Local Government Spending and the Labor-Market Multiplier: Evidence from Brazil*

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Abstract

In this study, we exploit a discontinuity in the assignment of federal transfers at the municipality level to estimate the effects of fiscal policy on local labor markets in Brazil. We find that effects of increased government revenue and spending on unskilled public sector employment and public sector wages are significant. However effects on overall wages and employment are extremely small: specifically, we cannot reject the null hypothesis of negligible (zero) effects on the labor market at large.

JEL-Classification: O11, O54, H72

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*Results are very preliminary. Comments are welcome. Please do not cite without authors' permission. Please direct comments at bpthomp@umich.edu.

1 Introduction

Fiscal policy and spending is a primary means by which the government can affect the state of the economy. The efficacy of these channels is of particular interest to macroeconomic policy makers. Accordingly, estimating fiscal multipliers and the effect of government spending on the economy are of great interest; however, isolating the effect of government spending on variables of interest proves challenging due to the lack of existing identifying variation in government spending.

Fiscal policy can entail vastly different outcomes for local economies in developing countries than it might in their developed counterparts. For instance, corruption, differences in the local business environment, and the mistrust of government can all severely limit the extent of the influence of government spending. In spite of these limitations, fiscal policy still plays an important role in a developing country context, and thus estimating its effects remains of great importance.

Our paper uses a known discontinuity in the allocation of government transfers in Brazil to estimate the effect of fiscal policy on income, wages, and employment at the local level. Amounts of government transfers to municipalities under the *Fundo de Participação dos Municípios* (FPM) program vary according to population thresholds such that municipalities in the same population bracket within the same state should all receive the same amount of transfers. These transfers make up a substantial portion of municipality budgets, and as such, revenue at the local level varies discontinuously around the population thresholds. We find that this revenue translates into spending based on discontinuity estimates. We then estimate the effects of government spending on local labor market variables. Our main results find limited effects of government spending on the overall economy. Specifically, while we find that unskilled public sector employment increases as a result of additional spending, we cannot reject the hypothesis that the effects of government spending on wages and overall employment are zero.

We then show results that may confound the estimation of fiscal spending and warrant further investigation. Firstly, the results of Brollo et al (2013) suggests that corruption jumps at the discontinuities we observe, we show that their results hold in a subset of our analytical sample; however we are only able to suggest that corruption may be a confounding factor in our analysis. Secondly, we find that the composition

of expenditure change discontinuously at the thresholds; specifically, staff spending as a fraction of general spending falls by a small but significant amount.

2 Literature Review and Contribution

The canon of literature on fiscal spending, including estimates based on military spending (Barro (1981)) and VAR models (Blanchard and Perotti (2002)) has been recently updated by literature attempting to use novel instruments to identify exogenous variation in spending. Several studies have utilized an instrumental variables approach, such as Serrato and Wingender (2011) and Shoag (2010); the former uses variation in census population counts to determine the allocation of government resources, and the latter uses variation in government pension windfalls. Both papers find that taking advantage of instrumental variables yields much larger, positive effects of government spending on local labor market variables, such as employment and income, compared with naive estimates derived from simply regressing labor outcomes on government spending and ignoring endogeneity. Specifically, Serrato and Wingender find estimates in the neighborhood of 1.45, and Shoag finds estimates around 2.12. While these results are interesting, the current literature on fiscal multipliers and, more generally speaking, government spending has focused almost exclusively on developed countries. The literature on multipliers in developing countries is scant, but existing studies do hint at the notion that developing country (GDP) multipliers are quite small. Kraay (2012) and Kraay (2013) use variation in World Bank spending projects to gain leverage on identifying the effects of fiscal spending on GDP. Kray (2012) fails to find multipliers significantly different from zero on a sample of 29 developing (and almost entirely African) countries, and Kraay (2013) finds multipliers in the range of 0.3 to 0.5 on a larger sample of 102 developing countries. The author suggests the lack of neo-classic mechanisms of wealth effects in developing countries a reason for such low multipliers.

We analyze the effects of government spending at the local level in a developing country. In order to deal of the endogeneity problem of government spending, our paper uses a known discontinuity in federal transfers to municipalities based on population cutoffs to identify 'shocks' in government spending. A number of studies have used the same discontinuity, however none has focused directly on the effects on labor market outcomes, though some examine a few related outcome variables in supporting analysis. Brollo et al (2013)

examines the political resource curse, or the idea that an influx of resources can actually prove detrimental to a state, using the same discontinuity to identify exogenous variation in government resources. The authors indeed find evidence of a political resource curse, and thus of non-negligible corruption at the municipality level. Litschig and Morrison (2012) also use FPM cutoffs to identify the effects of increased government spending on the reelection probabilities of incumbent officials. They find that the increased government spending due to the FPM cutoffs increased the reelection probability by about 10%. The authors explored the effects on wages and income per capita, finding positive effects, however the period they examined was from 1982-1985. [1]

Our paper extends the work of estimating the effects of recent fiscal policy at the local level in a developing country by focusing on the local labor market effects of presumably exogenous government spending. Because local funds are often directed at delivering tangible benefits to local citizens, exploring the extent to which these policies affect workers is an important topic. Our contribution is unique in a number of ways. Firstly, our paper estimates multipliers at the local-economy level, providing insight into the way local government spending potentially works; nearly all the literature on developing country multipliers focuses on national multipliers. Secondly, we are the first to use the widely-known population discontinuity in Brazil to examine the effects of government spending on labor market outcomes.

The paper proceeds as follows. Section III describes the data and the institutional framework in which the transfers take place. Section IV describes our estimation strategy, and Section V describes empirical results. Section VI concludes with an explanation of our results and some ideas for future research.

3 Data and Institutions

The Brazilian government operates in a decentralized manner. The 26 states of Brazil are subdivided into over 5,500 municipalities, or *municípios*, which have an average population of around 35,000 people; these municipalities are the lowest level of governance. Each of the 26 states has, on average, around 200 munic-

[1] Caselli and Michaels (2013) examine a different source of exogenous variation in oil revenue royalties paid to municipalities, finding small effects of oil abundance on government provision of services. They also do not explore labor market outcomes in detail.

ipalities. Local political power, including the allocation of government resources, is concentrated within the executive government of each of these municipalities. Each has an elected mayor, or *prefeito*, that has major influence over the distribution of municipal funds, along with an elected council, or *Camara dos Vereadores*.

In general, municipalities are heavily dependent on government transfers as a main source of revenue. For the an average municipality, tax revenues constitute a relatively small percentage of total revenues (around 6%) whereas transfers from the federal government under the transfer program *Fundo de Participação dos Municípios* constitute a much more sizable fraction (around 40%). While there are some restrictions on the distribution of federal funds at the municipality level, 70% is unrestricted (Brollo et al (2013)).

Each of the 26 states receives a different amount of federal transfers, but within each state, FPM funds to municipalities are distributed to municipalities according to population counts in each municipality. Municipalities within a state j predefined population brackets are assigned coefficients λ_i. The amount of federal transfers a municipality receives, or $FT_{i,j}$, is a fraction of the total amount allocated to a state, FT_j:

$$FT_{i,j} = \frac{FT_j \lambda_i}{\sum_i \lambda_i}$$

Table 1 gives a description of the population brackets and their corresponding coefficients. The coefficients, unsurprisingly, are increasing in population. The federal transfer framework presents an interesting discontinuous allocation of federal money in population. Variation in government spending along this dimension is plausibly exogenous, assuming that the running variable of population is not manipulated by mayors or other government officials. As we will explain further, for a subset of the data, there is sufficient evidence to believe that manipulation of the population counts is not a concern.

The discontinuous allocation of these federal funds must in some way translate into discontinuous government spending in order for us to estimate the effect of the impact on local labor markets. Non-fungible, general expenditures (*despesas não financeiras* are subdivided into expenditures on current expenditures and capital spending, and expenditures on labor (*pessoas*); about 45% of general spending goes toward labor expenditures. Of the labor spending, a large majority (around 85%) goes toward the existing workforce (i.e. not toward pensions or inactive workers). We will provide analysis on both the effect of general spending

and of the more specific active labor force spending.

Though we have outlined supporting statistics, it is helpful to see the distribution of government revenues and spending by population bracket. Figures 1 and 2 present scatter plots of actual government transfers and theoretical government transfers respectively. Theoretical government transfers are obtained by redistributing the total amount of federal transfers in a given state according only to population. While constructing this variable in this fashion might raise concern over endogeneity, including state fixed effects in regressions should capture any mis-allocation in federal funds FT_j across states.

Our data come from three main sources. Firstly, population and GDP data come from the Brazilian Institute of Geography and Statistics, or Instituto Brasileiro de Geografia e Estatística (IBGE); this information is provided annually to the public on the IBGE website. Public finance data, such as revenues, expenditures, and the distribution of expenditures come from the Finances of Brasil, Finanças do Brasil (FINBRA) annual survey of the Ministry of Finance, and employment and wages by sector and education level come from the Annual Report of Social Information, or Relaçao Anual de Informaçoes Sociais (RAIS) of the Ministry of Labor. It is important to note that information from RAIS only covers workers in the formal sector. The merged data contain information at the municipality level over the years 2000 to 2009.

3.1 Analytical Sample Construction

For our analysis, we use observations around the first three thresholds, comprising about 68% of all municipalities. We do this for several reasons. Firstly, the FPM program was intended for smaller municipalities, and these municipalities are much more dependent on the FPM transfers as a source of revenue. Therefore, the discontinuity should be more relevant and provide a cleaner identification of exogenously distributed government funds. Secondly, previous literature (Brollo et al (2013)) examine observations from the first three thresholds in isolation, and thus we follow their approach for comparability. [2]

[2]Out of this sample, we also discard observations with low fractions (less than 25%) of their total transfer revenue coming from FPM transfers. These observations constitute a small fraction of our original sample (around 4%), are outliers in terms of revenue and GDP with respect to the rest of our sample, and we do not consider them to be relevant as we do not expect their expenditures to depend on government transfers.

We also restrict our sample to municipalities within 2000 people of the nearest cutoff. While only using observations close to the threshold yields more accurate estimates of the discontinuity itself, most studies that attempt to do so suffer from drastic losses in statistical power; however, because we have observations over several years, this is not an issue in our study. [3]

Summary statistics for the analytical sample of municipalities are presented in Table 2. As aforementioned, FPM transfers constitute a sizable portion of revenues and GDP in our analytical sample. Additionally, tax revenues account for a much smaller portion of revenue and GDP. The private sector contains considerably more workers than does the public municipal sector, whereas public municipal workers are paid more on average. Also, as can be seen from Figures 1 and 2, most municipalities have populations around the smaller thresholds. FPM transfers matter substantially less for larger municipalities, as they are a smaller portion of the budget.

4 Identification

While the methodology behind obtaining reliable estimates in the context of regression discontinuities is far from canonical, verifying the validity and relevance of our discontinuity is an important first step. Therefore, in order to obtain a reliable idea of the fiscal multiplier, we should establish that the variation implicit in our identification is (1) generated exogenously, and (2) has a tangible means of affecting the outcome variables of interest (i.e. is relevant). When appropriate, we appeal to the the description of regression discontinuity analysis in Lee & Lemieux (2009). We argue that our analytical sample meets these criteria.

4.1 Validity of the Discontinuity

Our first task is to verify that the population cutoffs are indeed arbitrary and not associated with any other policies or phenomena that might affect our outcome of interest, including manipulation by agents. Litschig and Morrison (2012) provides an accessible brief history of the cutoffs. In 1964, a military junta deposed

[3] We also provide analysis using other 'bandwidths' around the discontinuity. The results remain robust to these changes in bandwidth, however, we find that using the half-bandwidth of 2000 people provides the most consistent estimates.

president João Goulart and assumed control of the government. One of the junta's main goals was to de-politicize public services, and so it distributed resources according to an objective measure of need (population). The exact numbers we see today were initially based on multiples of 2000, but were updated (as was specified by the law that put them in place) twice with census recounts (Litschig and Morrison (2012)). No other programs have been known to vary at these cutoffs.

The threat of precise manipulation by the mayors of the municipalities is unrealistic. The mayors almost surely do not have control over the annual population counts of the municipalities, in part due to the fact that the counts are not exact counts by rather projections by the Brazilian Institute of Geography and Statistics (IBGE). These projections are then reviewed by the Federal Court of Accounts (TCU), and so the mayors do not report the population counts of their municipalities directly. A more realistic fear is the threat of imprecise manipulation of population by the mayors, which could occur if mayors give residents in surrounding municipalities incentive to move to their respective municipalities. To this extent, we use McCrary (2008) tests for discontinuities in the density of the running variable to see if bunching at the first cutoff exists. Figure 3 shows a plot of the density function of population by bin averages for all municipalities in the sample from years 2001 to 2009. There seems to be evidence of a (significant) positive discontinuity at some cutoffs, however when we restrict the sample to only observations in years 2001 to 2007, we see that almost all the density discontinuities disappear in Figure 4. Imprecise manipulation of the running variable in years after 2008 can be explained as IBGE publicly announced a population recount that presumably brought the exact cutoffs to the attention of the mayors. For the rest of our analysis we restrict our sample to years before 2008.

4.2 Relevance

Our second task is to ensure that the 'ripple' caused by an exogenous increase in FPM transfers makes its way through government finances to expenditure. If we see a discontinuity in FPM transfers, we should see a corresponding discontinuous increase in revenues, and in turn a discontinuous increase in government expenditures. In order to estimate the magnitude and direction of the discontinuities and increase statistical power, we pool observations around the first three thresholds in our analytical sample by normalizing the running variable value to zero for all observations in the neighborhood of a given cutoff. Following the suggestions of Lee & Lemieux (2010), though our data have a panel structure, we pool the sample ignoring

time fixed effects. Table 3 shows regression results for observations before 2008. We estimate regressions of the form

$$y_i = \alpha + \beta D_i + \gamma f(p_i)$$

where y_i represents the outcome of interest (non-finance revenues and non-finance expenditures), D represents an indicator for a positive running variable value, and $f(p)$ represents a polynomial in the running variable (population).[4] Figures 5 and 6 show figures created using a local linear regression. Circles on the figures represent average values within bins of width 100, and the fits from the local linear regression are estimated on either side of the threshold and shown as colored lines. In both figures, the discontinuity is striking. [5]

Tables 3 and 4 show results in which $f(p)$ takes on different forms of polynomial. In the first two specifications, $f(p)$ takes on flexible slopes, and in the next three, we estimate $f(p)$ having the same slope on both sides of the threshold, where $f(p)$ is a first, second, and third order polynomial. Standard errors are robust and clustered at the municipality level. We find significant, positive discontinuities in both revenue and expenditures. In the pooled sample across all thresholds, being above the threshold resulted in between a 4% and 5% increase in revenue, which translated to between a 4% and 5% increase in expenditure. [6]

While the absence of significant discontinuities does not prevent us from obtaining consistent estimates of the effects of fiscal policy in the context of exogenous variation, it nevertheless clarifies the mechanism by which we expect FPM transfers to affect GDP and labor market outcomes and provides some reassurance of significant variation in these intermediate variables.

[4]Non-finance refers to the classification that such revenues and expenditures are aside from municipality-specific tax revenues. As later mentioned, we note that such tax revenues (1) do not comprise a large part of revenue and (2) do not jump at at the FPM thresholds.

[5]We follow an estimation strategy in which we estimate local linear regressions (free of functional form assumptions) and then attempt to verify the discontinuities using estimates with polynomials of the running variable included. Because the local linear regressions are less dependent on functional form assumptions, the figures can be taken as primary, rather than supporting evidence.

[6]We base these conclusions from the last three polynomial specifications in each table. While the first- order flexible slope estimates are also significant, these specifications are quite taxing on the data, and the discontinuity pictures would seem to indicate that the assumption of a similar slope on either side of the discontinuity is reasonable.

5 Results

We take advantage of both the regression discontinuity design and an instrumental variables approach to estimate the effects on local economic outcomes. Our results focus on municipality-level labor-market variables, specifically employment and wages. Our data include aggregate labor-market information stratified by sector and education level, and so we exploit variation along these dimensions to address possible crowding out of private spending. While there are an endless array of specifications and outcome variables we could examine, those which we display are the ones that best represent our findings. Additional supporting evidence is available by request.

5.1 Discontinuity Results

Our first set of results utilizes the regression discontinuity design of the study. Again, we create figures based on local linear regressions. We also estimate regressions of the form

$$y_i = \alpha + \beta D_i + \gamma f(p_i)$$

where y_i represents the outcome of interest (Real GDP in 2000 *reais*, average Wages across all sectors, and overall Employment across all sectors), D represents an indicator for a positive running variable value, and $f(p)$ represents a polynomial in the running variable (population).

As is evident from the Figures 7 and 8, and Tables 5 and 6, there does not seem to be a significant increase in either Monthly Wages or Employment as a result of increased government spending; the 'ripple' caused by the FPM transfers seems to stop at these outcome variables. While the point estimates of the effects on Log Employment seem somewhat sizable, indicating between a 2-3% increase (although insignificant), the point estimates on the Log of Monthly Wage seem to be quite small, indicating around a 0.5% increase in wages. This result is confirmed in later analysis.

We address concerns that our results may be driven by bandwidth selection. We repeat our analysis for different bandwidths around the population thresholds. Our results for the outcomes do not change. This analysis is available upon request.

5.2 IV Estimates

Our second set of results utilizes an instrumental approach to estimating the effect of government spending on outcomes. Because of the exogeneity of the thresholds, we can take municipality-observations around a given population threshold and predict government expenditure with an indicator for whether the municipality is above or below the threshold. The predicted government expenditure can then be used to estimate the effect of an exogenous increase in government funds on labor market variables. Specifically, we estimate the following first stage regression:

$$x_{i,t} = I(pop_{i,t} > c_{i,t}) + z_{i,t} + \mu_i + \delta_t + \eta_{i,t} \tag{1}$$

where $c_{i,t}$ is the nearest threshold to observation (i, t) and $z_{i,t}$ represents included regressors. μ_i and δ_t capture state and time fixed effects. We use the predicted values to estimate the following second stage regression:

$$y_{i,t} = \hat{x_{i,t}} + z_{i,t} + a_{i,t} + \nu_i + \gamma_t + \varepsilon_{i,t} \tag{2}$$

where $y_{i,t}$ is the labor market outcome variable of interest, and $a_{i,t}$ represents excluded regressors. ν_i and γ_t capture state and time fixed effects. Our estimates, where $y_{i,t}$ represents average wages or total employment at time t for municipality i are shown in Tables 8- 19. Across all specifications, we note the first F-statistic on our excluded regressors, which never dips below 10, and for the most part is well above most ranges of concern. We conclude that given the plausibility of the exogeneity of the population thresholds, our instrument is reasonably strong.

As can be seen in Tables 8 and 9, OLS estimates (ignoring endogeneity) seem to yield positive and significant (though small) effects. Specifically, when expenditures rise by 1%, average wages seem to rise by approximately 0.3%. The point estimates for employment seem quite sizable, as an increase of 1% in general spending is associated with an increase in employment of about 1.06%. However, when instrumenting for government expenditure, the results are different.

As can be seen in Tables 10, the estimated effects of spending on average wages are smaller and insignificant. The point estimates indicate that a 1% increase in general expenditures translates to an impact

on monthly wages of about 2%. We therefore fail to reject that additional municipality spending has any impact on average wages. The distinction between the OLS and IV estimates is more salient for employment. A 1% increase in general expenditures seems to imply an increase in employment of around 0.5% of total employment, about half of the OLS estimate, and the effect is insignificant.

Again, we address concerns that our results may be driven by bandwidth selection. We repeat our analysis for different bandwidths around the population thresholds. Our results do not change. This analysis is available upon request.

5.3 Effects on the Public Sector

While we are unable to obtain significant effects of government spending on the labor market as a whole, Tables 12-17 examine the effects of spending on the public sector. In Table 12 and 13, we estimate insignificant effects of a 1% increase in government spending on both the public and non-public sectors. This result is particularly surprising given than a large fraction (around 45%) of additional government spending is dedicated to the public workforce. The point estimates are sizable, but again, around half of the OLS estimates.

We also examine effects on employment by skill. In Table 14 and 15, we find that there are significant effects of additioanl government spending on unskilled public workers but not on skilled public workers; thus there seems to be suggestive evidence that imprecise estimates of increases in public sector employment may be driven by heterogeneity across skill.[7] Specifically, a 1% increase in government spending leads to an increase in unskilled labor of around 1.3%, significant at the 5% level, whereas the effects on skilled employment are about 0.3% and insignificant.

As for wages, we find that public wages increase significantly at the 10% level in response to a 1% increase in government expenditure, and the point estimates are much larger than the insignificant estimates of the effects on the non-public sector. Specifically, the elasticity of public wages with respect to expenditure is around 0.4, whereas the estimated elasticity of non-public wages with respect to expenditure is around

[7] We define unskilled as those without a high school diploma.

0.03.

Thus, the main finding is that while spending seems to have some significant effects on parts of the public sector, most notably among unskilled workers and overall public sector wages, these effects do not seem to be as salient or significant for the labor market as a whole. The public sector accounts for about 47% of the total employment in our sample, so it is possible that the effect of spending on the public sector is being 'swamped' by the small effects on the public sector; however more research is needed to confirm this story.

Though our estimates are largely insignificant, the reaction of the labor market to a presumably exogenous increase in demand can be used to obtain some idea of what the supply elasticity of labor looks like close to equilibrium with a potentially unbiased estimate. Given that the point estimates of the reaction of employment to an increase in government spending are much larger than the estimates of the reaction of wages, that the labor supply curve (measured in number of workers, not hours worked) in the public sector is highly elastic close to equilibrium, with an estimated elasticity of over 2 (based strictly on point estimates). Within the public sector, where the effect on wages is reasonably precisely estimated, point estimates also dictate a highly elastic labor supply curve. While there is a lack of comparable studies on Brazilian public sector labor markets, and more generally on labor supply in developing countries, past studies have generally found elastic (but only slightly elastic) labor supply curves in developing countries.

6 Potentially Confounding Factors and Other Findings

Because most estimates of the fiscal multiplier have been made in macroeconomic settings, typical explanations that have been proposed for variation in multiplier sizes have included variables such as openness to trade. As local multipliers operate on different scales, it is natural that explanations for smaller multipliers should differ from those proposed in the past literature. As aforementioned, we suggest several potentially confounding factors that may influence our estimates of the effects of fiscal policy, including corruption and changes in the composition of spending.

6.1 Corruption at the Threshold

Brazilian municipalities suffer from a noticeable degree of corruption. Corruption can have severely negative impacts on the distribution of government funds, and has been found to have negative impacts on economic growth rates. Beginning in 2003, locally-elected mayors and their governments were subjected to random audits. In a recent paper, Brollo and coauthors examine the political resource curse in the context of Brazilian municipalities, and take advantage of these audits. As part of their analysis, they find that the number of corrupt incidents 'jumps' at the population threshold with the receipt of FPM funds. For emphasis, we repeat their analysis here, using the intersection of the corruption data from Ferraz and Finan (2008) and our analytical sample. While the sample used here is only a subset of the sample used elsewhere in our paper, the results indicate that there seems to be an increase in the number of corrupt incidents at the threshold. The increase is significant with multiple specifications of the running variable polynomial. This result is striking, given the drastic reduction in sample size from our previous analysis.

On a macro level, corruption has been negatively linked to economic growth (Mauro (1995), Wei (1999)). A mechanism by which corruption can affect growth is through investment in physical capital (Gyimah-Brempong (2002)). To the extent that corruption itself can have negative impacts on the allocation of resources and by extension, GDP and economic activity, it would seem that a higher incidence of corruption at population thresholds may actually neutralize any positive effects of federal transfers at a more local level. Power issues currently prevent us from making causal statements regarding the existence of corruption and the lack of a consistently positive effect of spending on labor markets, and we do not presume to have a consistent story for how corruption may be affecting the labor-market multiplier; however, these results do seem to suggest corruption may be a confounding factor at the least.

6.2 Changes in the Composition of Expenditure

It is interesting to note that the average municipality does not spend its extra funds in the same proportion upon receipt of additional funds from being just over the threshold. Specifically, we find that municipalities that receive more funds in this manner spend a smaller fractions on staff/personnel. Specifically, an average municipality above the cutoff shows an approximately 0.7 percentage point drop in workforce spending share.

While the point estimates seem small, a 0.1 % of general expenditure translates to close to 12,000 *reais* . These results suggest that there may be some rerouting of funds among municipalities receiving extra revenue just above the cutoffs. While the amount being rerouted may seem small based on these point estimates, it may just be a sign of other such activity. While we do not wish to overstate the importance of our findings, we nonetheless present it as suggestive evidence for the manipulation of funds.

7 Conclusion

In this paper, we estimate that local government spending in Brazil has significant effects on parts of the public sector, but insignificant effects on overall wages and employment at the municipality level. Given the consistency of our results, we find the labor supply elasticity to be highly elastic. We explore potential confounding factors in the estimation of government spending effects, noting the results of past studies; however we are unable to reach a definitive conclusion.

Our work adds to the body of scant literature on fiscal spending in developing countries, which as thus far found government multipliers to be small compared to their counterparts in developed countries. Our regression discontinuity offers an interesting means by which to identify exogenous changes in government expenditure. Our finding of corruption and changes in the composition of expenditure as a primary confounder of multiplier estimates warrants further explanation. More precise estimates of explanations as to the efficacy of the fiscal multiplier may be possible with future data, and the mechanisms behind this are worth exploring in future studies.

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Tables and Figures

Table 1: Population Brackets and Coefficients

Population Bracket	Coefficient
Less than 10,189	0.6
10,189 - 13,584	0.8
13,585 - 16980	1
16,981 23,772	1.2
23,773 30,564	1.4
30,564 37,356	1.6
37,356 44,148	1.8
44,148 50,940	2
Greater than 50,940	2-4

Table 2: Descriptive Statistics for Analytical Sample

Sample means (Analytical Sample)	
General Revenues	124.66 *(100k reais in year 2000 prices)*
General Expenditures	122.43 *(100k reais)*
GDP	1016.67 *(100k reais)*
FPM Transfers/General Revenues	0.42
Total Employment	1124.16 (workers)
Public Employment	291.31 (workers)
Non-Public Employment	832.85 (workers)
Average Wage	705.04 (*reais in year 2000 prices*)
Average Public Wage	758.10 (*reais in year 2000 prices*)
Average Non-Public Public Wage	732.53 (*reais in year 2000 prices*)

Figure 1: Distribution of Municipalities by Population (Years 2000-2009)

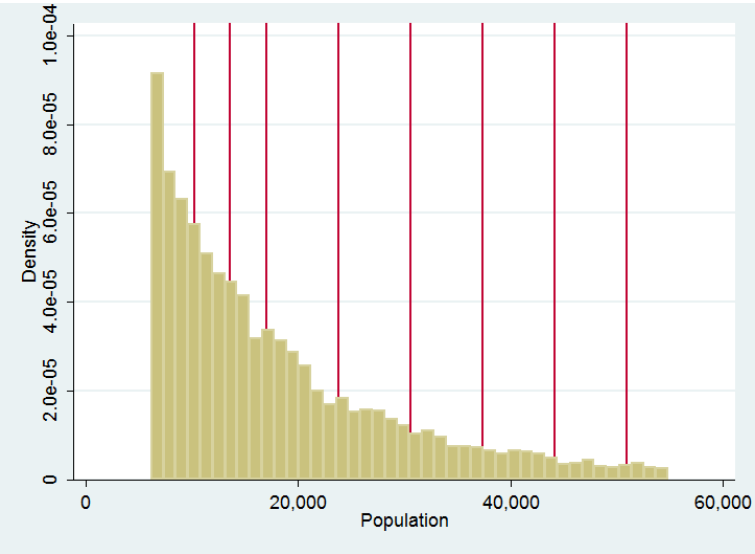


Table 3: Discontinuity Estimates: Revenues

	Ln Revenue				
	(1)	(2)	(3)	(4)	(5)
Above Threshold	0.129***	-0.012	0.051***	0.050***	0.056***
	(0.04)	(0.21)	(0.01)	(0.01)	(0.01)
Flexible Slope	1st Order	2nd Order	-	-	-
Same Slope	-	-	1st Order	2nd Order	3rd Order
Obs.	7698	7698	7698	7698	7698
R-Squared	0.711	0.711	0.710	0.711	0.711

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2: Distribution of Municipalities by Population (Years 2000-2007)

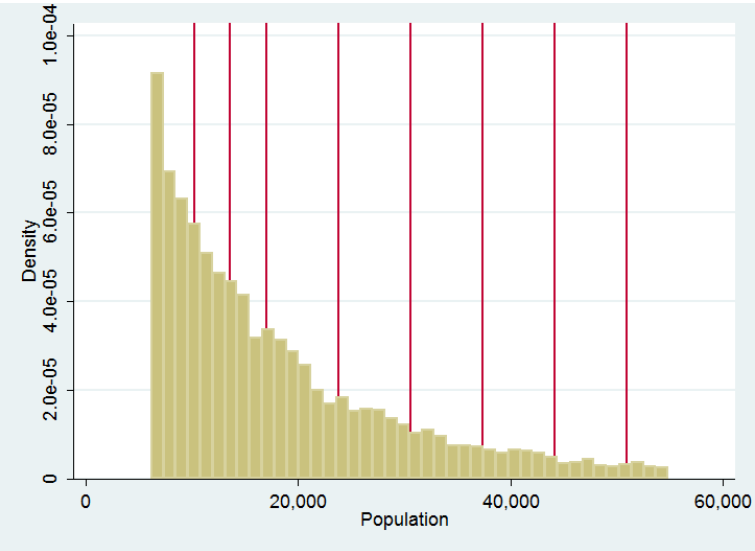


Figure 3: Density of Population - Nearest Cutoff, All Years

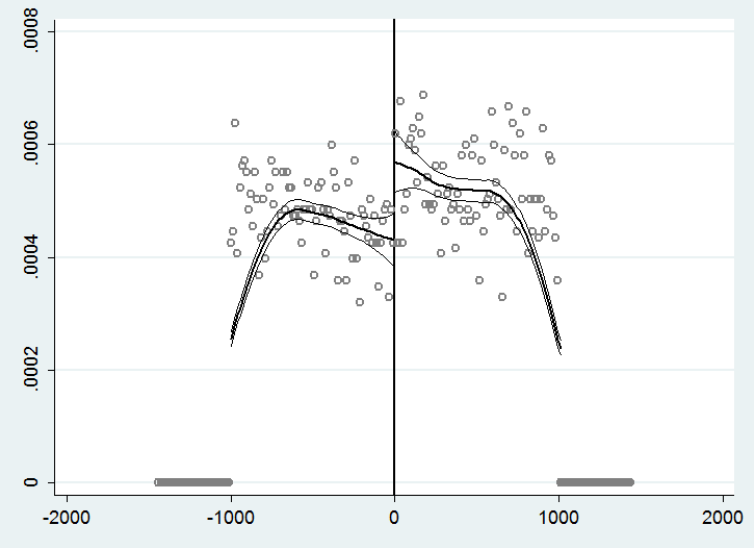


Table 4: Discontinuity Estimates: Expenditures

Ln Expenditure					
	(1)	(2)	(3)	(4)	(5)
Above Threshold	0.115***	0.032	0.049***	0.047***	0.052***
	(0.04)	(0.21)	(0.01)	(0.01)	(0.01)
Flexible Slope	1st Order	2nd Order	-	-	-
Same Slope	-	-	1st Order	2nd Order	3rd Order
Obs.	7696	7696	7696	7696	7696
R-Squared	0.687	0.687	0.686	0.687	0.687

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Density of Population - Nearest Cutoff, 2000-2007

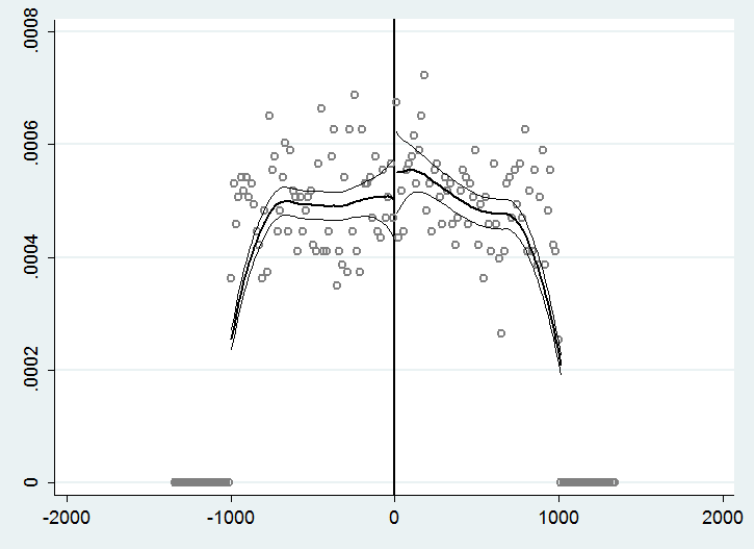
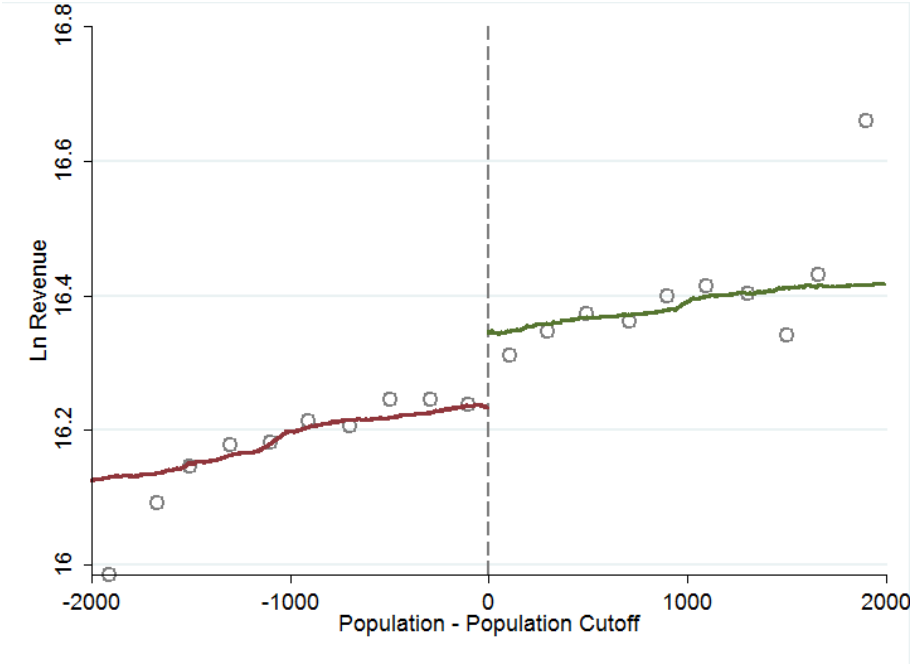
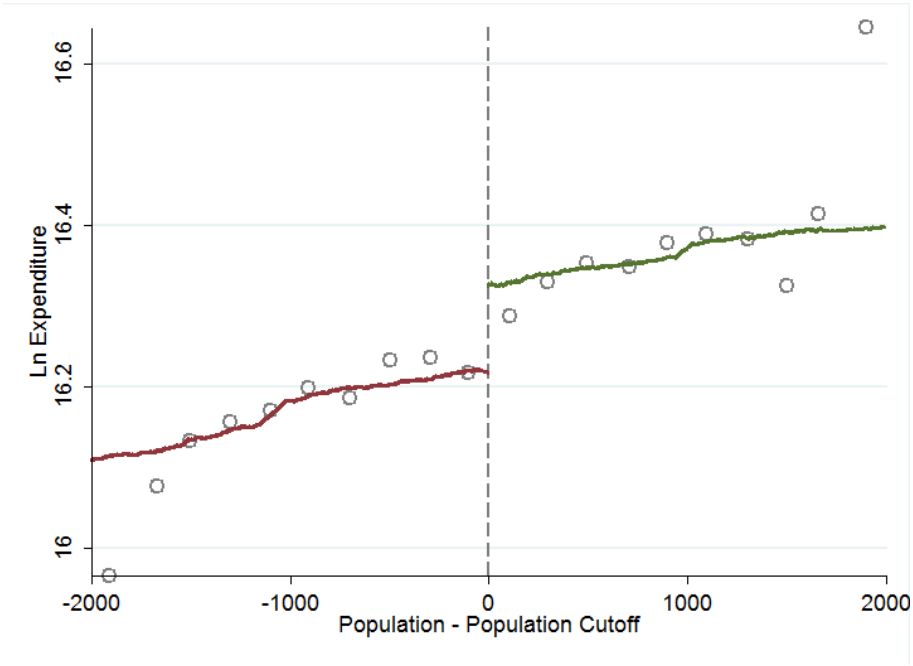


Figure 5: Revenues



Note: Open circles represent bin averages over bins of size 200, so that there are 10 bins on each side of the pooled cutoff. The sample is the analytical sample as described in Section 3.

Figure 6: Expenditures



Note: Open circles represent bin averages over bins of size 200, so that there are 10 bins on each side of the pooled cutoff. The sample is the analytical sample as described in Section 3.

Table 5: Discontinuity Estimates on Outcomes - Employment

Ln Employment					
	(1)	(2)	(3)	(4)	(5)
Above Threshold	0.124	0.845	0.030	0.026	0.019
	(0.11)	(0.67)	(0.02)	(0.02)	(0.03)
Flexible Slope	1st Order	2nd Order	-	-	-
Same Slope	-	-	1st Order	2nd Order	3rd Order
Obs.	8067	8067	8067	8067	8067
R-Squared	0.551	0.551	0.550	0.551	0.551

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Discontinuity Estimates on Outcomes - Wages

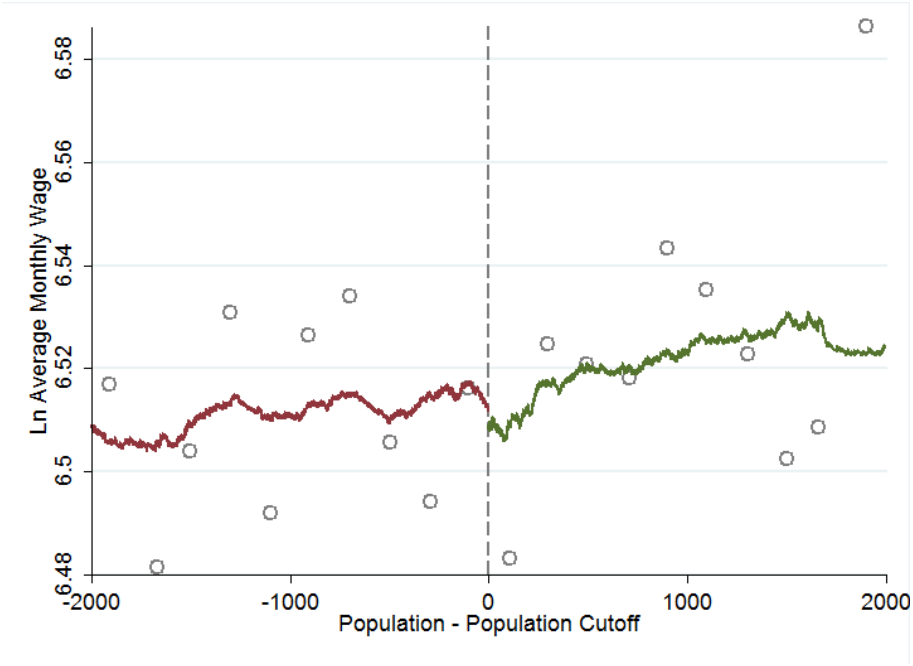
Ln Monthly Wage					
	(1)	(2)	(3)	(4)	(5)
Above Threshold	-0.030	0.220	0.005	0.007	0.007
	(0.03)	(0.22)	(0.01)	(0.01)	(0.01)
Flexible Slope	1st Order	2nd Order	-	-	-
Same Slope	-	-	1st Order	2nd Order	3rd Order
Obs.	8067	8067	8067	8067	8067
R-Squared	0.501	0.502	0.501	0.501	0.501

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 7: Outcomes - Employment

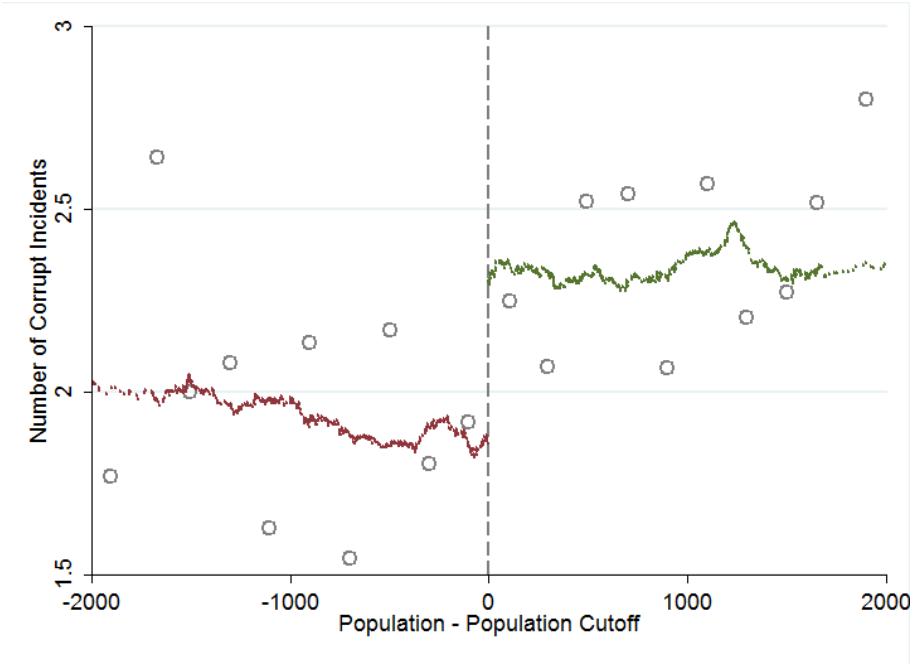
Note: Open circles represent bin averages over bins of size 200, so that there are 10 bins on each side of the pooled cutoff. The sample is the analytical sample as described in Section 3.

Figure 8: Outcomes - Wages



Note: Open circles represent bin averages over bins of size 200, so that there are 10 bins on each side of the pooled cutoff. The sample is the analytical sample as described in Section 3.

Figure 9: Number of Corrupt Incidents



Note: Open circles represent bin averages over bins of size 200, so that there are 10 bins on each side of the pooled cutoff. The sample is the analytical sample as described in Section 3.

Table 7: Discontinuity Estimates on Corruption - Number of Corrupt Incidents

Number of Corrupt Incidents					
	(1)	(2)	(3)	(4)	(5)
Above Threshold	0.472	-6.867	0.378**	0.378**	0.443**
	(0.89)	(4.84)	(0.17)	(0.17)	(0.17)
Flex Slope	1st order	2nd order	No	No	No
Same Slope	No	No	1st order	2nd order	3rd order
Obs.	928	928	928	928	928
R-Squared	0.365	0.371	0.365	0.365	0.367

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: OLS Estimates of General Spending Effects on Average Wage

Ln Monthly Wage				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.302***	0.304***	0.304***	0.304***
	(0.03)	(0.03)	(0.03)	(0.03)
Obs.	7696	7696	7696	7696
R-Squared	0.544	0.545	0.545	0.545

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: OLS Estimates of General Spending Effects on Total Employment

Ln Employment				
	(1)	(2)	(3)	(4)
Ln Expenditure	1.065***	1.063***	1.063***	1.060***
	(0.07)	(0.07)	(0.07)	(0.07)
Obs.	7696	7696	7696	7696
R-Squared	0.611	0.611	0.611	0.612

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: IV Estimates of General Spending Effects on Ln Monthly Wage

Ln Monthly Wage				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.147	0.183	0.194	0.194
	(0.16)	(0.17)	(0.16)	(0.16)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.533	0.539	0.540	0.540

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: IV Estimates of General Spending Effects on Ln Employment

Ln Employment				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.656	0.599	0.460	0.468
	(0.47)	(0.49)	(0.47)	(0.47)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.602	0.600	0.592	0.593

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: IV Estimates of General Expenditure Effects on Public Employment

Ln Public Employment				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.608	0.507	0.625	0.628
	(0.52)	(0.54)	(0.50)	(0.49)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.172	0.174	0.173	0.173

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: IV Estimates of General Expenditure Effects on Non-Public Employment

Ln Non-Public Employment				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.825	0.816	0.443	0.457
	(0.86)	(0.89)	(0.86)	(0.85)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.630	0.630	0.621	0.622

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: IV Estimates of Staff Expenditure Effects on Skilled Public Employment

Ln Skilled Public Employment				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.144	0.065	0.293	0.290
	(0.66)	(0.68)	(0.63)	(0.63)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.182	0.179	0.188	0.188

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: IV Estimates of Staff Expenditure Effects on Unskilled Public Employment

Ln Unskilled Public Employment				
	(1)	(2)	(3)	(4)
Ln Expenditure	1.325**	1.195*	1.285**	1.293**
	(0.65)	(0.68)	(0.62)	(0.62)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.120	0.131	0.124	0.124

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: IV Estimates of Staff Expenditure Effects on Public Wages

Ln Public Monthly Wage				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.399*	0.456**	0.394*	0.391*
	(0.21)	(0.22)	(0.20)	(0.20)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.515	0.512	0.516	0.517

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: IV Estimates of Staff Expenditure Effects on Non-Public Wages

Ln Non-Public Monthly Wage				
	(1)	(2)	(3)	(4)
Ln Expenditure	0.011	0.026	0.032	0.030
	(0.21)	(0.22)	(0.22)	(0.22)
Population Control Polynomial	1st Order	2nd Order	3rd Order	4th Order
1st Stage F-Statistic	39.59	37.93	44.34	44.66
Obs.	7696	7696	7696	7696
R-Squared	0.265	0.268	0.269	0.269

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Discontinuity Estimates of Staff Expenditure share of Expenditures

Workforce Share of Expenditure					
	(1)	(2)	(3)	(4)	(5)
Above Threshold	-0.007	0.023	-0.006**	-0.006**	-0.007***
	(0.01)	(0.06)	(0.00)	(0.00)	(0.00)
Flexible Slope	1st Order	2nd Order	-	-	-
Same Slope	-	-	1st Order	2nd Order	3rd Order
Obs.	7696	7696	7696	7696	7696
R-Squared	0.162	0.163	0.162	0.162	0.163

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$