Do Central Grants Affect Welfare Caseloads? Evidence from Public Assistance in Japan*

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Abstract: While a number of studies have empirically explored the determinants of welfare caseloads, very few have examined the effect of central grants on caseload size. Since in countries where localities implement social assistance programs the central government usually provides them with funds for the programs, the central grants should be considered an important determinant of welfare caseloads. In particular, the Japanese government has claimed that an increase in central grants makes localities lenient in granting eligibility for social assistance. While the issue of whether or not central grants affect local welfare caseloads is an empirical question, it is a difficult one to evaluate: the rates of matching subsidies are nationally uniform, and the amount of general-purpose grants is endogenous. This study circumvents these difficulties by taking advantage of institutional changes in the Japanese system of intergovernmental transfers. The results show support for the claim that more central grants lead to more welfare caseloads, even if its eligibility assessment procedures are supposed to be nationally uniform.

Keywords: *welfare caseloads, social assistance, fiscal transfers, Japan*

JEL codes: *H73, H75, H77*

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1. Introduction

A number of empirical studies have explored the various determinants of welfare caseloads. While there are some studies on this area from the 1960s (Brehm and Saving 1964), the majority of the studies emerged after 1990 in the US, being prompted by a large increase in caseload that started in the late 1980s but ended with an abrupt decrease after 1994. While almost all the studies examined the effects of economic factors like unemployment, many also explored the effects of institutional factors, including state demonstration programs (Schiller and Brasher 1993; Johnson et al. 1994), waivers from Aid to Families with Dependent Children (AFDC) (Schiller 1999), the difference between AFDC-Basic and AFDC-UP (Blank 2001), child support enforcement (Huang et al. 2004), and the introduction of Temporary Assistance for Needy Families (TANF) (Moffit 2003; Cadena et al. 2006; Danielson and Klerman 2008). The literature also highlights factors like benefits levels (Shah and Smith 1995), at-risk populations (Conte et al. 1998), local labor markets (Hill and Murray 2008), and minimum wages (Page et al. 2005). In addition, there are analogous studies on welfare caseloads in Canada (Spindler and Gilbreath 1979), Sweden (Gustafsson 1984), Spain (Ayala and Pérez 2005) and Japan (Suzuki and Zhou 2007).

In countries where subnational governments implement assistance programs, the central government usually provides them with funds for the programs. Central grants could thus be considered an important determinant of welfare caseloads. However, the literature has not examined their effect on the caseloads. There are indeed empirical studies on the effects of central grants on welfare *expenditures*, for example, in the US (Chernick 1998) and Canada (Baker et al. 1999). But, examining only the expenditures may not indicate whether or not central grants affect welfare caseloads, as subnational governments could adjust the expenditures by changing not only the size of caseload but also the level of the benefits. Therefore, it is more relevant to look at a country where the center sets uniform benefit levels. An example is Japan where welfare caseload size is the only variable its local governments could possibly adjust through their discretion. Japanese localities are indeed supposed to follow nationally uniform standards to assess the eligibility of welfare applicants. However, there should always be rooms for local discretion to creep in. For example, confronted with limited fiscal resources, localities may develop implicit procedures to limit assistance to the eligible (Lipsky 1984). In addition, if they obtain more central subsidies, they may help applicants at the margin of eligibility whom they would not otherwise.

While whether or not such an effect exists is a matter of empirical investigation, it is often difficult to examine the effect of central grants. First, the structure of matching grants may hinder the identification of their effects (Baker et al. 1998). For example, it is impossible to identify the effect of matching grants if matching rates are identical among localities. While changes in such an identical rate exhibit time-series variations, they may be marred with noise. For example, if a nationwide shock occurs in the same period of the rate change, it is difficult to separate their respective effects. Second, while it appears that the effect of general-purpose grants is identifiable since their amounts usually vary among localities, they may be correlated with the unobservable that affect welfare caseload (Holtz-Eakin 1986). We then could take advantage of variations in local tax revenues as their effects are theoretically considered equivalent to those of general-purpose grants (Bradford and Oates 1971). However, the literature on the flypaper effect may indicate otherwise (Oates 1979, Bailey and Connolly 1998).

To circumvent these difficulties, this study exploits the unique interplay of the central grant system in Japan, and contributes to the literature that utilizes institutional mechanisms to identify the effect of central grants on localities.[1] The system of central grants here involves the Central Government Subsidy for Public Assistance (CGS-PA) and the Local Allocation Tax (LAT). The CGS-PA is a matching grant for local "Public Assistance (PA)" programs,[2] while the LAT is a general-purpose grant whose amounts are calculated according to a specific formula. In particular, the LAT accounts for the costs of PA programs that are not covered by the CGS-PA. A change in the CGS-PA is offset by a counteracting change in the LAT. This implies that the status of the LAT receipts identifies the effect of changing central grants. In FY1989, the matching rate of

[1] Among such studies, Baker et al. (1998) investigated the Canadian reform in 1990 when federal grants for provincial welfare expenditures were converted from open-ended matching to closed-ended. Focusing on effects on municipal taxes, Buettner (2006) exploited discontinuities and changes in the mechanism of fiscal equalization in Baden-Württemberg, Germany. Dahlberg et al. (2008) also made use of a discontinuity in the grant system in Sweden to examine its effects on local spending and tax rates.

[2] This paper uses "social assistance" as a generic term for needs-based and tax-financed programs that aim to maintain the minimum costs of living of their recipients. On the other hand, "Public Assistance" is used to refer to a specific social assistance scheme in Japan.

the CGS-PA increased from .70 to .75. While all municipalities still faced the identical rate of .75, its benefits differ depending on the status of the LAT receipt. Only the non-recipients enjoyed the increase in the CGS-PA: the LAT offset the increase in the CGS-PA by reducing the LAT grants by the amount equal to the CGS-PA increase.

The current study also tries to substantiate the policy claim that more central grants lead to more welfare caseloads. The central government in Japan has traditionally displayed an aversion to fully funding local social assistance programs, claiming that doing so would make localities leniently grant more benefits (Okuno 1944). Such a claim was indeed made when the current PA system was designed (Kasai 1978): the programs are now partially funded through the CGS system. In addition, the center has attempted to reduce its funding from time to time. The most recent example was found in 2005. The Ministry of Health, Labour and Welfare (MHLW) tried unsuccessfully to reduce the CGS-PA, again claiming that more central grants would make localities more lenient in assessing assistance eligibility (Kimura 2006). Note that this study has thus implications to other countries where the center makes similar arguments to reduce central grants for locally implemented social programs.

The rest of the paper is organized as follows. Section 2 briefly introduces the Japanese PA system and intergovernmental transfers, and describes our identification strategies. Section 3 considers the case of the 1989 matching rate increase, performs a set of estimations. After Section 4 discusses the results, Section 4 concludes the paper.

2. Institutional Background and Identification Strategy

2.1. Japanese transfer system for Public Assistance

PA aims to guarantee that citizens can meet their basic costs of living. Its benefits are granted to applicants whose own earnings cannot cover their basic costs. Since the benefits are designed to supplement what individuals are able to earn through their own best efforts, means tests are implemented by cities (shi) and prefectures (ken) through their welfare offices, as is required by national law.[3] Towns (cho) and villages (son) are

[3] There are two levels of government in Japan, with municipalities (cities, towns, and villages) as the first tier, and prefectures as the second tier. Tokyo has a slightly different local system as the national capital.

not required to do so. Prefectural welfare offices cover residents in towns and villages without their own welfare offices.

The CGS and the LAT are the two major types of central grants. First, the CGS refers to a set of categorical grants that is directly disbursed from the budgets of central line-ministries to localities. In particular, the CGS-PA comes from the budget of the MHLW as 75% of PA benefits. The remaining 25% is included in the SFD for the LAT. Second, the LAT is a general-purpose grant, financed out of national taxes along with other central revenue sources.[4] The amount of LAT a locality receives is a non-negative difference between its SFD and SFR. The SFD estimates the local expenditures required to maintain a standard quality of public services within a given locality, while the SFR estimates local fiscal capacity.

2.2. The 1989 change in the CGS-PA matching rate

The matching rate of the CGS-PA changed from .70 to .75 in FY1989. Since the SFD includes the local costs for PA not covered by the CGS-PA, the increase in the CGS-PA was offset by a corresponding decrease in the LAT grants. In other words, the increase in central funding was only realized for the LAT non-recipients, and the burden remained intact for the LAT recipients. Figure 1 considers a locality whose unit cost of PA caseload is constant, measuring caseloads on the horizontal axis and local expenditures other than PA on the vertical axis. If a locality can discretionarily choose caseloads, it chooses point *a* on budget line *AB*. An increase in the matching rate results in a lower price for PA caseload. If the locality is a non-LAT recipient, it now chooses *b* on the rotated budget line *AC*. If it is a LAT recipient, its budget line moves down vertically to *A'C'* from *AC* as the LAT grants are general-purpose, and its amount decreases by *ad* to offset the increase in CGS-PA. The recipient then chooses *c*.

The status of the LAT receipt thus identifies the movement from *b* to *c*, as the effect of *general* grants. If the PA is a normal good, which is the case for Figure 1, the effect on caseload is expected to be positive: an increase in general grants make localities more lenient in granting PA benefits. On the other hand, if PA caseload is an inferior good, the effect will be negative. Note also that zero effect does not necessarily

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[4] The LAT consists of the Ordinary Local Allocation Tax (OLAT) and the Special Local Allocation Tax (SLAT). What I call LAT here is the OLAT, which shares 94% of the total LAT disbursements.

imply that localities do not have effective discretion, since the income effect could be zero even if they could choose caseload size with discretion.

Figure 1.

2.3. Relevance of the treatment variable

This study then uses the status of the LAT non-receipt as a treatment variable. While the treatment must be free from manipulation by cities to yield proper estimates, the LAT status may *seem* to be manipulated by controlling either SFD or SFR, or both, especially in localities with the SFR-SFD ratio in the neighborhood of $1 = SFR_{it}/SFD_{it}$. The SFD for city i in year t is given as $SFD_{it} = \sum_j a_{it,j}c_{t,j}x_{it,j}$ where j indexes expenditure items, $x_{it,j}$ is a measuring unit for jth item in i, $c_{t,j}$ is a unit cost (cost per measuring unit) for item j, and $a_{it,j}$ is an adjustment coefficient for item j in i. First, most of $x_{it,j}$ are measured by the number of beneficiaries or the physical size of the infrastructure relevant for item j in city i. For the SFD-PA, $x_{it,j}$ is local population. Second, the unit cost ($c_{t,j}$) is cost per measuring unit calculated for some "standard" city. Thus, $c_{t,j}$ for SFD-PA is a per capita PA benefits. There might be a possibility of a feedback from PA caseloads to $c_{t,j}$ for SFD-PA, since the former is based on the *national* growth rates of PA caseloads in the previous year. However, since there were hundreds of cities, it is unlikely for a single city to manipulate the national growth rate of PA caseloads. Third, $a_{it,j}$ accounts for cost differences that are not captured by $c_{t,j}$. Its value usually depends on characteristics in city i. Since the central government sets the formula for $a_{it,j}$ and uniformly applies it to each municipality, there is no room for cities to manipulate $a_{it,j}$.

As such, the possible manipulative element in the SFD is the measurement unit. However, I argue that an attempt by cities to change their LAT status is not plausible. The SFD consists of many items, and the SFD-PA occupies only a small part (about 3%). Given this share, the attempt by cities to change their LAT status by adjusting their PA caseloads would easily be undone by concurrent changes in the other SFD items that are larger and more volatile. However, cities might manipulate the other SFD items to become LAT recipients by adjusting the *other* measurement units. Still, such an attempt would also be undone by changes forced by the central government. The national aggregation of $\max\{SFD_{it} - SFR_{it}, 0\}$ usually exceeds the funds earmarked for the LAT grants. To balance the finances, the central government uniformly reduces the amount of the SFDs in every locality in a fixed proportion before their amounts are finalized. Of course, such reduction is unpredictable and exogenous for localities.

In addition, the SFR may also be difficult to manipulate. Rather, the *exact* amount of a change in the SFR is unexpected for a city. The SFR for city i in year t is given as $SFR_{it} = .75 \times R_{it} + S_{it}$ where R_{it} is estimated standard tax revenues and S_{it} is transfers other than the CGS and the LAT grants. Note that R_{it} only includes taxes listed in the Local Public Finance Law, and is a revenue projection which is based on *forecasted* changes in relevant tax bases evaluated at the corresponding statutory tax rates set by the central government which are uniform across localities: neither actual tax rates nor realized tax bases are reflected in the SFR. Since the forecasts are based on past trends of realized tax bases, manipulation might be possible only if a locality could control a *past stream* of actual tax bases. However, changes in the tax bases are mainly due to the business cycle which is exogenous and frequently volatile. Perfect manipulation of the past stream of tax bases is impossible.

Therefore, I argue that the LAT status are not subject to manipulation, which may be supported by the distribution of cities used in the analysis that ensues (i.e., the sample of cities pooled for FY1986-91) in Figure 2. If cities can change their LAT status through some manipulation, there would be a hump in the distribution just to the left side of $1 = SRD_{it}/SFD_{it}$. But such a hump is not visible in Figure 2.

Figure 2

3. Empirical Implementation

3.1. Regression DD and fixed-effects models

Let y_{it} refer to the index of PA caseload size in city i and year t (subscripts are defined analogously thereafter). Following Huang et al. (2004) and Danielson and Klerman (2008), I use the natural logarithm of caseload size as an index of PA caseload.[5] I specify a pair of years before and after the institutional change in FY1989. I set the starting year as the one immediately before the year of the change, a = FY1988.

[5] Another popular index in the literature is the natural logarithm of the ratio of caseload size to population (e.g., Blank 2002). The following estimation effectively allows for such an index by including the log of population as a regressor.

Meanwhile, I consider three possibilities for the ending year: b = FY1989, FY1990 and FY1991. I obtain the difference-in-differences (DD) estimator for the effect on the PA caseload size of the central grant increase in FY1989 as follows. I start with a regression DD with an additive error u_{it}:

$$y_{it} = \alpha + \delta \cdot D_{it} + \lambda \cdot d_t + \gamma \cdot g_i + u_{it} \tag{a}$$

where $g_i \equiv 1\{SFR_{it} > SFD_{it}\}$, $d_t \equiv 1\{t \geq 1989\}$, and $D_{it} \equiv g_i \cdot d_t$. The status of LAT non-receipts ($1\{SFR_{it} > SFD_{it}\}$) changed from year to year in some cities. Since I exclude from the samples such cities for the reasons I will elaborate on in Section 3.2, $g_i \equiv 1\{SFR_{it} > SFD_{it}\}$ is indexed only by i. Model (a) includes the effects for the group of LAT non-recipients (γ), the years after the institutional change (λ), and the LAT non-recipients after the change (δ). The last effect embodies the treatment effect of the increase in the central grants for PA program. I also consider a case which includes a set of covariates $\boldsymbol{x}_{it}$ with the vector of corresponding coefficients $\boldsymbol{\beta}$ as:

$$y_{it} = \delta \cdot D_{it} + \lambda \cdot d_t + \gamma \cdot g_i + \boldsymbol{x}'_{it}\boldsymbol{\beta} + u_{it}\,. \tag{b}$$

However, specifications (a) and (b) may be crude since they only allow for the groups before and after the treatment ($\lambda \cdot d_t$) and groups with and without the LAT ($\gamma \cdot g_i$). In fact, it is likely for unobserved heterogeneity α_i exist across cities. Indeed, previous studies on welfare caseloads have identified sources of such effects. First, Keiser and Soss (1998) argue that caseworkers tend to adopt the collective values shared within their organizations, and that such shared values exert a major effect on social-assistance implementation. Second, Grubb (1984) points out that the potential barriers to take-up of welfare include the spatial accessibility of welfare offices, which may be surrogated with the surface area of a city. Third, Grubb (1984) and Weissert (1994) argue that community attitudes is another important factor, since they may discourage eligible individuals from applying to welfare, or cause caseworkers to take tough positions on eligibility assessment. All these elements — the administrative values, surface area, and community values — are unlikely to change during a short period, but are likely to differ across regions. Therefore, the fixed-effects model may be able to capture their effects. In addition, the model also allows for other unspecified and potentially many determinants of PA caseloads that differ across localities but change little over time.

Therefore, the fixed-effects model complement the limited availability of controls $\boldsymbol{x}_{it}$. I then construct the fixed effect versions of (a) and (b) as

$$y_{it} = \delta \cdot D_{it} + \alpha_i + u_{it} \tag{c}$$

$$y_{it} = \delta \cdot D_{it} + \boldsymbol{x}_{it}'\boldsymbol{\beta} + \alpha_i + u_{it} \tag{d}$$

where α_i absorbs γg_i that appear equations (a) and (b).

3.2. Samples

Since the effects of central grants are materialized through changes in the LAT disbursements for the period that straddles FY1989, both the treated and the controls are naturally supposed to have continued their LAT status from FY1989 to *b*. I thus exclude from the sample those cities whose LAT recipient/non-recipient status changed during a specified period. First, I exclude those cities whose LAT status changed at least once from FY1989 to FY1991. This effectively excludes cities that locate near the threshold level 1 = SFR_{it}/SFD_{it} where cities are uncertain whether or not they will receive the LAT grants in coming years, since their SFR and SFD are close in amount and the relative amounts change almost by chance as I explained in Section 2.3.

This line of logic leads to an idea that the treatment effects more conspicuously reveal themselves with sample of cities that have firmer expectation for their LAT status in years after FY1989. Since such expectation may well depend on the past stream of the LAT status, I will consider another sample of cities that had longer history of unchanged LAT status — cities that had not changed their LAT status since FY1985 (two more years before the change). Excluding such cities should not cause a problem in estimation. As I have argued in Section 2.3, the possibility of manipulating the LAT recipient status is negligible and the changes in the LAT status could be considered exogenous. Therefore, problems this sample exclusion may cause would be negligible.

I also exclude cities that had merged from FY1986 to FY1991. Although the number of merged municipalities was small during the period, some bias might occur due to this exclusion. In particular, it does so when errors terms in the regression models are correlated with city's decisions to merge. But, it is not straightforward to show that they are correlated. Empirical studies in the literature show that decisions to merge were influenced by population, surface area, and fiscal climates (Hirota 2006). Since the model is conditioned on such factors as well, I could argue that the correlation is not so serious. Even if they were correlated, fixed-effects models (c) and (d) difference out the selection effect if its magnitude does not change.

3.3. Data and their sources

These exclusions make the first sample for the period FY1989-91 consist of 589 cities, and the second sample for the period FY1986-91 consist of 567 cities. Table 1 summarizes the sample statistics for the two data sets. PA caseloads are the number of households that receive PA benefits. The caseloads are calculated as daily averages within a fiscal year. The data are unpublished which I obtained by the courtesy of the MHLW and the Ministry of Internal Affairs and Communication (MIC).

Tables 1

The availability of relevant covariates is limited since I need the data on an annual basis.[6] I could only obtain three covariates: city population (in log), per capita income (in log), and the Fiscal Capacity Index (FCI). First, I include city population, since PA caseload size would be larger as the city size increases. Second, I construct per capita income as the ratio of taxable income — the only income variable available at the city level — to city population. I intended to use this to control the level of economic activity at the city level. Lastly, I include the FCI as an index for city's fiscal climates. The FCI, defined as a three-year average of the ratio of the SFR to the SFD, is a popular index for fiscal climates in the literature on Japanese local public finance. A large FCI is associated with the case where the standardized measure of local taxes is relatively larger than the standardized measure of fiscal needs in a given city. I obtain these variables, along with those for the SFR and the SFD from the Nikkei Chi'iki Data Base.

4. Estimation Results

Tables 2 and 3 list the estimation results. Table 2 lists cases with the sample with unchanged LAT status from FY1988 to FY1991. I have estimated four models (a)–(d) for three time spans that start in the same year a = FY1988 but end in three different

[6] Detailed city data are only available in the years of the national census, conducted every five years. For the period under consideration here, the census was conducted only in 1990.

years b = FY1989, FY1990 and FY1991. Note that a positive coefficient on "treatment" implies that more central grants would lead to an increase in PA caseloads. For the six cases of regression DD with or without three covariates, the estimates for the effect of central grants on PA caseloads are not insignificant at the standard levels of statistical significance. But they do exhibit positive signs. However, among the other six fixed-effect estimations, the coefficient estimates on the treatment are all positive for all the six cases, and are statistically significant except the two cases for FY1988-90 change. Since the fixed effect estimation allows for the factors I discussed in Section 3.1, the fixed-effect results may be more reliable than the regression DD results. However, even among the four significant cases, p value less than .05 is only found in (d) for FY1988-89. Therefore, although there is some evidence for the central grants to increase local PA caseloads, the evidence may not so unequivocal.

Table 2

This "not-so-emphatic" result may be due to the sample selection. The sample only includes the cities whose LAT status had not been changed during the three years. Recall that the LAT status is set almost by chance for those cities located in the neighborhood of $1 = SFR_{it}/SFD_{it}$. Cities with a few years of receiving no LAT grants would not think that it is likely for them not to receive the grants regularly. Some cities that happened to fail to receive LAT grants only in FY1989-91 may not expect to continue their status in the initial year of the matching rate change (FY1989).

This line of arguments leads to restricting the sample so that it consist of cities whose belief to keep their current LAT status is strong. Such expectation may depend on the past stream of the LAT status. I thus consider another sample of cities that had longer history of continual receipts or non-receipts of LAT grants, by selecting cities that had not changed their LAT status since FY1986 (three more years longer than the other sample). Table 3 lists the results from this sample. While the results for the six cases for the regression DD are almost the same, the effects of the central grants are now more revealing in all the six cases of the fixed-effect estimation. The coefficients are all positive and are larger than their counterparts in Table 2. In particular, they are now all statistically significant at the .05 level. Therefore, I may be allowed to argue that the central grants tend to have larger and more significant effects among cities that have longer history of unchanged status of the LAT receipts.

Table 3

However, the coefficient estimates on the "treatment" vary among the six fixed-effects estimations from .015 from .034. There are two patterns. First, the coefficient becomes larger as the time span becomes longer when the regressors are identical. Second, the coefficient becomes larger when the regression includes three covariates when the time span is the same. While the difference between the two estimates with the same span is the largest in the period FY1988-89 (.015 for c and .019 for d), it is rather small in the other periods, i.e., FY1988-90 (.021 for c and .022 for d) and FY1989-91 (.033 for c and .034 for d). When evaluated at the sample average of PA caseload for FY1986-91 (651), the minimum value (.015) is translated into an increase of 10 cases, and the maximum (.034) is translated into an increase of 23 cases. The results in Table 3 thus show that the 1989 change in the matching rate from .70 to .75 increased PA caseloads by the amount between 10 and 23, implying that more central grants would lead to more PA caseloads.

Lastly I briefly mention the effects of the covariates. As expected, population has positive signs in all the cases. However, its statistical significance is lost in the fixed-effects models, possibly because changes in city population during the periods were small so that its effects had been absorbed in the fixed effects. For the other covariates, the results are mixed and their signs are unexpected. Per capita income displays positive signs in the fixed-effects models, implying a richer city has more PA caseloads. A possible, but somewhat uncomfortable, interpretation is that rich cities are more lenient in accepting PA recipients. The coefficients on the FCI are all negative and statistically significant except (c) for FY1989-90 in both tables, implying that less fiscal stringency leads to a smaller size of PA caseload. While the causes for these unexpected signs are difficult to interpret, they may due to the fact that the regression models are in reduced form: there must be multiple routes where the effects of these variables interact so that the effects in aggregate could take on unexpected signs.

5. Concluding Remarks

This study has examined the effect of central grants on PA caseloads, by exploiting the reduction in the CGS-PA matching rate and the accompanying increase in the SFD in FY1989. It has indeed shown that more central grants would lead to a larger size of PA caseloads. Evaluated at the sample averages, the change in the CGS-PA matching rate from .70 to .75 resulted in an increase in PA caseloads by the amount between 10 and 23. This result therefore supports the claim by the central government that more central grants will lead to more PA caseloads. Recall that cities are supposed to follow the uniform national standards for assessing PA eligibility. The eligibility assessment is then monitored by layers of audits and supervision measures. Welfare offices are staffed with supervisors who oversee case workers; prefectures audit cities within their jurisdictions; and the central government audits prefectures and large cities. Despite these, the result in this paper implies the existence of discretion at the local level that falls outside of the central standard and monitoring. This should suggest that the administrative aspects of PA programs are another important factor. The next step would thus be to explore such aspects of PA programs.

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Figure 1. The effect of a reduction in the matching rate

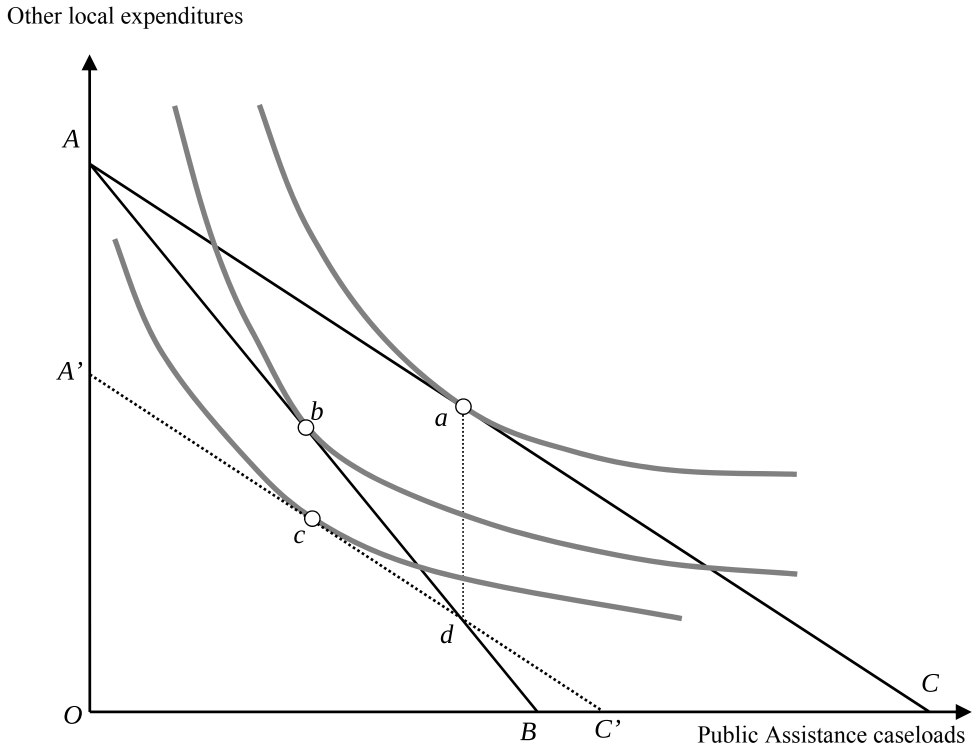

Figure 2. Distribution of cities (FY1986-FY1991)

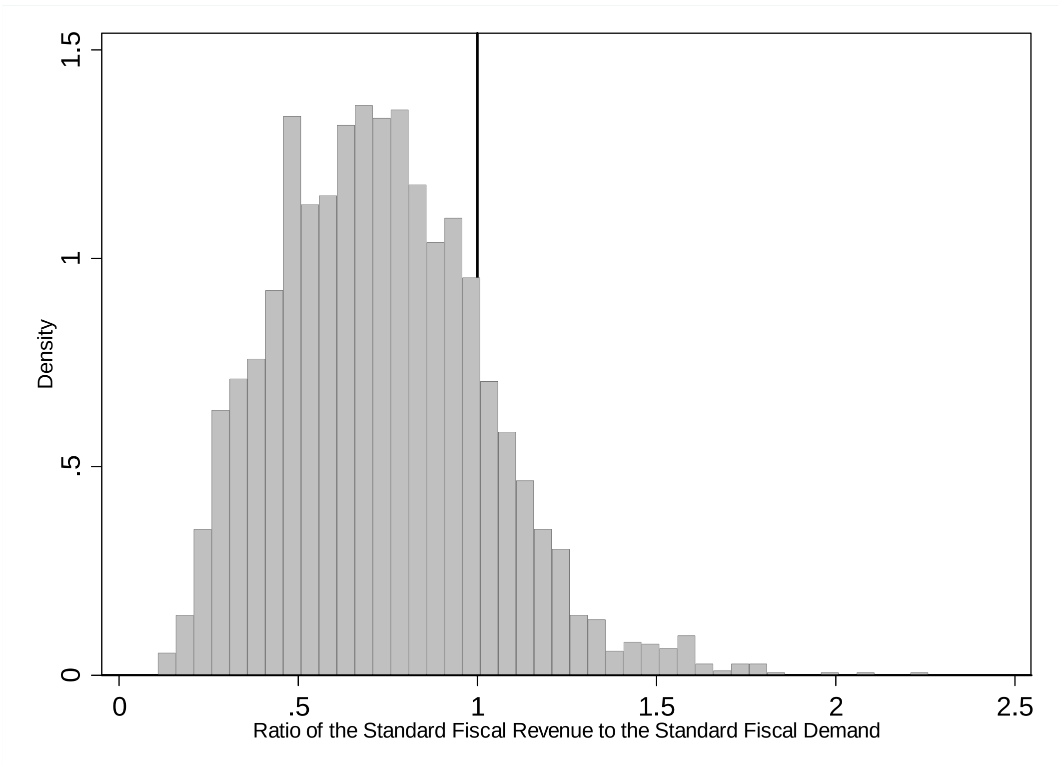

Table 1. Summary statistics

		Unchanged LAT status from FY1988 (N = 589)				Unchanged LAT status from FY1986 (N = 567)			
Variable	year	Mean	S.D.	Min.	Max.	Mean	S.D.	Min.	Max.
PA caseloads (households)	1988	687	1,854	19	20,383	687	1,882	19	20,383
	1989	664	1,808	19	19,783	665	1,836	19	19,783
	1990	636	1,745	21	19,134	637	1,772	21	19,134
	1991	614	1,693	21	18,682	615	1,719	21	18,682
Treatment	1988	.000	.000	.000	.000	.000	.000	.000	.000
	1989	.153	.360	.000	1.000	.132	.339	.000	1.000
	1990	.153	.360	.000	1.000	.132	.339	.000	1.000
	1991	.153	.360	.000	1.000	.132	.339	.000	1.000
Dummy for Non LAT receipts	1988	.153	.360	.000	1.000	.132	.339	.000	1.000
	1989	.153	.360	.000	1.000	.132	.339	.000	1.000
	1990	.153	.360	.000	1.000	.132	.339	.000	1.000
	1991	.153	.360	.000	1.000	.132	.339	.000	1.000
Population (1000 persons)	1988	119.2	207.2	9.3	3,121.6	117.3	207.7	9.3	3,121.6
	1989	119.9	208.8	9.0	3,152.7	117.9	209.3	9.0	3,152.7
	1990	120.5	210.4	8.8	3,176.0	118.5	210.8	8.8	3,176.0
	1991	121.1	212.0	8.4	3,210.6	119.1	212.5	8.4	3,210.6
Income per capita (1000,000 yen)	1988	.980	.264	.450	2.177	.971	.260	.450	2.177
	1989	1.042	.281	.479	2.466	1.032	.278	.479	2.466
	1990	1.126	.325	.486	2.839	1.114	.321	.486	2.839
	1991	1.253	.362	.550	3.251	1.241	.359	.550	3.251
Fiscal capacity index	1988	.729	.277	.150	1.790	0.719	.277	.150	1.790
	1989	.719	.285	.150	1.800	0.708	.285	.150	1.800
	1990	.709	.293	.150	1.810	0.698	.291	.150	1.810
	1991	.699	.297	.130	1.900	0.687	.296	.130	1.900

Table 2. DD estimates with a sample of cities with unchanged LAT status for four years (FY1989–FY1991)

Periods	FY1988–FY1989				FY1988–FY1990				FY1988–FY1991			
Models	Regression DD		Fixed-Effects Model		Regression DD		Fixed-Effects Model		Regression DD		Fixed-Effects Model	
Specifications	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment = $g_i \times d_t$	.011	.051	.011*	.015**	.140	.099	.014	.017	.023	.109	.023*	.027*
	(.185)	(.105)	(.006)	(.006)	(.185)	(.106)	(.010)	(.011)	(.185)	(.106)	(.013)	(.015)
$d_t = 1\{\text{year} \geq 1989\}$	−.042	−.021	−.042***	−.048***	−.092	−.036	−.092***	−.130***	−.132**	−.005	−.133***	−.215***
	(.074)	(.041)	(.003)	(.007)	(.074)	(.044)	(.004)	(.015)	(.074)	(.053)	(.005)	(.027)
$g_i = 1\{SFR_{it} > SFD_{it}\}$	.435***	.522***			.435***	.492***			.435***	.471***		
	(.131)	(.092)			(.131)	(.092)			(.131)	(.091)		
Log(population)		1.447***		.024		1.445***		.042		1.444***		.186
		(.025)		(.232)		(.025)		(.247)		(.025)		(.188)
Log(per capita income)		−.809***		.055		−.815***		.247**		−.887***		.305***
		(.125)		(.115)		(.125)		(.107)		(.125)		(.108)
Fiscal capacity index		−1.736***		−.167		−1.675***		−.217**		−1.595***		−.247**
		(.165)		(.110)		(.167)		(.108)		(.166)		(.099)
Constant	5.552***	−9.475***	5.598***	5.457**	5.531***	−9.499***	5.598***	5.300**	5.531***	−9.545***	5.598***	3.715**
	(.052)	(.271)	(.001)	(2.590)	(.052)	(.271)	(.001)	(2.266)	(.052)	(.272)	(.002)	(2.106)

Notes: $N = 589$. *** $p < 0.01$, ** $p < 0.05$; * $p < 0.10$. The robust standard errors are in parenthesis.

Table 3. DD estimates with a sample of cities with unchanged LAT status for six years (FY1986–FY1991)

Periods	FY1988–FY1989				FY1988–FY1990				FY1988–FY1991			
Models	Regression DD		Fixed-Effects Model		Regression DD		Fixed-Effects Model		Regression DD		Fixed-Effects Model	
Specifications	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)	(a)	(b)	(c)	(d)
Treatment = $g_i \times d_t$	.015 (.208)	.054 (.105)	.015** (.006)	.019*** (.007)	.021 (.208)	.100 (.118)	.021** (.010)	.022* (.012)	.033 (.207)	.115 (.117)	.033** (.014)	.034** (.015)
d_t = 1{year≥1989}	−.042 (.074)	−.023 (.041)	−.043*** (.003)	−.047*** (.007)	−.093 (.075)	−.041 (.045)	−.093*** (.004)	−.128*** (.015)	−.133* (.075)	−.004 (.054)	−.133*** (.005)	−.226*** (.027)
$g_i = 1\{SFR_{it} > SFD_{it}\}$	.454*** (.148)	.590*** (.102)			.454*** (.147)	.553*** (.101)			.454*** (.148)	.530*** (.101)		
Log(population)		1.447*** (.025)		.083 (.241)		1.465*** (.026)		.084 (.212)		1.464*** (.026)		.239 (.197)
Log(per capita income)		−.784*** (.175)		.058 (.116)		−.801*** (.131)		.229** (.111)		−.847*** (.131)		.268** (.111)
Fiscal capacity index		−1.862*** (.175)		−.162 (.118)		−1.786*** (.178)		−.188* (.109)		−1.712*** (.175)		−.253** (106)
Constant	5.553*** (.053)	−9.619*** (.280)	5.583*** (.001)	4.776* (2.590)	5.523*** (.053)	−9.652*** (.279)	5.583*** (.002)	4.798** (2.358)	5.523*** (.053)	−9.693*** (.280)	5.583*** (.002)	3.101 (2.201)

Notes: N = 567. *** $p < 0.01$, ** $p < 0.05$; * $p < 0.10$. The robust standard errors are in parenthesis.