The Interesting Case of Special and Extraordinary Items: What Are They and How Do They Influence Municipal Government Finances?

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Abstract: One-shot revenue shocks influence governments' budget decisions and the provision of public services. However, how governments respond to transitory income remains a theoretical and empirical puzzle. The permanent income hypothesis suggests that governments save revenue windfalls to smooth consumption across time. Alternatively, other theories suggest that windfalls will lead to significant spikes in current government spending. Extant studies on the effects of transitory income have produced mixed results because the revenue sources they examine may not be truly transitory. The case of special and extraordinary gains from uncommon and one-time events allows us to investigate the effects of truly transitory revenues. Taking advantage of the GASB requirement that governments report such gains in their financial statements, this study examines the effects of gains on governmental expenses for a sample of cities across ten years. Using a staggered adoption event study design, we find that transitory gains stimulate spending and that the size of gains matters before one observes the stimulatory effects. These results have substantial implications for budgetary transparency and fiscal sustainability in municipal governments.

Keywords: special and extraordinary gains, permanent income hypothesis, revenue windfall

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INTRODUCTION

External shocks such as natural disasters, economic recessions, and unexpected changes in intergovernmental transfers, among others, often cause drastic revenue fluctuations in local governments. For example, the recent COVID-19 pandemic resulted in a considerable loss of sales tax revenues in cities heavily dependent on travel and tourism (Chernick, Copeland, and Reschovsky 2020). Still, governments sometimes obtain one-time gains from intergovernmental transfers, the dissolution or annexation of other governments, and the sale of assets (Mead 2011). Revenue shocks, whether positive or negative, can cause spending policies to deviate from existing trends, affecting service provision. Facing sudden revenue decline due to external fiscal shocks, for example, local governments often engage in cutback budgeting, including reducing discretionary spending, delaying capital expenditures, and implementing layoffs (Jimenez 2017, 2022). Revenue windfalls, in contrast, can lead to the permanent expansion of budgets (Berset and Schelker 2020) or create opportunities for misuse of public funds (Nikolova and Marinov 2017), leading to future fiscal difficulties. Understanding the impacts of transitory revenue swings is crucial to ensuring the consistent and sustainable delivery of public services.

In this study, we explore the effects of windfalls on municipal government spending behavior. The literature on windfalls points to different potential effects of transitory income on local spending. One strand of the literature, which applies Friedman's (1957) permanent income hypothesis, argues that windfalls have a negligible effect on current budgets as governments use the one-time gain to increase savings and smooth spending across time (Holtz-Eakin, Rosen, and Tilly 1994; Dahlberg and Lindström 1998). This assumes that the average local official is a benevolent social planner and is forward-looking in making budgetary decisions. A different strand of the literature, which emphasizes politico-economic explanations of government fiscal behavior, suggests that vote-seeking politicians and budget-maximizing appointed officials are prone to budgetary myopia (Niskanen 1971; Brennan and Buchanan 1980; Persson and Tabellini 2000; Besley 2006) and that interest groups and some misinformed voters demand more services and patronage goods (Dougan and Kenyon 1988; Berset and Schelker 2020; Anzia and Moe 2015; Oates 1979). In this context, a revenue windfall can be a potent stimulus on current spending (Hines and Thaler, 1994; Berset and Schelker 2020). Extant studies provide mixed findings on the effects of windfalls, as they focus on revenues that are not truly transitory.

In this study, we focus on special and extraordinary gains as a type of transitory and non-recurring revenue. The Governmental Accounting and Standards Board (GASB) requires governments to report gains (and losses) from uncommon and one-time events separately from the common and recurring annual revenues and expenses. This requirement enables us to investigate the impacts of truly transitory income on spending. Using a sample of municipal governments with a population of at least 50,000 from 2004 to 2014, we find that after experiencing gains, cities increased expenses in succeeding years. To our knowledge, this is the first study of how special and extraordinary gains shape municipal government fiscal behavior. Our measures of government spending (and transitory gains) are based on full accrual accounting, which is more comprehensive than those used in previous studies and allows us to measure the full cost of operating government (Jimenez 2020). The empirical findings are robust to changes in estimation approach (difference-in-differences analysis, panel data models), model specification, and operationalization of key variables.

LITERATURE REVIEW

Revenue Windfall and Government Spending

There has been increasing attention in the literature on the impact of revenue windfalls on government budget decisions. Extant studies have focused on windfalls such as foreign aid, oil royalties, and excessive tax collections. Focusing on foreign aid, Abdelwahed (2021) proposes that governments may respond differently depending on the persistence and magnitude of aid. She finds that a permanent aid shock increases government spending by more than the amount of aid. In contrast, a temporary aid shock increases spending by less than the amount of aid, with the remaining used to reduce deficits. Focusing on oil royalties in Indonesia, Olsson and Valsecchi (2015) find that windfall increases district expenditure but has no effect on tax revenue. Cassidy's (2017) study, also of Indonesian local governments, distinguishes between permanent shock (stable increase in general grants) and transitory shock (revenues from the fluctuating oil and gas grant). Permanent shock stimulates greater provision of public schools, health facilities and personnel, and local roads, whereas transitory shocks have no significant effects. Besfamille et al. (2019) focus on Argentina and show that oil-producing provinces spend a portion of oil royalties to pay down debt but do not increase spending.

Ladd (1993) examines the effects of tax revenue windfalls from the Tax Reform Act (TRA) of 1986 and finds that a dollar of TRA windfall leads states to return around $0.6 to taxpayers and retain about $0.4 as state revenues. Berset and Schelker (2020) evaluate local governments' responses to excessive tax payments in the canton of Zurich in Switzerland. They find that the windfall increases personnel and administrative expenses and subsidies to local public entities and private individuals, and decreases income tax revenues. In a different study, however, Berset, Huber, and Schelker (2022) conclude that canton policymakers predominantly smooth expenditures using one-time tax gains.

The literature on intergovernmental grants also contributes to the discussion of revenue windfall by distinguishing between permanent and transitory trends. Deller and Walzer (1995) argue that local officials view intergovernmental transfers as either permanent (certain because the aid is increasing) or transitory (uncertain because of the downward trend). If there is a level of certainty or dependability to the aid money, local officials can count on those revenues and substitute aid for local money. If aid is viewed as transitory, local officials shift the use of funds to one-time expensive ventures such as construction projects or equipment purchases.

Empirical evidence so far indicates that local governments respond differently to permanent or transitory revenue shocks. A critical issue is that extant studies may not be comparable because they adopt diverse definitions and types of transitory incomes. Moreover, transitory revenues used in these studies may not be genuinely transitory. Foreign aid, oil royalties, and excessive tax payments may be unstable sources of income. Still, they are not unusual to state and local authorities (in the context they were studied) and will likely recur in the future. Similarly, the decline in intergovernmental grants does not necessarily indicate transitory income (and grant increase as permanent income). In this study, we focus on a one-time income in city governments in the United States, which is special and extraordinary gains.

Background on Special and Extraordinary Gains

GASB Statement No. 34 defines extraordinary items as "transactions or other events that are *both* unusual in nature and infrequent in occurrence," whereas special items are "significant transactions or other events within the control of management that are *either* unusual in nature *or* infrequent in occurrence" (GASB 1999, 23). GASB Statement No. 62 clarifies what "unusual" and "infrequent" mean (GASB 2010, 22). An event or transaction is unusual if it is highly abnormal and unrelated to, or only incidentally related to, the typical activities of the government. An event or transaction is infrequent if it is not reasonably expected to recur in the foreseeable future. The difference between extraordinary and special items is that the conditions for extraordinary items are stricter than those for special items. Specifically, extraordinary items are outside the control of management and are both unusual in nature and infrequent in occurrence. An example of special gains is the sale of properties for a government that does not commonly sell capital assets. Examples of extraordinary gains include the transfer of assets from other governments and gains accrued from discontinued operations.

Research on extraordinary and special gains has appeared mainly in the field of corporate finance and accounting.[1] Some studies show that in the private sector, managers use classification shifting of expenses and revenues between core items and special items to polish financial statements and meet the forecast earnings benchmark (McVay 2006; Fan et al. 2010; Esteban and Clemente 2008). Another line of research focuses on the relationship between unexpected gains and losses and private firms' performance. Studies find that extraordinary and non-recurring items influence firms' market value (Ballas 1999), future stock prices and returns (Dechow and Ge 2006; Burgstahler, Jiambalvo, and Shevlin 2002), as well as future profit margins and net income (Fairfield, Kitching, and Tang 2009; Jones and Smith 2011).

There are very few studies in the public budgeting and finance field that focus on special and extraordinary gains in government. The one published study that we found focuses on counties and cities in Virginia and concludes that gains generally meet the criteria of being unusual and/or infrequent (see Chase, 2007). This finding indicates that the reporting requirements stipulated by GASB are highly strict and that special and extraordinary gains can be viewed as truly transitory income in government budgeting. These gains allow us to test how

[1] The terms used in corporate accounting are slightly different from that of government accounting in that the Financial Accounting and Standards Board (FASB) uses the term "nonrecurring" instead of "special" items.

rarely occurring and recurring income sources affect local spending. We use the permanent income hypothesis to explore how gains influence municipal spending behavior.

THEORY

The permanent income hypothesis (or PIH) was originally developed by Friedman (1957) to model individual consumption behavior. It breaks down personal income into permanent and transitory components. Permanent incomes are stable revenue sources or expected long-term incomes, whereas transitory incomes are unexpected and infrequent. According to the PIH, individuals' expenditures do not respond to changes in current income but are driven by their expectations of lifetime average income. As a result, volatile transitory incomes will not change spending levels as individuals save that money for future consumption during negative transitory income shocks. That is, individuals engage in intertemporal consumption smoothing, maximizing utility by ensuring comparable consumption levels in current and future years by striking a balance between current spending and savings.

Holtz-Eakin, Rosen, and Tilly (1994) apply the PIH to explain state and local government spending trends. Applying the PIH to the public sector assumes that government officials and the median voter are forward-looking decision-makers who consider revenue and spending obligations in more than one period. In their adapted PIH, Holtz-Eakin, Rosen, and Tilly (1994) do not distinguish between the government official or representative voter, implying a generic decision maker. The assumption of a forward-looking decision-maker suggests two things. First, when making budget decisions, the decision maker will consider the utility of consuming current versus future services. Second, to smooth government consumption, the decision maker can either spend future income to finance current spending through borrowing or save current income for future consumption. When revenue increases in the current period, the decision maker faces the tradeoff of spending the revenue in the present or saving it for future consumption. The adapted PIH predicts that if the revenue spike is a one-shot event, the forward-looking decision-maker will save the windfall to avoid drastic spending fluctuations across time. This means that windfalls do not significantly increase current spending. Suppose the revenue increase is expected to last longer, such as a permanent expansion of the local tax base. In that case, the decision maker will increase the government's budget because it can now afford to spend more in future years without the need to increase savings in the present period.

[Figure 1 here]

Figure 1 depicts a local community's choice to spend in the present or future period. Specifically, the horizontal axis C_t represents the consumption of governmental goods in the current year, and the vertical axis C_{t+1} represents consumption in the succeeding year. The intertemporal budget constraint is represented by A_0A_1, and the community maximizes its utility at point e_0, where the indifference curve is tangent with the budget line. As a result, the expenditure level for the current period is at G_0. An increase in the current period income pushes the budget constraint outward from A_0A_1 to B_0B_1. However, according to the PIH, this is true only in the case of a permanent income increase. If the revenue growth is due to a permanent income flow, then current and future spending will increase, following the consumption path GCP_p. On the other hand, if it is a transitory shock that causes current revenues to rise, then the revenue windfall will be saved for future consumption, and the utility-maximizing point remains at e_0, indicating no change in governmental spending.

This theoretical framework has been applied to various contexts to test government spending behavior (see Holtz-Eakin, Rosen, and Tilly 1994; Dahlberg and Lindstrom 1998; Borge and Tovmo 2009; Donovan 2009; Persson 2016). However, the literature is not conclusive regarding whether forward-looking decision-makers govern subnational governments. Using aggregated state and local government data, Holtz-Eakin, Rosen, and Tilly (1994) find that permanent resources do not determine spending and that governments do not smooth consumption over time. On the other hand, Dahlberg and Lindstrom (1998) apply the PIH model to Swedish municipalities and find that 90 percent of Swedish municipal expenditures are associated with permanent incomes. Subsequent studies conclude that local government spending behavior tends to fall between perfect forward-looking and outright myopic (Berset and Schelker 2020; Berset, Huber, and Schelker 2022).

Although the mixed evidence on consumption smoothing in previous studies leads to questions about the assumption of a forward-looking government, the public budgeting literature has documented various types of rational financial planning practiced by subnational governments suggesting that government officials are not short-sighted. For example, Hou (2006) finds that the adoption of countercyclical fiscal tools like the budget stabilization fund by state governments helps in buffering budgets from external fiscal shocks. Jimenez (2013) examines how strategic or long-range planning affects cities' financial performance during the Great Recession. In his sample of 1,778 municipalities, 63 percent had a long-range plan, of which 74 percent had plans linked to budgets. Jimenez (2019) identifies three activity streams of financial recovery planning of city governments: budget diagnosis, short-term plan, and long-term plan. He finds that planning is associated with robust budgetary solvency. These studies suggest that the behavioral assumption of a forward-looking decision-maker is not far-fetched. Based on the adapted PIH, we propose that an increase in special and extraordinary gains will not systematically affect municipal government spending.

RESEARCH METHODOLOGY

To test the hypothesis, we focus on cities with a population of 50,000 or more. We identify the cities using the results of the 2007 Census of Governments, which lists 674 cities that meet the study's population threshold. This approach ensures that we have a consistent sample of cities across the years covered in the analysis or from 2004-2014. Using the initial list of cities, we gather data from different sources, such as the Census of Governments, Annual Survey of State and Local Government Finances, American Community Survey, and Annual Comprehensive Financial Reports or ACFRs.

Main Outcome and Independent Variables

We use data from ACFRs to measure the main outcome and independent variables. These reports follow Generally Accepted Accounting Principles (GAAP) and other guidelines issued by GASB (Mead 2011; Finkler et al. 2012).[2] Our primary source of information is the government-wide financial statements, which employ an economic resources measurement focus. This means that they report information on all economic resources of governments including all assets and liabilities – both current and non-current (GASB 1999). Current assets include cash and other resources readily convertible into cash, whereas non-current assets include, among others, capital assets such as buildings, equipment, or land. Current liabilities include payables within a year, such as amounts owed to vendors and employees, whereas non-current liabilities are mostly long-term debt (see Mead 2011). The statements are also prepared using full accrual accounting, which records revenue when it is earned, whether cash is collected or not, and reports an expense when an economic resource is used to provide services, whether

[2] Research staff downloaded ACFRs from city websites or requested directly from city officials, and manually recorded relevant financial information in Excel.

payment is made or not (Finkler et al. 2012). All costs incurred in the current period are reported in that period regardless of the timing of the resource outflow (GASB 1999).[3]

Our main outcome variable is per capita direct expenses of governmental activities (GA), which cover the government's basic services (e.g., police, fire, or park services) funded through taxes and grants. GA differ from business-type activities (BTA) in that the latter includes services financed by user fees and charges (Mead 2011). According to GASB Statement 34 (GASB 1999, 18) "Direct expenses are associated with a service, program, or department and thus are clearly identifiable to a particular function."[4] Direct expenses cover operating and nonoperating expenses, including the depreciation of long-lived assets. Our main independent variable is per capita GA special and extraordinary gains, previously defined.

Covariates

We deliberately include a parsimonious set of control variables that studies have shown to affect government spending. We control for sociodemographic factors. Per capita income proxies for the local revenue base. Wealthier jurisdictions have a greater capacity to support local spending (Jimenez 2022). Ethnic fragmentation can drive up patronage spending and reduce the willingness to pay taxes (Alesina, Baqir, and Easterly 1999). We also include variables that reflect differences in revenue and service authority as well as intergovernmental and local governance contexts. Cities' different revenue authority and service responsibilities determine their expenditure levels (Ladd and Yinger 1989). Cities are highly reliant on property tax to support local spending. To measure property tax dependence, we include property tax

[3] For example, the full labor costs in the current period include not only salaries or wages but also a portion of the benefits already earned by employees. Even though the benefits are to be paid out in the future, the government is obligated to set aside funding for them in the current period when the employee provided a service and earned a portion of the benefits as part of her compensation. If a city does not meet its required annual benefit plan contributions, the underfunding is reported as a liability for the fiscal year (Mead 2011).
[4] A "function" is a general category of related services e.g. public safety, which may include police, fire protection, emergency management, and inspection services, among others.

revenues as a percentage of total tax revenues. The functional performance index developed by Clark and Ferguson (1983) measures the differences in service responsibilities across cities. We include government form based on the finding that the mayor-council form is associated with city budget outcomes (see studies reviewed in Jimenez 2020). Lastly, cities receive grants from the federal and state governments to deliver services. The models include intergovernmental revenue as a percentage of the city's own-source revenues. We use natural log transformations of all continuous variables. In addition, we convert fiscal and income measures to the year 2000 dollars to adjust for inflation using the implicit price deflator from the US Bureau of Economic Analysis. Table 1 contains the basic descriptive statistics.

[Table 1 here]

Estimation Strategy

Our primary estimation strategy is to employ a staggered difference-in-differences regression (DID) from Callaway and Sant'Anna (2021), which is a quasi-experimental approach that attempts to establish causality even when the sample is not randomly selected. Traditional DID compares the outcomes between a control group (not exposed to a treatment) and a treated group (exposed) across time, or before and after treatment. Here, the treatment is the gains, and the outcome is expenses. We assess the effect of gains by comparing how expenses changed pre- and post-treatment between the treated and control groups. Briefly, the assumption behind DID is that in the absence of the treatment, the unobserved differences between treated and control groups will be the same across time. Thus, DID is useful even if exposure to the treatment is not random. DID removes biases in post-intervention comparisons caused by unobserved differences between the treated and control groups, as well as biases in pre- and post-treatment comparisons in the treated group caused by potential confounders (see Angrist and Pischke 2008).

Traditionally, DID regressions are estimated under a two-way fixed effects (TWFE) and event study framework where the treatment occurs at the same time point for all treated units. However, in our study, the treatment – the receipt of gains – does not conform to this standard. These gains occurred at distinct time points for different cities, leading to a staggered adoption of the treatment. Recent advancements in the study of panel data have emphasized potential drawbacks when using the TWFE model in staggered DID settings. A significant issue is the risk of creating flawed comparisons when early adopters serve as counterfactuals for late adopters (Goodman-Bacon 2021). In our study, using cities that received gains early in the panel series to serve as counterfactuals for those receiving them later can result in biased estimates, particularly if there is heterogeneity in the treatment effect as there is likely to occur under the permanent income hypothesis. Moreover, the TWFE model might not account for potential differences in treatment effects across various units, further undermining its reliability for DID regressions under these circumstances (Callaway and Sant'Anna 2021).

In light of these issues, we employ the doubly robust DID estimators proposed by Callaway and Sant'Anna (2021). This approach offers several advantages that address the issues associated with bad comparisons and heterogeneous treatment effects. Firstly, the doubly robust DID estimator ensures that only not-yet-treated or never-treated units are used as controls for the treated group, eliminating the potential bias introduced by early adopters. Secondly, this model incorporates covariates into the estimation process. Specifically, the covariates are leveraged in both the inverse probability weighting (IPW) stage as well as the final conditional outcome regression. Within the IPW model, covariates inform the estimation of the propensity score, which then serves to weigh the treated and control groups. This weighting ensures a balanced distribution of covariates between the groups, reducing the bias arising from pre-existing differences and solidifying the parallel trends assumption. In the outcome regression model, covariates adjust the estimated relationship between the treatment and the outcome for the control group. This adjustment predicts the expected outcome of the untreated units, accounting for variations in their covariate profiles. It ensures that the estimated treatment effect accurately reflects differences attributable to the treatment, rather than to other factors. This dual inclusion helps satisfy the conditional parallel trends assumption and ensures robustness against potential model misspecification.[5] We implement the DID event study following the equation:

$$\ln(E_{it}) = \alpha + \sum_{e=-K}^{L} \beta_e D_{it}^{e} + X'_{it}\delta + \epsilon_{it}$$

where E_{it} is the log of per capita expenses of city *i* in year *t*, D_{it}^{e} is an indicator of city *i* being *e* periods away from the initial revenue shock at year *t*, *K* and *L* are positive constants representing the largest pre- and post-treatment periods in our sample, respectively, and X' is a vector of control variables. The parameter of interest, β_e, denotes the group average stimulative effect of revenue shocks at different lengths of exposure to the shock.[6]

An additional issue that merits attention is that DID analysis is typically applied to settings where the treatment is a binary variable (treated or not treated). However, in the context of this study, the treated group received the treatment (gains) in different dosages, with some cities experiencing much larger revenue shocks than others. Considering this, our approach involves two distinct stages. Initially, we study the binary treatment effect, treating all cities within the treated group homogeneously, that is, without regard to differences in the size of gains. This approach provides a baseline understanding of the impact of the treatment.

[5] The structure of the doubly robust estimator ensures that it remains consistent if either the propensity score model or the outcome regression model is correctly specified.

[6] Unlike the traditional DID event study, the Callaway and Sant'Anna (2021) event study lacks a reference point due to its unique methodology of dissecting and then aggregating treatment effects across multiple groups and periods. This design facilitates a holistic view of treatment effects without anchoring results to a singular reference moment.

However, we are also interested in understanding whether the treatment effects vary depending on the magnitude of gains. To explore this, we divide the treatment group into two subgroups: cities that experienced high revenue shocks and those that experienced low revenue shocks. To divide cities into "high" and "low" treatment groups, we use a least squares dummy variable regression with a quadratic specification in the revenue shock size, and use the implied turning point to determine the low-to-high cut-off threshold.

RESULTS

Special and extraordinary gains are indeed very unusual and infrequent. Because some cities do not have ACFRs in certain years, the number of cities varies yearly. Focusing on cities where we have data for GA expenses, the total number of observations is 6,057, or an average of 605 cities per year. Of these, there are only 129 instances of cities reporting gains. The gains are economically significant, with an average of $26.9 million (in year 2000 dollars), but with a considerable range – a minimum of $10,392 and a maximum of $287 million. Cities with gains are scattered across 25 states, with more cities located in larger states such as California and Texas. This is expected as larger states have more mid-sized and large cities than smaller states. Figure 2 shows the average total gains by year and city size. Cities are categorized into tertiles based on population: small (population below 68,738), medium (68,739 to 111,145), and large cities (more than 111,145). Notable peaks in gains are observed in 2005 and 2012, particularly among medium and large cities. Conversely, the years between 2008 and 2010 as well as 2013 and 2014 exhibit minimal to no gains across all city sizes. Atop each bar, the numbers indicate the count of cities contributing to that year's average gain, with 2012 showing a higher count for medium and large cities. The chart also shows several years where gains are zero, especially for smaller cities, depicted with hatched bars.

[Figure 2 here]

Because ACFRs for some cities only include the basic financial statements (government-wide and fund) but not a Notes section (which contains required disclosures on gains and other transactions), we are unable to examine the nature of all gains reported in the sample.[7] We focus instead on a random sample of 30 percent of ACFRs with a Notes section. In general, gains resulted from either the transfer of assets from another public entity, or the sale of a city's capital assets. Some 51 percent of gains were from the transfer of assets from another government or agency (48 percent from a dissolved agency, and 3 percent from an annexed government such as a special district). Transferred assets include cash, short- and long-term investments, and fixed infrastructure. Approximately 49 percent of gains were from the sale of capital assets such as parks or buildings. That asset sales involve land and facilities is not surprising given that these are typically the most expensive tangible assets owned by cities.

Primary Results

Figure 3 presents the results of the doubly robust DID regression using an event-study approach with covariates.[8] Some cities report more than one gain or both gains and losses during the study period. To obtain a clean sample for the DID analysis, we focus on cities that report a single gain and no losses during the period, reducing the number of cases with gains from 129 to 71. Figure 3 represents the baseline DID analysis where the treatment variable is a binary variable equal to 1 if a city-year experiences a gain (regardless of the magnitude) and 0 otherwise. The horizontal axis displays the lengths of exposure to the revenue shock or gain, with 0 as the shock year. The vertical axis displays outcome differences (log of per cap GA expenses)

[7] Specifically, earlier year ACFRs that were scanned by city officials upon our request.
[8] Our robustness check confirms that the parallel trends assumption also holds without covariates. For a detailed examination, please refer to the supplementary materials available online.

between treated and control groups. Each bar denotes the point estimate (β_e) for the average treatment effect for a specific period e.g., $\beta_{-1\ (\text{to}\ -4)}$ represents pre-treatment differences one (to four) year(s) prior to the revenue shock year, and $\beta_{1\ (\text{to}\ 4)}$ represents post-treatment differences one (to four) year(s) after the shock period. The darker bars and circles represent pre-treatment differences and lighter ones with diamonds for post-treatment, with the bar width indicating a 95 percent confidence interval (thus our threshold for statistical significance is $p < .05$).

[Figure 3 here]

We focus on outcome differences rather than displaying separate trend lines for the control and treated groups because cities experienced gains in different years. Presenting the data in this manner also effectively provides evidence for parallel trends. Looking at Figure 3, the consistency in patterns before the shock year (0-point on the horizontal axis), especially with the clustering of the β_{-1} to β_{-4} coefficients around 0, indicates that the outcomes for both groups were largely similar before the shock, thereby supporting the parallel trends assumption. Post-treatment, compared to cities that did not experience gains, those that had gains initially reduced their expenses as indicated by the negative and statistically significant coefficient of β_1 (= -0.04, $p < 0.01$). However, they increased expenses in subsequent years as evidenced by the positive and statistically significant coefficients β_2 (= 0.09, $p < 0.05$) and β_3 (= 0.08, $p < 0.01$). The effects of gains dissipate in later periods, as indicated by the statistically insignificant coefficient in year four (β_4) (Subsequent analysis of subgroups of cities indicates that this result arises from the aggregation of data that includes cities with low gains – see below).

We estimate the magnitude of the positive impact of gains. In a DID with a log-transformed outcome variable, the coefficient $\beta_{e\geq 0}$ means that after experiencing the gain (i.e., D_{it}^{e} switches from 0 to 1 for $e \geq 0$), the per capita expenses in the treatment group increase by approximately 100*(exp(β)−1) compared to what we would expect had the gain not occurred, keeping all other factors constant.[9] Focusing on β_2, 100*(exp(0.09)-1) represents approximately a 9.71 percent increase in per capita expenses. Using the shock period 0 as the benchmark (because the change in expenses is 0 at this period), the average per capita expense in the treated group is $849.18. Multiplying this figure by 9.71 percent, the expected increase in per capita expenses when the average city experiences a gain is $82.49. Given that the average population among the treated group is 205,082, the expected increase in total expenses is $16,918,131.

As mentioned previously, the magnitude of the treatment, or the size of gains, varies considerably across cities. Therefore, relying solely on the binary treatment variable employed in our baseline analysis might mask the potential heterogeneous treatment effects of the revenue shock. Specifically, we postulate that the treatment effect differs based on the size of the gains. To explore the possible heterogeneous treatment effect, we modify our approach by segmenting the continuous treatment variable (gains) into two categories: low gains (or low revenue shock group) and high gains (or high revenue shock group). To motivate this division, we tested a series of Least-Squares Dummy Variables (LSDV) models with a quadratic term of the log of per capita gains, focusing solely on the treated group. This model serves several purposes. First, it serves as a robustness test, confirming DID findings on the effects of gains. Second, the LSDV regression includes a larger sample of cities with gains, improving the statistical power of the model. Third, by operationalizing gains as a continuous rather than a binary variable (as required in DID regression), LSDV provides us with straightforward information on how the magnitude of gains, rather than simply their occurrence, matters for spending. Finally, it provides empirical

[9] This interpretation is valid for coefficients that are relatively small in magnitude (typically less than 0.1 in absolute value) when using a log-transformed outcome.

backing for our subgroup analysis by highlighting any potential non-linear relationship between the gains and their subsequent effects.

The results of the LSDV models, presented in Table 2, indicate a U-shaped relationship between special gains and GA expenses. This is evidenced by the positive and statistically significant coefficients for the quadratic term, and a test confirming the presence of the U-shaped relationship suggests that it is not random (The online appendix provides a fuller discussion of the LSDV results). We use the results for panel 1 (with the largest sample size) to calculate the implied turning point, which is 3.48 in natural log or approximately $32.48 when exponentiated. Cities with per capita gains below this amount see a reduction in expenses even as gains increase. However, as per capita gains exceed $32.48, the stimulative effects become apparent with expenses growing larger for every additional dollar of gains. In the sample, the median per capita gain is $58.53. Hence, the stimulative effect begins quite early.

Next, we run DID event studies for subsamples using the previously calculated turning point of $32.48 as the threshold to divide the treatment group into high- and low-revenue shock groups. Figure 4 presents the results of these event studies, with the left panel illustrating the impact of high per capita gains on expenses, and the right panel depicting the impact of relatively low per capita gains. Our findings indicate that the effects observed earlier are primarily driven by the sample with higher gains, while per capita gains below $32.48 do not affect expenses in a statistically significant way. However, we should be careful about putting too much emphasis on this result as the subsample analysis involves subgroups with a smaller number of cities with gains, which can make it harder to detect true treatment effects.

[Figure 4 here]

Additional Tests

We tested other doubly robust DID event studies to further establish the robustness of our findings and to explore additional related questions. First, it is possible that governments spent part of the gains and saved the rest, assuming that government spending behavior falls between forward-looking and myopic.[10] We use the unrestricted net position of governmental activities as our measure of savings. Unrestricted net position can be used for whatever purpose and functions as a reserve (Mead 2011; Jimenez 2017). Figure 5 shows the DID event study results with the log of per capita unrestricted net position as the outcome variable. We find limited support for any systematic relationship between gains and savings.[11] Figure 6 shows the heterogeneous effects, using the same high and revenue shock subgroups in Figure 4. Gains do not show any statistically significant effects on savings in either group. Second, we operationalize the outcome and main independent variables differently, using total rather than per capita measures. The results are consistent with our main findings (See online appendix for other robustness tests).

[Figures 5 and 6 here]

DISCUSSION AND CONCLUSION

Our findings are consistent with Holtz-Eakin, Rosen, and Tilly's (1994) conclusion that state and local governments in the U.S. do not smooth their expenditures over time, and contrary to the results in some research focusing on European countries (Dahlberg and Lindstrom 1998; Berset, Huber, and Schelker 2022). In the LSDV models, we see a U-shaped relationship where smaller gains decrease expenses until after a threshold where larger gains produce the opposite effect. In the DID regression, we see that gains reduced expenses a year after the shock event and stimulated expenses in subsequent years. These combined results (LSDV and DID) suggest the

[10] Revisiting figure 1, this prediction means that the budget line shifts only slightly to the right of A_0A_1 but below B_0B_1

[11] We find that in the year of experiencing gains, the log of per capita unrestricted net position increased by 0.21. However, this effect is statistically significant only at the 90 percent level ($p < 0.1$).

presence of a U-shaped relationship not only between the size of gains and expenses but also between the timing of gains and expenses.

Why would gains initially reduce expenses? One explanation is responsibility shedding either through an asset sale or the transfer of liabilities to an independent entity. Our analysis of a random sample of gains showed that almost half of the instances of gains were a result of the sale of city capital assets. Once cities sold assets such as parks and buildings, they no longer needed to spend money for the operation and maintenance of such assets, thus reducing expenses. The remaining half arose from transfers of assets and liabilities from dissolved agencies. But even here, some cities had the option to shed inherited liabilities. In one state, for example, the state government's decision to dissolve redevelopment agencies led to the transfer of those agencies' assets and liabilities to city governments. Some of these cities then transferred the liabilities to an independent private-purpose trust fund for final disposal.[12] The reduction in city government expenses reflected the transfer of liabilities to the trust fund.

Another explanation, beyond responsibility shedding, is the potential efficiency returns from the annexation of another government such as a special district (Liner 1992). Municipalities can achieve economies of scale by merging with other governments. Economies of scale refer to the decrease in the average per unit price of a good or service as the scale of production increases (Oakerson 1999). Larger governments can expand production given a larger consumer base, the ability to invest in more sophisticated technical equipment, and better bargaining power to purchase inputs at lower prices (Boyne 1992). However, given that annexations constitute a very

[12] Briefly reviewing some aspects of government financial reporting is helpful here (A fuller discussion of the complex system of governmental financial reporting is beyond the scope of this study but an excellent and highly readable source is Mead 2011). Trust funds are fiduciary in nature and are not considered part of governmental activities. Government does not own fiduciary funds and, thus, cannot use resources in those funds to support governmental activities. Resources in trust funds are merely held in "trust" by the government on behalf of others outside the government (Mead 2011, 9-10).

small portion of the sample, the efficiency explanation for the initial reduction in expenses is less likely than the simpler explanation that cities shed responsibility for certain liabilities.

The unanswered question is, why would expenses increase after cities shed or transferred responsibilities for certain liabilities? This result strongly indicates that cities used the windfall to support expenses in succeeding years rather than increase savings, contrary to the prediction derived from the adapted PIH.[13] Indeed, we not only find that gains increased expenses, but that gains had no systematic effects on reserves.

Is it possible that the increase in expenses reflects liabilities incurred for acquiring expensive assets? Our measure of expenses does include operating and non-operating costs, and the latter covers expensive items such as the acquisition or construction of a long-lived fixed asset. GASB's (1999) rationale for including non-operating costs as part of current expenses in government-wide statements is that when a government provides a service, it does not only consume labor (e.g., police officers) and supplies (e.g., police uniforms) but also assets such as facilities (e.g., a building that houses the police department) or equipment (e.g., police cars). Thus, the full costs of providing a service in the current period do not only include personnel salaries and payments for supplies but also the costs of using long-lived assets, which is consistent with the economic resources measurement focus (GASB 1999). Yet, reporting the total construction or acquisition costs of the assets during the budget period when they were constructed or acquired overstates the government's current spending as these assets are not only very expensive but can be used for many years. It is important to highlight that because the data

[13] The case of gains from asset sales is straightforward: the proceeds from the sale flowed directly to government coffers and were used to fund current expenses. The case of gains from dissolved agencies is harder to explain because cities were supposed to transfer not only those agencies' liabilities to a trust fund, but also the assets. One study suggests that some cities were still able to use the dissolved agency's assets. Stephens and Fulton (2012) document how some city governments in one state were able to use redevelopment agencies' monies to pay for a portion of the salaries of some city personnel including city managers and police officers. A deeper examination of this issue requires a comprehensive forensic accounting investigation, which is beyond the scope of this study.

we use are based on full accrual accounting, the non-operating costs reflect annual depreciation expenses and not the full costs of the assets. The depreciation expense spreads the construction or acquisition costs (often in equal amounts) over the estimated number of years that the asset is expected to be used (Mead 2011). The annual depreciation expense, in essence, represents the cost of consuming a portion of the asset to provide a service each year. Thus, the increase in expenses that we observed is not caused by using gains to acquire expensive assets. Our findings on expenses (and savings) do not support the assumption about local officials' forward-looking behavior in budgetary decision-making.

Several factors can prevent local governments from making the rational decision to save windfall revenues to smooth future spending. Theories from political economy point to the role of politics, specifically how local institutional arrangements, political actors, and the incentives that they face shape local budget choices. One explanation focuses on the role of local government decision-makers – both elected and appointed. Short electoral cycles can disincentivize elected policymakers from taking a longer-term view of budget decisions (see Jimenez 2020). As Raveh and Sur (2020, 1) aptly summarize, "Re-election considerations shorten political time horizons and give rise to political myopia." Specifically, the pursuit of votes motivates politicians to use one-time gains to increase patronage spending (Weingast, Shepsle, and Johnsen 1981; Persson and Tabellini 2000; Besley 2006). It is not only vote-maximizing politicians who demonstrate a spending bias but also appointed public managers. Niskanen (1971; also see Schneider 1986), for example, points to bureaucrats who desire higher budgets to increase their salaries and non-monetary benefits from their official positions.

Local budgetary institutional arrangements give government officials wide latitude to shape budget policies, which may not help the goal of promoting fiscal discipline. Filimon, Romer, and Rosenthal's (1982) agenda control model describes a budgetary arrangement where the government official, or local budget setter, monopolizes budget formulation and presents voters with budget packages in a referendum. The budget setter hides information from voters about the true level of exogenous revenue that the community receives. The setter also limits voters' choice between a budget package preferred by the setter and higher than what the median voter demands, or a reversion budget that already incorporates the hidden exogenous revenue but is far below the median voter's preferred amount. Rather than accept the reversion budget and potential service cuts that it entails, voters select the higher budget. Of course, city budgets are seldom decided through referendums.[14] The point of the agenda control model is that the monopoly position of government officials in budgeting enhances those officials' ability to increase spending regardless of citizen preferences.

A second explanation points to the role of interest groups and lobbyists, who can exert significant influence on government spending decisions. Some argue that policymaking at lower-level governments is especially susceptible to interest group capture (see Miller 2008; Anzia and Moe 2015). When windfall revenues become available, interest groups may lobby for a greater share of the pie. This can lead to one-time gains being directed towards budget items, services, or projects with strong lobbying efforts or interest group backing, rather than being saved for future needs (see Dougan and Kenyon 1988; Berset and Schelker 2020; Anzia and Moe 2015).

Finally, taxpayers' fiscal choices might prevent the use of windfalls for savings and consumption smoothing. In the fiscal illusion hypothesis, residents misperceive the tax price of government services because the true costs are hidden or obscured. For example, studies show that local governments use unconditional lump sum grants from higher-level governments to

[14] Filimon, Romer, and Rosenthal (1982) developed their budget model for application to school districts.

supplement funding for local services (Wyckoff 1991; Oates 1999; Dahlberg et al. 2008).[15] Believing that the tax price of local public services is low (as these services are effectively subsidized with external grant money), residents demand more services, inducing government expansion. Gains may have a similar "illusionary" effect: they facilitate budgetary expansion without inflicting additional costs on residents in the form of tax increases, at least in the immediate term (After all, if one-time gains lead to a permanent increase in current expenses, then future governments will need to find other ways to finance the permanent expansion, whether through higher taxes, increased reliance on debt financing, or both).

The findings from this study have substantial implications for budgetary policymaking in city governments. In particular, our findings suggest that we cannot ignore how special and extraordinary gains can potentially shape government accountability and fiscal sustainability. It is useful to contrast gains against regular sources of revenues such as taxes and service fees. Own-source revenues such as taxes and fees tend to demonstrate greater predictability and constancy. In contrast, special and extraordinary gains are inherently unpredictable, arising from discrete, often unforeseen events, such as the sale of municipal assets or the uncommon dissolution of another governmental entity. Public acceptance and scrutiny of budgetary choices differ based on the revenue source. As Paler (2013, 706) avers, "windfalls undermine – and taxes strengthen – citizen demand for good government." Residents are accustomed to the routine nature of taxes and fees as they must pay these immediately, directly, and regularly. Any sudden increase in these revenue sources can lead to greater scrutiny of budget decisions. In contrast, gains do not involve direct and immediate costs to current taxpayers, and the gains' infrequent nature and high fluctuation mean that residents are likely to have minimal knowledge about the

[15] This is known as the flypaper effect because "money sticks where it hits" (Oates 1999).

availability, magnitude, and allocation of such gains. The fact that gains are "costless" to current taxpayers, combined with the information deficit, can undermine residents' willingness and ability to monitor local budgetary decision-making (Paler 2013). Largely hidden from the public's view, gains can be used to expand local budgets. Without any formal process of public input, the larger the windfall, the greater the ability of local officials to increase expenses.

It is possible, of course, that increasing government spending will improve residents' welfare. We cannot be certain that this is the case because we do not have information on whether the growth in current expenses for services aligns with residents' preferences. The stimulatory effects of windfalls suggest that greater transparency is needed to help voters understand the sources and nature of special and extraordinary gains and the implications of such gains on local spending and fiscal sustainability.

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Table 1
Basic Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Dependent Variables				
Per capita GA expenses (in $)	924.93	616.28	316.14	3,297.06
Total GA expenses (in million $)	242.00	538.00	20.80	4,160.00
Independent Variables				
Per capita GA special and extraordinary gains (in $)	223.64	420.07	0.05	2,274.50
Total GA special and extraordinary gains (in million $)	26.90	49.90	0.01	287.00
Per capita income	19,665.97	6,955.80	8,854.32	62,641.77
Ethnic fragmentation $1 - \sum_i^j (Race_i)^2$; where *Race i* denotes the share of population identified as race *i*, including white, black, Hispanic, Asian and Pacific Islander, and American Indian. Multiplied by 100 to convert to percentage, with higher values indicating greater ethnic heterogeneity	53.96	14.15	9.56	75.95
% Employed in public administration	4.68	2.59	0.27	14.63
Functional performance index $\sum(F_i W_i)$; where $W_i = E_i / N_i$ or the weight for subfunction *i*, E_i is per capita expenditure in all cities for subfunction *i*, N_i is the number of cities performing subfunction *i*, F_i is performance of subfunction *i*, which is 1 if city performs subfunction *i*, and 0 if city does not perform subfunction *i*.	5.84	30.22	0.31	333.46
Property tax as % of total taxes	54.76	19.18	9.83	100.00
IGR (intergovernmental revenues) as % of own-source revenues	15.76	15.41	1.89	78.94
Council-manager	0.76	0.43	0.00	1.00

Note: Summary statistics for a sample of 129 cities that experienced gains during the study period. Data sources include the Census of Governments, Annual Survey of State and Local Government Finances, American Community Survey, Annual Comprehensive Financial Reports, and the International City/County Manager Association Municipal Government Form Survey

Table 2

LSDV w/ Newey-West Heteroskedasticity- and Autocorrelation-Robust Standard Errors
Dependent Variable: Per Capita Direct Program Expenses of Governmental Activities

Independent Variables	**Quadratic Model**					
	Panel 1		**Panel 1**		**Panel 3**	
	Coef.	**SE.**	**Coef.**	**SE.**	**Coef.**	**SE.**
Per capita gains, 1-year lag (log)	-0.076*	0.035				
Per capita gains, 2-year lag (log)			-0.114**	0.039		
Per capita gains, 3-year lag (log)					-0.026	0.024
Squared term	0.011*	0.005	0.016**	0.005	0.003	0.004
Controls						
Per capita income, 1-year lag (log)	0.305***	0.100	0.401***	0.086	0.284*	0.114
Ethnic fragmentation, 1-year lag (log)	0.051	0.059	-0.003	0.056	-0.063	0.106
Employed in public administration, 1-year lag (log)	-0.132*	0.068	-0.158**	0.056	-0.260**	0.083
Functional performance index, 1-year lag (log)	0.489***	0.087	0.472***	0.101	0.249**	0.089
Dependence on property tax, 1-year lag (log)	-0.087	0.094	-0.024	0.108	0.007	0.126
Dependence on IGR, 1-year lag (log)	-0.071	0.044	-0.024	0.052	0.074	0.048
Council-manager	-0.155*	0.068	-0.103	0.070	-0.167**	0.058
Year dummies	Yes		Yes		Yes	
State dummies	Yes		Yes		Yes	
N	127		123		66	
R-Sq.	0.670		0.694		0.646	

Note: Using lags decreases the sample size. Following Baum, Schaffer, and Stillman (2010), the coefficients for singleton dummies (state dummies with one observation) have been "partialled out" to ensure that the robust covariance matrix estimator is of full rank. State and year dummies are still included in the models but their coefficients (as well as the constant) are not estimated. *** significant at .1%, ** at 1%, * at 5%, two-tailed tests.

Figure 1: Permanent Income Hypothesis

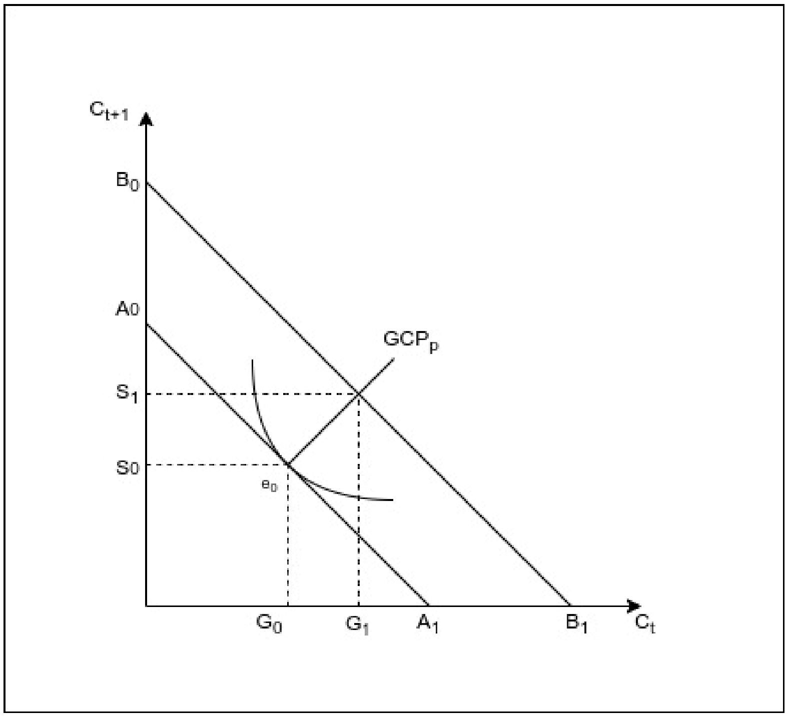
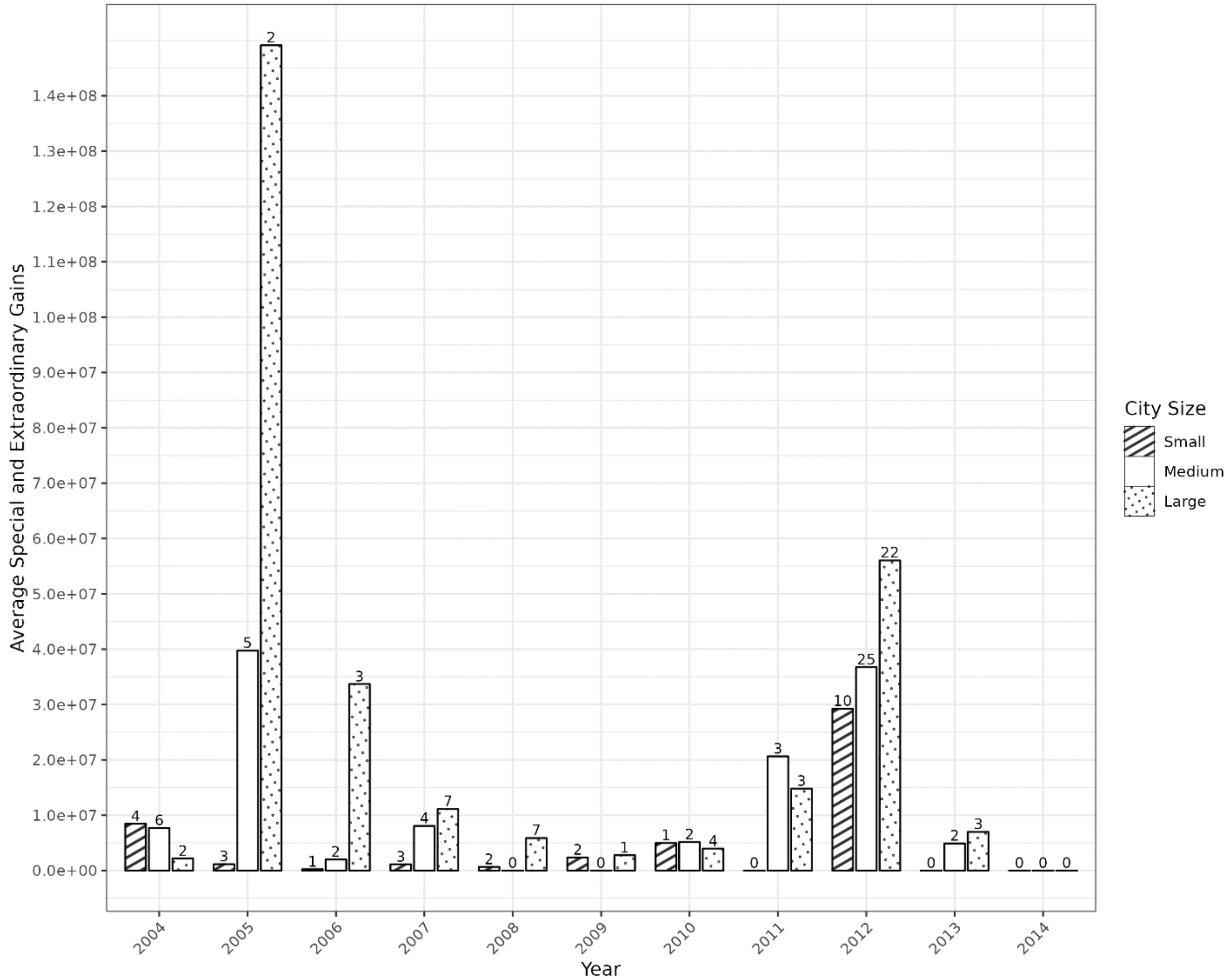
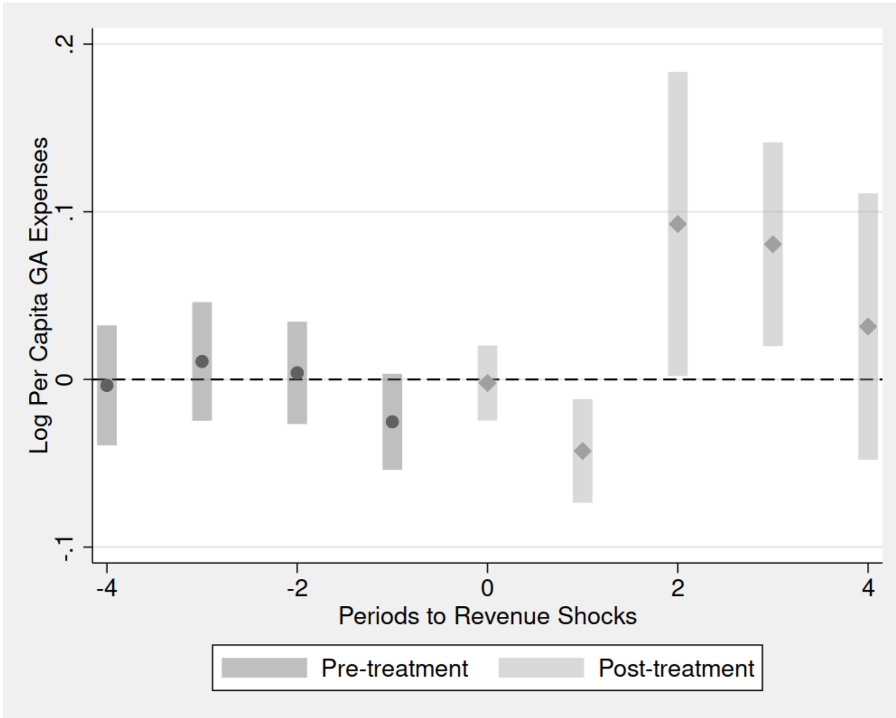


Figure 2: Annual Average Gains by City Size



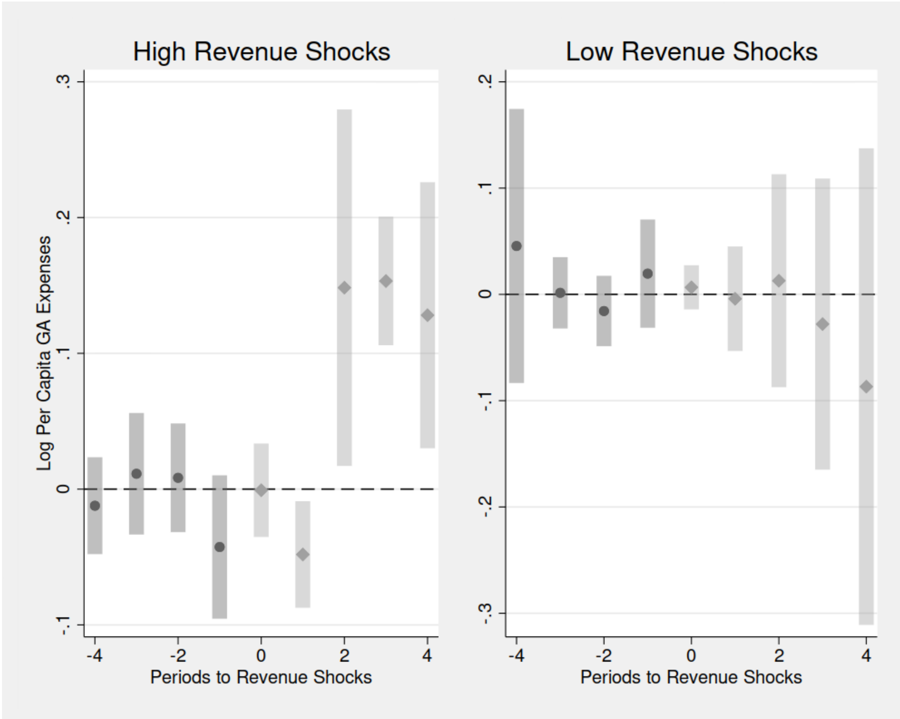
Note: "Small" indicates a population of less than 68,738, "Medium" between 68,738 and 111,145, and "Large" over 111,145. The number on top of the bar is the count of cities within the respective category.

Figure 3: DID Event Study on Per Capita Expenses of Governmental Activities (Full Sample)



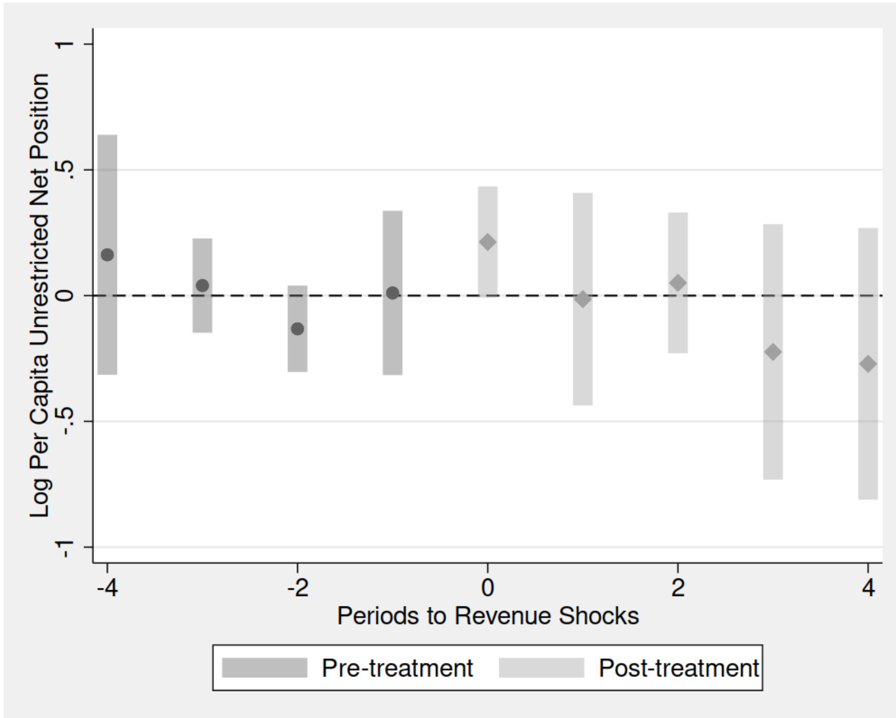
Note: The darker bars and circles represent pre-treatment differences and lighter ones with diamonds for post-treatment, with the bar width indicating a 95 percent confidence interval.

Figure 4: DID Event Study on Per Capita Expenses of Governmental Activities (Sub-samples)



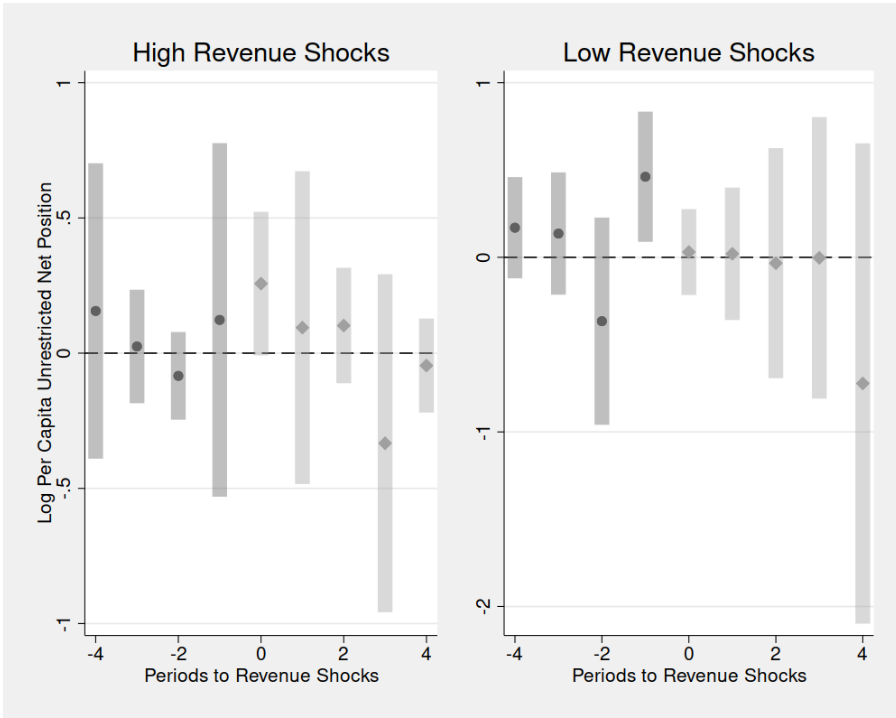
Note: The darker bars and circles represent pre-treatment differences and lighter ones with diamonds for post-treatment, with the bar width indicating a 95 percent confidence interval.

Figure 5: DID Event Study on the Per Capita Unrestricted Net Position of Governmental Activities (Full Sample)



Note: The darker bars and circles represent pre-treatment differences and lighter ones with diamonds for post-treatment, with the bar width indicating a 95 percent confidence interval.

Figure 6: DID Event Study on the Per Capita Unrestricted Net Position of Governmental Activities (Sub-samples)



Note: The darker bars and circles represent pre-treatment differences and lighter ones with diamonds for post-treatment, with the bar width indicating a 95 percent confidence interval.

APPENDIX

Least Squares Dummy Variable Regression

Table A-1 contains the full results of the LSDV models where the outcome variable is per capita expenses. State dummies address bias caused by omitting time-invariant variables at the state level, such as state history. The year effects address error correlation caused by the impact of national-level events, such as a national recession or changes in tax policy. We run the LSDV models using Newey-West HAC (heteroskedasticity- and autocorrelation-consistent) standard errors.

We first estimate base models (log-log regressions). To address potential simultaneous causation, we use different lags of gains. Panels 1 to 3 include one-, two-, and three-year lags. In panel 1, the estimate for the one-year lag of gains fails to reach conventional levels of statistical significance. In panels 2 and 3, the estimates for two and three-year lags of gains remain statistically insignificant.

We have reason to believe that the relationship between gains and expenses is U-shaped. Given that almost half of the gains are from the sale of capital assets, expenses can decline initially as cities shed responsibilities for operating and maintaining government-owned capital assets once these are sold. Proceeds from the sale can then be used to expand expenses in succeeding years. Panels 4 to 6 include one-, two-, and three-year lags of gains. The result of interest here is the coefficient for the squared term. Focusing first on panel 4, the coefficient is positive and statistically significant. The sign for the squared term confirms our suspicion of a U-shaped relationship: an initial increase in gains reduces expenses, but subsequent increases stimulate expenses. A test of the presence of a U-shaped produces a t-value = 1.95 and P>|t| = 0.027, supporting the alternate hypothesis of a U-shaped relationship.

Using year two lag, the estimate for the squared term remains positive and statistically significant. By year three, the sign still confirms the U-shaped relationship, but the estimate is no longer significant. Caution should be taken when interpreting this last result as there are only 66 observations in year three, weakening the statistical power of the model.

Using the results for panel 4 (with the largest sample size), the implied turning point is 3.48 in natural log or approximately $32.48 when exponentiated. How large is the effect of gains on expenses? Because the slope of a curve varies depending on the location in that curve, calculating the effects of gains requires focusing on specific points on the curve. Using the results for panel 4 again, we estimate the slopes at the lower and upper bounds of the actual data range for total gains. A 1% increase in per capita gains at the lower bound reduces per capita expenses by -0.141% (t-value = -2.194 and P>|t| = 0.015). At the upper bound, a 1% increase in per capita gains increases per capita expenses by 0.093% (t-value = 1.945 and P>|t| = 0.027).

Table A-1: LSDV w/ Newey-West HAC Standard Errors
Dependent Variable: Per Capita Direct Program Expenses of Governmental Activities (Log)

Independent Variables	Base Models						Quadratic Model					
	Panel 1		Panel 2		Panel 3		Panel 4		Panel 5		Panel 6	
	Coef.	SE.	Coef.	SE.	Coef.	SE.	Coef.	SE.	Coef.	SE.	Coef.	SE.
Per capita gains, 1-year lag (log)	0.010	0.015					-0.076**	0.035				
Per capita gains, 2-year lag (log)			0.015	0.017					-0.114***	0.039		
Per capita gains, 3-year lag (log)					-0.004	0.016					-0.026	0.024
Squared term							0.011**	0.005	0.016***	0.005	0.003	0.004
Controls												
Per capita income, 1-year lag (log)	0.308***	0.101	0.381***	0.084	0.300***	0.107	0.305***	0.100	0.401***	0.086	0.284**	0.114
Ethnic fragmentation, 1-year lag (log)	0.036	0.057	-0.012	0.059	-0.086	0.102	0.051	0.059	-0.003	0.056	-0.063	0.106
Employed in public administration, 1-year lag (log)	-0.144**	0.070	-0.155***	0.059	-0.251***	0.088	-0.132**	0.068	-0.158***	0.056	-0.260***	0.083
Functional performance index, 1-year lag (log)	0.491***	0.089	0.473***	0.104	0.242***	0.091	0.489***	0.087	0.472***	0.101	0.249***	0.089
Dependence on property tax, 1-year lag (log)	-0.080	0.091	-0.036	0.117	0.016	0.126	-0.087	0.094	-0.024	0.108	0.007	0.126
Dependence on IGR, 1-year lag (log)	-0.082*	0.043	-0.050	0.053	0.085*	0.048	-0.071	0.044	-0.024	0.052	0.074	0.048
Council-manager	-0.176**	0.069	-0.146*	0.076	-0.173***	0.059	-0.155**	0.068	-0.103	0.070	-0.167***	0.058
Year dummies	Yes		Yes		Yes		Yes		Yes		Yes	
State dummies	Yes		Yes		Yes		Yes		Yes		Yes	
N	127		123		66		127		123		66	
R-Sq.	0.654		0.655		0.638		0.670		0.694		0.646	

Note: Following the approach in Baum, Schaffer, and Stillman (2010), the coefficients for singleton dummies (state dummies with one observation) have been "partialled out" to ensure that the robust covariance matrix estimator is of full rank. State and year dummies are still included in the models but their coefficients (as well as the constant) are not estimated. *** significant at 1%, ** at 5%, * at 10%, two-tailed tests.

Heckman Two-Step Regression Models

We also use Heckman's two-step selection regression to address potential selection bias. Step one, or the selection equation, uses probit regression to estimate the probability that a city will have gains. The outcome variable is a dichotomous measure coded 1 for cities that registered gains and 0 otherwise. The predicted values from the selection model measure the selection hazard of inclusion in the sample. Called the inverse Mill's ratio, the transformed predicted values are included in the second step, or the level equation, to control for selection bias. The level equation only includes the sub-sample of cities with gains and is estimated using OLS, with expenses as the dependent variable. The exclusion restriction requires that the selection equation contains at least one variable that is not in the level equation. This requirement minimizes severe collinearity among the inverse Mill's ratio and control variables in the level equation. We include utility revenues in the selection equation with the expectation that cities with access to utility revenues will have less need for revenues from gains.

Table A-2 contains the results for the Heckman two-step regression models where the outcome variable is per capita expenses. In panels 7 and 9 or the selection equations, cities that are likely to have gains have ethnically heterogeneous populations, less intergovernmental revenues, and lower utility revenues. The results in panels 8 and 10 or the level equations confirm the results for the LSDV models. In panel 8, we do not see a relationship between the log of gains and the log of expenses. In panel 10, the estimate for the squared term is positive and statistically significant, confirming the U-shaped relationship detected in the LSDV models. Note that the selection hazard Lamda is not statistically significant, indicating that selection bias is not a serious concern.

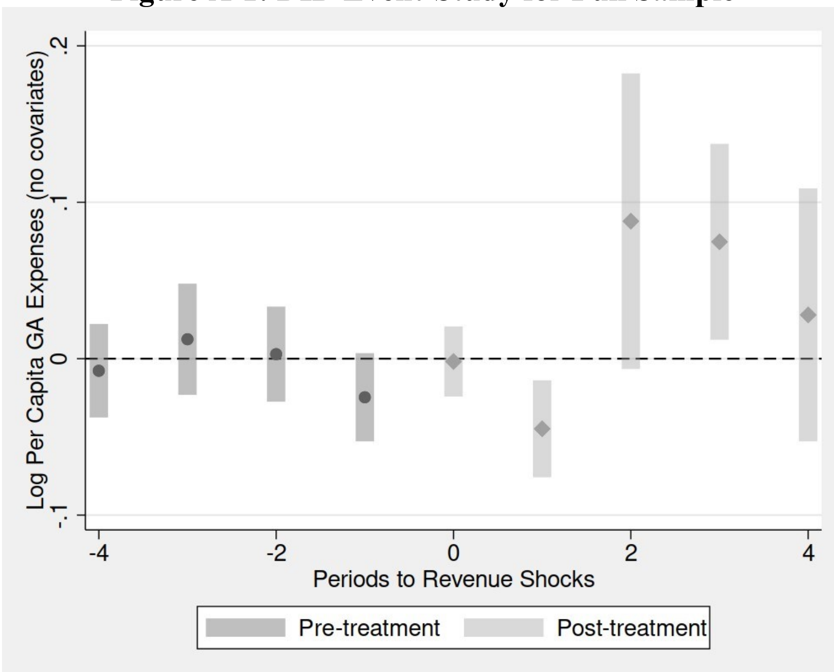
Table A-2
Heckman Selection Regression Models
Dependent Variable: Per Capita Direct Program Expenses of Governmental Activities (Log)

Independent Variables	Base Model				Quadratic Model			
	Panel 7 Selection Equation		Panel 8 Level Equation		Panel 9 Selection Equation		Panel 10 Level Equation	
	Coef.	SE.	Coef.	SE.	Coef.	SE.	Coef.	SE.
Per capita gains, 1-year lag (log)			0.015	0.015			-0.078*	0.042
Squared term							0.012**	0.005
Controls								
Per capita income, 1-year lag (log)	-0.126	0.131	0.256**	0.110	-0.126	0.131	0.244**	0.124
Ethnic fragmentation, 1-year lag (log)	0.312***	0.111	0.204	0.143	0.312***	0.111	0.245	0.162
Employed in public administration, 1-year lag (log)	0.049	0.115	-0.113	0.086	0.049	0.115	-0.094	0.098
Functional performance index, 1-year lag (log)	0.049	0.061	0.486***	0.062	0.049	0.061	0.483***	0.069
Dependence on property tax, 1-year lag (log)	0.076	0.077	-0.032	0.097	0.076	0.077	-0.032	0.108
Dependence on IGR, 1-year lag (log)	-0.189***	0.058	-0.149**	0.073	-0.189***	0.058	-0.146*	0.082
Council-manager	0.109	0.090	-0.117	0.081	0.109	0.090	-0.085	0.092
Utility revenues, 1-year lag (log)	-0.024**	0.012			-0.024**	0.012		
State dummies			Yes				Yes	
Year dummies			Yes				Yes	
Constant	-1.811	1.442	2.171	1.595	-1.811	1.442	2.031	1.801
Lambda	0.536	0.392			0.617	0.446		
N	6057				6057			
Selected	127				127			
Nonselected	5930				5930			
Wald chi2	376.370				301.250			
Prob > chi2	0.000				0.000			

Note: *** significant at 1%, ** at 5%, * at 10%, two-tailed tests.

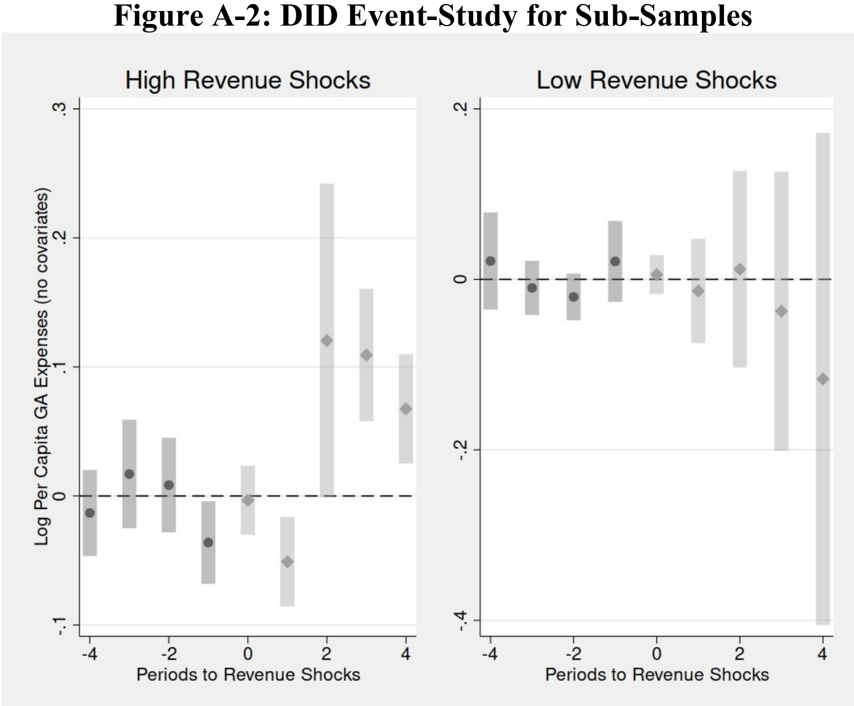
DID Regression for Per Capita Expenses of Governmental Activities, No Covariates

Figure A-1: DID Event-Study for Full Sample



Note: Darker bars and circles represent pre-treatment differences; lighter bars with diamonds for post-treatment; bar width indicates 95% confidence interval.

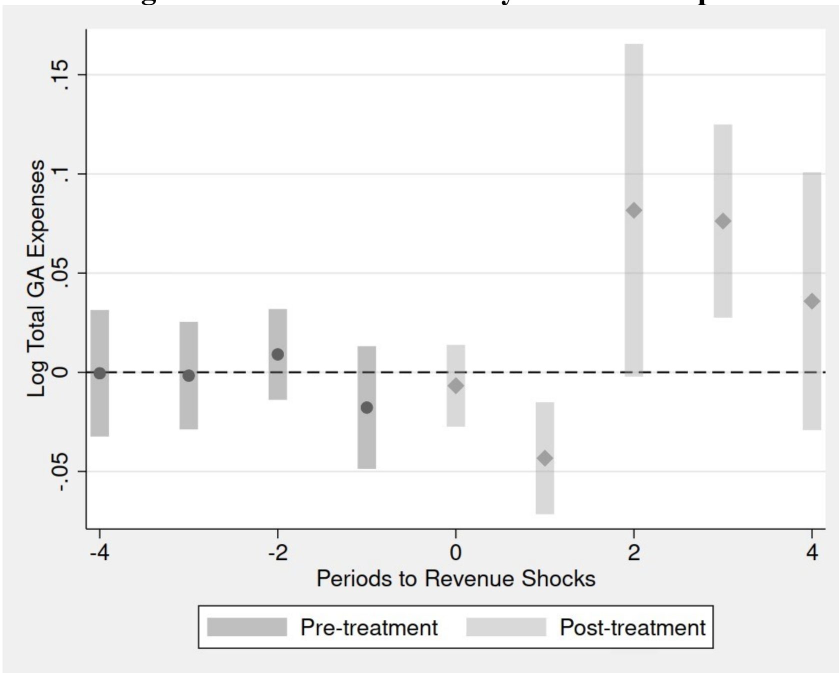
Figure A-2: DID Event-Study for Sub-Samples



Note: Darker bars and circles represent pre-treatment differences; lighter bars with diamonds for post-treatment; bar width indicates 95% confidence interval.

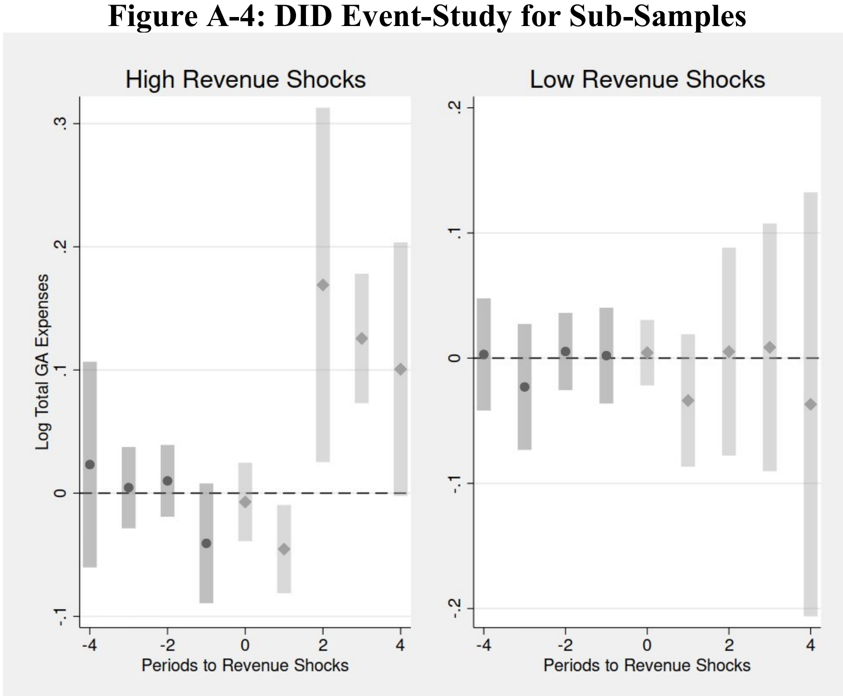
DID Regression for Total Expenses of Governmental Activities, With Covariates

Figure A-3: DID Event-Study for Full Sample



Note: Darker bars and circles represent pre-treatment differences; lighter bars with diamonds for post-treatment; bar width indicates 95% confidence interval.

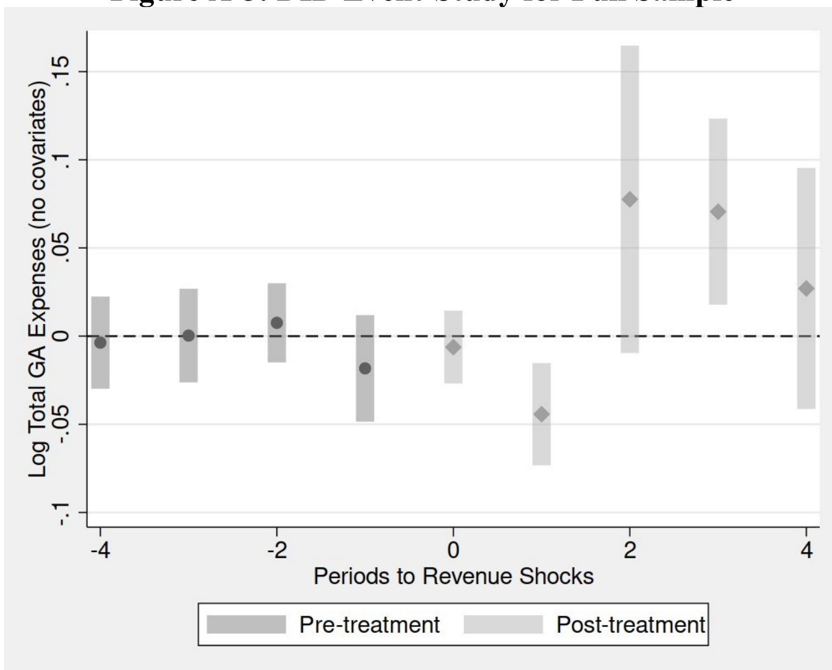
Figure A-4: DID Event-Study for Sub-Samples



Note: Darker bars and circles represent pre-treatment differences; lighter bars with diamonds for post-treatment; bar width indicates 95% confidence interval.

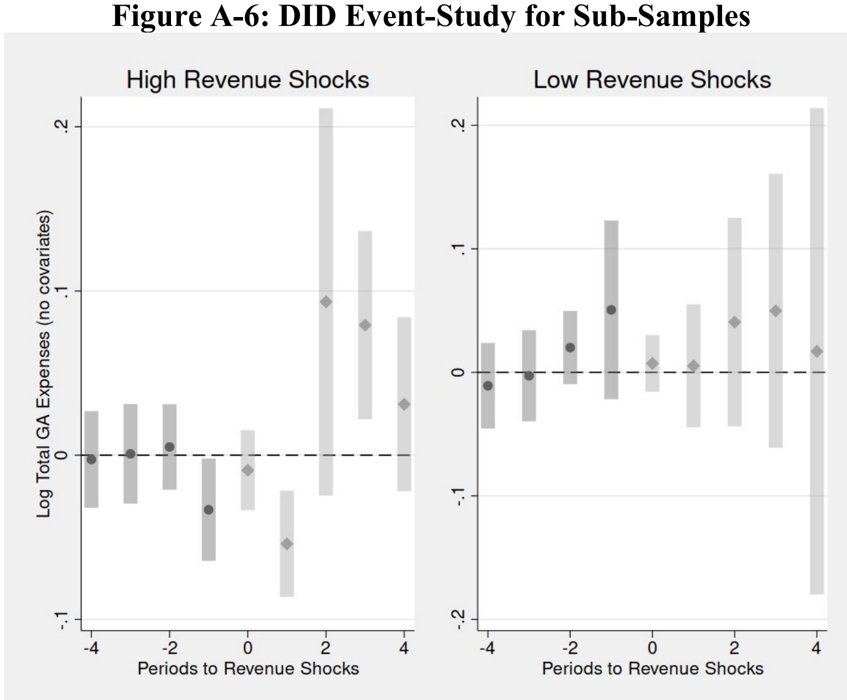
DID Regression for Total Expenses of Governmental Activities, No Covariates

Figure A-5: DID Event-Study for Full Sample



Note: Darker bars and circles represent pre-treatment differences; lighter bars with diamonds for post-treatment; bar width indicates 95% confidence interval.

Figure A-6: DID Event-Study for Sub-Samples



Note: Darker bars and circles represent pre-treatment differences; lighter bars with diamonds for post-treatment; bar width indicates 95% confidence interval.