare offices operating under relatively low workloads. In contrast, the effects on withdrawals are statistically significant in both subsamples. The estimated coefficients on both caseworker size and existing caseload size for applications and withdrawals are larger in the $RPC > 80$ subsample than in the $RPC \leq 80$ subsample. This suggests that increases in caseworker staffing have a greater impact on applications and withdrawals when the existing workload is high. Similar to the results for the full sample, the effects on rejections remain small and statistically insignificant across both subsamples. This implies that when welfare offices identify deficiencies in applications, they may prefer to avoid formally recording rejections and instead informally encourage applicants to withdraw, regardless of workload intensity.

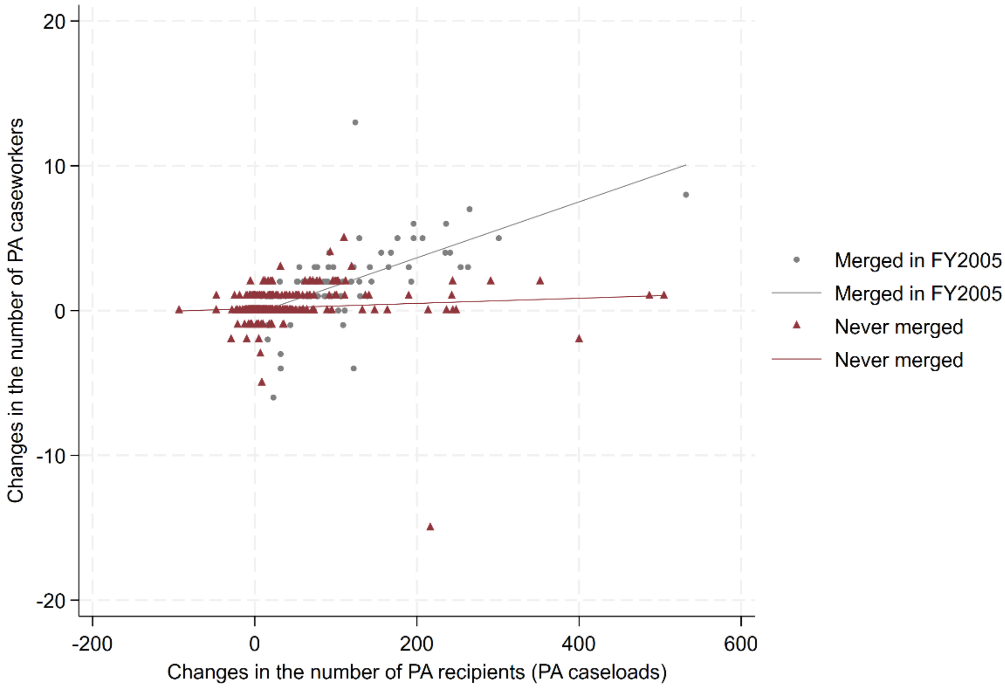
4.4 Robustness Check: Effects of outliers

Figures 3 and **4** identify two outliers among the cities that merged with TVs, each exhibiting caseload increases exceeding 1,200 and caseworker increases of 17 or more—more than twice the maximum observed in the remaining sample. To assess robustness, these outliers—present in both the full sample and the $RPC > 80$ subsample—are excluded. Panels (a) and (b) of **Figure 5** present scatter plots and fitted lines for the full sample and the $RPC > 80$ subsample, respectively, showing modest shifts in the fitted lines for merged cities. **Table 5** reports the slope coefficients and the PAHE estimates obtained from samples excluding the two outliers and compares them with the corresponding estimates from **Table 1**. Excluding the outliers yields steeper slopes and smaller PAHE values, although these differences are attenuated when covariates are included. **Table 6** presents the IV estimation results, which remain largely consistent with those in **Tables 2** and **4**. However, the instruments pass the weak instrument test only under higher threshold values ($\tau = 0.20$ for the full sample and $\tau = 0.30$ for the $RPC > 80$ subsample), suggesting a reduction in instrument strength.

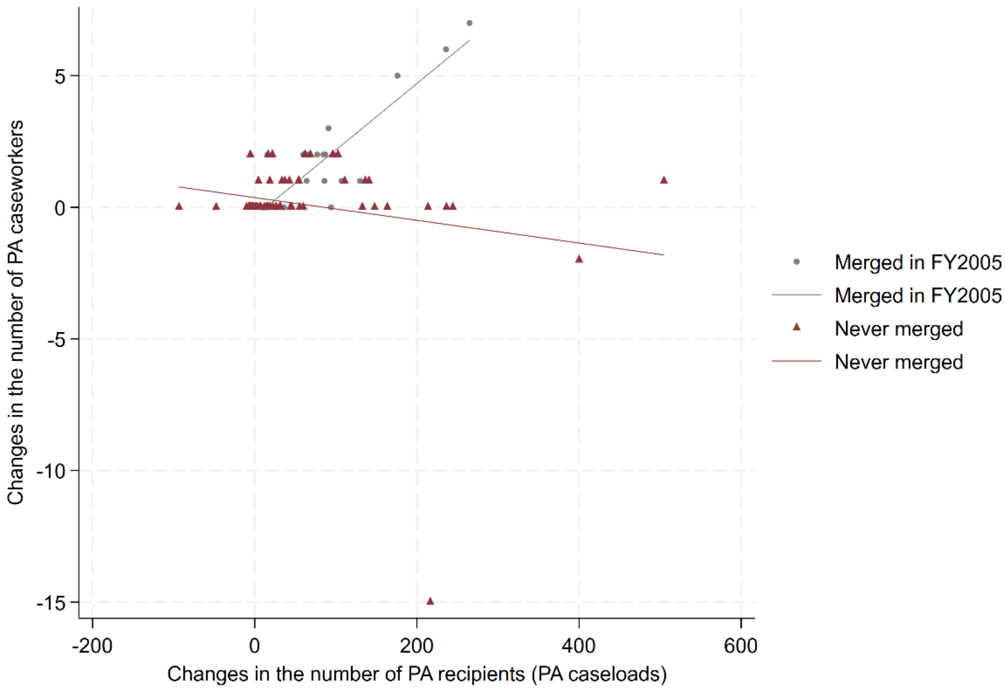
However, these two cities are not the only outliers. **Figure 5** also identifies an outlier among cities that did not undergo mergers, which experienced a caseload increase of over 200—despite the absence of a merger—the number of caseworkers *declined* by as many as by 15. Excluding this additional outlier further aligns the estimates with the baseline results. While **Figure 6** and **Table 7** continues to display some variation, the estimates exhibit closer convergence, particularly when the covariates are included. **Table 8** confirms that the IV estimates remain substantively unchanged. Notably, the instrument passes the weak instrument test under a lower threshold of $\tau = 0.10$ for both samples, and even under $\tau = 0.05$ for the $RPC > 80$ subsample.

Figure 5. Correlation between changes in caseloads and caseworker numbers by workload intensity

(a) Full sample excluding the two cities with largest changes



(b) Cities with higher workload Intensity excluding the two with the largest changes



Notes: Dots and their fitted line represent changes in cities that merged with TVs in FY2005; triangles and their fitted line represent cities that did not undergo mergers during the 2000s. For merged cities, changes in PA caseloads are calculated as the difference between the caseload in the month of the merger and that in the preceding month. The number of PA caseworkers is recorded on the first day of each fiscal year, with changes measured between the beginning of FY2005 and FY2006.

Table 5. Effects on the number of PA caseworkers

	Full sample				Cities with higher workload intensity			
Include covariates?	No	No	Yes	Yes	No	No	Yes	Yes
Exclude the two cities with the largest change?	Yes	No	Yes	No	Yes	No	Yes	No
Changes in PA caseloads due to mergers	0.0170*** (0.001)	0.0156*** (0.001)	0.0152*** (0.003)	0.0139*** (0.002)	0.0222*** (0.003)	0.0154*** (0.001)	0.0249*** (0.004)	0.0186*** (0.003)
F-value	133.2	405.0	23.9	63.9	66.6	283.8	12.5	45.1
R^2	0.311	0.588	0.329	0.599	0.269	0.755	0.305	0.767
Sample size	492	494	492	494	85	87	85	87
PA household equivalent (PAHE)	59	64	66	71	45	65	40	54

Notes: (i) *** $p \le .01$; ** $.01 < p \le .05$; * $.05 < p \le .10$. (ii) Standard errors are reported in parentheses. (iii) "PA household equivalent (PAHE)" refers to the increase in the number of Public Assistance (PA) recipient households required to induce a one-unit increase in the number of PA caseworkers.

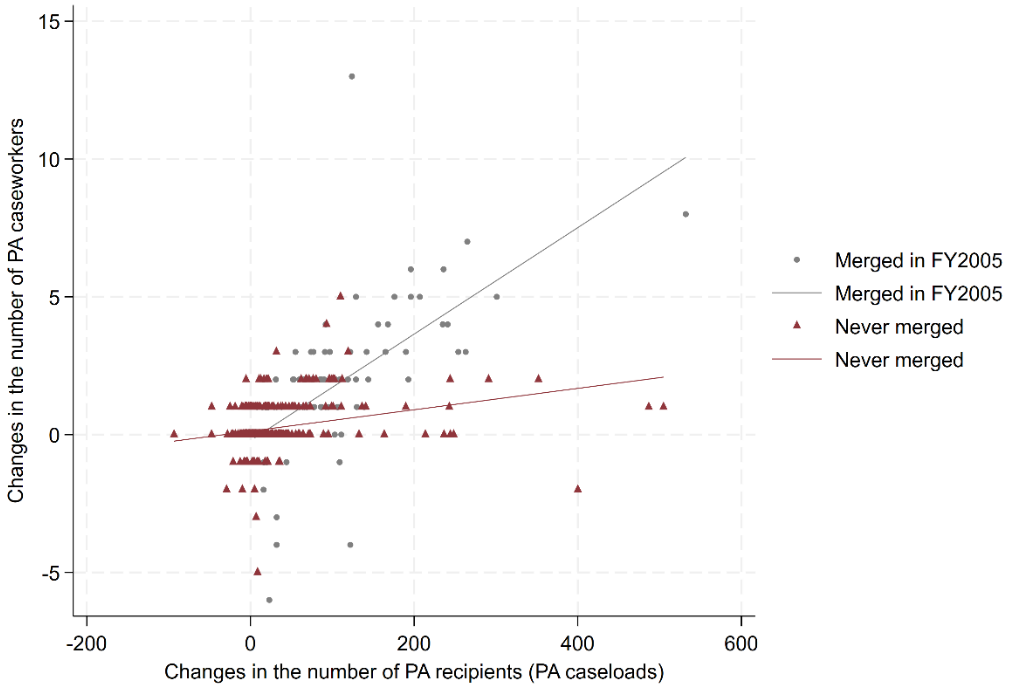
Table 6. Effects on rationing behavior

	Full sample						Cities with higher workload intensity					
	Applications		Withdrawals		Rejections		Applications		Withdrawals		Rejections	
Exclude the two cities with the largest changes?	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Number of caseworkers	25.43*** (7.61)	24.37*** (7.56)	5.45*** (1.53)	5.72*** (1.51)	0.18 (0.72)	0.05 (0.69)	22.23** (10.32)	24.65** (11.51)	5.66*** (1.52)	6.03** (2.66)	0.16 (0.59)	0.23 (0.65)
Existing caseloads	−0.25** (0.11)	−0.25** (0.11)	−0.06*** (0.02)	−0.06*** (0.02)	0.009 (0.01)	0.005 (0.01)	−0.27 (0.18)	−0.26 (0.17)	−0.06 (0.04)	−0.06* (0.04)	0.003 (0.01)	0.005 (0.01)
Sample size	492	494	492	494	492	494	85	87	85	87	85	87
The number of cities merged with TVs	122	124	122	124	122	124	22	24	22	24	22	24
Effective F ($\alpha = 0.05$)	15.79	19.44	15.79	19.44	15.79	19.44	11.13	13.07	11.13	13.07	11.13	13.07
Critical values for $\alpha = 0.05$, $\tau = 0.10$	16.64	18.05	16.62	18.01	16.56	17.99	16.99	15.99	17.04	15.85	16.92	15.77
Critical values for $\alpha = 0.05$, $\tau = 0.20$	11.11	11.97	11.09	11.94	11.06	11.93	11.32	10.70	11.35	10.62	11.30	10.57
Critical values for $\alpha = 0.05$, $\tau = 0.30$	9.01	9.68	9.00	9.65	8.97	9.64	9.18	8.70	9.20	8.64	9.16	8.60

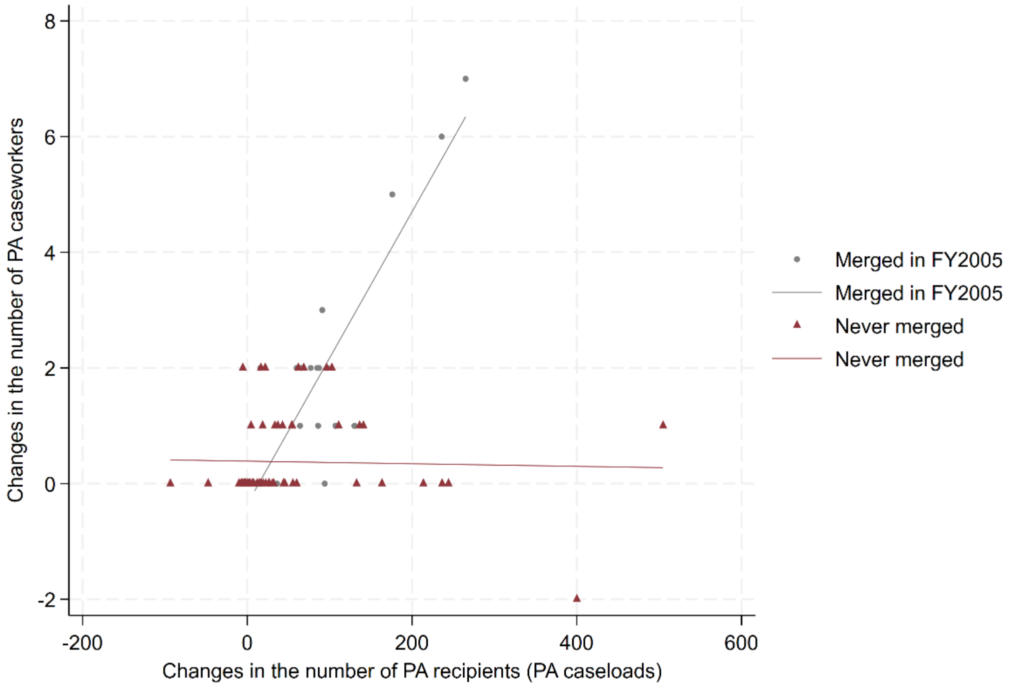
Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) The results in the last four rows are based on the weak instrument test proposed by Montiel Olea and Pflueger (2013).

Figure 6. Correlation between changes in caseloads and caseworker numbers by workload intensity

(a) Full sample excluding the three outliers



(b) Cities with higher workload intensity excluding the three outliers



Notes: Dots and their fitted line represent changes in cities that merged with TVs in FY2005; triangles and their fitted line represent cities that did not undergo mergers during the 2000s. For merged cities, changes in PA caseloads are calculated as the difference between the caseload in the month of the merger and that in the preceding month. The number of PA caseworkers is recorded on the first day of each fiscal year, with changes measured between the beginning of FY2005 and FY2006.

Table 7. Effects on the number of PA caseworkers

	Full sample				Cities with higher workload intensity			
Include covariates?	No	No	Yes	Yes	No	No	Yes	Yes
Exclude the three outliers?	Yes	No	Yes	No	Yes	No	Yes	No
Changes in PA caseloads due to mergers	0.0168*** (0.001)	0.0156*** (0.001)	0.0138*** (0.002)	0.0139*** (0.002)	0.0207*** (0.002)	0.0154*** (0.001)	0.0227*** (0.003)	0.0186*** (0.003)
F-value	134.6	405.0	26.5	63.9	71.8	283.8	10.0	45.1
R^2	0.375	0.588	0.417	0.599	0.616	0.755	0.637	0.767
Sample size	491	494	491	494	84	87	83#	87
PA household equivalent (PAHE)	59	64	72	71	48	65	44	54

Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) "PA household equivalent (PAHE)" refers to the increase in the number of Public Assistance (PA) recipient households required to induce a one-unit increase in the number of PA caseworkers.

Table 8. Effects on rationing behavior

	Full sample						Cities with higher workload intensity					
	Applications		Withdrawals		Rejections		Applications		Withdrawals		Rejections	
Exclude the three outliers?	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Number of caseworkers	25.12*** (8.03)	24.37*** (7.56)	5.28*** (1.43)	5.72*** (1.51)	0.41 (0.81)	0.05 (0.69)	22.16** (9.95)	24.65** (11.51)	5.37*** (1.52)	6.03** (2.66)	0.67 (0.60)	0.23 (0.65)
Existing caseloads	−0.25** (0.12)	−0.25** (0.11)	−0.06*** (0.02)	−0.06*** (0.02)	0.007 (0.01)	0.005 (0.01)	−0.26 (0.18)	−0.26 (0.17)	−0.06 (0.04)	−0.06* (0.04)	−0.0004 (0.01)	0.005 (0.01)
Sample size	490#	494	490#	494	490#	494	83#	87	83#	87	83#	87
The number of cities merged with TVs	122	124	122	124	122	124	22	24	22	24	22	24
Effective F ($\alpha = 0.05$)	20.23	19.44	20.23	19.44	20.23	19.44	26.08	13.07	26.08	13.07	26.08	13.07
Critical values for $\alpha = 0.05$: $\tau = 0.10$	15.96	18.05	16.28	18.01	15.58	17.99	11.75	15.99	11.49	15.85	11.53	15.77
Critical values for $\alpha = 0.05$: $\tau = 0.20$	10.68	11.97	10.86	11.94	10.46	11.93	8.12	10.70	7.97	10.62	8.00	10.57
Critical values for $\alpha = 0.05$: $\tau = 0.30$	8.68	9.68	8.81	9.65	8.51	9.64	6.74	8.70	6.63	8.64	6.65	8.60

Notes: (i) *** $p \leq .01$; ** $.01 < p \leq .05$; * $.05 < p \leq .10$. (ii) Standard errors are reported in parentheses. (iii) The results in the last four rows are based on the weak instrument test proposed by Montiel Olea and Pflueger (2013). (iv) # When estimating with covariates, Stata automatically drops one additional observation from the sample.

These findings indicate that the qualitative conclusions are robust to outliers, and the quantitative results remain relatively stable, particularly when the three cities identified as outliers are excluded from the analysis.

4. Concluding remarks

This study examined whether changes in workload affect rationing behavior in Japanese welfare offices by exploiting exogenous increases in public assistance (PA) caseloads in cities that underwent mergers with towns and villages (TVs) during the large wave of municipal mergers in the 2000s. Using these merger-induced caseload increases from FY2005 as an instrumental variable, the analysis estimated the impact of caseworker staffing on three key indicators of PA administration: the numbers of applications, withdrawals, and rejections. By controlling for the existing caseload size at welfare office, the analysis allowed the effect of caseworker numbers to be interpreted as the effect of reduced workload.

The results reveal that an increase in the number of caseworkers for a given caseload size (i.e., a decrease in workload) leads to more PA applications and withdrawals, but has no effect on rejections. Since the increase in applications exceeds that in withdrawals, this implies that expanding caseworker staffing results in a net increase in accepted applications. Moreover, heavier existing workloads are found to amplify the positive effects of caseworker size on both applications and withdrawals, whereas the effects on rejections remain statistically insignificant. These findings support the Type I error explanation of bureaucratic disentitlement, as emphasized in the literature, rather than the Type II error explanation underpinning the Japanese government's “cursory assessment” hypothesis. The results also suggest a tendency among Japanese welfare offices to avoid formal rejections, as evidenced by the statistically significant effects on applications and withdrawals, but not on official rejections. Finally, this study has limitations. In particular, as the analysis relies on historical data from FY2005 to FY2006, it remains uncertain whether the findings generalize to settings that differ substantially from the one examined in this study.

Appendix: Data description

Table A1 presents summary statistics for the variables used in this study. Although the estimations are based on differenced data, the values reported in the table are in levels, that is, values prior to differencing. First, monthly data on PA caseloads (recipient households), as well as the numbers of applications, withdrawals, and rejections, are obtained from the *Report on Social Welfare Administration and Services*, compiled by the Ministry of Health, Labour and Welfare. These confidential administrative data are recorded at the welfare office level. For cities with multiple welfare offices, the office-level data are aggregated to the city level. Additionally, for the regression analyses, monthly figures on applications, withdrawals, and rejections are aggregated into annual totals.

Second, the number of caseworkers in each municipality is recorded on the first day of each fiscal year (April 1). Although annual averages of daily caseworker counts are unavailable, these data should be sufficient because municipalities typically determine their staffing allocations at the beginning of the fiscal year and maintain them throughout, as noted by Nakajima and Arakawa (2004).

Table A1. Sample statistics

Nobs=496	FY2005				Fiscal Year 2006			
	Mean	St dev.	Min.	Max.	Mean	St dev.	Min.	Max.
Applications [i]	132.20	198.09	2	2,005	129.51	187.79	2	1,678
Withdrawals [I]	7.77	16.62	0	199	7.53	13.52	0	147
Rejections [I]	4.55	8.48	0	101	5.85	9.18	0	74
PA caseworkers [ii]	7.74	10.15	0	82	8.30	10.57	0	84
Caseworkers (PA + others) [ii]	12.63	15.09	0	113	13.46	16.08	0	127
Caseload changes by mergers [i]	25.74	102.19	0	1,542	0.00	0.00	0	0
Mergers in FY2005 (binary)[iii]	0.25	0.44	0	1	0.25	0.44	0	1
No mergers (binary) [iii]	0.75	0.44	0	1	0.75	0.44	0	1
PA caseloads [i]	676	1,055	19	9,484	730	1,118	24	10,011
Ratio of female population (%) [iv]	51	1	48	55	0.51	1	47	55
Ratio of elderly population (%)[iv]	21	5	10	40	0.22	5	10	52
Population (in thousands) [iv]	107.30	100.88	5.32	662.60	107.32	101.23	5.12	662.57
Households (in thousands) [iv]	42.19	42.09	2.75	280.64	42.70	42.67	2.65	283.31
Fiscal capacity index (%) [v]	70	27	11	172	71	28	12	172

Sources: (i) *Report on Social Welfare Administration and Services* (Fukushi Gyosei Hokokurei) by the Ministry of Health, Labour and Welfare. (ii) *Annual Survey of Municipal Human Resources* (Chiho Kokyo Dantai Teiin Kannri Chosa) by the Ministry of Internal Affairs and Communication. (iii) *List of Municipal Mergers Since FY1999* (Heisei Juichi Nendo Iko no Shi-cho-son Gappei no Jisseki) by the Ministry of Internal Affairs and Communication. (iv) *System of Social and Demographic Statistics* (SSDS: Shyakai Jinko Tokei Takei) by the Statistics Bureau. (v) *Annual Survey of Municipal Finance* (Shi-cho-son Kessan Jokyo Shirabe) by the Ministry of Internal Affairs and Communication.

Third, the regression models incorporate as many relevant covariates as possible. However, due to the limited availability of city-level data outside of census years (specifically FY2005 in this study), only the following covariates could be included: total population, number of households, the shares of females and elderly residents (aged 65 and above) in the total population, and the fiscal capacity index.

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