Working Paper Series (E)

No.25

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September 2015
http://www.ipss.go.jp/publication/e/WP/IPSS_WPE25.pdf

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Heterogeneous Effects of Fiscal Equalization Grants on Local Expenditures: Evidence from Two Formula-based Variations*

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First version: April 24, 2015
This version: September 9, 2015

Abstract

This paper investigates the effects of fiscal equalization grants on total expenditure and disaggregated expenditures by exploiting two different formula-based exogenous variations in grants. Examining the institutional settings of the Japanese fiscal equalization scheme and estimating local average grant effects with a regression kink design and an instrumental variable approach, I demonstrate that there exist heterogeneous grant effects for two groups of municipalities with different fiscal conditions. That is, estimated grant effects on total expenditure are approximately one-to-one for municipalities around the threshold of grant eligibility, but much more than one-to-one for municipalities that are heavily dependent on fiscal equalization grants. In addition, grant effects on disaggregated expenditures are dispersed across different expenditure items in the former type of municipality but concentrated on construction expenditures in the latter type. I then discuss that the observed grant effect heterogeneity is a consequence of the institutional settings of the fiscal equalization scheme.

JEL classification: C13, C21, H71, H72, H77
Keywords: Intergovernmental grants, Endogenous grants, Effect heterogeneity, Regression kink, Instrumental variable, Flypaper effect

*I am grateful to Shunichiro Bessho, Matz Dahlberg, Masayoshi Hayashi, Che-Yuan Liang, Eva Mörk, Reo Takaku, Janne Tukiainen, and seminar and conference participants at Uppsala, Tokyo, Osaka, and Montreal for very helpful comments and suggestions. This research was partly supported by JSPS KAKENHI Grant Number 15K17085. The views expressed in this paper do not necessarily reflect those of National Institute of Population and Social Security Research. All errors are my own.

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1 Introduction

In both developed and developing countries, intergovernmental grants are one of the largest revenue sources for sub-national governments and significantly affect local socio-economic circumstances. Reflecting the importance of grants in local public finance, the effects of intergovernmental grants on local policies and local economies have been extensively investigated since at least the early 1950s and the publication of Buchanan (1950) and Scott (1950).

This initial surge of empirical literature was followed by the intriguing finding of the "flypaper effect" in Henderson (1968) and Gramlich (1969). The flypaper effect indicates that the effect of unconditional lump-sum grants on local public expenditure is much larger than the counterpart effect of local income and rejects a strong theoretical prediction in Bradford and Oates (1971a;b) (hereafter B&O) concerning the equivalence of the effects of unconditional lump-sum grants and local income. There is currently a sizeable number of papers on the flypaper effect and relevant grant effects. Several review papers have also been published such as Hines and Thaler (1995), Bailey and Connolly (1998), Oates (1999), Gamkhar and Shah (2007) and Inman (2008).

The literature on grant effects matured further with the publication of Knight (2002), a seminal work that explicitly takes into account the endogenous determination of grant allocation that is caused by political and socio-economic factors. Recent studies of grant effects on local spending and revenue whose internal validity seems to be a significant improvement on that of earlier works include Gordon (2004), Baicker and Gordon (2006), Dahlberg et al. (2008), Lutz (2010), Cascio et al. (2013), Litschig and Morrison (2013), Lundqvist (2015), Dupor and Mehkari (2015) and Arvate et al. (2015).[1] Using an exogenous component in grant formulas for the identification of grant effects is also increasingly popular in studies on local employment and local multipliers such as Chodorow-Reich et al. (2012), Wilson (2012), Conley and Dupor (2013), Lundqvist et al. (2014) and Suárez Serrato and Wingender (2014).

[1] There is also relevant literature on the incentive effect (or price effect) of fiscal equalization grants on tax rates such as Smart (1998), Baretti et al. (2002), Buettner (2006) and Smart (2007), and recent studies by Dahlby (2011) and Dahlby and Ferede (2015) also investigate the price effect of fiscal equalization grants on local expenditure. I do not consider these arguments here, however, and interpret the estimated effects of fiscal equalization grants as income effects.

This paper adds further evidence of grant effects on local expenditures to this growing body of literature, but with a focus on heterogeneous grant effects caused by the institutional settings of *fiscal equalization*. More concretely, I investigate the grant effects that can differ among local governments even under a uniform fiscal equalization scheme because the size and role of fiscal equalization grants differ considerably depending on municipalities' fiscal conditions.

The main purpose of this paper is to estimate different causal effects of fiscal equalization grants on municipalities with different fiscal conditions, using a set of panel data from Japanese municipalities. By exploiting two exogenous variations in grant formulas and adopting two quasi-experimental research designs, I can take into account both the problem of grant endogeneity and differences in fiscal conditions that are expected to affect how grants are used in municipalities.

The contributions of this paper to the literature on grant effects are twofold. First, it adds more evidence of grant effects with two quasi-experimental research designs. As Dahlberg et al. (2008) argue, it is likely that different intergovernmental grant schemes may have different effects. The evidence of grant effects in the literature is still predominantly focused on the U.S., however, and few quasi-experimental studies have been implemented concerning intergovernmental grants in Asian countries. It is therefore worthwhile to provide new evidence of grant effects in Japan, where the system of intergovernmental transfers is quite different from that in the U.S. but have some similarities to some other countries.[2]

Second, to my knowledge, this is the first study to explicitly consider the grant effect heterogeneity that arises from the institutional settings of a uniform formula-based fiscal equalization scheme.[3] Such within-institution effect heterogeneity is

[2]According to a categorization based on equalization goals and allocation factors in Boex and Martinez-Vazquez (2007), Japanese fiscal equalization schemes have some similarities to those of Australia, China, Germany, Korea, Latvia, Rusia, UK, etc. Boex and Martinez-Vazquez (2007) group these countries together because their goal in engaging in equalization is "to enable similar levels of service at similar levels of taxation" (p.254) and their allocation factors are based on "*Fiscal gap* = Expenditure needs - Fiscal capacity, OR some other combination of need and capacity" (p.254).

[3]The sources of grant (or windfall revenue) heterogeneity are empirically examined in several recent papers. Strumpf (1998) suggests that the heterogeneity of the flypaper effect can be explained by the level of voter information. Based on the model of dynamic interactions between politicians and interest groups, empirical evidence presented in Singhal (2008) implies that seemingly unconditional grants are in fact implicitly tied to benefits to some special interest groups and grant effects vary depending on the power of such groups. Another important study is Lutz (2010), which examines grant effects

important when it comes to evaluating fiscal equalization grant policy, but is largely neglected in the literature.[4] Although my study focuses on a specific fiscal equalization scheme in Japan, explicit considerations of specific equalization mechanisms and resulting heterogeneous effects should have some general implications for the policy evaluation of intergovernmental grants in other countries that have similar equalization schemes.

My identification strategies rely on a regression kink (RK) design and an instrumental variable (IV) approach, and these strategies are meant to capture different grant effects arising from different exogenous variations in grant formulas. Estimation results show that estimated grant effects on total expenditure are about one-to-one for municipalities around the threshold of grant eligibility, but much more than one-to-one for municipalities that are heavily dependent on fiscal equalization grants. In addition, grant effects on disaggregated expenditures are dispersed across different expenditure items in the former type of municipality but concentrated on construction expenditures in the latter type.

I then discuss and interpret these results. My interpretation is that the observed grant effect heterogeneity is a consequence of the institutional settings of the Japanese fiscal equalization scheme. Examining the grant formulas of fiscal equalization, I argue that grant effects in municipalities around the threshold of grant eligibility can be considered to be outcomes of the municipality's decision making, whereas grant effects for municipalities with larger amounts of grants are to some extent driven by the policy objectives of the central government. This finding suggests that a plausible identification strategy and a precise understanding of institutional settings are both important when undertaking the causal analysis and policy evaluation of a fiscal equalization scheme.

The remainder of the paper is organized as follows. In Section 2 I review the literature of fiscal equalization in international and general contexts and then clar-

under direct democracy and also investigates grant effect heterogeneity by income level of municipality. Lutz suggests that effect heterogeneity by income is explained by concave Engel curves for education. None of these studies, however, consider grant effect heterogeneity induced by the institutional settings of a fiscal equalization scheme.

[4]For example, Gamkhar and Shah (2007), which provide a review chapter on recent studies of the impact of intergovernmental grants in Boadway and Shah (2007), describe fiscal equalization grants simply as "general-purpose nonmatching grants" in line with the framework of B&O and subsequent studies. This simplification of fiscal equalization grants is in sharp contrast to the several other chapters in the same book that describe conceptual and institutional details of actual fiscal equalization schemes around the world.

ify how Japanese fiscal equalization grants can result in different grant effects for different municipalities. Section 3 describes my dataset and then explains my two identification strategies to investigate heterogeneous grant effects. Section 4 provides preliminary analysis and Section 5 presents estimation results. In Section 6 I discuss how I can interpret my findings in Section 5. Section 7 concludes.

2 Institutional background

2.1 Expenditure needs and grant effects

Although the institutional structure of fiscal equalization and its consequences have not been seriously examined in most empirical studies on flypaper effects and other relevant grant effects, the characteristics of fiscal equalization grants are extensively studied in theoretical, institutional, and comparative studies (Boadway and Shah 2007; Martinez-Vazquez and Searle 2007; Bergvall et al. 2006; Blöchliger and Charbit 2008). The type of equalization that is most relevant to this paper is so-called "expenditure need equalization" (Shah 1996; 2012), which is also referred to as "cost equalization" (Blöchliger and Charbit 2008), "expenditure-based equalization" (Bird and Vaillancourt 2007), or "service capacity equalization" (Bergvall et al. 2006).

Expenditure need equalization is a common type of fiscal equalization and most developed countries adopt some kind of expenditure need equalization either at the level of federal/central governments or state/local governments (Reschovsky 2007; Shah 2012). A key feature of expenditure need equalization is that the amounts of fiscal equalization grants are (partly) based on "expenditure needs" for local public services as determined by factors such as the population size, the number of elderly people and children, the unemployment rate, the population density, etc.

The economic rationale for expenditure need equalization is well documented in Shah (1996), Bird and Vaillancourt (2007), and Shah (2012). In short, expenditure need equalization can take into account regional differences in local needs for specific public services that revenue capacity equalization cannot. Others are somewhat critical of this approach, however, because expenditure need equalization may inflate estimated expenditure needs and become a source of rent seeking (Blöchliger and Charbit 2008).

When it comes to the actual practice of expenditure need equalization in the real world, Shah (2012) distinguishes the following two main categories of equalization and provides a review of selection of countries in each group: (a) ad hoc determination of expenditure needs (Canada, Germany, Italy, South Africa, Switzerland, and USA), (b) a representative system using direct imputation methods (Australia, Denmark, Finland, Japan, Netherlands, Norway, Sweden and UK). He also proposes an alternative approach that is described as (c) a theory-based representative expenditure system.

Regardless of these various approaches, it should be noted that upper-level governments play a primary role in setting the expenditure needs of lower-level governments using a set of grant formulas or rules. In addition, since upper-level governments more or less need to ensure that all local bodies provide certain levels of local public services, formula-based expenditure needs explicitly or implicitly reflect the costs of mandatory local public services because fiscal resources for these services have to be guaranteed by intergovernmental fiscal transfers.

This feature of fiscal equalization grants implies that the allocation of these grants to different local policy fields may be strongly affected by centrally determined expenditure needs even if the grants are nominally unconditional and lump-sum. In an extreme case, if local governments cannot cover the costs of mandatory local services with their own revenues, fiscal equalization grants *have to* be used for these services without any discretion. If this is the case, the fundamental assumption of the optimization behavior of local governments under the B&O theorem is violated. Rather, estimated grant effects should be interpreted as a consequence of a lack of choices for local governments.

The interpretation of grant effects may thus heavily depend on the institutional settings of the fiscal equalization scheme in question. I explore this issue while focusing on the Japanese fiscal equalization scheme, but a similar issue may exist in any country that has expenditure need equalization in its fiscal equalization system.

2.2 Japanese fiscal equalization grants

The state of affairs in Japan is an appropriate case through which to investigate the institutional and empirical issues discussed in the above section due to Japan's unified large fiscal equalization scheme. On average, Japanese fiscal equalization grants (including fiscal equalization bonds discussed below) are around 38% of municipalities' revenue in my sample. In addition, as is stated in Boex and Martinez-Vazquez

(2007) or Shah (2012), Japan is not unique in adopting a fiscal equalization scheme with expenditure need equalization. In this subsection, I describe the Japanese fiscal equalization scheme, its expenditure need equalization, and its possible consequences.

2.2.1 The basic formula for fiscal equalization grants

"Local Allocation Tax (LAT)" grants, the major fiscal equalization scheme in Japan, allocates unconditional lump-sum grants to local governments (prefectures and municipalities) in order to compensate for the fiscal gap between their *fiscal need* and *revenue capacity*. LAT grants are intended to ensure a certain standard level of local public services to all citizens in Japan regardless of their place of residence. In the subsequent description I restrict my attention to the fiscal equalization for municipalities, but most of what I describe can be applied to the fiscal equalization for prefectures as well.

The distribution of LAT grants to local governments consists of a two-step procedure. In the first step the national-level total amount of LAT grants is determined based on the amount of central tax revenues and various political and bureaucratic processes. In the second step, the amount of the LAT grant for each municipality is determined by various socio-economic, political, and bureaucratic factors, but with the following simple formula[5]:

$$G_{it} = \begin{cases} 0 & \text{if } V_{it} \leq 0 \\ V_{it} & \text{if } V_{it} > 0, \end{cases} \quad \text{where } V_{it} = NEED_{it} - CAP_{it}. \tag{1}$$

In this equation $NEED_{it}$ is the *fiscal need* which indicates the amount of local expenditure required to cover the total cost of the "standard" levels of local public services. Note that $NEED_{it}$ is *not* an indicator that exclusively reflects certain objective levels of appropriate local public services. Rather, $NEED_{it}$ should be interpreted as a reference point for local fiscal needs the level of which is annually adjusted based on the fiscal conditions of local and central governments and other factors.

CAP_{it} is an index of *revenue capacity* which is calculated based on the potential

[5]Strictly speaking, the LAT grants that are distributed based on this formula consist of 94% of the total LAT grants and the other 6% of the LAT grants are distributed for specific purposes with more ad hoc rules. In this essay the term "LAT grants" refers to only the formula-based grants.

revenues that each municipality should be able to collect on its own under the standard local tax system. $NEED_{it}$ and CAP_{it} are officially referred to as "*Standard Fiscal Need* " and "*Standard Fiscal Revenue* " respectively, though I define them as per capita values whereas "*Standard Fiscal Need*" and "*Standard Fiscal Revenue*" are defined as total values for each municipality. Both indices are estimated annually by the central government's Ministry of Internal Affairs and Communications.

Thus V_{it} is the "need-capacity gap", which measures to what extent the local expenditure need is larger than the local revenue capacity. Dafflon (2007) discusses fiscal equalization systems based on the "need-capacity gap" in a more general context.

2.2.2 Graphical representation of pre and post equalization

Next, let us look at a graphical representation of how the revenues of local governments before and after fiscal equalization are changed by the fiscal equalization scheme as a whole. Because CAP_{it} is discounted from original pre-equalization revenue capacity, I first define true pre-equalization revenue capacity as $PreCAP_{it}$, which is roughly equal to $CAP_{it} \times 4/3$.[6]

I then define post-equalization revenue capacity as $PostCAP_{it}$, which is the sum of pre-equalization revenue capacity and fiscal equalization grants. The relationship between $PreCAP_{it}$ and $PostCAP_{it}$ can then be expressed as follows:

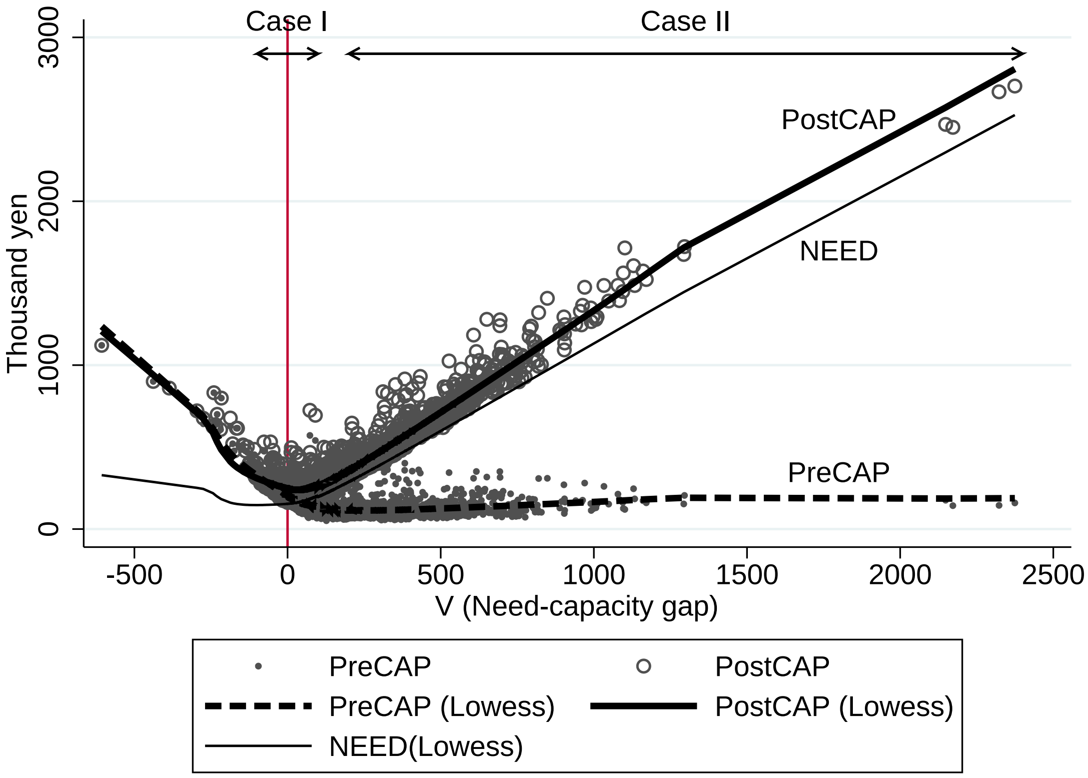
$$PostCAP_{it} = \begin{cases} PreCAP_{it} + B_{it}, & \text{if } \; V_{it} \leq 0 \\ PreCAP_{it} + G_{it} + B_{it}, & \text{if } \; V_{it} > 0, \end{cases}$$

where G_{it} is the LAT grant that is allocated based on the formula (1) and B_{it} is what I call "equalization bonds", the redemption of which is supposed to be perfectly compensated by future fiscal equalization grants.[7] I consider the sum of

[6]In other words, $CAP_{it} \simeq PreCAP_{it} \times 3/4$. The reason that CAP_{it}, the revenue capacity index used to calculate G_{it}, is defined as 3/4 of $PreCAP_{it}$ is to leave the remaining 1/4 of $PreCAP_{it}$ as revenue capacity that is not equalised by the fiscal equalization scheme. This measure generates post-equalization fiscal disparity among local governments but leaves some incentive for local governments to raise their pre-equalization revenue capacity. See also Appendix A for a further explanation.

[7]Equalization bonds B_{it} consist of Bonds for Extraordinary Financial Measures (BEFM) and Bonds for Tax Cut Compensation (BTCC). BEFM and BTCC are special bonds issued to provide complementary fiscal equalization and revenue. The future redemption cost of BEFM and BTCC are perfectly reflected in the calculation of $NEED_{it}$

Figure 1: $PreCAP_{it}$ and $PostCAP_{it}$ against V_{it}



Note: The Lowess smoothers are obtained using the `lowess` command in Stata 13 with the default setting. Source: Reports on the Municipal Public Finance (fiscal year 2003 and 2004)

G_{it} and B_{it} as the total amount of fiscal equalization grants.

Figure 1 presents scatter plots and Lowess smoothers of $PreCAP_{it}$ and $PostCAP_{it}$ against V_{it}, the need-capacity gap, using pooled data of 2003 and 2004.[8] I also add a Lowess smoother of $NEED_{it}$. This figure illustrates how the LAT grant phases in at the cutoff point $V_{it} = 0$ and increases along V_{it}. As a result, the distribution of $PostCAP_{it}$ against V_{it} is a clear V-shape.

and therefore municipalities do not have to cover the cost with their own revenue. See also Appendix A for further description.

[8]Although the amounts of equalization bonds issued by municipalities in $PostCAP_{it}$ are not available in officially collected municipality data, they can be approximately calculated using several official statistics. See also Appendix E in Ando (2013), which shows the same graphical representation of the LAT grants but data in 1980s and 1990s when no equalization bonds were issued. $PreCAP$ and $PostCAP$ in this essay correspond to $PreRev$ and $PostRev$ in Ando (2013).

2.2.3 Sources of grant effect heterogeneity

This paper estimates grant effects for municipalities around the threshold of grant eligibility ("Case I" in Figure 1) and for municipalities that are more dependent on fiscal equalization grants ("Case II" in Figure 1). I expect that grant effects will be different in these two subgroups for the following reasons.

First, when it comes to municipalities in Case I, I assume that the marginal increase in the LAT grants at the threshold of grant eligibility are truly unconditional and lump-sum and not affected by centrally determined expenditure needs. This assumption is justified by the fact that the introduction of the LAT grants happens at the threshold with a kinked assignment rule (1), whereas the level of centrally determined expenditure needs (or its indicator variable $NEED_{it}$) evolves smoothly around the threshold (see Figure 1). In other words, marginal increase in the fiscal equalization grants from zero to non-zero at this point is not driven by a systematic increase in expenditure needs defined by the central government.

In addition, municipalities at the threshold can be assumed to be able to cover basic expenditure needs with their own revenues, because pre-equalizatin revenue capacity ($PreCAP_{it}$) is larger than centrally determined expenditure needs ($NEED_{it}$) at the threshold by definition (see Figure 1).[9] It is thus reasonable to assume that these municipalities can use their additional revenue from fiscal equalization grants as unconditional and lump-sum grants.

Second, fiscal equalization grants for municipalities in Case II, on the other hand, need to be interpreted more cautiously. For these municipalities, the increase in fiscal equalization grants ($G_{it}+B_{it}$) is mostly driven by the increase in their centrally determined expenditure needs ($NEED_{it}$).[10] Given the fact that the expenditures on local public services which are reflected in $NEED_{it}$ are often mandatory or to some degree regulated by the central government, the effects of fiscal equalization grants on local expenditures for these municipalities may be the consequences of central policies, rather than the results of discretionary choices made by municipalities.

In fact, Hayashi (2000; 2006) provides critical reviews of empirical studies on flypaper effects in Japan and points out that the previous studies do not consider such institutional properties of LAT grants. DeWit (2002) also argues that Japanese LAT

[9] Remember that $NEED_{it} = CAP_{it} \simeq PreCAP_{it} \times 3/4$ at the threshold.

[10] Note that the amount of per capita equalization bonds (B_{it}) was determined by a similar formula to that of $NEED_{it}$ during the sample period, implying that B_{it} also reflects expenditure needs.

grants were exploited by the central government to "force-feed" local governments public-works funding. A book written by a former top bureaucrat of the Ministry of Home Affairs (renamed the "Ministry of Internal Affairs and Communications" in 2001) explicitly states that the allocation of LAT grants was significantly affected by the intention of the central government to distribute more investment funds to under-developed regions (Ishihara 2000).

Table 1 provides expected grant effects in Case I and Case II, taking into consideration the institutional characteristics of Japanese fiscal equalization schemes. The expected grant effect on total expenditure is approximately one-to-one or greater in both cases because it is a well-known fact that the local tax system in Japan is basically homogeneous: tax items, tax rates and tax bases are more or less uniform across local municipalities.[11] A greater than one-to-one effect is also possible in Japanese local public finance because local public services and local investment projects are often partly financed by matching grants and local bonds. For example, it is possible that a one-unit increase in grants will result in a two-unit increase in total expenditure, with the additional one-unit increase coming from matching grants with a matching rate of 50%. Similarly, a one-unit decrease in grants can lead to a greater than one-unit decrease in total expenditure.

Expected grant effects on disaggregated expenditures are rather unclear for Case I if I do not presuppose a specific political-economic model for local governments. But I can at least argue that observed grant effects in Case I should reflect municipalities' own preferences and decision making as in the classical B&O model, because in this case grants can be interpreted as being truly unconditional and lump-sum. In Case II, on the other hand, I expect that grant effects are more clearly observed in construction expenditures because some portions of LAT grants are implicitly tied to construction expenditures via the calculation of centrally determined fiscal needs $NEED_{it}$.

3 Data and identification strategies

In order to separately estimate grant effects for Cases I and Case II, I rely on two different quasi-experimental research designs with an identical dataset of Japanese municipalities. Although the comparison of estimates that are obtained by ex-

[11] Nagamine (1995) argues Japan has an "institutionalized" flypaper effect because local governments have little discretion over local taxes.

Table 1: Expected grant effects in Case I and Case II

	Subgroup for analysis	Expected grant effect on total expenditure	Expected grant effects on disaggregated expenditures
Case I	Municipalities around the threshold of grant eligibility	About One-to-one or more	No unique a priori prediction. Grant effects may reflect municipalities' own preferences.
Case II	Municipalities with relatively high need-capaicty gaps	About One-to-one or more	Construction expenditures may be most significantly affected by grants.

ploiting different local variations require some caution because different estimators have different interpretations, I can utilize the fact that different local identification strategies produce different local estimates of grant effect.

3.1 Data

In studies on Case I and Case II, the same panel data of 2,267 municipalities (cities, towns, and villages) in the fiscal year 2003 and 2004 are used for estimation. The number of municipalities is 2,521 at the end of fiscal year 2004, but the municipalities that experienced amalgamation during 1999-2004 are excluded from the sample because they received special financial support. 23 Special districts in Tokyo prefecture and one municipality (Miyake village) are also dropped from the sample.[12]

Outcome variables consist of total expenditure and disaggregated expenditures that are categorized by the types of expenditure. The main explanatory variable is the fiscal equalization grants, which consist of the ordinary LAT grants and equalization bonds BEFM and BTCC. Although the amount of the equalization bonds that were actually issued by municipalities are not available in officially collected municipality data, the amount of equalization bonds whose redemption is perfectly compensated by the central government can be approximately calculated using several official statistics.[13]. Finally, I also use several covariates to mitigate possible biases that may arise in my estimation.

Descriptive statistics for these variables as well as the instrument and covariates are shown in Table 2. In Appendix B, I also provide basic pooled OLS and first-differenced (FD) OLS estimates for total expenditure and disaggregated expen-

[12]Special districts fall under a different fiscal equalization scheme implemented by Tokyo Metropolitan Government. Miyake village on Miyake Island had no residents during 2003 and 2004 because of an evacuation necessitated by a volcanic eruption.

[13]See Appendix A for further descriptions about BEFM and BTCC.

ditures, where I regress an expenditure variable on the variable of fiscal equalization grants (LAT grants + equalization bonds) and the set of covariates. When I check the estimated coefficients of the grant variable, both OLS and FD estimates for the total expenditure are larger than two and most OLS and FD estimates for disaggregated expenditures are significantly different from zero.

Table 2: Descriptive statistics

Variable	Cross-sectional data (2003, 2004)			First-differenced data (2004-2003)		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.
Expenditures (1000 yen)						
Total expenditure	4534	588.27	456.73	2267	-12.83	121.34
Personnel	4534	114.93	73.05	2267	-0.49	6.88
Supplies and services	4534	72.56	60.21	2267	-0.99	10.38
Maintenance and repairment	4534	6.26	9.36	2267	0.07	2.84
Social assistance	4534	32.59	16.06	2267	2.67	2.16
Subsidies	4534	66.46	46.28	2267	-0.59	19.78
Subsidized construction	4534	55.13	129.86	2267	-12.64	84.60
Unsubsidized construction	4534	68.82	83.39	2267	-5.31	57.44
Debt service	4534	88.27	91.96	2267	0.41	15.56
Addition to reserve funds	4534	17.94	38.67	2267	-1.26	32.43
Transfers to other accounts	4534	49.09	30.79	2267	1.73	14.96
Fiscal equalization grants (1000 yen)						
LAT grants (G)	4534	180.55	183.02	2267	-6.22	14.21
Equalization bonds (B: BEFM and BTCC)	4534	41.02	29.21	2267	-11.66	10.97
Fiscal indices (1000 yen)						
Need-capacity gap (V)	4534	178.12	186.82	2267	-6.75	17.04
Fiscal need (NEED)	4534	279.18	182.01	2267	-4.19	12.95
Revenue capacity (CAP)	4534	101.06	46.68	2267	2.56	10.75
IV in Study II (1000 yen)						
Simulated grant reduction by revision	-	-	-	2267	-1.92	1.86
Covariates*						
Pre-equalization revenue capacity (1000 yen)	4534	131.79	61.97	2267	2.70	14.33
Populaion	4534	42210.74	142931.10	2267	16.06	763.95
Population density (pop/km^2, 2003)	2267	738.75	1515.45	-	-	-
Sectoral ratio (primary industry, %, 2000)	2267	14.04	11.12	-	-	-
Sectoral ratio (tertiary industry, %, 2000)	2267	53.87	10.69	-	-	-
Population growth rate (%, 1995-2000)	2267	-1.40	5.54	-	-	-

*The primary sector consists of agriculture, forestry, fisheries and mining. The tertiary sector includes all industries that are not included in the primary and secondary sectors (construction and mining).
Note: All fiscal variables are divided by population, meaning that they are per-capita values.
Sources: Reports on the Municipal Public Finance, official documents of the Ministry of Internal Affairs and Communications, and my own calculations.

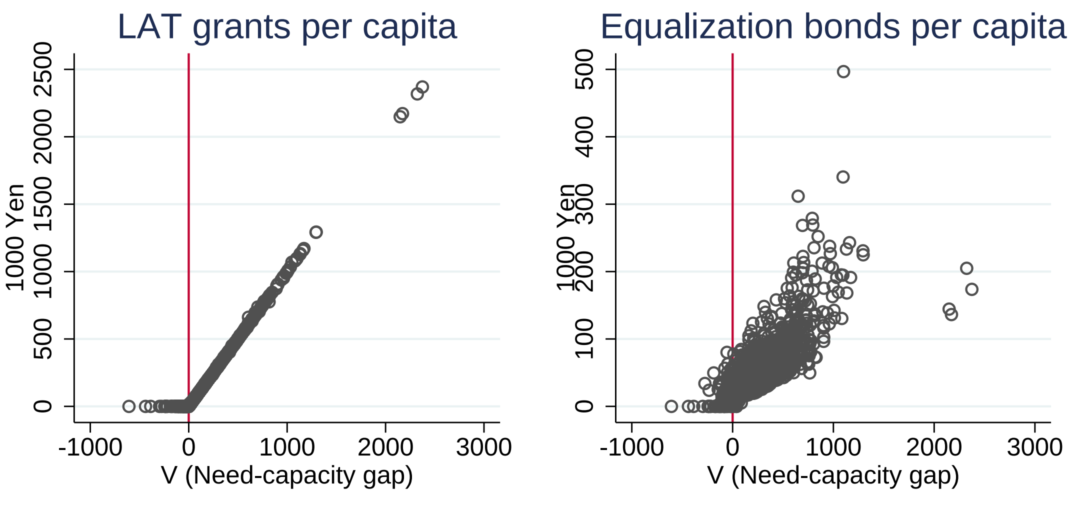
3.2 Case I: A regression kink design

In Case I, I estimate grant effects for relatively wealthy municipalities that receive a small amount of LAT grants at the margin of grant eligibility. As is investigated in section 2.2, LAT grants for municipalities around the threshold of grant eligibility should be more discretionary because the kinked assignment of LAT grants at this point is not directly tied to a smooth increase in expenditure needs. In addition, municipalities around this threshold are relatively wealthy and can cover their mandatory expenditures using their own pre-equalization revenues, setting aside fiscal equalization grants as additional funds for discretionary use.

In order to examine grant effects for these municipalities around the kinked grant introduction threshold, I estimate the local average treatment effect of fiscal equalization grants by exploiting the regression kink (RK) design as in Ando (2013).[14] I am able to use the RK design in the institutional setting of Japanese LAT grants because the grant assignment rule (1) in the last subsection has a deterministic kink at $V_i = 0$.

Figure 2 presents the scatter plots of fiscal equalization grants against the need-capacity gap. The left-hand graph indicates that LAT grants G_{it} have a clear deterministic kink at the threshold whereas the right-hand graph shows that equalization bonds B_{it} do not have such a kink. In my baseline case I therefore estimate the effect of LAT grants on local expenditures exploiting the deterministic kink of LAT grants. In addition, because a change in the trend of B_{it} around the threshold may induce biased RK estimates, I implement a robustness check in which I use B_{it} as an additional covariate.

Figure 2: Fiscal equalization grants against V



Source: Reports on the Municipal Public Finance, official documents of the Ministry of Internal Affairs and Communications, and my own calculations (fiscal year 2003 and 2004).

[14] See Card et al. (2015) for theoretical and methodological discussions of the RK design.

The sharp RK estimand can be expressed as follows, following the approach used in Nielsen et al. (2010) and Card et al. (2015):

$$\beta_{RK} \equiv \frac{\lim_{e\to 0} \left.\frac{dE(Y_{it}|V_{it}=v)}{dv}\right|_{v=+e} - \lim_{e\to 0} \left.\frac{dE(Y_{it}|V_{it}=v)}{dv}\right|_{v=-e}}{\lim_{e\to 0} \left.\frac{dE(G_{it}|V_{it}=v)}{dv}\right|_{v=+e} - \lim_{e\to 0} \left.\frac{dE(G_{it}|V_{it}=v)}{dv}\right|_{v=-e}}. \quad (2)$$

This quantity denotes a change in the slope of $E(Y_i|V_i = v)$ at $V_i = 0$ divided by a change in the slope of $E(G_i|V_i = v)$ at $V_i = 0$. That is, a sharp RK estimand identifies a local average treatment effect as the ratio of a kink size in an outcome variable to the kink size in a treatment variable at the threshold. In my specific case, I know that the kink size in the treatment variable G_{it} is one[15], so the sharp RK estimand β_{RK} is identical to the numerator of the right-hand size of the equation (2).

Although the causal interpretation of the sharp RK estimand is relatively clear, the estimand (2) is inherently local. The application of the RK design therefore forces me to focus on grant effects for a subgroup of municipalities around $V_{it} = 0$. This is in fact preferable, however, in the sense that this subgroup consists of relatively affluent municipalities that are at the margin between grant receivers and non-receivers and have a relatively large amount of their own tax revenues. As is mentioned in Section 2.2.3, estimated grant effects at this margin can be more or less considered to reflect municipalities' discretionary policy preferences or propensities rather than any implicit or explicit institutional restrictions imposed on them.

For empirical analysis, I use a linear or quadratic polynomial model with a certain bandwidth of V_{it} to estimate the magnitude of a kink in an outcome variable.[16]

[15] G_{it} changes from $0 \cdot V_{it}$ to $1 \cdot V_{it}$ at $V_{it} = 0$.

[16] I do not use higher order polynomials such as third-order (cubic) and fourth-order (quartic) polynomials in estimation because an RK estimate generally becomes very imprecise with these model specifications. Monte Carlo simulations in Ando (2013) also suggest that RK estimates with higher-order polynomials tend to be very imprecise. Card et al. (2012)'s discussion of a substantial cost in variance when local quadratic polynomials are used instead of local linear polynomials is relevant here. Simonsen et al. (2015) also adopt only RK estimation with a linear polynomial in their baseline analysis because of the high variance of RK estimation with a quadratic polynomial. In the context of RD designs, Gelman and Imbens (2014) argue that high-order polynomials should not be used for RD estimation because of several unfavourable properties of high-order polynomials.

The linear polynomial model is expressed as follows,

$$Y_{it} = \theta_0 + \theta_1 V_{it} + \theta_2 V_{it} \cdot D_{it} + \varepsilon_{it}, \quad |V_{it}| < h \tag{3}$$

where $D_{it} = 1$ if $V_{it} > 0$ and otherwise 0, ε_{it} is an error term and h is the size of a bandwidth. The quadratic specification adds the terms V_{it}^2 and $V_{it}^2 \cdot D_{it}$ and their coefficients to the right-hand side of the above equation. The parameter of interest is θ_2, which captures the size of the kink in Y_{it} at $V_{it} = 0$.

When it comes to identifying assumptions for a valid sharp RK analysis, I primarily assume that the density function of V_{it} and pre-determined covariates are smoothly distributed (continuously differentiable) at $V_{it} = 0$ as is discussed by Card et al. (2015). I confirm these assumptions in the preliminary analysis subsection (Section 4).

In addition, as Ando (2013) demonstrates using Monte Carlo simulations and an RK estimation of LAT grant effects on total expenditure with Japanese city data between 1980-1999, RK estimation may not be able to separate out a true "kink" in an outcome variable from confounding nonlinear relation ("curves") between an assignment variable and an outcome variable. I therefore include several covariates and their quadratic terms among the control variables in my baseline RK estimations to mitigate bias from this confounding nonlinearity.

Finally, I also provide a set of placebo RK estimates to show the validity of my RK analysis. In a placebo trial, the same RK estimation is repeatedly implemented with a placebo threshold as if a formula-driven kink were located at this placebo threshold. In other words, I implement the following linear RK estimation with a different placebo threshold value c_{plcb}:

$$Y_{it} = \pi_0 + \pi_1(V_{it} - c_{plcb}) + \pi_2(V_{it} - c_{plcb}) \cdot D_{it} + \eta_{it}, \quad |V_{it} - c_{plcb}| < h \tag{4}$$

where $D_{it} = 1$ if $(V_{it} - c_{plcb}) > 0$ and otherwise 0 and η_{it} is an error term. A placebo RK estimate is captured as the estimate of π_2, which is the counterpart parameter of θ_2 in model (3).

This placebo test is akin to the Fisher-style permutation inference for RK estimation proposed by Ganong and Jäger (2015) with the assumption of "Random Kink Placement" and the sharp null hypothesis of no policy effect. I, however, provide the results of placebo tests not as a formal p-value under a sharp null hypothesis but as a graph of placebo RK estimates $\widehat{\pi_2}$ against placebo threshold values c_{plcb}. If RK estimation is valid (i.e. no bias-inducing confounding nonlinearity exists), a placebo RK estimate should be around zero if the placebo threshold is not very close to the true threshold.

3.3 Case II: An instrumental variable approach

Next, I consider a different identification strategy that is meant to capture grant effects for a different subgroup of municipalities. That is, by exploiting an exogenous formula revision in a component of $NEED_{it}$ in the early 2000s as an instrument, I estimate grant effects for the municipalities with relatively high expenditure needs that are significantly affected by this revision.

As is explained in Section 2.2.1, the amount of the LAT grants is determined by the gap between fiscal need $NEED_{it}$ and revenue capacity CAP_{it}. In turn, $NEED_{it}$ formula for a public service k consists of the following three components:

$$NEED^k_{it} = Unit\ Cost^k_t \times Measurement\ Unit^k_{it} \times Adj.Coeff^k_{it}, \tag{5}$$

where $Unit\ Cost^k_t$ is the uniform standard cost of providing one unit of public service k in year t, $Measurement\ Unit^k_{it}$ is a measurement unit for public service k in municipality i in year t, and $Adj.\ Coeff^k_{it}$ is the set of adjustment coefficients that reflect various environmental factors in municipality i.[17]

I utilize the fact that so-called *Scale Adjustment Coefficients* in $Adj.\ Coeff$, which takes into account the scale efficiency in the provision of local public services, were revised and reduced for relatively small municipalities during 2002-2004, resulting in exogenous decreases in their LAT grants. The objective of this revision was, according to several official documents, to make the local public finance of small municipalities more efficient by suppressing their LAT grants. Because small municipalities are in general municipalities with high expenditure needs and a high need-capacity gap, this exogenous reduction in grants can be utilized to investigate grants effects for Case II.

As is shown in Appendix C, the simulated amount of grant reduction caused by the revision of *Scale Adjustment Coefficients* is a deterministic function of the size of lagged population.[18] This simulated grant reduction can be used as an instru-

[17]See Appendix A for further description.

[18]The simulated amounts of grant reduction are calculated by the author with the of-

mental variable (IV) for actual grant changes, conditional on the flexible polynomial function of lagged population that controls for the direct effect of population on an outcome variable.

In Figure 3, I provide scatter plots of the instrumental variable and changes in LAT grants and equalization bonds from 2003 to 2004 against the need-capacity gap in 2003.[19] This figure suggests that my instrument is appropriate for capturing grant effects for municipalities with relatively high need-capacity gaps. Figure 3 indicates that a municipality with a higher need-capacity gap is faced with a larger grant reduction under the revision, resulting in a larger reduction in LAT grants and equalization bonds.

My estimation strategy is based on an instrumental variables approach with a first-differenced (FD-IV) model:

$$\begin{aligned} &\text{First stage} &&: \triangle G^*_{it} = \lambda_0 + \lambda_1 Z_{it} + g(pop_{i,t-1}) + \eta_{it} \\ &\text{Second stage} &&: \triangle Y_{it} = \rho_0 + \rho_1 \triangle G^*_{it} + f(pop_{i,t-1}) + \varepsilon_{it}, \end{aligned}$$

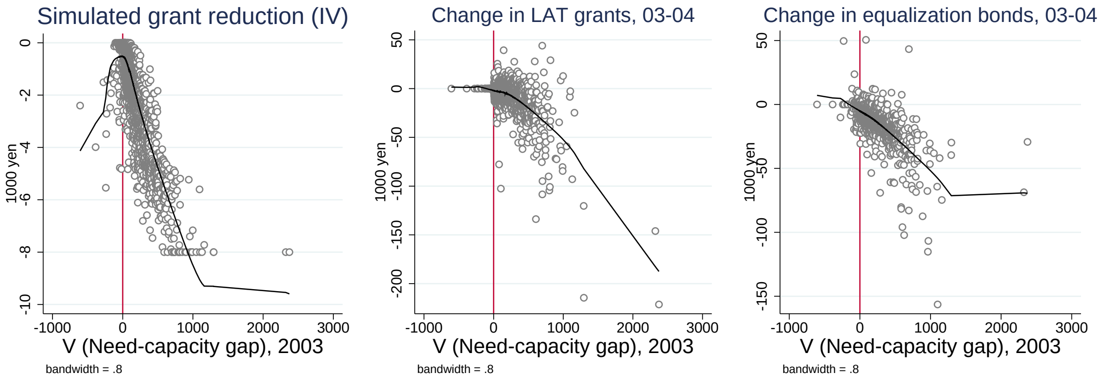
where $\triangle Y_{it}$ and $\triangle G^*_{it}$ are defined as first-differenced variables of expenditure $Y_{it} - Y_{i,t-1}$ and fiscal equalization grants $G^*_{it} - G^*_{i,t-1}$ respectively and $G^*_{it} = G_{it} + B_{it}$. Z_{it} is the instrumental variable mentioned above, $g(pop_{i,t-1})$ and $f(pop_{i,t-1})$ are polynomial functions of lagged population, and η_{it} and ε_{it} are error terms. Unlike in Case I, I include equalization bonds B_{it} in the treatment variable G^*_{it}. I do this because, as the right graph in Figure 3 suggests, my instrument seems to affect B_{it}, which is calculated by a formula similar to the formula of $NEED^k_{it}$ expressed in Equation (5).

Because I use only two periods (fiscal yaer 2003 and 2004), the actual estimation

ficial simulated data (at several standard population levels) presented by the Ministry of Internal Affairs and Communications (MIC) and interpolation of this data using hyperbolas. Interpolation with hyperbolas is the interpolation method that is adopted in the actual calculation of the LAT grants and I follow the same method. See also Appendix C.

[19] I cannot use the first-differenced data of 2001-2002 and 2002-2003 because equalization bonds in these years were issued such that these bonds (in particular BEFM) partly cancelled out the reduction of the LAT grants: smaller municipalities that were faced with a greater reduction in their LAT grants tended to be entitled to issue larger amounts of equalization bonds. It was only in fiscal year 2004 that both the LAT grants and equalization bonds were significantly reduced from the previous year. This significant reduction in fiscal equalization grants from 2003 to 2004 is referred to as "*Chizai Shock*"(local public finance shock).

Figure 3: Changes in fiscal equalization grants (2003-2004) against $V_{i,2003}$



Notes: The graph shows the simulated amount of LAT grant reduction caused by the revision of *scale adjustment coefficients*, which is a deterministic function of lagged population (see Appendix C.). The central graph and the left graph show the actual changes in the LAT grants and equalization bonds from 2003 to 2004 respectively. The smoothed curve in each graph is a Lowess smoother based on the default setting of the `lowess` command in Stata 13. All variables are per capita values. Sources: Reports on the Municipal Public Finance, official documents of the Ministry of Internal Affairs and Communications, and my own calculations.

is identical to cross-sectional 2SLS estimation using first-differenced variables and lagged population. Note that Z_{it} is a deterministic function of $pop_{i,t-1}$, so controlling for the direct effect of $pop_{i,t-1}$ should be sufficient to ensure the validity of the exclusion restriction in this IV strategy. In addition, time-invariant fixed effects are assumed to be cancelled out by first-differencing.

4 Preliminary analysis

Before presenting estimation results, I provide several preliminary analyses for Cases I and II. Because both Cases I and II rely on seemingly exogenous variations that can be graphically observed, expected effects may also be observable in graphical representation. In addition, the identifying assumptions of the RK design in Case I can also be checked visually.

4.1 Case I

4.1.1 Graphical analysis

In Figure 4, I provide bin-mean plots of total expenditure and disaggregated expenditures against the assignment variable V_{it}. These graphs can be used to check whether there are observable kinks at the threshold of the need-capacity gap, $V_{it} = 0$.

Graphical implications are somewhat ambiguous, but total expenditure and some expenditure items such as supplies and services, subsidies, subsidized construction, and unsubsidized construction appear to have some trend changes around $V_{it} = 0$.

Although from the graph it is not clear whether these trend changes around the threshold are "kinks" or just "curves", they implies that the kinked assignment of the LAT grants at the threshold may affect the total expenditure and disaggregated expenditures trends.

4.1.2 Density distribution

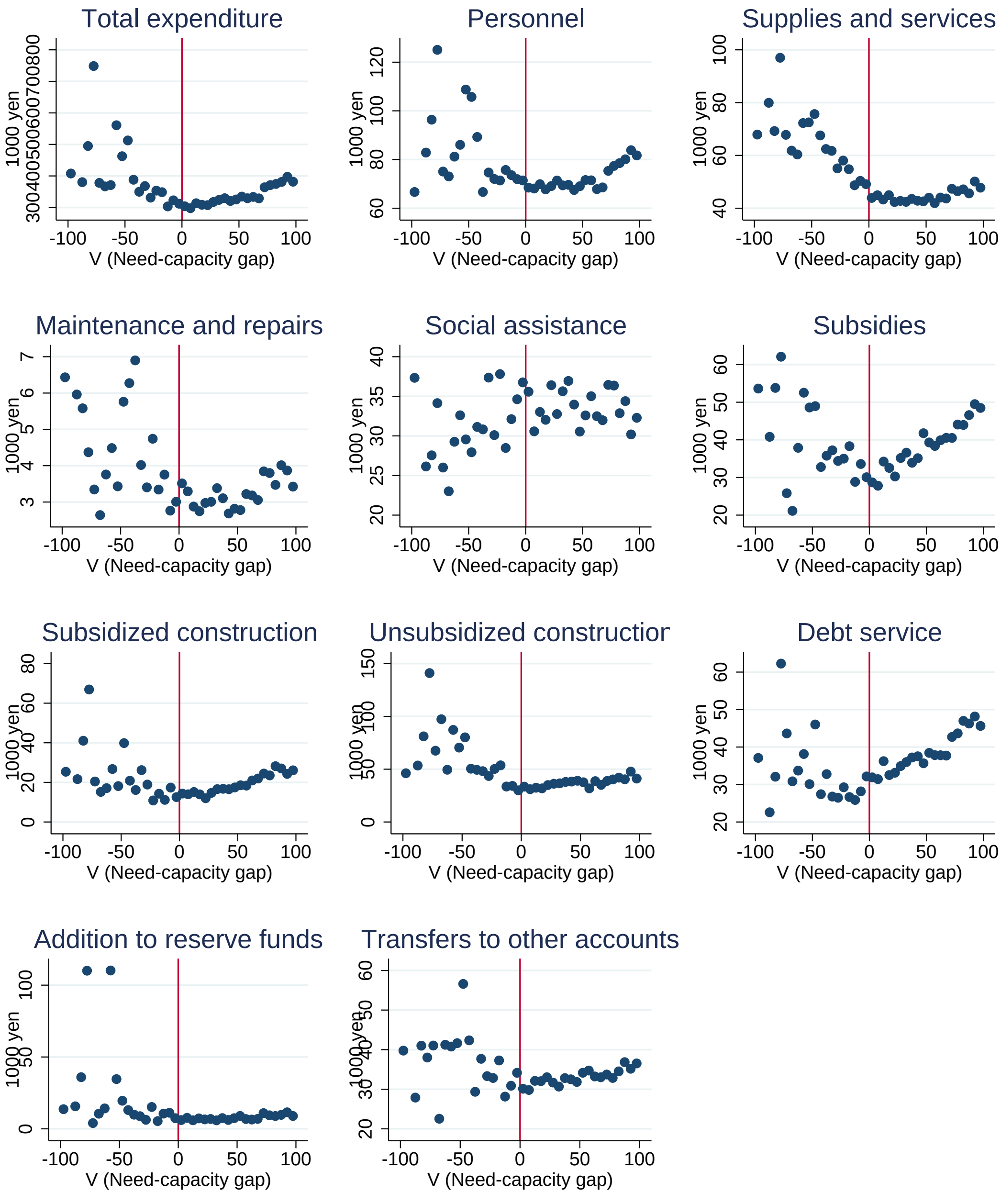
Second, a key identifying assumption for a valid RK design is that the density of the assignment variable is continuously differentiable at the threshold (Card et al. 2015). Since the LAT grant is calculated using uniform centrally determined formulas, there is little possibility that municipalities or the central government can precisely manipulate the need-capacity gap around the threshold. It is possible, however, that some institutional settings or unknown factors systematically affect the determination of whether or not a given municipality near the threshold becomes an LAT-grant receiver. I therefore conduct a density test analogous to that proposed by McCrary (2008) and presented by Card et al. (2015) in the context of an RK design. Both graphical analysis and estimation results in Appendix D indicate that the density of the need-capacity gap is smooth at the threshold.

4.1.3 Covariate distributions

Third, according to Card et al. (2015), an important implication under the required conditions for a valid RK design is that any pre-determined covariate should have a conditional distribution which evolves smoothly around the threshold. In other words, there should be no kink at the threshold for any pre-determined covariate against an assignment variable.

However, a smooth nonlinear relation between a covariate and an assignment variable around the kink point can be picked up as a kink in RK estimation (Ando

Figure 4: Bin-mean plots of disaggregated expenditures



Notes: All variables are per capita values. Bin size is 5 and bandwidth is $V_{it} < 100$. Source: Reports on the Municipal public Finance

2013). It may thus be difficult to assert that there are no kinks whatsoever at the threshold for any covariate. Bin-mean scatter plots of covariates in Appendix E at least indicate that no such kinks are visually apparent in the graphical representation of the data, although some trend changes around the threshold are observed. To mitigate possible bias arising from these nonlinearities, I include these covariates and their quadratic terms among the control variables in my baseline RK estimations.

4.2 Case II

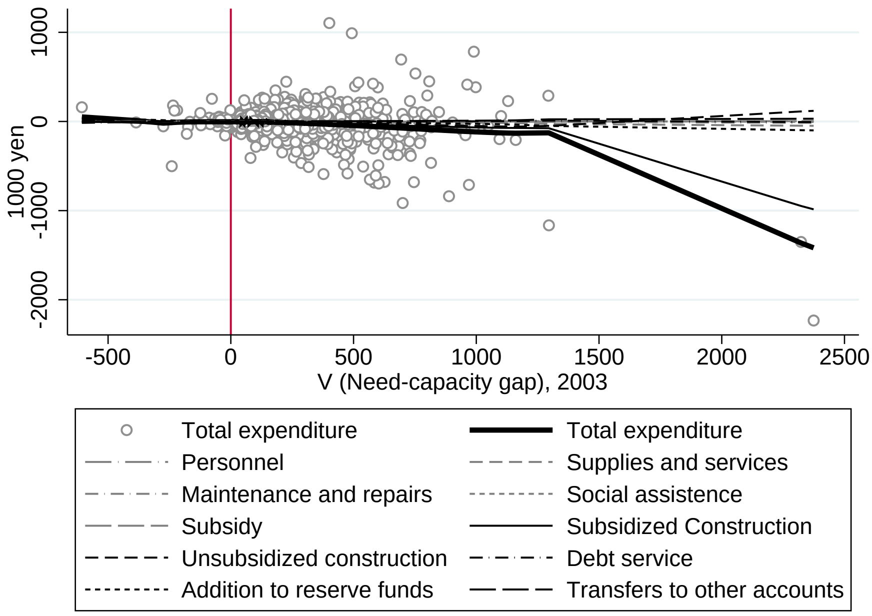
Figure 5 presents Lowess smoothers of changes in total expenditure and disaggregated expenditures (2003-2004) against the need-capacity gap in 2003. This figure shows that total expenditure decreases in municipalities with higher need-capacity gaps, and this seems to be mostly explained by expenditure decreases in subsidized construction (solid black line), implying that the grant effect caused by grant reduction from 2003 to 2004 is observed mostly in subsidized construction expenditures.

According to the scatter plots of changes in total expenditure in Figure 5, this result may appear to be driven by a few municipalities with the highest need-capacity gaps of over 2,000, but the Lowess curves around the need-capacity gaps of 500-1,500 suggest that the most decreased expenditure item is subsidized construction regardless of municipalities' need-capacity gaps. To address this issue, I also investigate possible grant effect heterogeneity (or homogeneity) with different subsamples in the following FD-IV estimation.

5 Results

In this section I start by estimating grant effects for municipalities around the threshold of grant eligibility with the RK design (Case I) and then I obtain the counterpart grant effects for less wealthy municipalities that are located far away the threshold using the FD-IV approach (Case II). In Appendix G, I also provide additional analysis on revenues.

Figure 5: Lowess curves of changes in expenditures



Notes: Expenditures are per capita values. Lowess smoothers are obtained using the default setting of the `lowess` command in Stata 13. Source: Reports on the Municipal Public Finance

5.1 Case I

5.1.1 RK estimates for total expenditure

Table 3 provides RK estimates for total expenditure with a linear or quadratic polynomial specification and different bandwidths.[20] I also added the set of control variables and their quadratic terms listed in Table 2. RK estimates with a linear polynomial for total expenditure show that the grant effect on total expenditure is more or less one. Although estimates with a quadratic polynomial are relatively unstable, probably due to imprecise RK estimation with higher order polynomials (Card et al. 2012; Ando 2013), they also indicate that grant effects on total expenditure are not far from one.[21] The null hypothesis that an RK estimate is equal to one cannot be rejected in most cases.

In Table F.1 in Appendix F, I provide further results of RK estimation employing

[20] I provide various RK estimates with different bandwidths and show the range of RK estimates as a robustness check rather than presenting a single optimal RK estimate based on the developing literature of optimal bandwidth in RD and RK designs such as Imbens and Kalyanaraman (2012), Calonico et al. (2014) and Card et al. (2015).

[21] These results are similar to the results of Ando (2013), which implements a similar RK estimation (but only for total expenditure) with Japanese city data between 1980-1999.

Table 3: RK estimates for total expenditure

Bandwidth	Estimate	Robust S.E.	P(H0: θ2=1)	Sample size
Linear polynomial with full covariates				
No	0.597	(0.501)	0.421	4,534
|V|<100	0.708***	(0.177)	0.0995	1,845
|V|<90	0.775***	(0.202)	0.267	1,674
|V|<80	0.805***	(0.221)	0.379	1,472
|V|<70	0.827***	(0.242)	0.473	1,287
|V|<60	1.056***	(0.254)	0.826	1,114
|V|<50	1.144***	(0.262)	0.584	931
|V|<40	1.166***	(0.297)	0.575	731
|V|<30	0.884**	(0.434)	0.789	522
|V|<20	0.873	(0.770)	0.869	335
Quadratic polynomial with full covariates				
No	0.598**	(0.288)	0.163	4,534
|V|<100	0.991**	(0.459)	0.984	1,845
|V|<90	0.837*	(0.492)	0.740	1,674
|V|<80	0.602	(0.625)	0.524	1,472
|V|<70	1.309*	(0.737)	0.675	1,287
|V|<60	0.823	(0.937)	0.850	1,114
|V|<50	1.069	(1.082)	0.949	931
|V|<40	0.969	(1.168)	0.979	731
|V|<30	0.678	(1.487)	0.829	522
|V|<20	-0.038	(3.195)	0.746	335

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All covariates in Table 2 and their quadratic terms are included among the regressors.

RK estimates without covariates, with additional covariates of equalization bonds, and with an additional covariate of the lagged outcome variable using cross-sectional data for 2004.[22] Results suggest that although RK estimates without any covariates are apparently biased (Column 1 in Table F.1), the introduction of pre-determined covariates (Table 3) or a covariate of the lagged outcome variable (Column 3 in Table F.1) significantly reduces this bias. The inclusion of equalization bonds B_{it} among the covariates also does not significantly change RK estimates (Column 2

[22] I use a lagged outcome variable as an additional control variable rather than adopting fixed-effect or first-differenced RK estimation in accordance with Lee and Lemieux (2010)'s recommendation to do so for regression discontinuity (RD) analysis.

and 4 in Table F.1).

5.1.2 Placebo RK estimates for total expenditure

Two graphs in Figure 6 also support the plausibility of my RK estimation. These graphs provide the plots of placebo RK estimates against placebo thresholds without and with covariates respectively[23]. For estimation, a linear placebo RK model (4) with a bandwidth of 40 is used. The left graph shows that placebo RK estimates with placebo thresholds of around -20 are much larger than the true RK estimate with a threshold of zero, implying that there exists severe bias-inducing confounding nonlinearity around $V = -20$ and the true RK estimate is also biased due to this nonlinearity.

On the other hand, the right graph indicates that this confounding nonlinearity goes away once the control variable of pre-equalization revenue capacity and its quadratic term are introduced in the placebo RK model (4): a placebo RK estimate is significantly different from zero and close to one only when a placebo threshold is close to the true threshold of zero. These two graphs suggest that bias-inducing confounding nonlinearity is effectively eliminated once I control for pre-equalization revenue capacity.

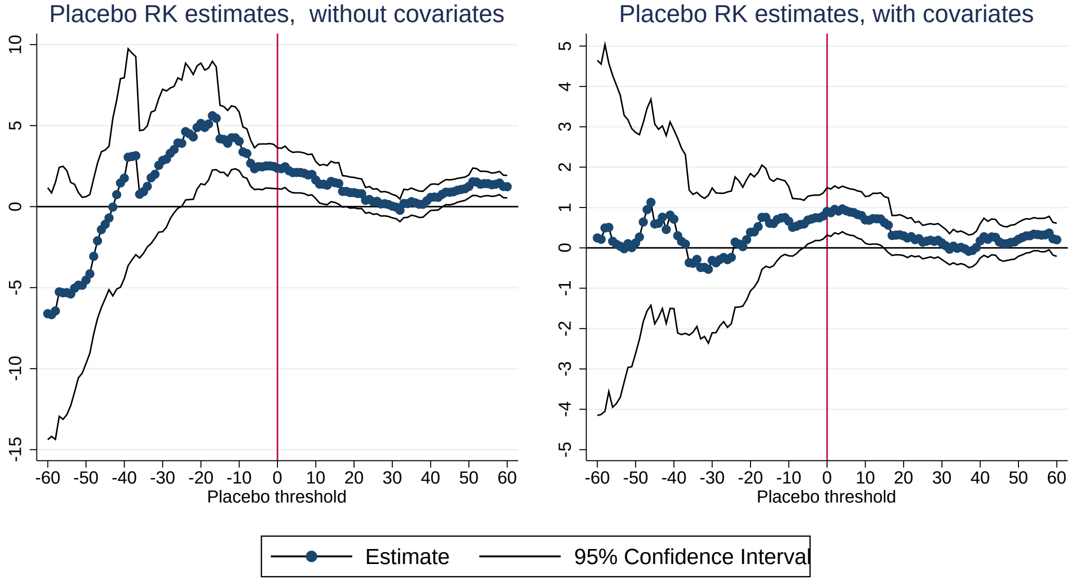
5.1.3 RK estimates for disaggregated expenditure

Next, table 4 provides selective RK estimates for disaggregated expenditures with several interesting results.[24] First, most RK estimates for personnel expenditure are not statistically different from zero, although personnel expenditure is the largest component in local public expenditure according to Table 2. Second, RK estimates for subsidized construction are not significantly different from zero, whereas RK estimates for unsubsidized construction are around 0.3-0.7 and mostly statistically significant. Third, RK estimates for supplies and services and social assistance are also significantly different from zero in most cases, and point estimates are around

[23]See Section 3.2 for the details of the placebo test.

[24]The selection of bandwidths with local and quadratic polynomials are based on the plausibility of RK estimation for total expenditure in Table 3. I drop RK estimates with a linear polynomial and larger bandwidths due to the possibility of biased estimates and RK estimates with a quadratic polynomial and smaller bandwidths due to the possibility of imprecise estimates.

Figure 6: Placebo RK estimates for total expenditure



Notes: The left graph shows how placebo estimates and their confidence intervals change (Y axis) with different placebo thresholds (X axis) using RK estimation without conditioning on covariates. The right graph provides the same placebo estimates and their confidence intervals using RK estimation, conditioning on the covariate of pre-equalization revenue capacity and its quadratic term.

0.2 for supplies and services and about 0.3-0.5 for social assistance.[25] RK estimates for the other remaining expenditure items are mostly statistically insignificant and close to zero.

In sum, RK estimation with both linear and quadratic polynomials suggests 1. an approximately one-to-one effect on total expenditure, 2. no statistically significant effect on personnel expenditure and subsidized construction, and 3. statistically significant effects on supplies and services, social assistance, and unsubsidized construction.

[25]Figure 4 shows no kink for social assistance but some kink (or curve) for subsidies. In actual estimation with covariates, however, the results are quite the opposite. This also implies that RK estimation without covariates may be easily biased if some confounding nonlinearity exists around the threshold.

Table 4: RK estimates for disaggregated expenditures

Polynomial order and bandwidth	Sample size	(1) Personnel	(2) Supplies and services	(3) Maintenance and repairment	(4) Social assistance	(5) Subsidies	(6) Subsidezed Construction	(7) Unsibsidized Construction	(8) Debt service	(9) Addtion to reserve funds	(10) Transfers to other accounts
Linear polynomial with full covariates											
|V|<60	1,114	-0.048	0.157***	0.020	0.322***	0.003	0.153	0.285**	-0.058	0.112	0.071
		(0.073)	(0.052)	(0.015)	(0.051)	(0.062)	(0.094)	(0.120)	(0.047)	(0.076)	(0.047)
|V|<50	931	-0.095	0.185***	0.027	0.290***	0.014	0.203*	0.493***	-0.033	-0.011	0.039
		(0.076)	(0.057)	(0.019)	(0.057)	(0.073)	(0.110)	(0.141)	(0.055)	(0.058)	(0.059)
|V|<40	731	-0.160*	0.189**	0.042**	0.389***	0.016	0.154	0.539***	-0.082	-0.026	-0.035
		(0.089)	(0.075)	(0.020)	(0.088)	(0.092)	(0.110)	(0.154)	(0.068)	(0.078)	(0.074)
|V|<30	522	0.024	0.107	0.017	0.256*	0.028	-0.020	0.600***	-0.110	-0.073	-0.017
		(0.120)	(0.100)	(0.022)	(0.132)	(0.131)	(0.131)	(0.223)	(0.089)	(0.115)	(0.105)
Quadratic polynomial with full covariates											
|V|<100	1,845	-0.097	0.198**	0.017	0.474***	-0.048	-0.011	0.437**	-0.170*	0.046	0.190*
		(0.117)	(0.094)	(0.027)	(0.106)	(0.110)	(0.201)	(0.211)	(0.101)	(0.192)	(0.097)
|V|<90	1,674	-0.154	0.190*	0.012	0.473***	0.024	-0.096	0.376*	-0.176	-0.073	0.213*
		(0.125)	(0.110)	(0.030)	(0.113)	(0.127)	(0.210)	(0.227)	(0.111)	(0.198)	(0.111)
|V|<80	1,472	-0.155	0.107	0.044	0.451***	0.173	0.112	0.282	-0.325***	-0.323	0.194
		(0.142)	(0.128)	(0.032)	(0.144)	(0.149)	(0.189)	(0.279)	(0.125)	(0.253)	(0.126)
|V|<70	1,287	-0.071	0.203	0.057*	0.553***	0.150	0.095	0.686**	-0.239*	-0.300	-0.016
		(0.159)	(0.150)	(0.034)	(0.160)	(0.173)	(0.204)	(0.338)	(0.136)	(0.283)	(0.144)

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Robust standard errors are in parentheses. All covariates in Table 2 and their quadratic terms are included among the regressors.

5.2 Case II

5.2.1 FD-IV estimates for total expenditure

Now let us look at Case II. Table 5 presents FD-IV estimates for total expenditure with linear, quadratic, and cubic polynomials of population and different sets of covariates. Although estimates change slightly with different model specifications, they largely suggest that grant effect on total expenditure is around 1.6-2.0, much larger than the RK estimates in Case I. An interpretation of this result is provided in Section 6.

Table 5: FD-IV estimates for total expenditure

Polynomial	Estimate	Robust S.E.	Covar.	First stage F-stat	Sample size
Linear	1.557***	(0.414)	No	297.7	2,267
	1.600***	(0.420)	Covar.1	290.8	2,267
	1.954***	(0.525)	Covar.2	180.2	2,267
	1.884***	(0.543)	Covar.3	171.2	2,267
Quadratic	1.611***	(0.429)	No	278.1	2,267
	1.640***	(0.432)	Covar.1	276.6	2,267
	1.982***	(0.532)	Covar.2	176.9	2,267
	1.915***	(0.550)	Covar.3	167.7	2,267
Cubic	1.667***	(0.446)	No	257.4	2,267
	1.681***	(0.447)	Covar.1	259.7	2,267
	1.979***	(0.533)	Covar.2	176.6	2,267
	1.914***	(0.551)	Covar.3	167.4	2,267

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. In "Covar.1", first-differenced variables of pre-equalization revenue capacity and population and their quadratic terms are included among the control variables. In "Covar.2" lagged population density, lagged sectoral ratios of primary and tertiary industries, a population growth rate (1995-2000) and their quadratic terms are also added to the control variables. In "Covar.3" prefecture fixed effects are further added. In "Covar.2" and "Covar.3", lagged population density is used rather than first-differenced population density because the variation of density over time is only caused by a change in population, which is already included among the control variables. The lagged rather than first-differenced variables of sectoral ratios and population growth are also used due to a lack of annual data.

5.2.2 FD-IV estimates for disaggreagated expenditures

Table 6 in turn provides FD-IV estimates for disaggregated expenditures with different polynomials and two covariate settings: controlling for no covariates and controlling for full covariates. First, FD-IV estimates for personnel expenditures are always near zero and statistically insignificant. Second, FD-IV estimates for subsidized construction are greater than one regardless of the introduction of covariates, whereas FD-IV estimates for unsubsidized construction are around 0.4 but statistically insignificant if covariates are introduced. This implies that, contrary to Case I, grant effects are larger on subsidized construction expenditure. Third, estimates for supplies and services and allocations to reserve funds are also positive and statistically significant, while estimates for social assistance are small and insignificant when covariates are introduced.

The above findings can be summarized as follows: 1. a greater than one-to-one effect on total expenditure, 2. no statistically significant effect on personnel expenditure, 3. statistically significant effects on supplies and services, subsidized construction, and allocations to reserve funds, and possibly to unsubsidized construction, and 4. the size of the effect on subsidized construction stands out.

Table 6: FD-IV estimates for disaggregated expenditures

Polynomial order	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Personnel	Supplies and services	Maintenance and repairment	Social assistance	Subsidies	Subsidezed Construction	Unsibsidized Construction	Debt service	Addtion to reserve funds	Transfers to other accounts
					No covariates					
Linear	-0.007	0.128***	-0.002	0.017***	0.029	1.158***	0.397*	-0.041	0.180*	-0.084
	(0.022)	(0.040)	(0.011)	(0.006)	(0.062)	(0.346)	(0.204)	(0.047)	(0.103)	(0.065)
Quadratic	-0.002	0.129***	-0.001	0.014**	0.028	1.174***	0.409*	-0.040	0.190*	-0.082
	(0.023)	(0.041)	(0.011)	(0.006)	(0.064)	(0.360)	(0.211)	(0.048)	(0.107)	(0.068)
Cubic	0.002	0.130***	-0.001	0.011*	0.026	1.190***	0.425*	-0.038	0.202*	-0.080
	(0.024)	(0.043)	(0.011)	(0.006)	(0.067)	(0.377)	(0.220)	(0.050)	(0.111)	(0.071)
					Covariates 3 (Full covariates)					
Linear	-0.016	0.109**	-0.003	0.007	-0.013	1.272**	0.352	-0.016	0.286**	-0.047
	(0.024)	(0.052)	(0.013)	(0.007)	(0.077)	(0.507)	(0.296)	(0.057)	(0.127)	(0.070)
Quadratic	-0.017	0.108**	-0.004	0.006	-0.012	1.273**	0.371	-0.017	0.295**	-0.048
	(0.025)	(0.052)	(0.014)	(0.007)	(0.078)	(0.519)	(0.302)	(0.057)	(0.128)	(0.070)
Cubic	-0.016	0.109**	-0.004	0.005	-0.011	1.276**	0.368	-0.017	0.295**	-0.049
	(0.025)	(0.052)	(0.014)	(0.007)	(0.078)	(0.519)	(0.301)	(0.057)	(0.128)	(0.070)
Sample size	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Robust Standard errors are in parenthesis. "Covariates 3 (Full covariates)" indicates Covar.3 in Table 5.

5.2.3 FD-IV estimates with varying subsamples

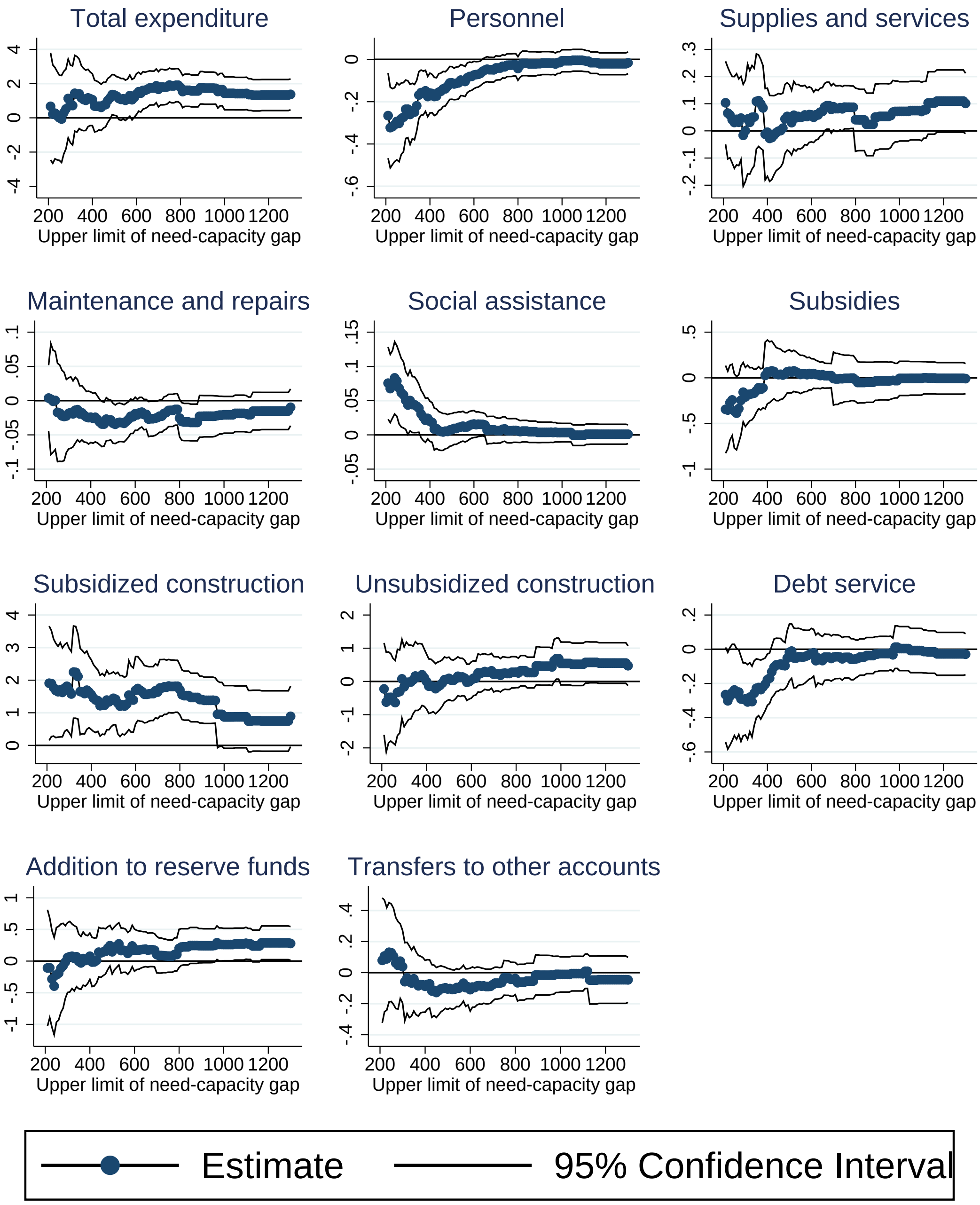
In this subsection, I examine how the above findings in Case II are robustly observed when I restrict observations in a sample to wealthier municipalities with lower need-capacity gaps. Given the fact that my instrument in Case II has greater variation in less wealthy municipalities with higher need-capacity gaps, this subsample analysis may result in more imprecise estimation. This is nonetheless an interesting trial because estimated grant effects between Case I and Case II can be made more comparable by limiting observations to wealthier municipalities in Case II. In other words, because RK estimates in Case I are interpreted as local grant effects around a need-capacity gap of zero, it is intriguing to investigate whether FD-IV estimates get closer to counterpart RK estimates when FD-IV estimation is implemented with a subsample that is more similar to that in RK estimation in terms of its need-capacity gap.

Results are shown in Figure 7. In these graphs, I provide FD-IV estimates and their confidence intervals (Y axis) with different criteria of subsample selection (X axis). For control variables, I use a quadratic polynomial of lagged population and full covariates (Covar.3 in Table 5). In each FD-IV estimation, I limit observations to municipalities whose need-capacity gaps in 2003 are smaller than a certain value, which is expressed as the "upper limit of need-capacity gap" on the X axis. By changing this criterion in increments of 10 from 1300 to 200 (1300,1290, 1280,...,200), I obtain 111 FD-IV estimates for each expenditure item.[26].

When it comes to FD-IV estimates for total expenditure, they are robustly around 1.5-2.0 and significantly different from zero when the upper limit of the need-capacity gap is 600 or larger, but otherwise they are statistically insignificant. Because it is unlikely that grants do not affect total expenditure in the context of Japanese local public finance, this result suggests that FD-IV estimation may not be plausible when observations are limited to the municipalities in which the need-capacity gap is lower than 600, possibly due to insufficient variation in an outcome variable. Statistically significant negative estimates for personnel and debt service with an upper limit of around 500 or less also imply that some FD-IV estimates with low upper limits are unreliable.

[26]I do not show FD-IV estimates when the values of the upper limit (X axis) are smaller than 200 because standard errors become too large to show in one graph. I also exclude FD-IV estimates when the upper limit is higher than 1300 because there are only two observations in which need-capacity gaps are between 1300 and 2500 (see Figure 5)

Figure 7: FD-IV estimates with varying subsamples



Notes: For each expenditure item, 111 FD-IV estimates and their 95% confidence intervals are obtained by using subsamples with different upper limits of the need-capacity gap. Note that the first-stage F-statistic is at its minimum when the upper limit is 200 but it is still 40.49.

In turn FD-IV estimates for subsidized construction, the size of which is greater than one in the baseline analysis, are again around 1.0-1.5 in most cases and significantly different from zero at the 90% or 95% significance level. This indicates that a grant effect on subsidized construction is rather homogeneous across municipalities with different need-capacity gaps in Case II. At the same time, this result shows that the sizes of FD-IV estimates do not converge on those of RK estimates when I restrict observations to municipalities closer to the kinked grant eligibility threshold.[27]

6 Discussion

In my empirical investigation, I found quite different grant effects in Case I and Case II and these differences seem to remain even when I restrict observations in Case II to municipalities that are relatively close to the kink point in Case I. In this subsection I briefly discuss how I can interpret my findings in relation to the institutional settings of Japanese fiscal equalization schemes.

First, the one-to-one effect on total expenditure in Case I clarifies that a one-unit increase in LAT grants at the threshold of grant eligibility directly leads to a one-unit increase in local expenditure.[28] Because pre-equalization revenue capacity exceeds centrally determined expenditure needs at the threshold of grant eligibility (see footnotes 6 and 9), the marginal increase in LAT grants and resulting increase in expenditure at this point can be interpreted as a consequence of the decision making of municipalities.

Second, grant effects on disaggregated expenditures in Case I show that the one-to-one grant effect on total expenditure can be decomposed into grant effects on supplies and services, social assistance, and unsubsidized construction. Such dispersed grant effects are plausible because unconditional lump-sum grants can be spent in any domain of public policy, and it should be difficult for any interested parties inside or outside of municipalities to exclusively gain benefits from tiny LAT grants around the eligibility threshold.

Third, on the other hand, in Case I, no effect is found on personnel expenditure, which is the largest expenditure component. From a theoretical point of view, this suggests that the subgroup of municipalities around the threshold is not a

[27]According to Table 4, RK estimates for subsidized construction are much smaller than FD-IV estimates and in most cases statistically insignificant.

[28]Although I cannot exclude the possibility of some sort of crowding-out effect on local tax revenues, this effect should not be very large.

budget-maximizing or slack-maximizing type, where bureaucrats try to maximize their own benefits with additional revenues.[29] Another possible interpretation is that bureaucrats try to maximize their benefits through more flexible and minor expenditure categories such as including outsourcing costs in "supplies and services" rather than in relatively rigid and legally regulated personnel costs.[30] Unfortunately, more detailed expenditure categories that would enable me to investigate this issue are not available.

Fourth, the much greater than one-to-one effect on total expenditure in Case II is surprising at first glance. But given the fact that municipalities with a high capacity-need gap in general rely heavily on a combination of fiscal equalization grants and conditional matching grants, it may be quite plausible that a marginal change in fiscal equalization grants leads to a marginal change in conditional matching grants and therefore a greater than one-to-one marginal change in total expenditure.

Fifth, in Case II, construction expenditures, particularly when subsidized, receive the largest grant effect while some other expenditure items are more modestly affected.[31] These results seem plausible because municipalities with higher expenditure needs, which are more severely affected by the instrument in FD-IV estimation, are in general more dependent on public construction financed by upper-level governments. Because a large part of other major expenditures such as personnel, supplies and services, and social assistance are often mandatory costs, these municipalities might have had no choice but to significantly reduce construction investment in the short run.[32]

Sixth and finally, FD-IV estimation with varying subsamples in Case II shows that FD-IV estimates do not get closer to RK estimates when a subsample is more

[29] See, among others, Niskanen (1971) and Wyckoff (1988b) for the theory of budget-maximizing and slack-maximizing bureaucrats. Wyckoff (1988a) applies the theory of budget-maximizing bureaucrats to the flypaper effect.

[30] Note that almost all municipal official personnel costs are included in "personnel costs".

[31] The grant effect is larger and more robust in subsidized construction than in unsubsidized construction. This may simply reflect the fact that a one-unit change in construction expenditure from fiscal equalization grants results in a more than one-unit change in construction expenditure only in the case of subsidized construction due to the subsidization of matching grants.

[32] In fact, it is often pointed out that the significant reduction in fiscal equalization grants and conditional matching grants in the early 2000s had a huge negative impact on the local construction industry, although there have been few reliable empirical studies addressing this issue.

similar to that in Case I in terms of need-capacity gaps. This implies that a small kinked variation in grants at the grant eligibility threshold and a relatively modest change in grants around medium-level need-capacity gaps still have different impacts on local expenditures.

7 Concluding remarks

The effects of intergovernmental grants heavily depend on the political, economic, and institutional circumstances of local governments. If the primary interest of one's research is to properly estimate and interpret the effects of existing intergovernmental grants for policy evaluation, one must inevitably examine how the grant endogeneity problems can be solved and what an estimated grant effect, which often comes from some local variation of grants, captures.

This paper studied the effects of fiscal equalization grants on local expenditures when grant endogeneity and grant effect heterogeneity were expected to exist due to the institutional settings of the fiscal equalization scheme. My two studies that exploited different formula-based variations in grants suggested that the effect of Japanese fiscal equalization grants on total expenditure was one-to-one in one case and more than one-to-one in the other case. When it comes to grant effects on disaggregated expenditures, the two studies also provided different findings and interpretations. I then argued that their causal interpretations were not the same because both the fiscal conditions of the subgroups of municipalities and exogenous variations in grants exploited for identification in the two cases were quite different.

These results may be understood as consequences of weak external validity in my identification strategies. This weak external validity, however, is not necessarily misleading and is even informative when it comes to policy implications. To begin with, my study clarifies that fiscal equalization grants for relatively wealthy municipalities around the grant eligibility threshold can be regarded as unconditional lump-sum grants, and these municipalities allocate the grants to different policy areas such as social assistance and unsubsidized construction. This implies that a tiny adjustment in the level of fiscal equalization grants for these municipalities will lead to more or less dispersed or balanced adjustments in disaggregated expenditures.

On the other hand, when it comes to fiscal equalization grants for less wealthy municipalities with higher need-capacity gaps, there is a striking estimated grant effect on subsidized construction expenditures. This large effect on subsidized construction seems to be explained by the fact that municipalities with high expenditure needs rely heavily on public construction financed by intergovernmental grants and had no choice but to reduce public construction when faced with abrupt grant reduction. This may suggest that a future change in fiscal equalization grants for these municipalities will again lead to concentrated grant effects on construction expenditures and a significant impact on those who work in construction industry.

My two studies thus imply that a plausible identification strategy and a serious examination of the fiscal equalization scheme in question are both essential if we are to properly interpret and understand the causal mechanisms of grant effects on local public policies. Although my studies are exclusively based on Japanese data and Japanese fiscal equalization grants, this implication is generally applicable to other countries that include expenditure-need elements in their fiscal equalization formulas. How fiscal equalization grants affect local public policies is still largely unknown and worthy of further study.

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Appendices

A Description of fiscal variables

In this appendix[33], I describe fiscal variables that are used in my estimation. Note that in the following descriptions I discuss *total* amounts of fiscal variables in each municipality, but in actual estimation these fiscal variables are weighted by population and per capita values are used.

Expenditure need: $NEED_i$

This index measures the cost of a "standard" level of local public services for a municipality. It is officially referred to as "*Standard Fiscal Need*" (*Kijun Zaisei Juyo Gaku*) and calculated annually by the Ministry of Internal Affairs and Communications. *Standard Fiscal Need* is calculated as follows:

$$NEED_i = \sum_k (Unit\ Cost^k \times Measurment\ Unit_i^k \times Adj.Coeff_i^k),$$

where k expresses kth public service. $Unit\ Cost_k$ is a kind of net standard cost per measurement unit for each service item. $Measurement\ Unit_i^k$ is in most cases the number or size of the beneficiaries of a particular service. $Adj.Coeff_i^k$ is the set of adjustment coefficients that reflect the socio-economic diversity of a local body and modifies the unit cost in order to make it fit the local body's socio-economic circumstances.

Revenue capacity: CAP_i

CAP_i is an index that measures the fiscal revenue capacity of a municipality before fiscal equalization. It is officially referred to as "*Standard Fiscal Revenue*" (*Kijun Zaisei Syunyu Gaku*) and calculated annually by the Ministry of Internal Affairs and Communications. CAP is calculated as follows:

$$CAP_i = Standard\ Tax\ Revenues_i \times \frac{3}{4} + Local\ Transfer\ Tax, etc_i,$$

where *Standard Tax Revenues* are estimated based on standard tax rates, standard tax collection rates, and estimated tax bases calculated using relevant statistics or

[33] This appendix is partly based on the online Appendix of Ando (2013).

past tax revenues. *Local Transfer Tax, etc.* represents the sum of revenues from *Local Transfer Tax* and *Special Grant for Traffic Safety Measures*. In brief, CAP_i captures the potential amount of local general revenues before fiscal equalization, and cannot be manipulated by municipalities in the short run.

There are two main reasons that *Standard Tax Revenue* is multiplied by 3/4. First, the remaining 1/4 of *Standard Tax Revenue* is excluded from the fiscal equalization process and left for municipalities so that they can cover remaining fiscal needs that are not taken into account by the *Standard Fiscal Needs* calculation. Second, this portion of tax revenue is excluded from the fiscal equalization process so that municipalities have some incentive to increase their local tax revenues by enhancing local economic growth. In other words, if the exact amount of *Standard Tax Revenue* were taken into account in CAP_i, LAT-receiving local bodies would have less incentive to enhance local economic growth because the increase in *Standard Tax Revenue* caused by this economic growth would be completely cancelled out by the decrease in the LAT grant.

Pre-equalization revenue capacity: $PreCAP_i$

As is explained above, CAP_i itself does not represent true pre-equalization revenue capacity as it takes into account some policy objectives of the fiscal equalization scheme. We can, however, easily recover true pre-equalization revenue capacity by simply replacing 3/4 for 1 in the above definition of CAP_i.

Because the available statistics are only CAP_i and *Local Transfer Tax*, I have to assume that revenue from *Special Grant for Traffic Safety Measures* is negligible to recover $PreCAP_i$ from CAP_i. This assumption should not be a major problem because the amount of *Special Grant for Traffic Safety Measures* is in general much smaller than the sum of *Standard Tax Revenues* and *Local Transfer Tax*.

I therefore estimate $PreCAP$ as follows:

$$\begin{aligned}
& PreCAP_i \\
= \; & Standard\ Tax\ Revenues_i + Local\ Transfer\ Tax, etc._i \\
= \; & (CAP_i - Local\ Transfer\ Tax, etc._i) \times \frac{4}{3} + Local\ Transfer\ Tax, etc._i \\
\simeq \; & (CAP_i - Local\ Transfer\ Tax_i) \times \frac{4}{3} + Local\ Transfer\ Tax
\end{aligned}$$

Disaggregated expenditures

1. Personnel: Expenditures on personnel costs such as pay and retirement allowance for employees and pay for assembly members

2. Supplies and services: Expenditures on general consumption by local bodies, excluding other consumption expenses such as personnel costs

3. Maintenance and repairs: Expenditures on maintenance and repairs for public facilities

4. Social assistance: Expenditures on social assistance benefits under various laws and orders, including benefits independently provided by local bodies

5. Subsidies: Expenditures on payments for other authorities, regional associations, local public enterprises, etc.

6. Subsidized and unsubsidized Construction: Expenditures on the construction of social infrastructure such as roads, urban planning, schools, and welfare facilities

7. Debts service: Expenditures on debts service such as repayment of interest and principal on a debt

8. Addition to reserve funds: Expenditures on addition to reserve funds, which are allowed to be established for particular purposes

9. Transfers to other accounts: Transfers to other accounts such as special accounts for local public enterprises, National Health Insurance, Health and Medical Care for the Elderly, Long-Term Care Insurance, etc.

Equalization bonds: BEFM and BTCC

Bonds for Extraordinary Financial Measures (BEFM) and Bonds for Tax Cut Compensation (BTCC) are special local bonds set by the central government for complementary fiscal equalization and revenue guarantee. BEFM are alternative fiscal revenues that compensate for the reduction of LAT grants. BTCC are alternative fiscal revenues that compensate for the reduction of local taxes determined by the central government.

The central government compensates the redemption costs of these equalization bonds by adding annual redemption costs to future $NEED_{it}$ in coming years. The reason that these equalization bonds are perfectly compensated in the future is that both BTCC and BTCC are issued so that local governments can cover the reduction of LAT grants and local tax revenues caused by central government policies. Equalization bonds can be regarded as fiscal equalization grants whose fiscal resource is not current tax revenues but future tax revenues.

Note that even if a municipality does not fully issue the entitled amount of equalization bonds it can still receive full compensation in the future as if it had issued the full amount of equalization bonds. This is because the central government calculates future redemption costs based on the entitled amount of equalization bonds, not the actual amount of bonds issued. Due to this measure, municipalities with the same amount of entitled equalization bonds can receive the same amount of compensation in the future regardless of whether or not they decide to actually issue the bonds.

B OLS and FD estimation

Table B.1: OLS and FD estimates for expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome variable	Total Expenditure	Personnel	Supplies and services	Maintenance and repairment	Social assistance	Subsidies	Subsidezed Construction	Unsibsidized Construction	Debt service	Addtion to reserve funds	Transfers to other accounts
Pooled OLS											
OLS estimates	2.202***	0.321***	0.249***	0.024***	-0.007***	0.141***	0.475***	0.295***	0.448***	0.084***	0.099***
	(0.101)	(0.013)	(0.014)	(0.003)	(0.001)	(0.011)	(0.084)	(0.021)	(0.021)	(0.013)	(0.007)
Observations	4,534	4,534	4,534	4,534	4,534	4,534	4,534	4,534	4,534	4,534	4,534
Adjusted R^2	0.845	0.82	0.68	0.444	0.443	0.611	0.449	0.423	0.87	0.175	0.544
First-differenced OLS											
FD estimates	2.877***	0.026	0.136***	0.019	0.004	0.117*	0.592**	0.592**	0.179***	0.372***	0.029
	(0.625)	(0.024)	(0.044)	(0.013)	(0.007)	(0.063)	(0.267)	(0.267)	(0.051)	(0.132)	(0.073)
Observations	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267	2,267
Adjusted R^2	0.218	0.030	0.093	0.066	0.078	0.037	0.057	0.057	0.058	0.047	0.027

Notes: Robust standard errors are in parenthesis. ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. OLS and FD estimates are the estimated coefficients of the grant variable $G_{it} + B_{it}$. In pooled OLS estimation I introduce covariates that are identical to those used in Table 3. In FD estimation, the set of covariates is identical to Covar.3 in Table 5.

C An instrumental variable in Case II

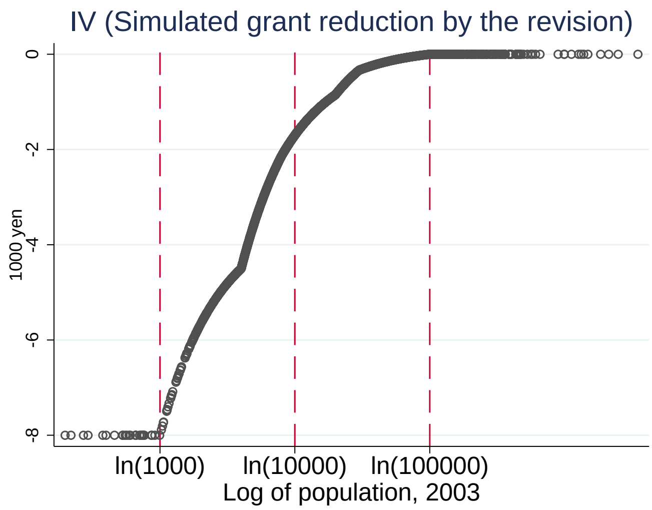
In Case II, my instrument $Z_{i,2004}$ is constructed based on the simulated amounts of grant reduction caused by a formula revision during 2002-2004. These simulated amounts of grant reduction are provided by the Ministry of Internal Affairs and Communications and are presented in Table C.1. Based on these simulated data points, I further interpolate the data between simulated data points using interpolation with hyperbolas, a method that is actually adopted in the calculation of LAT grants. The resulting data is presented in Figure C.1.

Table C.1: Simulated grant reduction by formula revision

Population	Simulated grant reduction	Simulated grant reduction per capita
1,000	-8,000,000	-8,000.000
4,000	-18,000,000	-4,500.000
8,000	-17,000,000	-2,125.000
12,000	-17,000,000	-1,416.667
20,000	-17,000,000	-850.000
30,000	-10,000,000	-333.333
100,000	0	0.000

Note: Simulated amounts of grant reduction are provided by the Ministry of Internal Affairs and Communications (MIC).

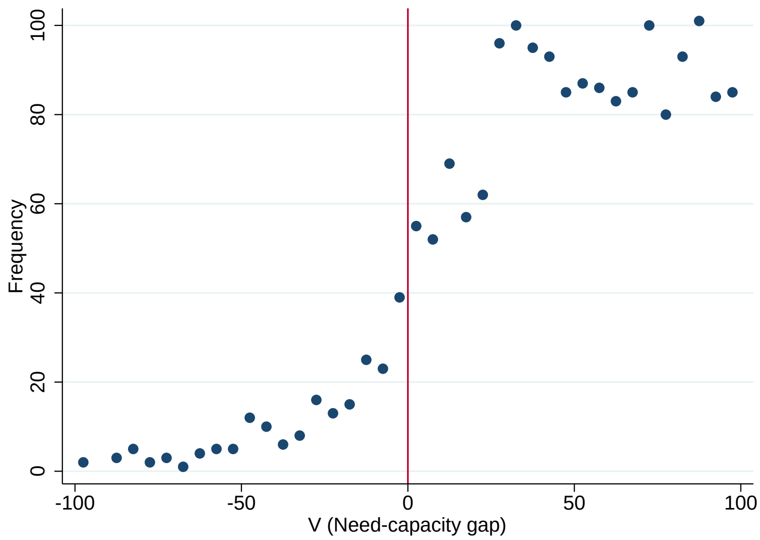
Figure C.1: Simulated grant reduction by formula revision



Note: Based on simulated data points in Table C.1, I further interpolate the data between simulated data points using interpolation with hyperbolas, a method that is actually adopted in the calculation of LAT grants.

D Density test for Case I

Figure D.1: Bin-mean plots of the number of observations



Note: Bin size is 5 and bandwidth is $|V| < 100$. Source: Reports on the Municipal Public Finance

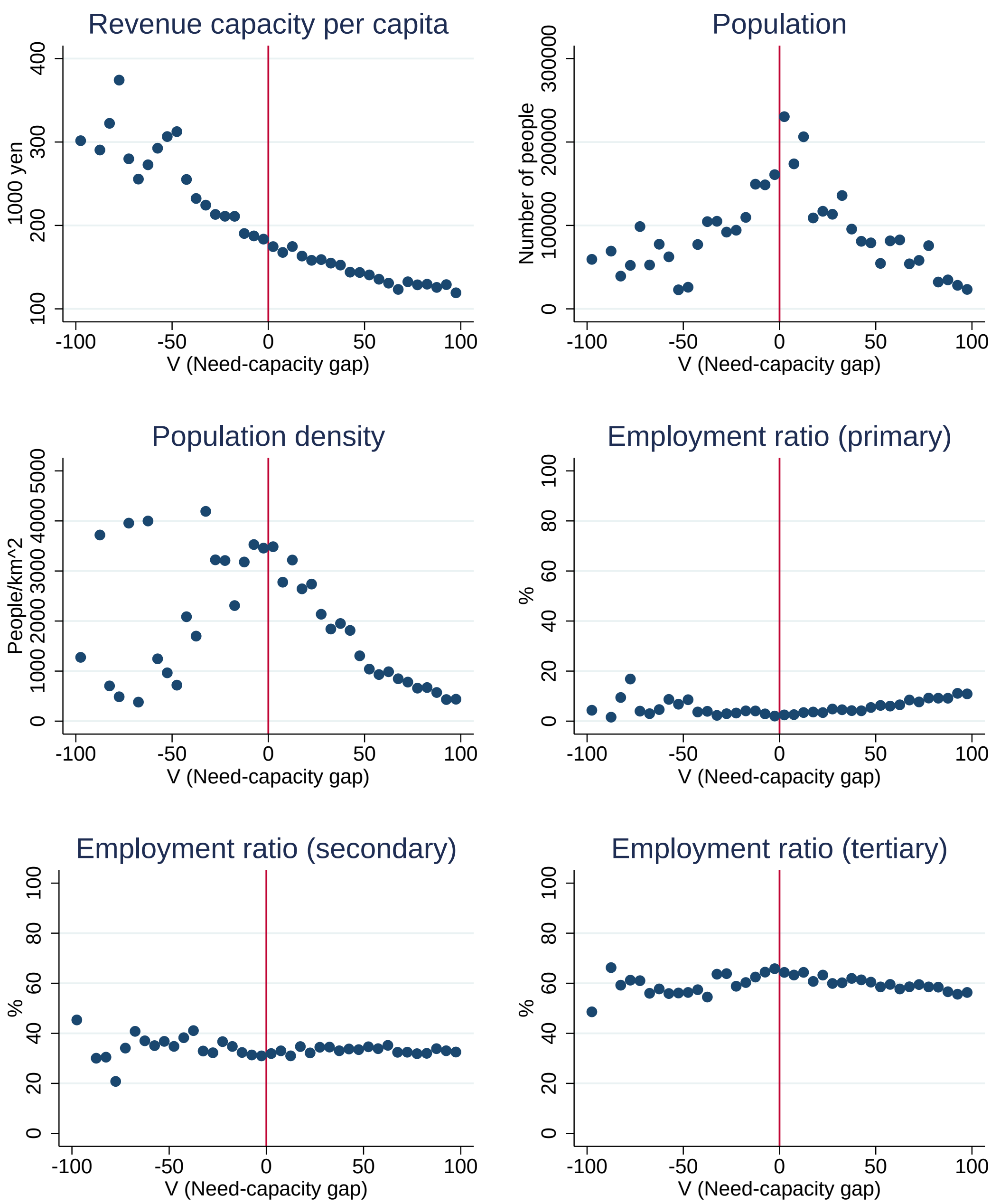
Table D.1: RK estimates for the number of observations (bin size=5, $|V| < 100$)

Polynomial order	One	Two	Three	Four
RK estimates	0.005	0.471	0.834	0.015
	(0.165)	(0.316)	(0.876)	(1.883)
Observations	39	39	39	39
Adjusted R^2	0.856	0.955	0.963	0.961
AIC	323.1	279.7	273.7	276.8

Note: Robust standard errors are in parenthesis. ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$

E Bin-mean plots of covariates for Case I

Figure E.1: Bin-mean plots of covariates



Note: Bin size is 5 and bandwidth is $|V| < 100$. Source: Reports on the Municipal Public Finance

F Further results in Case I

Table F.1: RK estimates for total expenditure with various specifications

Bandwidth	Pooled data, 2003-2004					Cross-sectional data, 2004				
	(1): No covar.		(2): Covar.+B		Sample size	(3): (1)+lagged Y		(4): (2)+lagged Y		Sample size
	Estimate	Robust S.E.	Estimate	Robust S.E.		Estimate	Robust S.E.	Estimate	Robust S.E.	
Linear polynomial										
No	5.539***	(0.578)	1.247**	(0.543)	4,534	1.082***	(0.299)	1.129***	(0.246)	2,267
|V|<100	3.505***	(0.409)	0.986***	(0.211)	1,845	0.583***	(0.136)	0.795***	(0.154)	939
|V|<90	3.536***	(0.443)	1.097***	(0.252)	1,674	0.679***	(0.151)	0.897***	(0.184)	852
|V|<80	3.662***	(0.590)	1.149***	(0.283)	1,472	0.807***	(0.199)	0.933***	(0.209)	752
|V|<70	3.194***	(0.587)	1.092***	(0.285)	1,287	0.737***	(0.231)	0.869***	(0.228)	656
|V|<60	3.692***	(0.698)	1.214***	(0.314)	1,114	0.736***	(0.269)	0.809***	(0.226)	572
|V|<50	3.361***	(0.594)	1.125***	(0.319)	931	0.567***	(0.194)	0.732***	(0.204)	485
|V|<40	2.368***	(0.642)	0.926***	(0.319)	731	0.817***	(0.266)	0.977***	(0.291)	387
|V|<30	1.923**	(0.918)	0.404	(0.393)	522	0.719**	(0.299)	0.875***	(0.322)	275
|V|<20	1.908	(1.668)	0.420	(0.726)	335	0.819	(0.617)	0.765	(0.636)	180
Quadratic polynomial										
No	6.274***	(0.478)	1.587***	(0.329)	4,534	1.432***	(0.324)	1.449***	(0.267)	2,267
|V|<100	3.183***	(0.997)	1.405***	(0.485)	1,845	1.243***	(0.441)	1.351***	(0.421)	939
|V|<90	2.566**	(1.088)	1.186**	(0.511)	1,674	1.047**	(0.452)	1.034***	(0.399)	852
|V|<80	1.227	(1.619)	1.076*	(0.605)	1,472	0.664	(0.467)	0.901**	(0.445)	752
|V|<70	2.553*	(1.460)	1.745***	(0.642)	1,287	0.022	(0.693)	0.780*	(0.471)	656
|V|<60	-0.013	(2.134)	1.259*	(0.762)	1,114	-0.180	(1.047)	0.718	(0.628)	572
|V|<50	-0.630	(2.017)	1.004	(0.917)	931	0.996	(0.690)	1.548**	(0.670)	485
|V|<40	1.134	(2.370)	0.451	(1.019)	731	0.994	(0.979)	1.432	(0.916)	387
|V|<30	1.124	(3.203)	-0.355	(1.574)	522	1.323	(1.324)	1.771	(1.196)	275
|V|<20	-2.314	(6.198)	-0.848	(3.356)	335	3.964	(2.843)	4.030	(2.890)	180

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. "Covar." in column (2) indicates all covariates used in the RK estimation in Table 3.

G Additional analysis of local revenues

RK estimation in Case I and FD-IV estimation in Case II show that the grant effect on total expenditure is one-to-one or greater, implying that conditional matching grants or local bonds may be induced by a marginal increase in fiscal equalization grants. In this subsection, I briefly investigate whether matching grants and local bonds are actually affected by fiscal equalization grants.

In addition, I also examine grant effects on local revenues because there may be some room for municipalities to discretionarily adjust the levels of local tax revenues. First, the urban planning tax is arguably the most discretionary local tax that municipalities can levy on the land and buildings within urban planning areas. Second, Doi (2000) argues that municipalities can adjust the effective tax rates of local property taxes. Third, although only a few municipalities reduce local income tax rates, some local governments impose extra local taxes. Fourth, tax collection rates may also be affected by fiscal equalization grants, although Ishida (2012) does not find such an effect. In any case, I do not expect strong grant effects on local revenues because the Japanese local tax system is relatively homogeneous in so far as the tax rates and bases of major local tax revenues such as local personal and corporate income taxes and local property taxes are mostly uniform across municipalities.

Due to a lack of detailed local revenue statistics, I cannot investigate the details of possible grant effects on local tax rates and bases. I can, however, estimate grant effects on disaggregated local revenues such as total local tax revenues, user charges and fees, miscellaneous revenues, conditional matching grants from upper-level governments, and local bonds. Although these investigations may be more demanding than the estimation of grant effects on the expenditure side due to increased noise, in this section I will examine how fiscal-equalization grants affect these local revenues.

One advantage when it comes to the analysis of local revenue in my case is that I can control for the pre-equalization revenue capacity that the central government measures annually. By controlling for this variable, I can investigate the partial variation of local revenues conditioning on the exogenous revenue capacity of municipalities, not the total variation of local revenue. This may improve the precision of my estimation. See Table G.1 for descriptive statistics of revenue variables.

Table G.2 provides RK estimates for local revenues with the same baseline spec-

Table G.1: Discriptive statistics of revenue variables

Variable	Cross-sectional data (2003, 2004)			First-differenced data (2004-2003)		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.
Revenues (1000 yen)						
Conditional matching grants	4534	85.95	141.32	2267	-8.49	68.91
Local bonds (except for equalization bonds)	4534	36.28	57.65	2267	-3.57	47.28
Local tax revenues	4534	107.73	60.88	2267	0.52	8.35
Local transfer tax revenues	4534	8.88	6.58	2267	2.13	0.94
Charges and fees	4534	14.27	16.98	2267	-0.07	3.18
Miscellaneous revenues	4534	18.95	30.87	2267	0.76	15.78

Note: All variables are per capita values. Source: Reports on the Municipal Public Finance

ifications used as in Table 4. Results with a linear polynomial suggest that conditional matching grants and local bonds (except for equalization bonds) may be increased by fiscal-equalization grants, whereas local tax revenues may be reduced around the threshold. However, if I believe the sizes of these estimates, the total increase in matching grants and local bonds clearly outweighs the decrease in local tax revenues by 0.3-0.5. This implies that the grant effect on total expenditure should be around 1.3-1.5 given the fact that total revenue and total expenditure are balanced. This is not perfectly consistent with the estimated grant effect on total expenditure, which is around 0.9-1.2 when the counterpart bandwidths are used (Table 3). In addition, RK estimates with a quadratic polynomial are more unstable and mostly insignificant. Thus these RK estimates may suffer from high imprecision and I do not reach a clear conclusion about how grants affect local revenues around the threshold.

Second, Table G.3 presents FD-IV estimates with the same baseline specifications used in Table 6. When it comes to estimates for conditional matching grants and local bonds, the former are around 0.84 and significantly different from zero and the latter are around 0.31 but statistically insignificant. It is rather puzzling that estimates for local tax revenues are positive and significantly different from zero and estimates for local transfer tax revenues are negative and also statistically significant, but the magnitudes are small and close to zero. In sum, given the fact that the estimated grant effects on total expenditure with FD-IV estimation in Section 5.2 are around 1.6-2.0 (Table 5), it is plausible that in Case II a marginal change in fiscal equalization grants induces a marginal change in conditional matching grants and results in a very large change in total expenditure.

Table G.2: RK estimates for revenues in Case I

Polynomial order and bandwidth	Sample size	(1) Conditional matching grants	(2) Local bonds	(3) Local tax revenues	(4) Local transfer tax revenues	(5) Charges and Fees	(6) Miscellaneous revenues
Linear polynomial with full covariates							
|V|<60	1,114	0.326***	0.052	-0.053	-0.000	-0.010	-0.039
		(0.109)	(0.063)	(0.077)	(0.006)	(0.016)	(0.064)
|V|<50	931	0.454***	0.138*	-0.047	0.007	0.009	-0.052
		(0.140)	(0.074)	(0.090)	(0.007)	(0.018)	(0.074)
|V|<40	731	0.534***	0.201**	-0.227**	-0.007	0.015	0.023
		(0.137)	(0.090)	(0.103)	(0.008)	(0.019)	(0.083)
|V|<30	522	0.415**	0.236**	-0.293**	-0.014	0.043	0.007
		(0.185)	(0.119)	(0.136)	(0.011)	(0.026)	(0.100)
Quadratic polynomial with full covariates							
|V|<100	1,845	0.352	-0.062	-0.017	-0.041**	0.023	-0.068
		(0.221)	(0.131)	(0.117)	(0.017)	(0.028)	(0.103)
|V|<90	1,674	0.216	-0.057	-0.026	-0.042**	0.002	-0.068
		(0.255)	(0.139)	(0.131)	(0.019)	(0.029)	(0.108)
|V|<80	1,472	0.430	0.104	-0.173	0.005	-0.037	-0.081
		(0.372)	(0.187)	(0.147)	(0.014)	(0.033)	(0.138)
|V|<70	1,287	0.992***	0.249	-0.269	0.005	-0.003	0.085
		(0.277)	(0.176)	(0.201)	(0.014)	(0.038)	(0.165)

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Robust standard errors are in parentheses. All covariates in Table 2 and their quadratic terms are included among the regressors. Equalization bonds B_{it} are excluded from local bonds in Column (2).

In sum, both RK and FD-IV estimation imply that fiscal-equalization grants induce conditional matching grants and local bonds, or at least some of them. Partial crowding-out effects on local tax revenues are also weakly indicated in RK estimations. Other disaggregated revenues seem not to be significantly affected by fiscal equalization grants according to both RK and FD-IV estimation.

Table G.3: FD-IV estimates for revenues in Case II

Polynomial order	(1) Conditional matching grants	(2) Local bonds	(3) Local tax revenues	(4) Local transfer tax revenues	(5) Charges and Fees	(7) Miscella-neous revenues
Linear	0.842**	0.313	0.025*	-0.018***	0.017	0.030
	(0.386)	(0.215)	(0.015)	(0.002)	(0.013)	(0.061)
Quadratic	0.842**	0.315	0.027*	-0.018***	0.017	0.033
	(0.394)	(0.217)	(0.014)	(0.002)	(0.013)	(0.062)
Cubic	0.841**	0.313	0.028**	-0.018***	0.017	0.035
	(0.394)	(0.217)	(0.014)	(0.002)	(0.013)	(0.062)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	2,267	2,267	2,267	2,267	2,267	2,267

Notes: ***: $P < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Robust standard errors are in parentheses. "Covar.3" in Table 5 are included among the regressors. Equalization bonds B_{it} are excluded from local bonds in Column (2).