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How Much Should We Trust Regression-Kink-Design Estimates?

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Abstract

In a Regression Kink (RK) design with a finite sample, a confounding smooth nonlinear relationship between an assignment variable and an outcome variable around a threshold can be spuriously picked up as a kink and result in a biased estimate. In order to investigate how well RK designs handle such confounding nonlinearity, I firstly implement Monte Carlo simulations and then study the effect of fiscal equalization grants on local expenditure using a RK design. Results suggest that RK estimation with a confounding nonlinearity often suffers from bias or imprecision and estimates are credible only when relevant covariates are controlled for.

JEL classification: C13, C21, H71, H72, H77

Keywords: Regression Kink Design, Endogenous regressors, Intergovernmental grants, Flypaper effect

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1. Introduction

To find a plausible exogenous variation which is generated by a quasi-experimental situation has become an essential element of recent econometric policy evaluation, particularly when it is not possible to randomly assign the treatment of interest.

Regression Kink (RK) designs have recently been added to the methodologies available for the implementation of this trend. The basic idea of a RK design is similar to a Regression Discontinuity (RD) design, which is now one of the most popular approaches in applied microeconometrics. Whereas a RD design utilizes a “discontinuity” or “jump” in treatment status at a threshold of an assignment variable, a RK design exploits a “kink” in treatment status at a threshold. The applicability of RK designs may be potentially broader than that of RD designs in policy evaluation because various public policies avoid abrupt discontinuity at a threshold in the treatment and outcome status of relevant parties and result in a continuous but often kinked transition in status following introduction of the treatment in question.

Two such examples are progressive taxation and welfare transfer programs. Because marginal income tax rates and social welfare benefits are often determined by the income levels of taxpayers and welfare recipients, the budget constraints of individuals are often kinked at eligibility thresholds. The existence of such kinks in budget constraints is well known and is taken into account in empirical studies of labor supply and other relevant topics.[1]

Nonetheless, kinks generated by institutional or policy settings have not been recognized as explicit sources of identification. One notable recent exception is studies of bunching such as Saez (2010), which exploits the bunching behavior of taxpayers at a kink point to estimate labor supply elasticity. While the study of bunching utilizes endogenous sorting at a kink for identification, RK designs exploit the seemingly exogenous variation that is generated by a kink in a manner similar to the way RD designs use a discontinuity as a source of exogenous variation.

The empirical applications of RK designs are still limited but increasing. Before the seminal work of Nielsen et al. (2010) introduced the term "Regression Kink design", a few papers had already exploited a kink as a means of identification (Guryan, 2001 and Dahlberg et al., 2008). After Nielsen et al. (2010) (working paper 2008), Card, Lee, & Pei (2009) and Card, Lee, Pei, and Weber (2012) (hereafter referred to as CLP(2009) and CLPW(2012) respectively) formally discussed nonparametric identification and estimation using an RK design. Other recent applications include Simonsen et al. (2010), Lundqvist et al. (forthcoming), Bravo (2011), Engström (2011), Turner (2012), Landais (2013) and Ek (2013).

Although an RK design seems to be a fruitful identification strategy which can be applied to various institutional settings, there is some concern about the

[1] See, among others, Hausman (1985) and Moffitt (1986, 1990) for econometrics issues concerning nonlinear budget sets. Studies of nonlinear budget sets have mostly appeared in the labor-supply literature, but Moffitt (1984), for example, applies these techniques to the study of the effects of federal grants on state and local expenditures.

applicability of RK designs to real-world finite samples. That is, although an RK design tries to capture a discontinuous change in slope at a kink point, it may be incapable of providing accurate estimation with a finite sample if a kinked distribution in an outcome variable against an assignment variable is significantly confounded by noise around the cutoff point. While a similar problem arises in RD designs, RK designs may have a more severe problem due to the intrinsic subtleness of the estimation of a "kink" when compared with a "jump".

In particular, a confounding smooth nonlinear relationship, e.g. a quadratic or more complicated nonlinearity, between an assignment variable and an outcome variable around a cutoff point could be problematic in a real-world sample, because this smooth nonlinearity could be spuriously captured as a kink. This problem is pointed out and considered by Landais (2013), but has not been directly investigated in the literature.[2]

In this paper, therefore, I study how well the RK design can eliminate confounding smooth nonlinearity around the threshold and capture only the "kink" that is generated by a treatment variable. I first examine this problem with Monte Carlo simulations and then apply an RK design to a real-world

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[2] In order to cancel out or reduce estimation bias coming from a quadratic relation which is spuriously picked up as a kink, Landais (2013) proposes a double-difference RD design which exploits the shifting of kink positions over time and eliminates this spurious kink by taking the difference of two RK estimates, one of which is supposed to pick up both a true kink and a spurious kink and the other is assumed to capture only the spurious kink at the same point of an assignment variable. Although his strategy is attractive, its applicability is restricted to situations where the kink point is shifted over time. In addition, this double-difference RD design requires the additional assumption that the distribution of outcomes against an assignment variable would not have changed before and after the kink shift if there had been no treatment intervention.

situation where I estimate the effect of a fiscal equalization grant on local expenditure with panel data from Japanese municipalities.

In my Monte Carlo simulations, I use two different data generating processes (DGPs). The first DGP is based on a minimum setting that generates both an endogeneity problem and confounding smooth nonlinear relation between an assignment variable and an outcome variable. The second DGP is an extension of the first DGP and can also be interpreted as a stylized version of a Japanese fiscal equalization scheme, which I subsequently investigate. In both the first and second DGPs, I also add fixed effects that generate another source of confounding nonlinearity.

I then examine the plausibility of an RK design using Japanese local public finance panel data. Specifically, I investigate the effect of Japanese fiscal equalization grants on local expenditure by exploiting the kinked formula of the fiscal equalization grant.

In the Monte Carlo simulations, my finding is that the RK estimates can be biased when there is a confounding nonlinearity around the kink point, even if I use a reasonably small bandwidth or a quadratic polynomial in my estimation. This bias could be mitigated or eliminated by using an even smaller bandwidth or a higher order polynomial, but either procedure comes at a cost of reduced precision and power and this cost is often prohibitive. The simulation results also show that the performance of RK estimation is improved by controlling for observed covariates, but that problems could still remain if there are unobserved confounding factors. In addition, simulations with a larger sample provide some evidence that the imprecision and the low power of a RK estimator can be significantly improved by adding more observations.

When it comes to the real-world application of a RK design to the Japanese municipality panel data, my results show that their implications are more or less consistent with those of the Monte Carlo simulations in at least two respects. First, in both the Monte Carlo simulations and the empirical application, RK estimation without covariates can be easily biased, and this problem can be mitigated by adding basic covariates to the regressors. Second, in both cases, a smaller bandwidth and/or a higher order polynomial, even a quadratic polynomial, tends to result in imprecise estimates.

The rest of the paper is organized as follows. In Section 2, I briefly explain the RK design and discuss potential problems in this approach. Section 3 presents Monte Carlo simulations with the simplest DGP and only the minimum necessary variables. In Section 4, I implement Monte Carlo simulations with an extended DGP that can also be interpreted as a stylized fiscal equalization scheme. Section 5 provides an empirical application of the RK design to a real-world situation by studying the effects of fiscal equalization grants on local expenditures in Japanese municipalities. Section 6 concludes.

2. Identification in RK designs

2.1 Estimation in RK designs

Consider the following constant-effect and additive model that is presented by Nielsen et al.(2010):[3]

$$Y = \tau B + g(V) + \varepsilon, \tag{1}$$

where $B = b(V)$ is a deterministic and continuous function of V with a kink at $v = 0$, $g(V)$ is an unrestricted function and ε is an error term. They show that if $g(\cdot)$ and $E(\varepsilon|V = v)$ have derivatives that are continuous in v at v=0, then the RK estimand τ can be expressed as follows:

$$\tau = \frac{\lim\limits_{v_o \to 0^+} \left.\frac{dE[Y|V = v]}{dv}\right|_{v=v_0} - \lim\limits_{v_o \to 0^-} \left.\frac{dE[Y|V = v]}{dv}\right|_{v=v_0}}{\lim\limits_{v_o \to 0^+} \left.\frac{db(v)}{dv}\right|_{v=v_0} - \lim\limits_{v_o \to 0^-} \left.\frac{db(v)}{dv}\right|_{v=v_0}}. \tag{2}$$

Intuitively speaking, the numerator of the RK estimand is the change in the slope of the conditional expectation function $\mathrm{E}(Y|V = v)$ at the kink

[3] See also CLPW (2012) for a theoretical discussion of generalized RK estimation based on a nonseparable model with a heterogeneous treatment effect, $Y = y(B, V, U)$ and the "fuzzy" version of the RK design which allows for unobserved determinants of B and measurement errors in B and V. CLPW(2012) show that an RK estimand can be interpreted as the same "local average response" parameter of Altonji and Matzkin (2005) or equivalently "treatment on the treated" parameter of Florens et al. (2008) if certain conditions are satisfied. Because the objective of this paper is to investigate the performance of the RK design in the presence of a confounding nonlinearity around a cutoff point, I exclusively rely on constant-effect and additive models in the following discussion and Monte Carlo simulations.

point $(v = 0)$ and the denominator is the change in the slope of the deterministic assignment function $b(V)$ at the kink. As CPLW (2012) discuss, one important feature of the RK design is that it allows for other less extreme forms of endogeneity if the density of the assignment variable is smooth and rules out deterministic sorting at the kink point.

For clarification, suppose a linearly incremental treatment variable $B = b(V) = \kappa V$ where $\kappa > 0$ if $v > 0$ and otherwise $B = b(V) = 0$ in equation (1). Given that $g(\cdot)$ is a smooth function that is differentiable in V at $v = 0$, the slope of Y, dY/dV, changes discontinuously at $v = 0$ from $g'(0)$ to $\tau\kappa + g'(0)$. Because the change in the slope of $B = b(V)$ *at* $v = 0$ is κ, the treatment effect τ can be recovered as

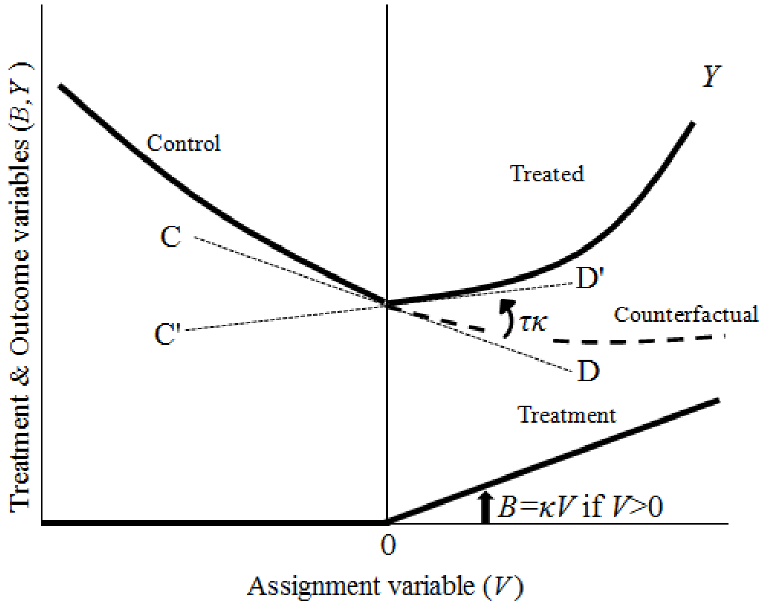
$$\frac{g'(0) + \tau\kappa - g'(0)}{\kappa - 0} = \frac{\tau\kappa}{\kappa} = \tau,$$

using the RK estimand in (2).

The stylized features of the RK design in this setting can also be graphically described as in Figure 1. Here the effect of B on Y at $v = 0$ is depicted as the ratio of the change in tangent from the line CD (at $v \to 0^-$) to the line C'D' (at $v \to 0^+$) to the change in the slope of the treatment variable at $v = 0$.

For estimation with a RK design, CLP(2009) propose local polynomial models which are analogous to the local polynomial models in a RD design (Lee and Lemieux, 2010):

Figure 1. Stylized features of the Regression Kink Design



$$Y = \alpha_0 + \sum_{p=1}^{\bar{p}}[\alpha_p v^p + \beta_p v^p \cdot D] + \varepsilon \; where \; |v| \leq h, \tag{3}$$

where ε is a usual random error term and D is a dummy variable which takes one when the assignment variable V exceeds the threshold $v = 0$ and otherwise takes zero. $\bar{p}$ is the degree of a polynomial and h is the bandwidth that determines the window $[-h; +h]$ within which the sample is selected. Note that equation (3) does not have the term D, implying that this model imposes a kink at the threshold. In this approach the numerator of equation (2) is estimated by the OLS estimator of β_1. Thus the RK estimator $\hat{\tau}$ can be obtained by dividing the OLS estimator $\hat{\beta}_1$ by the slope change of $B = b(V)$ at the threshold.[4]

[4] CLPW(2012) discuss the asymptotic properties of RK estimators with local linear and

2.2 Potential problems of a RK design with a finite sample

Equation (2) holds true only at the cutoff point $v = 0$. In a real-world sample, however, we often need to include observations that are not very close to the threshold and this inclusion of less relevant observations might lead to a biased estimate. For example, suppose $g(\cdot)$ can be expressed as a smooth nonlinear (e.g. quadratic) function as is depicted in Figure 1. In this case it could be difficult to separate out this confounding nonlinearity from a kink generated by the treatment with a finite sample. The resulting estimator $\hat{\beta}_1$ can thus be biased and different from $\tau\kappa$.

This is of particular concern if the first order polynomial ($\bar{p} = 1$) is used in equation (3) for RK estimation. To illustrate this problem, Figure 2 replicates Figure 1 with an arbitrary bandwidth $[-h; +h]$. The kinked line of the treatment variable is dropped for simplicity. In this graph, a kinked line EFG expresses fitted values with the piecewise linear model $Y = \alpha_0 + \alpha_1 V + \beta_1 V \cdot D + \varepsilon$ with bandwidth $[-h; +h]$, which is equivalent to equation (3) with $\bar{p} = 1$. In other words, the line EF is a linear fit for observations with $v \leq 0$ and the line FG is a linear fit for observations with $v > 0$, where the continuity of the two lines (kink) at $v = 0$ is imposed. Here, the estimated coefficient $\hat{\beta}_1$ is the difference between the slopes of the lines FG and EF, which is clearly different from $\tau\kappa$.

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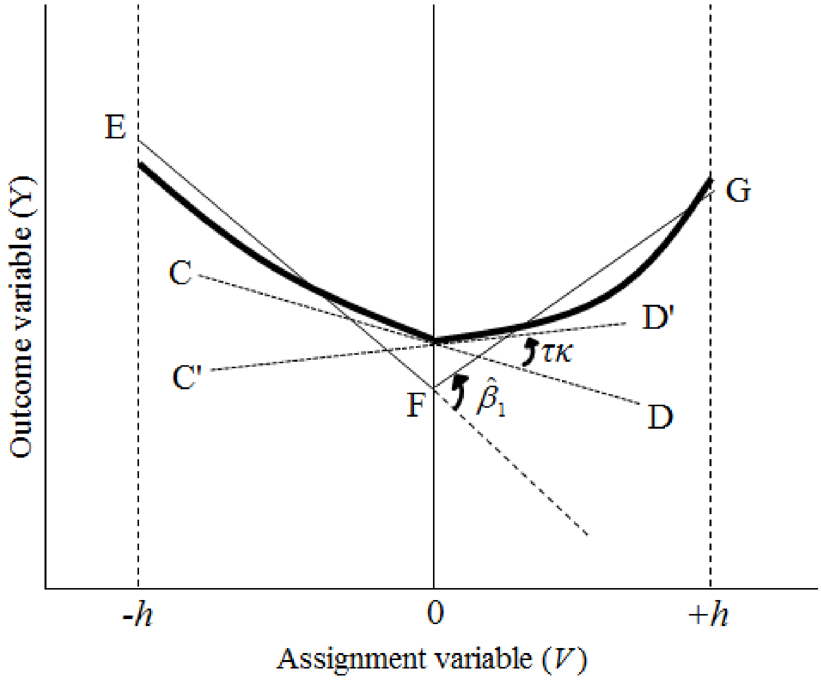
quadratic regressions ($\bar{p} = 1$ *and* $\bar{p} = 2$) without imposing continuity at the threshold. In their empirical application, however, they use local linear and quadratic models that do impose such continuity.

Figure 2. Bias in a RK estimate with a linear polynomial



The reason for the difference between $\hat{\beta}_1$ and $\tau\kappa$ is intuitively quite straightforward. $\hat{\beta}_1$ is different from $\tau\kappa$ because the linear fits EF and FG are not identical with the tangent lines CD and C'D' when $g(\cdot)$ is a nonllinear function around $v = 0$. This gap between $\hat{\beta}_1$ and $\tau\kappa$ can be reduced by making bandwidth $[-h; +h]$ smaller and goes to zero as $h \to 0$. However, with a finite sample, the bandwidth may not be sufficiently narrowed to perfectly remove this discrepancy.

In this case, a RK design with a local linear regression results in a biased estimator of τ. In addition, even if a smaller bandwidth might reduce or even eliminate this bias, it comes at the cost of less precision due to a smaller sample size and less data variation around the cutoff point. An alternative solution is to use a global or local polynomial regression. For example, in Figure 2, quadratic fits in the both sides of the threshold seem to recover the slope change $\tau\kappa$ at $v = 0$. However, this procedure may not always work well since RK estimation with a higher order polynomial incurs the substantial cost of larger variance in the estimator.[5]

In sum, although RK designs are as explicit and straightforward as RD designs, there is some concern about their applicability to real-world finite samples. In the following sections I discuss the potential defects of a RK design with a finite sample using Monte Carlo simulations and real-world empirical data.

3. Monte Carlo simulations: baseline settings

In this section and the next section I implement two types of Monte Carlo simulations in order to examine the performance of RK estimators with a finite sample and in the presence of noise around the cutoff point. In particular, I focus on the cases where there exists a confounding smooth nonlinear relation between the assignment variable V and the outcome variable Y. The first set of Monte Carlo simulations in this section are based on a simple data generating process (DGP) which generates confounding nonlinearity between V and Y through three factors: an observed covariate, an unobserved time-varying covariate, and unobserved time-invariant fixed effects.

[5] CLPW(2012) argue that, unlike in the case of the kind of local polynomial regression for an interior point discussed by Fan & Gijbels (1996), there is a substantial cost in variance incurred by using a local quadratic polymomial instead of a local linear polynomial in RK designs.

3.1 Data Generating Process

Consider equation (1) again. One primary reason a RK design is required is that endogeneity results in a biased estimate if Y is regressed on B with a simple OLS. In this subsection, I construct a simple DGP which has the following three properties in order to investigate the performance of the RK design: 1.an endogeneity problem when simply regressing Y on B, 2.a deterministic assignment rule $B = b(V)$ with a kink at $v = 0$, and 3. confounding nonlinearity between V and Y through an unobserved covariate U. I also generate additional confounding nonlinearity by an observed covariate X and unobserved fixed effects FE to see how adding an observed covariate and introducing a fixed effects model can alleviate bias caused by confounding nonlinearity.

One DGP which satisfies these requirements is described as follows:

$$X_{it} = x_{it} \tag{4}$$

$$U_{it} = u_{it} \tag{5}$$

$$FE_i = \delta_i \tag{6}$$

$$\begin{aligned} V_{it} = \eta_1 X_{it} + \eta_2 X_{it}^2 + \theta_1 U_{it} + \theta_2 U_{it}^2 \\ + \lambda_1 FE_i + \lambda_2 FE_i^2 + \psi_{it} \end{aligned} \tag{7}$$

$$B_{it} = \begin{cases} V_{it} & if \;\; V_{it} > 0 \\ 0 & if \;\; V_{it} \leq 0 \end{cases} \tag{8}$$

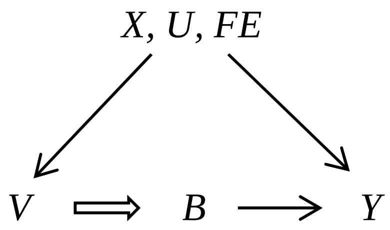
$$\begin{aligned} Y_{it} = \tau B_{it} + \rho_1 X_{it} + \rho_2 X_{it}^2 + \sigma_1 U_{it} + \sigma_2 U_{it}^2 \\ + \phi_1 FE_i + \phi_2 FE_i^2 + \omega_{it}, \end{aligned} \tag{9}$$

where cross-sections and time periods are denoted by $i = 1, \dots N$ and $t = 1, \dots T$ respectively, and u_{it}, x_{it}, δ_i, ψ_{it} and ω_{it} are all NID(0,1). $\eta_m, \theta_m, \lambda_m, \rho_m$, σ_m and ϕ_m $(m = 1,2)$ are the coefficients of the covariates and τ, the coefficient of B, denotes the size of the constant treatment effect.

In this DGP, the three requirements listed above are fulfilled because the effect of B on Y cannot be estimated with a simple pooled regression or fixed effects regression due to an omitted variable bias from time-varying unobserved U (1), B is non-stochastically determined by V with a kink at $v = 0$ (2), and there is a confounding nonlinear relation between V and Y through U (3). An observed covariate X and unobserved fixed effects FE are also introduced, and they too generate nonlinear relations between V and Y.

Figure 3 shows a causal diagram for (4)~(9). The outlined arrow from V to B indicates a deterministic kinked relation based on the assignment rule (8). As is shown later in the Monte Carlo simulations, in this DGP, simple pooled regression with observed covariates or fixed effects regression cannot properly identify τ, because a confounding effect from U cannot be controlled for. The treatment variable B, however, deterministically depends on V with the kinked formula (8), and I can thus exploit a RK design by using V as an assignment variable and $V = 0$ as a threshold.

Figure 3. Causal diagram under the baseline setting



Note: A solid arrow shows causality and an outlined arrow represents a deterministic kinked relation.

Next, I consider the values of the parameters in (7) and (9) and other settings in Monte Carlo simulations. First, I impose a one-to-one constant treatment effect by setting $\tau = 1$. Second, for a baseline analysis, I set $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,1,1,1,1,1)$ in (7) and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) = (1,0,1,0,1,0)$ in (9) so that X, U and FE have nonlinear quadratic effects on V, but linear one-to-one effects on Y. In this DGP there is a nonlinear smooth correlation between V and Y through X, U and FE.[6] Third, the sample size is set to 10,000 ($N = 500$ and $T = 20$), which is close to empirical application I discuss later.

In simulation analysis based on this DGP, τ in (9) is estimated with the RK design using (3). Because (8) shows that the slope of $B = b(V)$ changes from 0 to 1 at the threshold, the denominator of the RK estimand (2) is 1. The

[6] Note that this confounding nonlinear relation between V and Y in this parameter setting is different from a straightforward quadratic U-shape relation as is depicted in Figure 1.; in this parameter setting, a unit increase in X, U or FE leads to a direct linear increase in Y and quadratic increase in V, not vice versa, and therefore the resulting distribution of g(V) in equation (1) can be more complicated. One important thing to note, however, is that there is no kinked relation between V and Y other than through B, implying that the condition that $g(\cdot)$ has a derivative that is continuous at the cutoff is valid.

point estimate of β_1 in (3) can therefore be interpreted as a RK estimate itself. Equation (3) is estimated with several polynomial orders ($\bar{p}$) and bandwidth choices (h). I test all of the combinations of the polynomial degrees $\bar{p} = 1,2,3, \text{and } 4$ and bandwidth sizes $h = \infty, 1, \text{and } 0.5$. I present the rejection rates of the null hypotheses of $\beta_1 = 1$ (size of test) and $\beta_1 = 0$ (power of test), as well as the means and standard deviations of $\hat{\beta}_1$. Test levels are set at a conventional 5 percent and I use heteroskedasticity-robust standard errors for pooled regressions and clustered robust standard errors (clustering on individuals) for fixed effects regressions. Finally, the number of simulations conducted is 1,000.

In the following subsection I present and discuss the simulation results of RK estimation with the first and second order polynomials. The results with the third and fourth order polynomials are given in online Appendix A. In Appendix A I also provide simulation results with OLS estimation in order to compare them to results with RK estimation. In addition, since the sample size and parameter settings stated above are rather arbitrary, in this Appendix A I also provide simulation results with a larger sample size and different parameter setting.[7]

Before presenting the results of the Monte Carlo simulations, Figure 4

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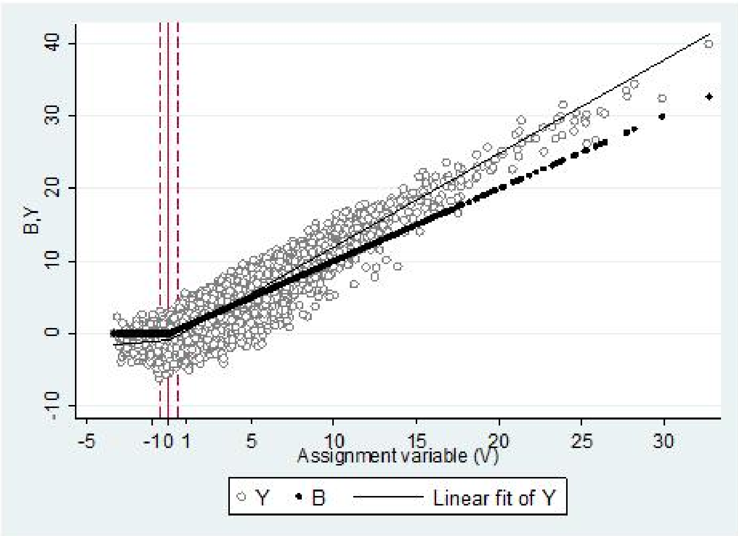
[7] In online Appendix A, I firstly present the results of Monte Carlo simulations with the same parameter settings as above but with a ten-times larger sample size, 100,000 ($N = 5000$ and $T = 20$). Second, I change parameters to $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,0,1,0,1,0)$ in (9) and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) = (1,1,1,1,1,1)$ in (11), which indicate a linear one-to-one effect of U on V and a nonlinear quadratic effect of U on Y. This DGP generates a simpler U-shape relationship between V and Y through X, and FE. The number of simulations is 1,000.

provides an intuitive graphical representation of the estimation bias that could be generated by the RK design with a linear polynomial. First, in Figure 4, the scatter plots of Y and B against V are generated based on the above DGP. Second, the kinked linear fits of Y against V are estimated using RK estimation with (5). The estimation bias in $\hat{\beta}$ is indicated by a difference between the slope change in the linear fits and the slope change in B at the threshold $V=0$. With these particular parameter settings, it is not graphically clear how the slope change in the linear fits is different from the slope change in B.

Figure 4. One example of scatter plots and linear fits with the baseline settings



Note: Vertical dashed lines indicate ± 0.5, which represents the smallest bandwidth used in the simulations.

3.2 Results

The results of the Monte Carlo simulations are shown in Table 1. The first three rows present the results of simulations using the RK estimation with no covariates. Row 1 shows that, with both linear and quadratic polynomials, the means of estimates are larger than 1 and test sizes are far larger than 5 percent if no bandwidth is selected. But the biases are reduced and the test size is closer to 5 percent when the bandwidth is $|V| < 1$ or $|V| < 0.5$ (Row 2, 3). However, RK estimation with a smaller bandwidth and/or second order polynomial leads to higher standard deviations and lower power.

Rows 4 to 6 provide the results of simulations using a RK model that incorporates the direct effect of a covariate X by adding X and X^2 to the control variables. They indicate that the performance of the RK design does not improve much, although the standard deviations are slightly reduced. Rows 7 to 9 show the results of estimation with a fixed effects model, and once again no significant change is observed. Finally, Rows 10 to 12 present simulation results with full covariates where U, which is otherwise assumed to be unobservable, is also added to the regressors. Obviously these results show the best performance and the means of estimates are always close to 1, although a smaller bandwidth and a quadratic polynomial tend to result in higher standard deviation and low power.

Table 1. Monte Carlo simulations with the baseline settings

	Band-width	Obs. (Mean)	(I) Linear polynomial			(II) Quadratic polynomial		
			Mean (S.D.)	Size $H_0:\beta=1$	Power $H_0:\beta=0$	Mean (S.D.)	Size $H_0:\beta=1$	Power $H_0:\beta=0$
			No covaraites					
(1)	No	10,000	1.156 (0.053)	90.4	100	1.302 (0.125)	74.8	100
(2)	$\|V\|<1$	2,458	1.085 (0.208)	6.8	100	1.022 (0.782)	3.2	24.0
(3)	$\|V\|<0.5$	1,273	1.060 (0.551)	4.3	46.5	0.981 (2.244)	4.6	7.8
			With covaraites *X*, *X*^{2}					
(4)	No	10,000	1.191 (0.050)	98.2	100	1.285 (0.117)	75.5	100
(5)	$\|V\|<1$	2,458	1.065 (0.188)	5.9	100	1.012 (0.712)	3.1	26.8
(6)	$\|V\|<0.5$	1,273	1.035 (0.496)	3.5	52.0	0.989 (2.090)	5.7	7.3
			With *X*, *X*^{2}, *fixed effects*					
(7)	No	10,000	1.212 (0.037)	100.0	100	1.255 (0.101)	74.2	100
(8)	$\|V\|<1$	2,458	1.042 (0.190)	6.2	100	1.030 (0.697)	3.8	28.2
(9)	$\|V\|<0.5$	1,273	1.045 (0.552)	4.6	47.0	0.986 (2.229)	6.0	7.9
			With *X*, *X*^{2}, *U*, *U*^{2}, *fixed effects*					
(10)	No	10,000	1.000 (0.033)	2.8	100	1.001 (0.084)	5.7	100
(11)	$\|V\|<1$	2,458	1.002 (0.164)	5.2	100	1.010 (0.599)	5.6	35.9
(12)	$\|V\|<0.5$	1,273	1.006 (0.478)	4.6	57.1	1.021 (1.885)	5.7	9.1

Notes: The number of simulations is 1,000. Standard deviations (S.D.) of estimates are in parenthesis. The null hypothesis is tested with a 5 % significance level. Heteroskedasticity-robust standard errors are used for pooled regressions and clustered robust standard errors (clustering on individuals) are used for fixed effects regressions. Fixed effects estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. Average number of cross-sections is: N=421.6 when $|V| < 1$ and N=385.5 when $|V| < 0.5$.

In sum, the estimation results imply that, in this particular DGP, the combination of a smaller bandwidth and a first order polynomial somewhat reduces the RK bias caused by smooth nonlinearity, but does not eliminate it completely. The combination of a smaller bandwidth and a second order polynomial seems to reduce the bias further, but at the cost of imprecision. Introduction of the observed covariate improves the performance of RK regression by reducing the bias and increasing the precision of an estimate, but the magnitude of this improvement is rather modest. Additional control for fixed effects does not change results in a significant way.

Finally, online Appendix A provides additional simulation results. Tables A1 and A2 present the results of OLS estimates and RKD estimates with higher order polynomials. According to Table A1, OLS estimates are precise but biased, and the test size is always around 1 unless the "unobservable" U is controlled for. Table A2 shows that the RK estimates with third and fourth order polynomials also suffer from biased estimates when global regression (without bandwidth) is used. On the other hand, when local regression (with bandwidth) is adopted, simulation results present high standard deviation and extremely low power. These results suggest that RK estimation with a linear or quadratic polynomial works better than OLS estimation and RK estimation with a higher order polynomial. Table A3 presents the results with a ten-times larger sample and, as expected, shows reduction in standard deviation and increase in test power. Figure A shows one example of scatter plots with a different parameter setting and Table A4 presents Monte Carlo simulation results with this setting. Although quadratic RK estimation seems to work better in this case than in the above baseline DGP, the main implications mentioned above remain unchanged.

4. Monte Carlo simulations: stylized fiscal equalization

In this section I implement Monte Carlo simulations in which the DGP has been modified from that used in the simulations presented in the previous section and is similar to the real-world situation I investigate in the next section. Because the situation I examine is the effects of Japanese fiscal equalization grants on local expenditure, I set up a DGP based on a stylized fiscal equalization scheme.

4.1 Data Generating Process

Although fiscal equalization schemes differ considerably across countries, it can be argued there are in general two important components in fiscal equalization transfers from an upper-level government to a lower-level government. The first is the equalization of fiscal revenue capacity (revenue equalization) and the second is the equalization of expenditure needs (needs equalization).[8] According to Dafflon (2007), the fiscal equalization approach that takes into account both revenue equalization and needs equalization is referred to as "need-capacity gap" equalization. Because Japan has a unified fiscal equalization scheme which takes into account both revenue and needs equalization, the Japanese fiscal equalization scheme can be categorized as a need-capacity gap equalization scheme.

In this section, I construct a stylized need-capacity gap equalization

[8] See Boadway and Shah ed. (2007) for extensive studies on fiscal equalization programs.

scheme which can also be interpreted as a simplified version of the Japanese fiscal equalization scheme. Under this stylized scheme, fiscal equalization general grants are distributed to individual local bodies based on the following kinked assignment rule:

$$GRANT_{it} = \begin{cases} V_{it} & if\ V_{it} > 0 \\ 0 & if\ V_{it} \leq 0 \end{cases} \tag{10}$$

where $GRANT_{it}$ is the amount of the fiscal equalization grant for municipality i at t. V_{it} is the "need-capacity gap" of the municipality and defined as follows:

$$V_{it} = NEED_{it} - CAP_{it} \tag{11}$$

where $NEED_{it}$ is the expenditure need, which indicates the total cost of the standard levels of local public services for municipality *i* at period *t*. CAP_{it} is the revenue capacity of municipality *i* at period *t* which represents the amount of its own tax revenues that municipality i can collect under the standard local tax system.

In short, under this fiscal equalization scheme, grants ensure that all municipalities can provide a standard level of local public services by filling the need-capacity gap in cases where $NEED$ outweighs CAP. On the other hand, if the need-capacity gap is negative, a municipality is considered rich enough to cover their expenditure needs on their own and no grant is provided.

It is not difficult to convert this setting into a DGP for Monte Carlo simulations that extends the first DGP given above. First, $NEED$ is defined as follows:

$$\begin{aligned} NEED_{it} = NEED^0 + \eta_1 X_{it} + \eta_2 X_{it}^2 \\ + \theta_1 U_{it} + \theta_2 U_{it}^2 + \lambda_1 FE_i + \lambda_2 FE_i^2 + \psi_{it} \end{aligned} \quad (12)$$

where $NEED^0$ indicates a constant basic expenditure need, X, U, and FE are defined as before and ψ_{it} is a random component with NID(0,1). Second, CAP_{it} is determined as:

$$CAP_{it} = CAP^0 + \zeta_{it} \quad (13)$$

where CAP^0 is a constant basic revenue capacity and ζ_{it} is a random term with NID(0,1). For simplicity, I assume that CAP is not affected by X and U and does not contain fixed effects. Finally, the outcome variable, expenditure (denoted as EXP), is set to be affected by CAP as well as the same left-hand side variables in (9). One straightforward interpretation of the effect of CAP on EXP is that expenditure should be primarily determined by tax revenue capacity.

Thus the modified DGP is described as follows:

$$X_{it} = x_{it} \tag{14}$$

$$U_{it} = u_{it} \tag{15}$$

$$FE_i = \delta_i \tag{16}$$

$$V_{it} = NEED_{it} - CAP_{it} \tag{17}$$

$$GRANT_{it} = \begin{cases} V_{it} & if \ \ V_{it} > 0 \\ 0 & if \ \ V_{it} \leq 0 \end{cases} \tag{18}$$

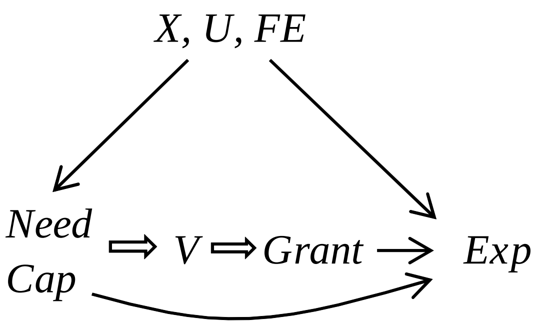
$$\begin{aligned} EXP_{it} = {} & \tau GRANT_{it} + \pi_1 CAP_{it} + \pi_2 CAP_{it}^2 \\ & + \rho_1 X_{it} + \rho_2 X_{it}^2 + \sigma_1 U_{it} + \sigma_2 U_{it}^2 \\ & + \phi_1 FE_i + \phi_2 FE_i^2 + \omega_{it}, \end{aligned} \tag{19}$$

where cross-sections and time periods are defined as before and x_{it}, u_{it} and ω_{it} are all NID(0,1). τ is the homogeneous treatment effect of $GRANT$ on EXP and other parameters in (19) are the coefficients of covariates. There are essentially only two differences between the first DGP in the last section and this second DGP. First, the assignment variable V_{it} is now the gap between $NEED_{it}$ and CAP_{it}. Second, the outcome variable EXP_{it}, local expenditure, is directly affected by CAP_{it}, not only by the treatment variable, X_{it}, U_{it}, and FE_i. Figure 5 shows a causal diagram for equation (14)~(19), where the definitions of the two types of arrows follows Figure 3.

This DGP is identical to the first DGP if CAP is always zero, but in general CAP adds another source of nonlinear relation between V and EXP if $\pi_2 \neq 0$. In my baseline analysis, I set $(\pi_1, \pi_2) = (1,1)$ and keep other parameters unchanged from the first DGP, namely $\tau = 1$, $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,1,1,1,1,1)$ and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) =$

(1,0,1,0,1,0). $Need^0$ and Cap^0 are set at 5. Although this DGP looks more complicated than the first one, the key identification strategy is the same; because $Grant$ is an endogenous treatment variable and a deterministic kinked function of V, I can exploit an RK design to identify the effect of $Grant$ on EXP at $v = 0$.

Figure 5. Causal diagram of the stylized fiscal equalization scheme



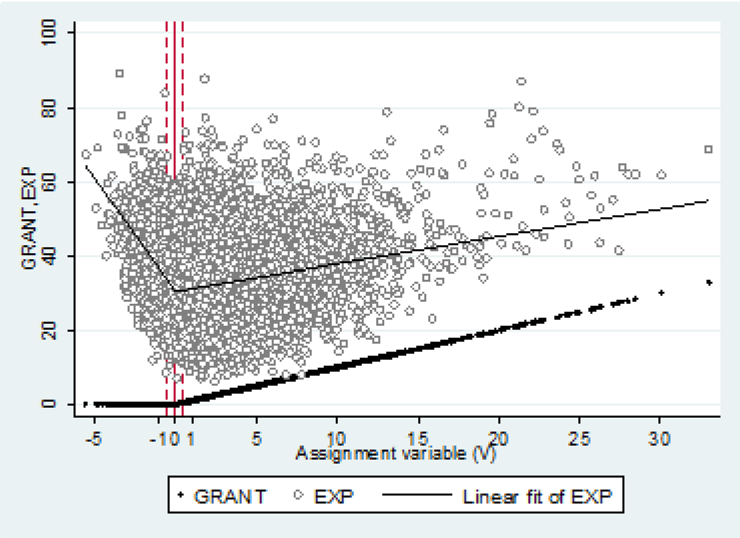
Note: A solid arrow shows causality and an outlined arrow represents a deterministic relation.

The other simulation settings are also the same as in the first DGP. I test all combinations of the polynomial degrees $\bar{p} = 1,2,3 \text{ and } 4$ and bandwidth sizes $h = \infty, 1 \text{ and } 0.5$ in (3). The sample size is 10,000 where the number of cross-sections N is 500 and the time period T is 20. The number of simulations is 1,000. I present the means and the standard deviations of $\hat{\beta}_1$ and the rejection rates of the null hypotheses of $\beta_1 = 1$ (test size) and $\beta_1 = 0$ (test power). Test levels are set at a conventional 5 percent and heteroskedasticity-robust standard errors are used for pooled regressions and clustered robust standard errors (clustering on individuals) are used for fixed effects regressions.

In online Appendix B I provide results corresponding to those given in online Appendix A: simulation results of OLS estimation and RK estimation with the third and fourth order polynomials. Simulation results with a larger sample size and different parameter values are also provided.

Figure 6 presents the scatter plots of EXP and $GRANT$ against V with this DGP. The figure shows that the slope change of linear fits is clearly greater than the slope change of $GRANT$ at the kink point, implying a bias in a RK estimate with a linear polynomial.

Figure 6. One example of scatter plots and linear fits with the stylized fiscal equalization



Note: Vertical dashed lines indicate ± 0.5, which represents the smallest bandwidth in simulations.

4.2 Results

The results of the second set of Monte Carlo simulations are presented in Table 2. The "no covariates" case in Rows 1~3 shows that RK estimates with a linear polynomial are severely biased even in the smallest bandwidth $|V| < 0.5$. RK estimates with a quadratic polynomial also present biased estimates in the no-bandwidth case and very imprecise estimates with low power in the cases of bandwidth $|V| < 1$ and $|V| < 0.5$ (Rows 1~3).

Once the covariates of CAP and CAP^2 are introduced, however, estimation bias and standard deviations are significantly reduced. Particularly in local regressions with $|V| < 1$ and $|V| < 0.5$, the size of test is always around 5 percent, although the standard deviations are high and the power of test is still very low in a quadratic polynomial (Rows 4~6).

The introduction of additional covariates of X and X^2 seems to improve RK estimations by subtly reducing bias and standard deviations, but there is no significant improvement in size and power (Rows 7~9). There is also no considerable improvement in RK estimations when a fixed effects model which also includes full observed covariates is used (Rows 10~12). I also implement simulations with a fixed effects model that includes both observed and unobserved covariates to see how the RK design works when all relevant covariates can be included in the regressors. The results show an overall improvement, with RK estimation without a bandwidth in particular working very well while local RK regressions with a quadratic polynomial still result in high standard deviation and low power (Row 13~15).

Table 2. Monte Carlo simulations with the stylized fiscal equalization scheme

	Band-width	Obs. (Mean)	(I) First order polynomial Mean (S.D.)	Size $H_0{:}\beta=1$	Power $H_0{:}\beta=0$	(II) Second order polynomial Mean (S.D.)	Size $H_0{:}\beta=1$	Power $H_0{:}\beta=0$
					No covaraites			
(1)	No	10,000	6.758	100	100	5.647	100	100
			(0.255)			(0.653)		
(2)	$\|V\|<1$	2,244	2.022	10.0	24.8	1.062	4.8	5.3
			(1.507)			(6.051)		
(3)	$\|V\|<0.5$	1,145	1.399	4.7	6.0	0.242	4.1	4.2
			(4.209)			(16.34)		
					With *CAP*, *CAP*^{2}			
(4)	No	10,000	1.072	54.7	100	1.174	52.6	100
			(0.040)			(0.092)		
(5)	$\|V\|<1$	2,244	1.022	5.1	99.7	0.953	4.7	19.8
			(0.229)			(0.877)		
(6)	$\|V\|<0.5$	1,145	0.973	5.2	35.0	0.913	5.3	7.6
			(0.615)			(2.468)		
					With *CAP*, *CAP*^{2}, *X*, *X*^{2}			
(7)	No	10,000	1.100	82.9	100	1.156	49.8	100
			(0.039)			(0.087)		
(8)	$\|V\|<1$	2,244	1.016	5.9	100	0.956	5.8	22.6
			(0.211)			(0.804)		
(9)	$\|V\|<0.5$	1,145	0.974	5.7	40.1	0.906	6.0	8.5
			(0.567)			(2.268)		
					With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *fixed effects*			
(10)	No	10,000	1.118	97.5	100	1.135	46.3	100
			(0.030)			(0.074)		
(11)	$\|V\|<1$	2,244	1.012	6.1	99.9	0.959	5.2	22.3
			(0.198)			(0.778)		
(12)	$\|V\|<0.5$	1,145	0.962	4.5	33.9	0.967	5.4	8.4
			(0.612)			(2.505)		
					With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *U*, *U*^{2}, *fixed effects*			
(13)	No	10,000	1.000	5.3	100	1.000	5.5	100
			(0.026)			(0.062)		
(14)	$\|V\|<1$	2,244	0.996	4.7	100	0.963	3.5	31.5
			(0.162)			(0.639)		
(15)	$\|V\|<0.5$	1,145	0.964	4.9	46.8	0.951	5.6	7.7
			(0.509)			(2.083)		

Notes: The number of simulations is 1,000. Standard deviations (S.D.) of estimates are in parenthesis. The null hypothesis is tested with a 5 % significance level. Heteroskedasticity-robust standard errors are used for pooled regressions and clustered robust standard errors (clustering on individuals) are used for fixed effects regressions. Fixed-effects estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=438.6 when $|V| < 1$ and N=397.5 when $|V| < 0.5$.

Finally, online Appendix B provides additional simulation results. Their implications are similar to those of Appendix A. Table B1 presents precisely biased OLS estimates and Table B2 provides biased or imprecise RK estimates. Table B3 implies that a larger sample results in more precise estimates with larger power. Table B4, which provides simulation results with a different parameter setting, shows that quadratic RK estimation seems to work better under these conditions, but the main implications of the results remain the same.

Overall, it is hard to fully demonstrate the validity of RK estimation at least in this specific DGP when we do not include any relevant covariates in the regressors. The simulation results suggest that RK estimation without covariates could generate biased estimates, particularly when a linear regression is used, because its functional form does not take into account a confounding nonlinear relation between an assignment variable and an outcome variable. This bias could be mitigated or even removed by using a quadratic regression and/or a smaller bandwidth, but the resulting highly imprecise and often statistically insignificant estimates make it difficult to obtain a robust conclusion about the causal effect of interest. The inclusion of covariates and the introduction of a fixed effects model may significantly improve RK estimations (i.e. CAP in this DGP), but their contributions may also be modest (i.e. X and fixed effects in this DGP).

4.3 Implications for empirical RK design

My main findings from the first and second Monte Carlo experiments can be summarized as follows. Although RK estimation can mitigate a classic endogeneity bias, it may generate other problems such as biases caused by confounding nonlinearity, imprecise estimation, and low power. While the introduction of observed covariates and a fixed effects model may improve the performance of the RK design to some extent, it may not be sufficient to resolve these issues. In addition, fixed effects regressions in a real-world sample may introduce another problem in the form of insufficient sampling variability when within-group variation is small.

Despite these problems that may undermine the internal validity of estimation as well as inherently limited external validity as a local "treatment on the treated" parameter, it can be argued that the RK design still has some advantages over standard OLS regression as a method of causal inference. First, the RK design can potentially avoid the endogeneity bias that stems from many unobserved or unrecognized correlations by exploiting an explicit source of identification and somewhat testable identifying assumptions. Second, the availability of robustness checks using different model specifications and bandwidth choices is also an attractive feature. In the following section, I examine the usefulness of the RK design by using a sample of Japanese municipality panel data and exploiting a kinked formula in the Japanese fiscal equalization scheme.

5. An empirical application

In this section, I apply the RK design to a real-world situation. I exploit the kinked formula of Japanese fiscal equalization grants to identify the effect of these grants on local expenditures. The first subsection briefly explains the related theoretical and empirical literature in economics. In the second subsection I provide a description of the institutional setting I exploit and discuss why this is suitable for investigating the plausibility of the RK design. The third and fourth subsection present my data and results respectively.

5.1 Background

The effects of intergovernmental grants on local public expenditures have been extensively studied in the context of the "flypaper effect". Following seminal theoretical studies such as Scott (1952) and Wilde (1968, 1971), Bradford and Oates (1971a, b) show that general lump-sum grants and local private income have an exactly equivalent effect on local spending and local taxation under the assumption of representative voters. This is called the "Bradford-Oates equivalence theorem". However, many empirical studies have demonstrated that local spending is much more stimulated by grants than by private income. This has been called the "flypaper effect", and various theoretical and empirical studies have been published which try to fill the gap between the equivalence theorem and the flypaper effect.[9]

Recent empirical studies along these lines have increasingly taken

[9] Relatively recent reviews of the flypaper effect are available in Hines and Thaler (1995), Bailey and Connolly (1998), Oates (1999), Gamkhar & Shah (2007) and Inman (2008).

accounting for the endogeneity of grants seriously. Many previous empirical studies implicitly or explicitly assume that the variation of grants is generated exogenously and a model specification error and an omitted variable bias do not seriously harm their estimation of the flypaper effect. However, this assumption does not hold true if the institutional and political circumstances of grants are seriously taken into consideration. For example, Dahlberg et al. (2008) list four possible sources of endogeneity in intergovernmental grants: (1) political negotiations between central and local politicians, (2) central politicians' preferences for specific economic and political characteristics of local governments associated with their spending priorities, (3) socio-economic characteristics of municipalities simultaneously influencing spending, taxation and grant allocation, and (4) unobserved characteristics correlated with both local spending and grant allocation.[10]

As an empirical application of a RK design, I study the effects of Japanese fiscal equalization grants on the total expenditures of local municipalities by exploiting the kinked assignment rule of these grants. The RK approach seems

[10] Besley and Case (2000) discusses endogenous policy decisions. When it comes to the endogeneity of grants, an earlier study, Holtz-Eakin (1986), carefully discusses this problem and uses "a large variety of instrumental variables estimators" to check the validity of its estimation. An increasing number of studies use exogenous variations caused by grant-related institutional settings or policy reforms to identify the impacts of endogenous grants on local expenditure, revenue, employment, and other socio-economic outcomes. The most popular approach is the instrumental variables approach, the approach employed in Knight (2002), Gordon (2004), Conley and Dupor (2011), Feyrer and Sacerdote (2011), Suárez Serrato and Wingender (2011), Chodorow-Reich et al. (2012) and Wilson (2012). As has already been mentioned, Dahlberg et al. (2008), Lundqvist et al.(forthcoming) and Bravo (2011) use the RK design. Lundqvist (forthcoming) adopts Difference-in-Difference estimation as a primary identification strategy as well as the instrucmental variables approach. Litschig and Morrison (2013) exploit a Regression Discontinuity design.

particularly advantageous in this case because it may be able to disentangle the complicated endogenous causality inherent in the "fiscal equalization" procedure, something that cannot be easily dealt with by other approaches like fixed effects regression or the instrumental variables approach.[11]

5.2 Institutional setting

As is mentioned in the last section, the Japanese fiscal equalization scheme distributes general grants to local governments (prefectures and municipalities) in order to compensate for the "need-capacity gap" of each local government and ensure a certain standard of local services for all citizens. This fiscal equalization grant is called a *Local Allocation Tax* (LAT) grant.[12] The detailed allocation mechanism of LAT grants is fairly complicated, but the basic framework can be explained as follows.[13] First, the national-level total amount of LAT grants is determined based on the amount of central tax revenues and political and bureaucratic processes in the central government.[14]

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[11] Two papers closely related to this empirical analysis are Dahlberg et al. (2008) and Lundqvist et al. (forthcoming). Although Dahlberg et al. (2008) do not use the term "regression kink design", they exploit a kinked relation between one part of general grants and out-migration rates in Sweden in order to identify the effects of grants on local taxes and spending using an instrumental variables approach. Lundqvist et al. (forthcoming) also utilize the same kinked relation as Dahlberg et al. (2008) and adopt a "fuzzy" version of the RK design in order to estimate the effects of grants on local public sector employment.

[12] The confusing name "Local Allocation Tax" comes from the fact that the total amount of the grants is, in principle, a given proportion of revenues from five national taxes. See also note 14.

[13] Strictly speaking what I refer to as LAT grants in this paper are the "ordinary" LAT grants which consist of 94% of the total LAT grants. The other 6% of the LAT grants are called "special" LAT grants and are distributed for specific purposes with more ad hoc rules.

[14] According to the legal framework in which they are described, the total amount of the LAT grants is stipulated to be a fraction of the revenues from five major national taxes. In

Second, the LAT grant is distributed to individual local bodies based on the following kinked assignment rule:

$$GRANT_{it} = \begin{cases} V_{it} & if\ V_{it} > 0 \\ 0 & if\ V_{it} \leq 0 \end{cases} \tag{20}$$

where $GRANT_{it}$ is the amount of the LAT grant for municipality *i* in year *t*. V_{it} is the "revenue-capacity gap" of the municipality and defined as follows:

$$V_{it} = NEED_{it} - CAP_{it} \tag{21}$$

where $NEED$ indicates the expenditure needed to cover the total cost of the standard levels of local public services. This necessary expenditure is officially referred to as "*Standard Fiscal Need*" and is calculated annually by the central government. CAP is the revenue capacity index which reflects the potential revenues that each municipality can collect on its own under a standard local tax system. CAP is officially referred to as *Standard Fiscal Revenue* and is also calculated annually by the central government.[15]

In brief, LAT grants ensure that each municipality can provide a level of local public services that meets a certain minimum standard, the expenditures required for which being measured as $NEED_{it}$ for each i and each t, by

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reality, however, the total amount is also influenced by other socio-economic and political factors.

[15] See online Appendix E and F for more detailed definitions and institutional descriptions of $NEED$ (or *Standard Fiscal Need*) and CAP (or *Standard Fiscal Revenue*).

filling the positive "revenue-capacity gap" between $NEED_{it}$ and CAP_{it}.[16] On the other hand, if the "need-capacity gap" is negative, a municipality is considered rich enough to cover their expenditure needs on their own and no grant is provided.

The assumed DGP under this scheme is similar to the DGP in the second set of Monte Carlo simulations described above. One difference is that observed and unobserved variables (X, U and FE in Figure 5) should affect both $NEED$ and CAP, whereas the DGP in the second set of Monte Carlo simulations is constructed such that these variables affect only $NEED$. This difference, however, does not affect my empirical strategy of employing a RK design that exploits the deterministic kink in the assignment variable V.

Because the objective of this paper is to investigate the performance of a RK design with a confounding nonlinearity, it is preferable to use a clean dataset that does not contain unnecessary noise and to establish have some expected bounds of the treatment effect. From this standpoint, there are at least four advantages in studying the kinked assignment of Japanese fiscal equalization grants.

First, the problem of potential endogenous sorting can be almost completely ignored. Because my assignment variable is the indicator of a fiscal gap between the expenditure needs and revenue capacity of a municipality which is calculated by the central government, local governments

[16] There may be some doubt about whether the LAT grant can be interpreted as a pure general and lump-sum grant because the amount of the grant is strongly affected by fiscal needs for specific public services. See online Appendix F for an interpretation of the lump-sum characteristic of the LAT grant around the kink point.

cannot, in a precise manner at least, manipulate their positions regarding (or int terms of) this variable. Second, the variation of the grant is expected to be economically meaningful even around the kink point because the size of the equalization grant is relatively large. Third, because it is a well-known fact that Japanese municipalities have relatively homogeneous revenue systems, it is highly expected that an increase in the grant will lead to a statistically significant increase in total expenditure. For example, if municipalities have a totally uniform revenue system and the LAT grant does not draw any additional revenue sources such as matching grants, the effect of the LAT grant on total expenditure should be one-to-one. Although this seems to be an extreme case, a one-to-one effect ratio is at least a good benchmark when examining the effect on total expenditure. In addition, as a robustness check it is possible to estimate the effect of the LAT grant on total revenue from other sources: If the effect on total expenditure is expected to be one-to-one, the effect on total revenue minus the LAT grant should be zero. Fourth, possessing rich 20-year panel data allows for RK estimates with fixed-effect models for robustness checks.

5.3 Data and preliminary investigation

In estimations, I use the panel data for cities (*shi*) covering fiscal years 1980~1999.[17] I exclude from the sample the cities which experienced

[17] Japanese municipalities consist of cities (*shi*), towns (*cho*), villages (*son*) and special districts in Tokyo (*ku*). Because the duties of municipalities differ depending on their type, the levels of expenditure per capita are also affected by the type of municipality in

amalgamation during the sample period because merged municipalities follow a special fiscal equalization scheme, but a large part of the cities remain in the sample. All of the fiscal data are from Reports on the Municipal Public Finance (*Shichoson-betsu Kessan Jokyo Shirabe*), which are published annually by the Ministry of Internal Affairs and Communications (MIC). When it comes to observed pre-determined covariates, I use revenue capacity[18], population, population density, population ratios of the elderly cohort and the young cohort, and the sectoral ratios of employment. All the covariates, except for revenue capacity, are from Census data. Because Census data is only available for every 5th year, I impute annual data by linear interpolation.

Table 3 shows the summary statistics of the variables that I use for the empirical application in this section. All the fiscal variables are expressed as per capita values and deflated by Consumer Price Index (CPI: the reference year is 2005) published by MIC. On average, the size of the LAT grant is about 16% of total expenditure. The sum of revenue capacity and the LAT grant is smaller than total expenditure because there are other important fiscal revenues, including earmarked grants from the central and prefectural governments and prefectures and debt financing.

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question. I therefore use only city (*shi*) data, excluding other types of municipalities. I also drop the 13 largest "designated" cities (*seirei shitei toshi*) from the sample because they have some extra duties compared with normal cities. See onine Appendix G for further details about my data arrangement.

18 The revenue capacity variable is similar to CAP(or *Standard Fiscal Revenue*), but some modifications have been made in order to reflect real revenue capacity of municipalities. See online Appendix E.

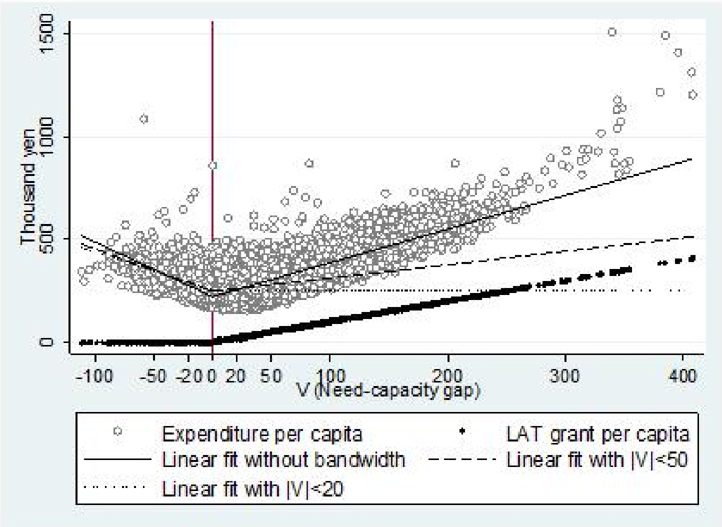
Table 3. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Outcome (Thousand yen)					
Total expenditure	12666	308.98	96.58	155.25	1506.90
Total revenue (excluding the LAT grant)	12623	269.06	71.56	125.33	1167.13
Treatment (Thousand yen)					
LAT grant	12623	48.17	46.84	0.00	408.26
Assingment (Thousand yen)					
Need-capacity gap	12666	45.59	50.29	-111.15	408.50
Covariates					
Revenue capacity (modified, thousand yen)*	12666	117.92	40.05	24.02	320.32
Population	12666	102890	103892	6178	810482
Population density (pop/km^2)	12666	1653.98	2306.80	20.35	14131.37
Population ratio (age 0-15, %)	12666	19.35	3.41	9.11	32.47
Population ratio (age 65-, %)	12666	13.10	4.54	3.67	32.42
Sectoral ratio (primary industry, %) **	12666	8.90	7.96	0.10	46.78
Sectoral ratio (tertiary industry, %) ***	12666	57.06	10.01	26.63	85.05

Notes: All fiscal variables are divided by population, meaning that they are per-capita values. The fiscal variables are also deflated by CPI (the reference year is 2005). There are some missing values for the LAT grant.
Sources: Reports on the Municipal Public Finance, Census, and CPI
*See online Appendix E for a precise definition of revenue capacity as a pre-determined covariate.
**The primary Sector consists of agriculture, forestry, fisheries and mining.
***The tertiary sector includes all the sectors that are not included in the primary sector and secondary sectors (construction and mining).

Before proceeding to econometric analysis, I conduct several preliminary analyses in order to examine the validity of my identification strategy. First, Figure 7 shows the scatter plots of the LAT grant and total expenditure against the need-capacity gap for municipalities (only cities). This graph indicates that the LAT grant has a clear deterministic kink at the threshold and the size of the grant is not negligible for many LAT-receiving municipalities. The linear fits of total expenditure based on RK estimation with a first-order polynomial show that the size of an estimated kink decreases when the bandwidth is shifted from $|V_{i,t}| < 50$ to $|V_{i,t}| < 20$, implying the existence of some confounding nonlinear relation between the assignment variable and expenditure per capita.

Figure 7. Total expenditure and LAT grant against need-capacity gap



Note: All of the variables are per-capita variables. Linear fits of expenditure per capita are obtained by RK estimation with a first-order polynomial based on (3). Sources: Reports on the Municipal Public Finance, Census, and CPI

Second, a key identifying assumption for a valid RK design is that the density of the assignment variable is continuously differentiable at the threshold. Since the LAT grant is calculated by centrally-determined uniform formulas, there is little possibility that municipalities or the central government can precisely manipulate the need-capacity gap around the threshold. It may be suspected, 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 CLP (2009) and CLPW(2012) in the context of an RK design. Both estimation results and graphical analysis indicate that the density of the need-capacity gap is smooth at the threshold (these results are given in online Appendix C.1).

Third, according to CLP (2009) and CLPW (2012), 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 the assignment variable.

However, the argument presented in Section 2 and the Monte Carlo simulations described in Sections 3 and 4 imply that a smooth nonlinear relation between a covariate and an assignment variable around the kink point could be estimated as a kink using RK estimation. It may thus be hard to assert that there are no kinks whatsoever at the threshold for any covariate. The figures in online Appendix C.2, however, at least indicate that no such kinks are visually apparent in the graphical representation of the data.

5.4 Results

Table 4 presents the results of RK estimates generated by applying linear and quadratic polynomials to the empirical model (3).[19] In order to check whether estimates differ significantly from the "benchmark" value of one, p-values with the null hypothesis of $\beta_1 = 1$ are also presented.

[19] OLS estimates and RK estimates with third and fourth order polynomials are presented in online Appendix D.

First, I examine RK estimation with a linear polynomial (Column I). When no covariate is introduced, estimates are almost always larger than one and significantly different from zero, while estimates get smaller when the bandwidth is smaller (Row 1~6). With the smallest bandwidth of $|V| < 10$, the estimate sharply decreases with a very high standard error (Row 6). However, once the covariate of revenue capacity is introduced, estimates tend to be around 1 with most bandwidths (Row 7~24), while they are again sharply reduced and feature high standard errors with $|V| < 10$ (Row 12,18,24). As expected, p-values with the null of $\beta_1 = 1$ are often around zero with no covariates, but they become much larger when the covariates and a fixed-effect model are introduced and the bandwidth is larger than $|V| < 10$.

Second, in RK estimation with a quadratic polynomial (Column II), estimates fluctuate to a much greater degree. When the bandwidth is equal to or larger than $|V| < 40$, however, estimates are mostly around 0.8~1.5 and p-values with the null of $\beta_1 = 1$ are often around 0.5 or higher. Although these are much less conclusive results than those obtained using a linear polynomial, they also indicate the effect of the grant on total expenditure is not too far from one-to-one.

These estimation results are also more or less compatible with the implications of the Monte Carlo simulations. First of all, the introduction of covariates can reduce the biases of RK estimates, particularly RK estimates with a linear polynomial. In my empirical analysis, once the covariates of revenue capacity and its quadratic term are introduced, RK estimates with a linear regression decrease considerably, while the introduction of additional covariates and a fixed-effect model have only a modest effect on these estimates. This implies that the introduction of revenue capacity may eliminate the largest part of the confounding nonlinearity around the threshold. Second, RK estimates with a quadratic regression fluctuate a lot and are statistically insignificant when the bandwidth is smaller than $|V|$=40. This is again compatible with the Monte Carlo results which indicated that the standard deviation of RK estimates is often too high and power too low if a quadratic regression is used with a smaller bandwidth.

In Tables D1 and D2 of online Appendix D, I also present the results of OLS estimates and RK estimates with higher order polynomials. The OLS estimates, which are obtained by simply regressing the total expenditure on the grant, change considerably if I introduce covariates or fixed effects, implying severe endogeneity. It is difficult to draw useful conclusions about the magnitude of the treatment effect on the basis of these OLS estimates alone. The RK estimates with a third and fourth order polynomial are very unstable and mostly statistically insignificant. This could be because RK estimates with higher order polynomials tend to be very imprecise. Table D3 provides RK estimates for total revenue *minus* the LAT grant and shows that no effect is observed once the covariate of revenue capacity is controlled for.

Overall, relatively reliable RK estimation with a lower order polynomial suggests that the effect of the grant on total expenditure is around 1, and, as I have already discussed, this result appears reasonable on the basis of our institutional knowledge of Japanese local public finance. When it comes to the validity of the various RK estimations examined, the empirical results and the Monte Carlo simulations suggest that an RK design employing a local linear or quadratic regression with a modestly small bandwidth and additional covariates provides arguably the most reliable estimate.

Table 4. RK estimates for total expenditure

	Band-width	Obs.	(I) Linear polynomial			(II) Quadratic polynomial		
			Estimate	S.E.	$p(H_0:\beta_1=1)$	Estimate	S.E.	$p(H_0:\beta_1=1)$
			No covaraites					
(1)	No	12,666	3.697***	(0.261)	0.000	2.813***	(0.358)	0.000
(2)	|V|<50	7,750	2.329***	(0.261)	0.000	1.469**	(0.634)	0.460
(3)	|V|<40	6,430	2.254***	(0.311)	0.000	0.919	(0.764)	0.915
(4)	|V|<30	5,013	1.992***	(0.354)	0.005	0.339	(0.954)	0.489
(5)	|V|<20	3,451	1.796***	(0.461)	0.085	-1.236	(1.687)	0.186
(6)	|V|<10	1,741	0.286	(0.997)	0.475	-3.116	(3.818)	0.282
			With covaraites of revenue capacity					
(7)	No	12,666	0.735***	(0.154)	0.087	1.096***	(0.308)	0.756
(8)	|V|<50	7,750	1.081***	(0.193)	0.675	1.371***	(0.430)	0.389
(9)	|V|<40	6,430	1.216***	(0.238)	0.364	0.876*	(0.524)	0.813
(10)	|V|<30	5,013	1.260***	(0.260)	0.317	0.064	(0.675)	0.166
(11)	|V|<20	3,451	1.100***	(0.355)	0.779	-1.069	(1.149)	0.073
(12)	|V|<10	1,741	-0.123	(0.677)	0.099	-2.404	(2.522)	0.179
			With full covaraites					
(13)	No	12,666	0.965***	(0.175)	0.842	1.492***	(0.277)	0.076
(14)	|V|<50	7,750	1.126***	(0.198)	0.526	1.272***	(0.403)	0.501
(15)	|V|<40	6,430	1.174***	(0.242)	0.473	0.980**	(0.479)	0.967
(16)	|V|<30	5,013	1.266***	(0.256)	0.301	0.060	(0.657)	0.153
(17)	|V|<20	3,451	1.054***	(0.337)	0.874	-1.064	(1.068)	0.054
(18)	|V|<10	1,741	-0.071	(0.617)	0.084	-0.724	(2.535)	0.497
			With full covariates and fixed effects					
(19)	No	12,666	0.966***	(0.263)	0.896	1.031***	(0.313)	0.922
(20)	|V|<50	7,750	1.006***	(0.272)	0.982	0.799**	(0.372)	0.589
(21)	|V|<40	6,430	0.972***	(0.305)	0.927	0.360	(0.376)	0.089
(22)	|V|<30	5,013	0.890***	(0.280)	0.695	-0.021	(0.525)	0.053
(23)	|V|<20	3,451	0.896***	(0.272)	0.701	-0.244	(0.731)	0.090
(24)	|V|<10	1,741	0.228	(0.369)	0.038	0.292	(1.383)	0.609

Note: Standard errors are clustered by the municipality level. ***: P<0.01, **:p<0.05, *:p<0.1. Covariates are listed in Table 4 and both linear and quadratic terms of these covariates are introduced into regressors. Fixed-effect estimation with a defined bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth.

6. Conclusion

Regression Kink (RK) designs, which have several attractive features similar to those of Regression Discontinuity (RD) designs, are potentially quite useful, but the weakness of this approach has been largely ignored in the emerging literature. In order to investigate the validity of RK designs, I first examined the finite sample properties of RK estimation in the presence of a confounding smooth nonlinearity around the kink point, using Monte Carlo simulations. Then I applied an RK design to the study of the causal effects of fiscal equalization grants on the spending of local governments.

The results of the Monte Carlo Simulations suggested that RK estimation often resulted in biased estimates when there was a confounding nonlinear relation between an assignment variable and an outcome variable. Introduction of a higher order polynomial or a smaller bandwidth could mitigate this bias, but this procedure often resulted in an imprecise estimate and low power that might significantly undermine the results obtained. The simulations also provided evidence that the introduction of observed covariates and a fixed effects model can improve RK estimation, but the improvement may be insufficient if a strong confounding nonlinear relation is generated by unobserved time-varying covariates.

When it comes to the real-world application, I first found seemingly very large biases in the estimated effect of the equalization grant on local expenditure when I did not include any additional covariate in my RK estimation. After introducing the covariate of revenue capacity to regressors, however, relatively robust estimates of one-to-one effect were obtained when a linear polynomial was used for RK estimation. RK estimates with a quadratic polynomial were less robust, but they also imply that the effect of the grant on local expenditure is not far from one.

In conclusion, my Monte Carlo simulations and real-world applications provide mixed answers concerning the usefulness of an RK design with a finite sample. On the one hand, it can be argued that RK analysis is more reliable than a simple pooled or fixed-effect OLS regression when it comes to causal interpretation because an RK design has an explicit identification strategy and transparent tools for validity and robustness checks. On the other hand, if unobserved covariates generate a confounding nonlinearity around the kink point and the sample size around this point is relatively small, a biased or imprecise estimate may be obtained. In empirical studies employing an RK design, this possible weakness should be explicitly addressed by careful robustness checks. The construction of formal criteria for a sufficiently credible RK design is an important area for further study.

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Online Appendix

Appendix A. Additional simulation results with the baseline setting

Table A1. Simulation results for OLS estimates

Obs.	Mean	S.D.	Size H_0:β=1	Power H_0:β=0
No covaraites				
10,000	1.313	0.011	100	100
With covaraites V, V^2				
10,000	1.275	0.060	99.5	100
With covaraites V, V^2, X, X^2				
10,000	1.251	0.057	99.2	100
With covaraites V, V^2, X, X^2, *fixed effects*				
10,000	1.214	0.044	99.6	100
With covaraites V, V^2, X, X^2, U, U^2, *fixed effects*				
10,000	1.001	0.037	3.8	100

Note: Simulation settings are the same as the baseline setting in Chapter 3.

Table A2. Simulation results for RK estimates with third & forth order polynomials

Band-width	Obs. (Mean)	(I) Third order polynomial				(II) Fourth order polynomial			
		Mean	S.D.	Size H_0:β=1	Power H_0:β=0	Mean	S.D.	Size H_0:β=1	Power H_0:β=0
No covaraites									
No	10,000	1.361	0.232	37.1	100	1.311	0.415	12.7	89.4
$\|V\|$<1	2,458	1.015	2.008	4.2	8.1	1.011	4.002	5.0	6.4
$\|V\|$<0.5	1,273	1.115	5.479	4.6	4.5	0.504	11.30	5.0	5.1
With covaraites X, X^2									
No	10,000	1.319	0.214	34.6	100	1.255	0.381	11.9	90.4
$\|V\|$<1	2,458	0.981	1.853	5.8	8.5	0.991	3.762	5.8	6.0
$\|V\|$<0.5	1,273	1.057	5.152	6.0	4.9	0.673	10.60	5.9	6.5
Fixed effects model with covaraites X, X^2									
No	10,000	1.248	0.192	25.8	100	1.195	0.347	9.2	93.6
$\|V\|$<1	2,458	0.984	1.842	4.6	8.8	0.968	3.657	4.8	6.4
$\|V\|$<0.5	1,273	0.967	5.619	4.9	6.2	0.564	10.75	4.7	4.5
Fixed effects model with covaraites X, X^2, U, U^2									
No	10,000	1.000	0.162	5.6	100	0.983	0.290	5.8	92.0
$\|V\|$<1	2,458	0.956	1.568	5.3	10.5	1.026	3.098	6.3	7.0
$\|V\|$<0.5	1,273	0.989	4.743	5.6	6.1	0.812	9.348	4.7	5.4

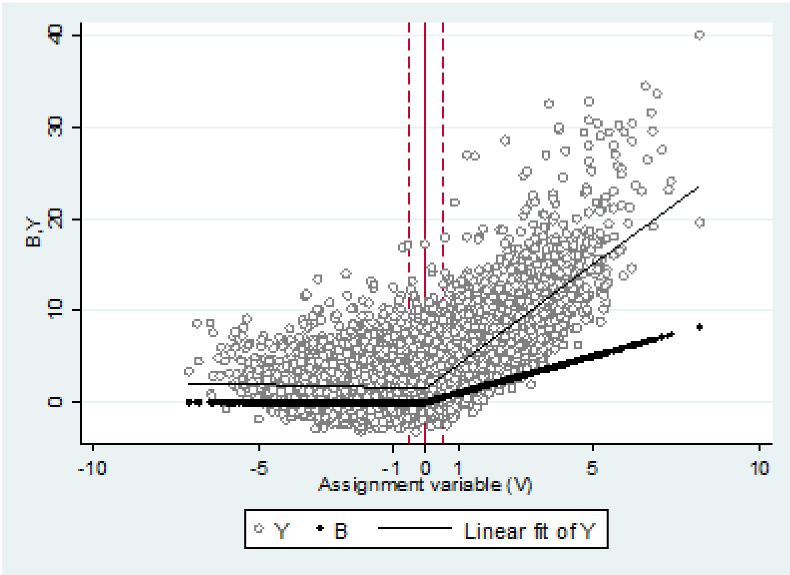
Note: Simulation settings are the same as the baseline setting in Chapter 3. Fixed-effect estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=421.6 when $|V| < 1$ and N=385.5 when $|V| < 0.5$.

Table A3. Simulation results for RL estimates with a larger sample

	Band-width	Obs. (Mean)	(I) Linear polynomial				(II) Quadratic polynomial			
			Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$	Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$
						No covaraites				
(1)	No	100,000	1.154	0.017	100	100	1.297	0.040	100	100
(2)	$\|V\|<1$	24,613	1.087	0.065	27.0	100	1.014	0.254	5.0	97.2
(3)	$\|V\|<0.5$	12,744	1.057	0.176	5.8	100	0.994	0.713	5.1	28.6
						With covaraites X, X^2				
(4)	No	100,000	1.189	0.016	100.0	100	1.280	0.038	100	100
(5)	$\|V\|<1$	24,613	1.069	0.060	21.6	100	1.011	0.233	4.9	98.8
(6)	$\|V\|<0.5$	12,744	1.046	0.159	5.8	100	0.992	0.642	4.9	33.5
						Fixed effects model with covaraites X, X^2				
(7)	No	100,000	1.212	0.012	100	100	1.251	0.032	100	100
(8)	$\|V\|<1$	24,613	1.046	0.059	11.8	100	1.014	0.231	5.3	99.4
(9)	$\|V\|<0.5$	12,744	1.037	0.171	4.8	100	1.010	0.700	5.5	31.4
						Fixed effects model with covaraites X, X^2, U, U^2				
(10)	No	100,000	1.000	0.011	4.8	100	1.000	0.026	4.5	100
(11)	$\|V\|<1$	24,613	1.004	0.050	4.7	100	1.014	0.193	4.7	99.9
(12)	$\|V\|<0.5$	12,744	1.013	0.146	4.8	100	1.017	0.603	5.3	40.7

Note: The sample size is 100,000 ($N = 5000$ and $T = 20$). The other settings are the same as the baseline setting in Chapter 3. Fixed effects estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=4220 when $|V| < 1$ and N=3857 when $|V| < 0.5$.

Figure A. Scatter plots with a different parameter setting



Note: Parameters are set as $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,0,1,0,1,0)$ in (9) and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) = (1,1,1,1,1,1)$ in (11). The other settings are the same as the baseline setting in Chapter 3.

Table A4. Simulation results for RK estimates with a different parameter setting

	Band-width	Obs. (Mean)	(I) Linear polynomial Mean	S.D.	Size $H_0{:}\beta{=}1$	Power $H_0{:}\beta{=}0$	(II) Quadratic polynomial Mean	S.D.	Size $H_0{:}\beta{=}1$	Power $H_0{:}\beta{=}0$
						No covaraites				
(1)	No	10,000	2.646	0.075	100.0	100	1.001	0.142	5.7	100
(2)	$\|V\|<1$	3,827	1.367	0.280	26.8	100	1.005	1.109	5.4	15.0
(3)	$\|V\|<0.5$	1,973	1.181	0.788	7.2	35.2	1.089	2.961	4.0	6.3
						With covaraites *X*, *X*^{2}				
(4)	No	10,000	2.161	0.081	100.0	100	1.005	0.131	6.5	100
(5)	$\|V\|<1$	3,827	1.220	0.241	16.4	100	1.012	0.936	5.2	21.2
(6)	$\|V\|<0.5$	1,973	1.103	0.648	5.8	40.7	1.145	2.541	4.7	8.1
						With covaraites *X*, *X*^{2}, *fixed effects*				
(7)	No	10,000	1.618	0.043	100.0	100	0.996	0.107	5.4	100
(8)	$\|V\|<1$	3,827	1.106	0.202	8.9	100	1.006	0.817	5.6	27.9
(9)	$\|V\|<0.5$	1,973	1.051	0.607	6.0	43.9	1.177	2.279	5.2	7.3
						With covaraites *X*, *X*^{2}, *U*, *U*^{2}, *fixed effects*				
(10)	No	10,000	1.000	0.018	4.5	100	1.000	0.048	4.2	100
(11)	$\|V\|<1$	3,827	0.999	0.122	4.9	100	0.980	0.489	5.6	53.4
(12)	$\|V\|<0.5$	1,973	0.980	0.365	6.3	77.3	1.061	1.433	4.7	11.0

Note: The number of simulations is 1000. As in Figure A1, Parameters are set as $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,0,1,0,1,0)$ in (7) and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) = (1,1,1,1,1,1)$ in (9). The other settings are the same as the baseline setting in Chapter 3. Fixed-effect estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=499.3 when $|V| < 1$ and N=489.8 when $|V| < 0.5$.

Appendix B. Additional simulation results with the stylized fiscal equalization

Table B1. Simulation results for OLS estimates

Obs.	Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$
		No covaraites		
10,000	0.349	0.056	100	100
		With *CAP, CAP^2*		
10,000	5.128	0.331	100	100
		With CAP, *CAP^2, Z, Z^2*		
10,000	1.165	0.049	95.7	100
		With CAP, *CAP^2, Z, Z^2,X, X^2*		
10,000	1.143	0.047	93.0	100
		With CAP, CAP^2, Z, Z^2,X, X^2, fixed effects		
10,000	1.115	0.036	89.0	100
		With *CAP, CAP^2, Z, Z^2, X, X^2, U, U^2, fixed effects*		
10,000	1.001	0.030	4.6	100

Note: Simulation settings are the same as the baseline setting in Chapter 4.

Table B2. Simulation results for RK estimates with a third and fourth order polynomial

	Band-width	Obs. (Mean)	(I) Third order polynomial				(II) Fourth order polinomial			
			Mean	S.D.	Size H_0:β=1	Power H_0:β=0	Mean	S.D.	Size H_0:β=1	Power H_0:β=0
						No covaraites				
(1)	No	10,000	4.434	1.316	79	95	2.819	2.134	14.0	28.3
(2)	$\|V\|$<1	2,244	0.0553	14.84	4.3	4.6	-0.0206	30.06	5.4	5.6
(3)	$\|V\|$<0.5	1,145	1.316	42.46	4.7	4.9	3.901	84.83	5.3	5.8
						With *CAP*, *CAP*^{2}				
(4)	No	10,000	1.184	0.179	22.2	100	1.104	0.297	6.9	96.1
(5)	$\|V\|$<1	2,244	0.897	2.171	4.3	7.7	0.840	4.383	5.4	5.0
(6)	$\|V\|$<0.5	1,145	0.921	5.996	4.0	4.1	0.541	11.80	4.7	5.0
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}				
(7)	No	10,000	1.156	0.164	20.7	100	1.083	0.269	6.2	98.0
(8)	$\|V\|$<1	2,244	0.896	2.012	4.6	8.0	0.832	3.991	5.4	5.0
(9)	$\|V\|$<0.5	1,145	0.867	5.507	4.6	4.9	0.730	10.67	4.5	4.1
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *fixed effects*				
(10)	No	10,000	1.109	0.145	13.9	100	1.056	0.244	4.3	98.8
(11)	$\|V\|$<1	2,244	0.942	2.008	5.2	8.1	0.953	3.975	5.9	5.9
(12)	$\|V\|$<0.5	1,145	0.965	6.053	5.5	6.1	0.787	12.33	5.9	5.5
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *U*, *U*^{2}, *fixed effects*				
(13)	No	10,000	0.999	0.120	5.0	100	0.997	0.204	4.8	100
(14)	$\|V\|$<1	2,244	0.943	1.608	4.4	7.9	0.994	3.352	5.7	7.4
(15)	$\|V\|$<0.5	1,145	0.985	5.065	4.6	6.1	0.589	10.40	5.5	5.5

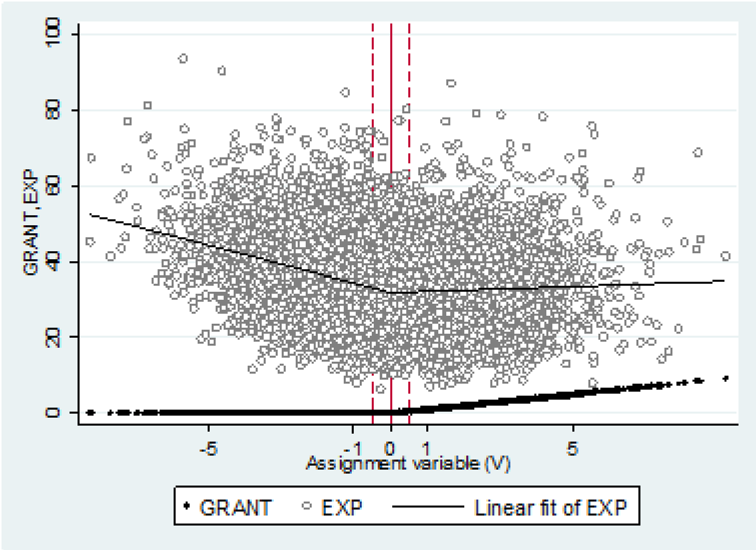
Notes: Simulation settings are the same as the baseline setting in Chapter 4. Fixed-effect estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=438.6 when $|V| < 1$ and N=397.5 when $|V| < 0.5$.

Table B3. Simulation results for RK estimates with a larger sample size

	Band-width	Obs. (Mean)	(I) First order polynomial				(II) Second order polynomial			
			Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$	Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$
						No covaraites				
(1)	No	100,000	6.751	0.079	100	100	5.640	0.219	100	100
(2)	$\|V\|<1$	22,483	2.033	0.484	54.5	98.8	0.915	1.894	5.4	7.4
(3)	$\|V\|<0.5$	11,473	1.433	1.319	5.9	18.8	1.001	5.619	6.2	7.3
						With *CAP*, *CAP*^{2}				
(4)	No	100,000	1.074	0.013	100	100	1.177	0.030	100	100
(5)	$\|V\|<1$	22,483	1.032	0.071	7.6	100	1.015	0.283	5.4	95.4
(6)	$\|V\|<0.5$	11,473	1.017	0.194	4.9	99.9	1.016	0.779	5.6	25.2
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}				
(7)	No	100,000	1.101	0.012	100	100	1.160	0.029	100	100
(8)	$\|V\|<1$	22,483	1.027	0.065	7.5	100	1.014	0.256	5.3	97.6
(9)	$\|V\|<0.5$	11,473	1.018	0.177	5.2	100	1.024	0.708	5.7	30.5
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *fixed effects*				
(10)	No	100,000	1.119	0.009	100	100	1.135	0.023	100	100
(11)	$\|V\|<1$	22,483	1.018	0.066	6.4	100	1.008	0.254	5.0	98.4
(12)	$\|V\|<0.5$	11,473	1.013	0.193	5.2	99.9	0.993	0.757	4.3	24.9
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *U*, *U*^{2}, *fixed effects*				
(13)	No	100,000	1.000	0.008	3.7	100	0.999	0.020	6.7	100
(14)	$\|V\|<1$	22,483	1.000	0.054	4.5	100	1.006	0.213	5.2	99.6
(15)	$\|V\|<0.5$	11,473	1.004	0.164	5.6	100	0.996	0.636	4.7	32.4

Note: The sample size is 100,000 ($N = 5000$ and $T = 20$). The other settings are the same as the baseline setting in Chapter 4. Fixed-effect estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=4391 when $|V| < 1$ and N=3983 when $|V| < 0.5$.

Figure B. Scatter plots with a different parameter setting



Note: Parameters are set as $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,0,1,0,1,0)$ in (14) and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) = (1,1,1,1,1,1)$ in (21). The other settings are the same as the setting in Chapter 4.

Table B4. Simulation results for RK estimates with a different parameter setting

	Band-width	Obs. (Mean)	(I) First order polynomial				(II) Second order polynomial			
			Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$	Mean	S.D.	Size $H_0:\beta=1$	Power $H_0:\beta=0$
						No covaraites				
(1)	No	10,000	2.570	0.180	100	100	0.985	0.479	5.6	58
(2)	$\|V\|<1$	3,451	1.291	1.335	6.9	18.4	0.859	5.125	5.9	5.7
(3)	$\|V\|<0.5$	1,768	1.087	3.575	4.9	5.1	0.657	14.88	5.6	5.4
						With *CAP*, *CAP*^{2}				
(4)	No	10,000	2.220	0.066	100.0	100	1.003	0.128	6.0	100
(5)	$\|V\|<1$	3,451	1.221	0.298	12.2	99.1	0.961	1.153	4.7	14.3
(6)	$\|V\|<0.5$	1,768	1.095	0.811	5.0	26.9	1.029	3.278	5.3	6.0
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}				
(7)	No	10,000	1.842	0.068	100.0	100	1.001	0.116	6.3	100
(8)	$\|V\|<1$	3,451	1.132	0.254	9.1	100	0.967	0.960	4.5	15.4
(9)	$\|V\|<0.5$	1,768	1.059	0.680	4.5	33.2	1.068	2.792	5.6	7.1
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *fixed effects*				
(10)	No	10,000	1.438	0.038	100.0	100	0.998	0.094	6.1	100
(11)	$\|V\|<1$	3,451	1.065	0.214	6.3	99.9	0.966	0.819	4.5	20.4
(12)	$\|V\|<0.5$	1,768	1.029	0.645	4.5	37.6	1.003	2.529	5.4	6.2
						With *CAP*, *CAP*^{2}, *X*, *X*^{2}, *U*, *U*^{2}, *fixed effects*				
(13)	No	10,000	1.000	0.017	5.6	100	1.001	0.044	5.3	100
(14)	$\|V\|<1$	3,451	0.999	0.122	4.1	100	0.991	0.505	4.8	48.6
(15)	$\|V\|<0.5$	1,768	1.004	0.397	5.4	71.6	1.068	1.522	4.7	10.2

Note: Parameters are set as $(\eta_1, \eta_2, \theta_1, \theta_2, \lambda_1, \lambda_2) = (1,0,1,0,1,0)$ in (12) and $(\rho_1, \rho_2, \sigma_1, \sigma_2, \phi_1, \phi_2) = (1,1,1,1,1,1)$ in (19). The other settings are the same as the setting in Chapter 4. Fixed-effect estimation with a bandwidth uses unbalanced panel data because observations are dropped when they exceed the bandwidth. The average number of cross-sections is: N=499.5 when $|V| < 1$ and N=487.0 when $|V| < 0.5$.

Appendix C. RK estimation validity check

C.1 Smooth density of the assignment variable

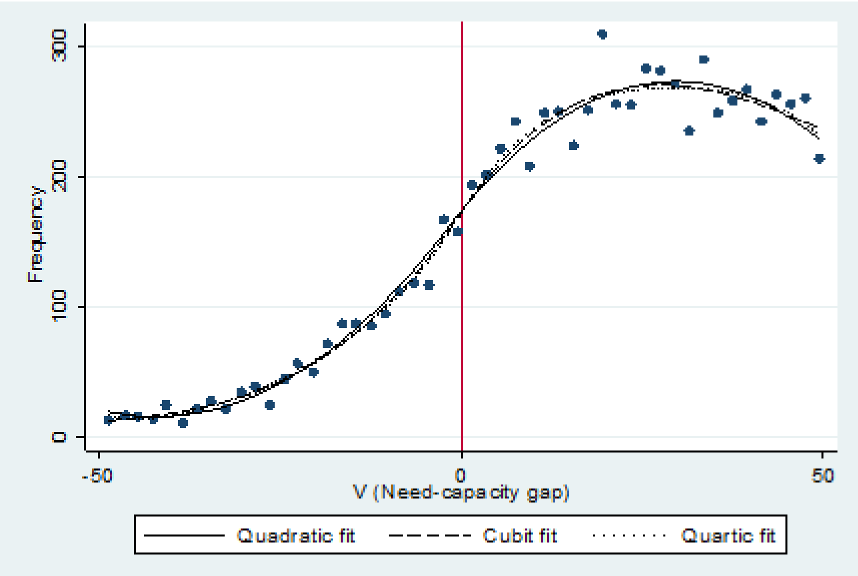
Following a density test applied to a RD design in McCrary (2008), CLP (2009) and CLPW (2012) present a density test applied to a RK design using collapsed data with equal-sized bins based on an assignment variable. Two required variables in this collapsed data set are the number of observations in each bin and the midpoint values of the assignment variable in each bin. I use bins with width 2 and bandwidth [-50,+50], which is a benchmark bandwidth for local regressions in this paper. Table C shows that in each sample a RK estimate is statistically insignificant if the order of polynomial is equal to or larger than two. The value of Akaike Information Criteria (AIC) is smallest when the order of polynomial is two. Figure C1 graphically illustrates that there seems to be no kink at the threshold.

Table C. RK estimates for need-capacity gap (bin size=2, |V|<50)

	Order of Polynomical			
	(1)	(2)	(3)	(4)
Variables	One	Two	Three	Four
RK esitmates	-2.161***	-0.869	-0.357	0.325
	(0.672)	(1.172)	(2.944)	(5.030)
Observations	50	50	50	50
R-squared	0.901	0.980	0.981	0.981
AIC	494.1	419.3	420.3	423.3

Notes: Heteroscedasticity-robust standard errors are in parenthesis.
***: P<0.01, *:p<0.05, *:p<0.1.

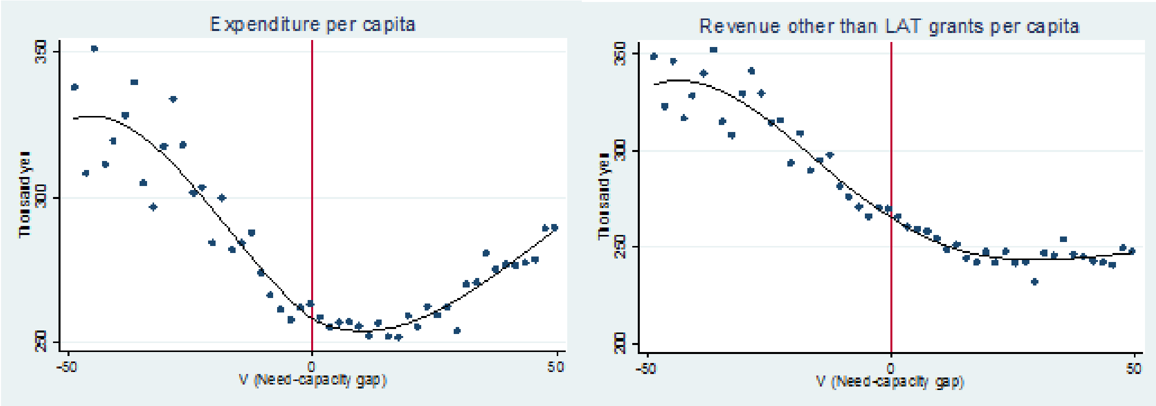
Figure C1. Density of need-capacity gap (bin size=2, bandwidth |V|<50)



Notes: Bin size is 2 and fitted curves are based on RK estimation with local polynomial regressions. Fitted curves are generated based on the estimation with equation (3).

C.2 Bin-mean plots and cubic fits of outcomes and covariates against V

Figure C2-1. Outcome variables against need-capacity gap (bin size=2, bandwidth |V|<50)



Note: Cubic fits are based on the estimation with equation (3).

Figure C2-2. Covariates against need-capacity gap (bin size=2, bandwidth |V|<50)

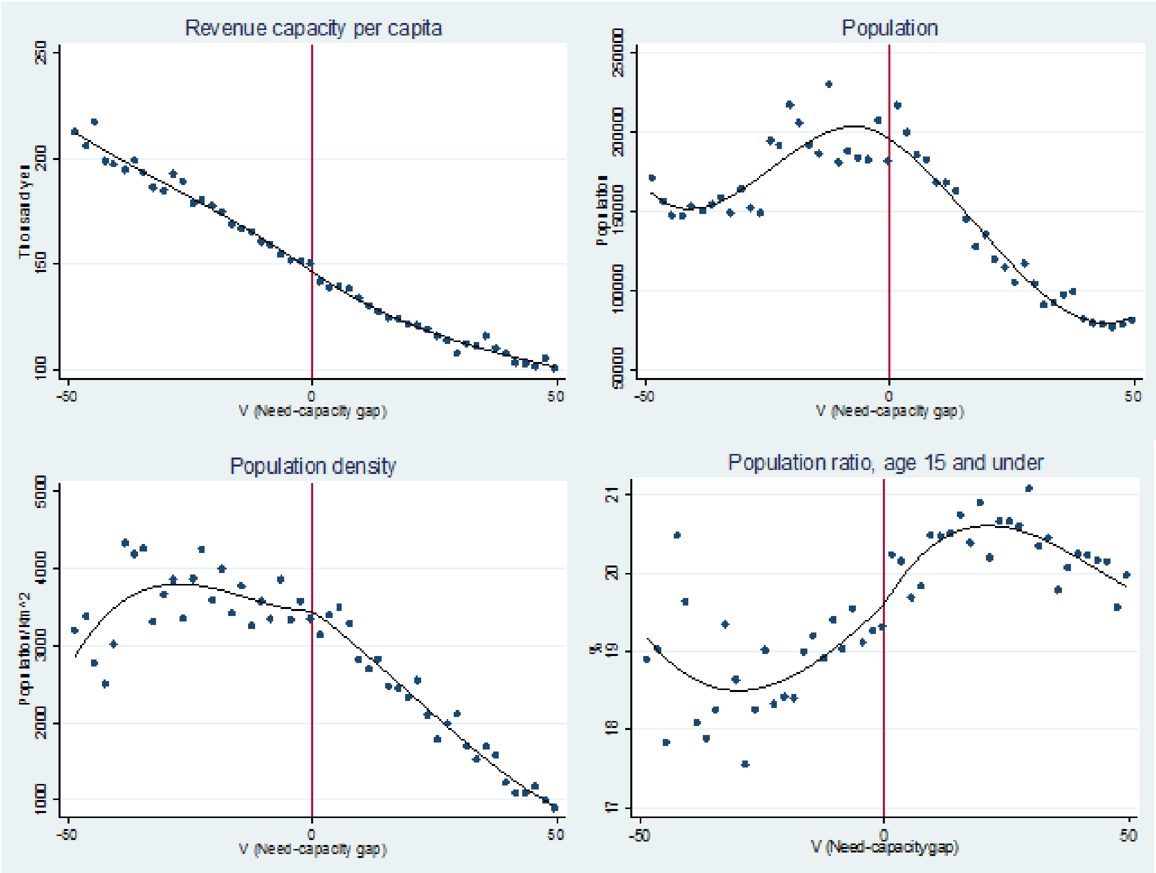


Figure C2-2. Covariates against need-capacity gap, cont. (bin size=2, bandwidth |V|<50)

Note: Cubic fits are based on the estimation with equation (3).

Appendix D. Additional results from the empirical application

Table D1. OLS estimates for total expenditure

	Band-width	Obs.	Estimate	S.E.	$p(H_0:\beta_1=1)$	Covariates
(1)	No	12,623	1.321***	(0.084)	0.000	No
(2)	No	12,623	2.278***	(0.085)	0.000	Revenue capacity
(3)	No	12,623	2.328***	(0.098)	0.000	Full covairates
(4)	No	12,623	1.582***	(0.100)	0.000	Full covairates & FE

Notes: Standard errors are clustered by the municipality level. ***: P<0.01, **:p<0.05, *:p<0.1

Table D2. RK estimates for total expenditure with higher order polynomials

	Band-width	Obs.	(I) Third-order polynomial			(II) Fourth-order polynomial		
			Estimate	S.E.	$p(H_0:\beta_1=1)$	Estimate	S.E.	$p(H_0:\beta_1=1)$
				No covaraites				
(1)	No	12,666	2.503***	(0.562)	0.008	1.224	(0.830)	0.787
(2)	|V|<50	7,750	-0.157	(1.264)	0.361	-2.186	(2.086)	0.127
(3)	|V|<40	6,430	-0.809	(1.519)	0.234	-1.777	(2.935)	0.345
(4)	|V|<30	5,013	-0.929	(2.218)	0.385	-3.383	(4.098)	0.285
(5)	|V|<20	3,451	-0.736	(3.322)	0.602	-9.437	(6.694)	0.120
(6)	|V|<10	1,741	1.095	(8.818)	0.991	2.056	(17.969)	0.953
				With covaraites of revenue capacity				
(7)	No	12,666	2.057***	(0.433)	0.015	0.621	(0.603)	0.530
(8)	|V|<50	7,750	-0.287	(0.910)	0.158	-2.333*	(1.402)	0.018
(9)	|V|<40	6,430	-1.081	(1.016)	0.041	-2.119	(2.116)	0.141
(10)	|V|<30	5,013	-0.992	(1.533)	0.195	-3.590	(2.822)	0.105
(11)	|V|<20	3,451	-2.223	(2.273)	0.157	-5.038	(4.390)	0.170
(12)	|V|<10	1,741	1.613	(6.415)	0.924	1.153	(12.324)	0.990
				With full covaraites				
(13)	No	12,666	2.180***	(0.424)	0.006	0.420	(0.542)	0.286
(14)	|V|<50	7,750	-0.039	(0.859)	0.227	-2.236*	(1.325)	0.015
(15)	|V|<40	6,430	-1.065	(0.995)	0.039	-2.025	(2.007)	0.133
(16)	|V|<30	5,013	-0.919	(1.437)	0.183	-2.527	(2.751)	0.201
(17)	|V|<20	3,451	-1.119	(2.291)	0.356	-2.381	(4.276)	0.430
(18)	|V|<10	1,741	4.422	(6.375)	0.592	5.742	(12.126)	0.696
				With full covariates and fixed effects				
(19)	No	12,666	1.180***	(0.373)	0.629	0.352	(0.456)	0.156
(20)	|V|<50	7,750	-0.495	(0.603)	0.014	-1.293	(1.124)	0.042
(21)	|V|<40	6,430	-0.582	(0.828)	0.057	-1.297	(1.683)	0.173
(22)	|V|<30	5,013	-0.014	(1.002)	0.312	-3.003	(2.077)	0.055
(23)	|V|<20	3,451	-0.477	(1.653)	0.372	-1.787	(2.491)	0.264
(24)	|V|<10	1,741	4.829	(3.346)	0.254	16.252**	(6.521)	0.020

Notes: Standard errors are clustered by the municipality level. ***: P<0.01, **:p<0.05, *:p<0.1. Covariates are listed in Table 4 and both linear and quadratic terms of these covariates are introduced into regressors.

Table D3. RK estimates for total revenue excluding the LAT grant

	Band-width	Obs.	(I) Linear polynomial		(II) Quadratic polynomial	
			Estimate	S.E.	Estimate	S.E.
			No covaraites			
(1)	No	12,623	2.831***	(0.258)	1.924***	(0.358)
(2)	|V|<50	7,711	1.404***	(0.256)	0.679	(0.628)
(3)	|V|<40	6,396	1.365***	(0.308)	0.061	(0.776)
(4)	|V|<30	4,981	1.079***	(0.348)	-0.387	(0.965)
(5)	|V|<20	3,423	0.897*	(0.467)	-2.004	(1.717)
(6)	|V|<10	1,725	-0.458	(1.002)	-4.305	(3.873)
			With covaraites of revenue capacity			
(7)	No	12,623	-0.110	(0.159)	0.261	(0.316)
(8)	|V|<50	7,711	0.209	(0.197)	0.557	(0.427)
(9)	|V|<40	6,396	0.347	(0.243)	0.039	(0.531)
(10)	|V|<30	4,981	0.371	(0.270)	-0.750	(0.682)
(11)	|V|<20	3,423	0.182	(0.364)	-1.853	(1.175)
(12)	|V|<10	1,725	-0.850	(0.694)	-3.780	(2.652)
			With full covaraites			
(13)	No	12,623	0.087	(0.179)	0.624**	(0.283)
(14)	|V|<50	7,711	0.208	(0.204)	0.406	(0.410)
(15)	|V|<40	6,396	0.268	(0.251)	0.068	(0.495)
(16)	|V|<30	4,981	0.337	(0.272)	-0.777	(0.670)
(17)	|V|<20	3,423	0.110	(0.352)	-1.811	(1.107)
(18)	|V|<10	1,725	-0.800	(0.648)	-1.747	(2.701)
			With full covariates and fixed effects			
(19)	No	12,623	0.060	(0.291)	0.139	(0.333)
(20)	|V|<50	7,711	0.084	(0.303)	-0.080	(0.389)
(21)	|V|<40	6,396	0.061	(0.338)	-0.597	(0.389)
(22)	|V|<30	4,981	-0.027	(0.308)	-1.068**	(0.533)
(23)	|V|<20	3,423	-0.030	(0.280)	-1.327*	(0.791)
(24)	|V|<10	1,725	-0.677*	(0.383)	-0.851	(1.448)

Notes: Standard errors are clustered by the municipality level. ***: P<0.01, **:p<0.05, *:p<0.1. Covariates are listed in Table 4 and both linear and quadratic terms of these covariates are introduced into regressors.

Appendix E. Description of fiscal variables

Expenditure need: $NEED$

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:

$$NEED_i = \sum_k (Mesaurement\ Unit_{ik} \times Unit\ Cost_k \times Adjustment\ Coefficient_{ik}),$$

where k expresses k^{th} public service. *Measurement unit* is in most cases the number or size of the beneficiaries of a particular service. *Unit cost* is a kind of net standard cost per measurement unit for each service item. *Adjustment coefficient* is a modification ratio that reflects 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

CAP 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:

$$CAP_i = Standard\ Tax\ Revenues_i \times \frac{3}{4} + Transfer\ Tax\ revenue, etc._i$$

where *Standard tax revenues* are estimated based on standard tax rates, standard tax collection rates, and estimated tax bases which are calculated using relevant statistics or past tax revenues. *Transfer Tax Revenue, etc.* represents the sum of revenues from the *Local Transfer Tax* and the *Special Grant for Traffic Safety Measures*. In brief, CAP captures the potential amount of local general revenues before fiscal equalization, which canno be manipulated by municipalities in the short run.

There are two main reasons that *Standard Tax Revenue* is multiplied by 3/4.[20] 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 some remaining fiscal needs that are not taken into account by the *Standard Fiscal Needs* (SFN) 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*

[20] See also online Appendix F, where I describe the stylized features of the Japanese fiscal equalization grants with some equations and graphs.

Revenue were taken into account in CAP, 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.

Revenue capacity (modified for a pre-determined covariate)

As is explained above, CAP itself does not represent "real" pre-equalization revenue capacity as it takes into account some policy objectives of the fiscal equalization scheme such as providing economic incentives to municipalities. We can, however, easily recover real pre-equalization revenue capacity by simply replacing 3/4 for 1 in the above definition of CAP.

When I use pre-equalization revenue capacity as a control variable in Section 5, I use this modified version of revenue capacity that reflects the real pre-equalization revenue capacity of municipalities. However, because available statistics are only CAP and *Local Transfer Tax*, I have to assume that revenue from *Special Grant for Traffic Safety Measures* is negligible. This assumption should not be a major problem because the amount of the *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 this "real" CAP as follows:

$$RealCAP_i = Standard\ Tax\ Revenues_i \times 1 + Transfer\ Tax\ Revenue, etc._i$$

$$= (CAP_i - \ Transfer\ Tax\ Reveneu, etc._i\ ) \times \frac{4}{3} + Transfer\ Tax\ Reveneu, etc._i$$

$$\approx (CAP_i - \ Transfer\ Tax\ Revenue_i) \times \frac{4}{3} + Transfer\ Tax\ Revenue_i$$

Appendix F. Stylized description of Japanese fiscal equalization

In this appendix, I explain the stylized features of the Japanese fiscal equalization scheme and describe how the kink based on the LAT grants is generated in more detail. In order to make this description as concise as possible, throughout this appendix I redefine CAP as follows:

$$CAP_i = Standard\ Tax\ Revenue_i \times \frac{3}{4}.$$

In other words, compared with the actual definition in Appendix E, CAP is simplified by dropping the second term (local transfer tax and some miscellaneous revenues), which is actually much smaller than the first term (standard local tax revenue) in the majority of municipalities.

Then, further assuming that there are no additional revenues other than local tax revenues and LAT grants, the relation between pre-equalization standard revenue (denoted as PreRev) and post-equalization standard revenue (denoted as PostRev) can be expressed as follows[21]:

$$\begin{cases} PostRev_i = PreRev_i, & if\ V_i \leq 0 \\ PostRev_i = PreRev_i + GRANT_i, & if\ V_i > 0. \end{cases}$$

By inserting $PreRev_i = Standard\ \ Tax\ Revenue_i$, $V_i = NEED_i - CAP_i$, $GRANT_i = V_i = NEED_i - CAP_i (if\ V_i > 0$, and the above definitions of CAP_i into these equations, they can be rewritten as

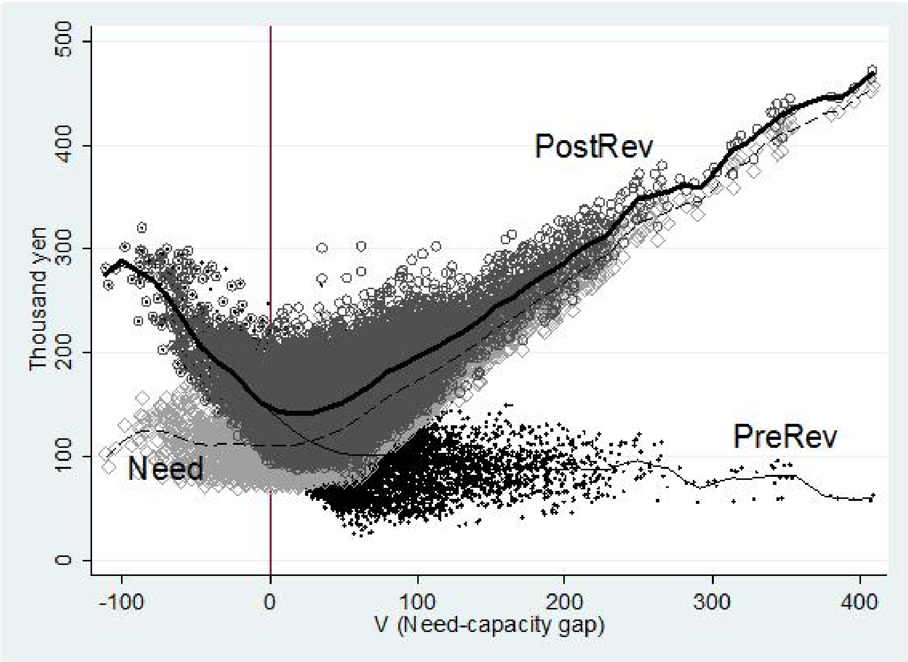
$$\begin{cases} PostRev_i = CAP_i + \ Standard\ Tax\ Revenue_i \ \times \frac{1}{4}, & if\ CAP_i \geq NEED_i \\ PostRev_i = NEED_i + Standard\ Tax\ Revenue_i \times \frac{1}{4}, & if\ CAP_i < NEED_i. \end{cases} \quad \text{(I)}$$

These two equations represent an essential function of the Japanese fiscal equalization scheme. First, if CAP is larger than $NEED$, no LAT grant is distributed and post-equalization standard revenue is identical to the sum of *CAP* and *Standard Tax Revenue×1/4*. Second, when CAP is smaller than $NEED$, the LAT grant ensures that municipalities receive the sum of $NEED$ and *Standard Tax Revenue×1/4*. In both cases, this additional amount, *Standard Tax Revenue×1/4*, exists due to the fact that CAP is

21 I use the phrase "standard" revenue to emphasize that this is not the actual revenue of local municipalities but the estimated revenue that the central government evaluates under some "standard" local taxation setting.

calculated by *Standard Tax Revenue×3/4* and the other 1/4 of *Standard Tax Revenue* is excluded from the fiscal equalization formula. Because of this excluded part of *Standard Local Tax Revenue*, which is officially referred to as "reserved revenues", a richer municipality is always richer even after fiscal equalization. Figure F presents actual scatter plots and local polynomial smoothing of $PreRev$, $PostRev$, and $Need$ against the assignment variable V. It graphically illustrates how the LAT grant phases in at the cutoff point $V = 0$.

Figure F: Scatter plots of $PreRev$, $PostRev$, and $Need$



Notes: The same sample that is described in Section 5.3 is used for this scatter plot. The local polynomials are obtained using the lpoly command in STATA 12 with the default setting.
Sources: Reports on the Municipal Public Finance, Census, and CPI

Notice that in this graph $PostRev$ is well above $Need$ around $V = 0$. This implies that municipalities just after $V > 0$ have ample additional fiscal resources in excess of $NEED$. These additional fiscal resource come from the term *Standard Local Tax Revenue×1/4* in the second equation of (I). This fact benefits our empirical analysis because LAT grants, which phase in after $V > 0$, can be plausibly considered as "general" and "lump-sum" around the threshold, without any difficulties caused by complicated institutional settings of these grants. I conclude this appendix by examining this issue in greater detail.

In this paper, I implicitly assume that LAT grants are "general" and "lump-sum" and local bodies have full discretion in their decision-making on spending and taxation. In other words, I presuppose that an estimated coefficient can be straightforwardly interpreted as the effect of general lump-sum grants on local spending under the full discretion of local municipalities.

But it could be misleading to simply assume that LAT grants are completely “general” and “lump-sum” as the Bradford-Oates equivalence theorem and some previous empirical studies have done. The LAT grant is nominally a general grant that a local body can spend on whatever it wants, but at the same time the LAT grant is the grant that guarantees every single municipality a sufficient amount of revenues to cover centrally-determined “standard costs” for local public services, which is referred to as *Standard Fiscal Needs* and denoted as $NEED$ in this paper. It is sometimes pointed out that the central government takes advantage of LAT grants to control local spending by arbitrarily adjusting $NEED$. In addition to these possibly “centralized” aspects of LAT grants, the provision of local public services is often strongly regulated by the central government through various centralized legal frameworks.

In sum, although local bodies do not have to strictly follow these centrally-determined standards, they quite often cannot control their expenditures on some local public services because the basic legal and provisional frameworks of these local services are centrally determined. Hayashi (2000, 2006) provides critical reviews of empirical studies on flypaper effects in Japan and points out that these previous studies do not consider these institutional settings of the Japanese general grant and naively treat it as a “general” and “lump-sum” grant as studies of similar schemes in the U.S. do.

In fact, this obligatory and centralized feature of local public services is part of the institutional basis of LAT grants: since the central government forces all local bodies to provide particular levels of local public services, fiscal resources for these services have to be guaranteed by the intergovernmental fiscal transfer which reflects the expected costs of these services. This feature of the LAT grant is officially referred to as a function of “*fiscal resource guarantee*”.

According to Figure F1, however, I would argue that LAT grants can be considered to be “general” and “lump-sum” around the threshold $V = 0$ regardless of the centralized features of these grants and local administration. In other words, around the threshold, $PostRev$ is well above $NEED$ and therefore the relatively “obligatory” local public services that are reflected in the calculation of $NEED$ can be financed even without the LAT grant. It is thus possible to assume that the marginal increase in the LAT grant around the threshold affects local bodies’ expenditure in exactly the same way that standard “general” and “lump-sum” grants do.

Appendix G. Description of data arrangement

Japan is a unitary state which has three-tiers of administrative authorities: the central government, 47 prefectures, and 1750 municipalities (as of 2010). Municipalities are classified into four categories: cities (shi), towns (cho), villages (son) and special districts (ku). Cities are generally larger than towns and villages and in principle the minimum population required to become a city is 50,000. Even if the population of a city becomes less than 50,000, however, it does not have to become a town or village. The 23 special districts are all located in Tokyo prefecture and have similar duties to other municipalities but follow a different vertical fiscal equalization scheme managed by the prefecture. Cities and towns/villages have similar duties under the LAT grant fiscal equalization scheme, but cities have more responsibilities in some areas.

In this paper I only use the datasets for cities, but not all cities are included in my analysis. First, I exclude so-called “designated“ cities, which consisted of the 12 largest cities in Japan during the sample period. I drop these cities from the sample because their response to the marginal increase in their LAT grants might be institutionally different from other cities as a result of the fact that some of the duties normally assigned to prefectures are delegated to them and their administrative responsibility is thus larger than that of normal cities. Second, I also remove the cities that experienced amalgamation between 1975 and 1999 because the calculation of the LAT grants for these merged cities was affected by special measures. Because this special measure was in effect for 5 years after amalgamation, municipalities which merged before 1975 were not affected by this measure after 1980. Finally, there are some LAT-receiving municipalities whose need-capacity gap is apparently different from the amount of their LAT grant, possibly due to measurement errors or typos. Therefore, I drop 18 observations in which |Need-capacity gap – LAT grant per capita| is larger than 10,000 yen .

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