Does Transparency Improve Public Policy? Evidence from a Tax Incentive Transparency Initiative*

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Abstract

Transparency of governance can lead to a number of different theoretical or empirical outcomes. We examine the effect of increased transparency in the cost of local governance in a major U.S. policy area: local economic development. Past work suggests that tax incentives, a common economic development tool employed by local governments, are bad policy but good politics: they fail to attract investment, but offer electoral returns for politicians who give them out. One explanation for this overuse is that the costs are not transparent to taxpayers. We leverage an exogenous increase in the transparency of the costs of local governments' tax incentive reporting requirements, General Accounting Standards Board's Statement 77, to test whether transparency improves local economic development policy (e.g., reduces tax incentive use). Using a difference-in-differences design, we estimate that GASB 77 had no discernible effect on local governments' use of tax incentives. We discuss three potential interpretations of the null result: first, it could be explained by imperfect compliance with the policy change. Second, it could be explained by GASB 77's low salience among elected officials. Finally, it is possible that there are limits to cost transparency's ability to improve the quality of policymaking.

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Governmental transparency has become a major policy initiative in countries around the world. Even in areas that are notoriously opaque, such as trade negotiations, there is increasing pressure to open up the policy process to the public. Transparency has been posited to accomplish everything from reducing corruption, to improving public policy, to increasing trust in government. Does transparency really accomplish these goals?

In this paper we examine an exogenous change in U.S. transparency standards in a major policy area: local economic development policy. In the United States, and in many countries around the world, governments offer individual companies financial incentives, such as tax breaks to relocate or expand operations. The most high profile case was in 2017, when U.S. technology giant Amazon began searching for a U.S. municipality in which to locate its second headquarters (dubbed "Amazon HQ2"). The competition was intense; Amazon claimed to receive bids from 238 U.S. municipalities, each one a detailed document touting the municipality's suitability for a new Amazon HQ. Cities competed to offer Amazon the largest and most attractive tax incentive package: for example, Newark, New Jersey offered a package (endorsed by NJ governor Chris Christie) worth nearly \$7 billion.¹

Although this is an extreme example, local politicians frequently offer tax incentives in attempt to woo other companies as well; Bartik (2017) estimates that U.S. municipalities gave out \$45 billion dollars in incentives in 2015. The economic logic of the firm specific incentives is that the new jobs and capital expenditure that investing firms bring to town outweigh the cost of forgone tax revenue. However, a wealth of academic evidence suggests that such incentives do not play a major role in firms' location decisions; rather, firms look for favorable labor markets and geographic locations (Jensen and Malesky, 2018).

One explanation for the overuse of incentives is the lack of transparency in this policy space. Many of these economic incentive deals, including for Amazon HQ2 are shrouded in secrecy, and numerous economic development programs are exempt from public records laws. We take advantage of a unique and exogenous policy change - the Governmental Accounting

¹See, e.g., https://lasvegassun.com/news/2017/oct/23/amazon-says-it-received-238-proposals-for-2nd-head/

Standards Board (GASB)'s Statement 77 - to examine how an increase in cost transparency shapes incentive use.

GASB is an oversight board that sets standards for state and local government finance. Enacted in 2015, GASB Statement 77 required U.S. municipalities to include information on their total tax incentive spending in their (publicly available) annual financial reports. Up to this point, most cities provided no comprehensive accounting of the amount of tax abatements offered to firms.

GASB 77 constituted a plausibly exogenous increase in the transparency of local governments' tax incentive spending; however, non-tax incentives (such as grants and low-interest financing) were not affected by the new policy, nor were municipalities in states that do not require their cities to adhere to these standards. We use a difference-in-differences design to test whether or not GASB 77 caused cities to reduce their tax incentive spending. This allows us to examine if the enacting of this transparency standard led to fewer incentives deals or smaller amounts of tax abatements.

We find that GASB 77 did not cause affected municipalities to reduce their tax incentive spending, regardless of whether nontax incentives or tax incentives in non-GAAP states are used as the comparison group. In the concluding section, we discuss potential explanations for this null result and suggest avenues for future research.

Government Use of Tax Incentives

Governments around the world use different forms of firm specific incentives to facilitate economic development. Although these incentives include tax abatements, cash grants, fee waivers, and dedicated infrastructure for companies, a study by Danzman et al. (2016) finds that the vast majority of economic development incentives are provided through tax incentives. By 1999, 95 percent of cities were using firm-specific incentives for development Jensen and Malesky (2018). The total dollar value of incentives in the United States has been estimated between 45–90 billion per year, although the lack of transparency of both programs and individual deals makes this value difficult to estimate (Parilla and Liu, 2018).

Despite incentives being one of the primary tools used by government officials, there is limited evidence that incentives are an effective economic development strategy. Slattery and Zidar (2020) review the literature as well as original analysis of "close deals", where cities attracting companies through incentives see employment increases, but find no evidence of broader economic benefits for spillovers from these subsidized firms. This is consistent with previous work that finds incentives suffer from poor targeting, where the majority of incentives are allocated to companies that would have invested absent these incentives. There is a general concensus that economic development incentives practice is in need of reform. Jensen and Malesky (2018) argue that, while corporate tax breaks are not efficient tools for attracting investment, they are tools that local politicians can use to attach their name to local investment projects. Jensen and Malesky argue that, even if they fail to bring new firms to town, local officials can use incentives to deflect blame for a lack of investment. These same government officials minimize the oversight of these programs, often not even requiring a simple cost-benefit analysis for offering taxpayer support. Corporate tax breaks may be bad economics, but they are good politics.

Central to the political economy of economic development incentives is the lack of transparency around these programs and deals. Some high profile programs, such as Georgia's film incentives provide not details on the companies receiving incentives. Numerous states maintain exceptions to economic development activities, allowing broad exceptions to the release of information on economic deals (Jensen and Thrall, 2021).

In the next section we further discuss the political economy of transparency and specifically discuss the potential implications of increasing transparency of local economic development programs. In this paper we address a specific type of transparency, the costs of abatements through lost revenues. This transparency mechanism has been linked with lower levels of spending and could plausibly limit the overuse of economic development incentives.

Transparency and Local Economic Development

It is often noted that transparency is necessary for democratic governance; without transparency, voters cannot accurately determine what their elected officials are doing and thus cannot hold them accountable for their actions (Adsera, Boix and Payne, 2003). However, while cross-national evidence suggests that democracies are indeed more transparent than nondemocracies (Hollyer, Rosendorff and Vreeland, 2011), there is substantial variation in transparency between (and even within) democratic governments. What explains this variation in transparency, and what are its effects?

The leading explanation for variation in transparency across and within democracies centers on electoral competition. The logic is that competitive elections foster uncertainty about whether or not the incumbent party will remain in power during the next cycle; knowing they may be removed from office, incumbent politicians in competitive democracies will pass transparency regulations in order to constrain future opposition parties. Wehner and de Renzio (2013) find that democracies with more competitive elections tended to have higher levels of budgetary disclosure. Berliner (2014) finds that democracies with more electoral competition are more likely to implement freedom of information laws, which constitute major steps towards transparency. Transparency may thus vary within and across democracies for strategic reasons rather than normative ones.

Germane to this paper's topic is the literature on the effects of fiscal transparency: what happens when citizens are better informed about how the government is spending their tax money? Two findings are particularly worth discussing. First, greater fiscal transparency is associated with more balanced budgets (Benito and Bastida, 2009) and lower levels of debt Alt and Lassen (2006).

Second, Alt and Lassen (2006) find that fiscally transparent democracies experience less pronounced electoral cycles in government spending than non-transparent democracies. This result suggests that transparency, by way of increasing voters' information about government spending activity, limits the extent to which politicians can spend taxpayer money in ways that are economically suboptimal but electorally efficient. Electoral cycles - the ramping up of public spending in the year preceding an election - bring electoral returns to incumbent politicians because they temporarily boost the economy just before voters decide whether to vote the proverbial bums out. However, Healy and Lenz (2012) argue that most voters actually want to evaluate politicians' aggregate economic performance, but they simply lack the information necessary to do so and thus rely on the current/recent state of the economy as a proxy. They find that experimentally increasing voters' information about incumbents' aggregate performance substantially reduces the recency bias.

These findings suggest that government transparency should reduce government spending, as voters will be able to hold incumbents accountable for profligate use of their tax dollars. However, transparency can also lead to greater spending by encouraging greater government effort. For example, Ferejohn (1999) links increased transparency to higher taxes and transfers. This is also consistent with work finding that transparency can increase trust in government and lead to higher levels of government spending (Alt, Lassen and Skilling, 2002; Alt and Lowry, 2010).

Theoretically and empirically, fiscal transparency can have different effects on government spending. We believe that examining local economic development transparency provides some answers to these broader questions about the implications for transparency.

Tax incentives are similar to electoral budget cycles in the sense that they allow incumbent politicians to use taxpayer funds to maximize their odds of reelection, rather than to maximize aggregate welfare. Other economic development tools may be more effective for governments, limited information about the costs of incentives can make them an effective political strategy (Patrick, 2016). Politicians can often use message control to extol the benefits of their economic development efforts while minimizing information on the costs Jensen and Malesky (2018).

We argue that existing practices allow governments to selectively provide information on incentive use. Politicians already use incentive announcements to pander to the public (Jensen and Malesky, 2018) and selectively reveal information about the costs as well as the benefits of incentives (Jensen and Thrall, 2021). What is missing in the current transparency regime is a full and systematic accounting for the costs of incentives.

GASB 77 was an exogenous shock to local transparency, requiring a very specific type of disclosure by governments. We believe that this transparency doesn't affect the government's ability to pander, and doesn't necessarily even signal major changes in a cities financial health. The main benefit of this transparency is providing detailed information on why government revenues are lower than one might otherwise think, and to show systematically on how much economic development efforts lead to annual reductions in revenues. Thus by only revealing additional information on the costs of incentives, we hypothesize that this disclosure can shape the provision of incentives.

This type of transparency has been linked with reductions in government spending, where the public is provided additional information about the costs of a policy (tax abatements) but no additional information on the benefits of these programs.

Formally, this paper's sole hypothesis can be stated as follows:

H1: All else equal, an increase in the transparency of tax incentive spending should result in a decrease in the amount of incentive spending.

Research Setting: GASB 77

In 2015, U.S. state and local governments experienced a sudden increase in transparency requirements for their tax incentive spending. That increase was the result of GASB Statement 77, an accounting rule change that required state and local governments to report their incentive spending in a standardized format on their annual financial reports. This rule change provides an ideal setting in which to test the above hypothesis about transparency and tax incentive spending.

In the years following the Great Depression, the U.S. government took several steps

to standardize and regulate accounting practices for companies, school districts, and local governments. One of the most important pieces of legislation related to this mission was the Securities Exchange Act of 1934, which created the Securities and Exchange Commission (SEC), the federal agency tasked with regulating the financial reporting practices of public and private entities (Strother, 1975). Shortly after its creation, the SEC adopted a common set of standards for financial reporting called the Generally Accepted Accounting Principles (GAAP) and required that companies and local governments comply to them. The GAAP includes both broad, general commitments (e.g., the commitment that financial results be presented honestly) as well as more specific rules (e.g., unrealized income cannot be reported as revenue).

In 1984, a number of groups including the National League of Cities and the National Conference of State Legislatures came together to create the Governmental Accounting Standards Board (GASB).² GASB is a private organization tasked with setting financial reporting standards for GAAP-compliant local and state governments; it sets standards "through a transparent and inclusive process intended to promote financial reporting that provides useful information to taxpayers, public officials, investors, and others who use financial reports."³

Since its creation, GASB has issued 94 rule changes, called "Statements," that affect the manner in which state and local governments must prepare their annual financial reports and/or the information that governments must include in the reports. The focus of this paper is GASB Statement 77 (hereafter GASB 77), issued in August 2015, which required for the first time that local and state governments must disclose their tax incentive spending in their annual reports. Specifically, GASB 77 requires governments to report three things:⁴

- 1. The dollar amount (gross) of taxes abated during the reporting period.
- 2. "Brief descriptive information" about the incentives, such as the specific tax being abated, eligibility requirements for recipients of the abatement, and any provisions

²https://www.fasb.org/jsp/FASB/Page/TimelinePage&cid=1175805309640

³See https://www.gasb.org/jsp/GASB/Page/GASBSectionPage&cid=1176168081485

⁴The full text of GASB 77 can be found here.

that may be in place to reclaim or terminate the incentive in certain situations.

3. Other non-tax commitments made by a government as part of a tax incentive deal.

GASB 77 markedly increased the transparency of the affected governments' incentive spending by requiring governments to report their total annual spending in a standardized, public format. Prior to GASB 77, information on a local government's total incentive spending would need to either be pieced together from different news articles/press releases (time intensive), calculated using proprietary incentive data (cost intensive), or accessed via Freedom of Information Act (FOIA) request (time and cost intensive). In many cases, these deals were exempt form FOIA requests and the costs were never reported.

Research Design:

Identification Strategy

We test the effect of GASB 77 on local governments' incentive spending using a differencein-differences (D-in-D) design. D-in-D designs are appropriate for panel data where one or more groups receive some plausibly exogenous treatment, and both treatment and control groups are observed pre- and post-treatment (Angrist and Pischke, 2009). In order to achieve causal identification, it is necessary to select treatment and control groups that serve as appropriate counterfactuals. We take two different approaches to control/treatment selection in this paper.

First, we select local governments' economic development tax incentive spending as the treated group and local governments' *non*tax economic development incentive spending as the control group. This allows us to take advantage of the fact that GASB 77 only required local governments to report incentives that abate tax revenue; other types of incentive spending, such as grants or low-interest loans, were unaffected. One potential concern about this approach is that governments may ramp up their nontax incentive spending in response to

GASB 77, meaning that the treatment really affected both types of incentive spending and rendering the comparison invalid. However, governments are typically more constrained in their ability to use nontax incentives such as grants or low-interest financing, as they require large upfront costs.



Incentive type - Non-tax - Tax

Figure 1 displays the average monthly level of spending on tax incentives (blue line) and nontax incentives (red line) across reporting cities; the dashed vertical line indicates the issuance of GASB 77. While the pre-treatment trends are not parallel they seem to be reasonably consistent, peaking in February and July 2015. Further, while nontax incentives experienced a slight increase post-treatment, it is clear that governments have not simply transferred their tax incentive spending into nontax formats.

Figure 1: Tax vs. Nontax Incentive Spending, pre- and post-GASB 77

It is important to note that many non-tax incentives, such as grants, required financial outlays that would be reported, at least in aggregate, in local financial reports. Tax incentives where the least transparent form of economic development incentives, and thus it isn't clear if shifting to nontax incentives would allow governments to minimize scrutiny of their incentive deals.

Second, we exploit state-level variation in GAAP requirements. All U.S. state governments are required to file their own financial reports in accordance with GAAP; however, some states require their municipal governments to follow GAAP standards while others do not.⁵ Twenty-nine U.S. states fall in the latter group, and thus local governments in these states face no legal requirement to comply with GASB's Statements.⁶ The separation is not perfect, as many local governments in non-GAAP states still report in accordance with GAAP standards (presumably to maintain favorable bond/credit ratings), but it allows for a second control/treatment split: We select tax incentives issued by local governments in GAAP states as the treatment group, and tax incentives issued by local governments in non-GAAP states as the control group.

The baseline difference-in-differences model is specified as follows:

$$ln(Incentive)_{ist} = \lambda_t + \gamma_s + \sum_{\tau=-7}^{-2} \gamma_\tau D_{st} + \sum_{\phi=0}^{16} \gamma_\phi D_{st} + \epsilon_{ist}$$
(1)

Time (year-months) is indexed by t, treatment group (tax vs. non-tax incentives, or GAAP mandated vs. non-GAAP mandated) is indexed by s, and municipality is indexed by i. Fixed effects are included at the treatment group and year-month levels. Finally, as is standard in event study D-in-D designs, we estimate the difference-in-differences parameter in several pre- and post-treatment time periods (omitting the first pre-treatment lag to serve

 $^{^{5}}$ For a detailed report on state-level reporting requirements, see https://gasb.org/resources/ccurl/336/337/GAAP_Research_Brief.pdf.

⁶The states are Alabama, Alaska, Arkansas, California, Delaware, Idaho, Illinois, Indiana, Iowa, Kansas, Michigan, Missouri, Montana, Nebraska, New Hampshire, New Jersey, New York, North Dakota, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Texas, Vermont, Washington, West Virginia, and Wyoming.

as the baseline).

Incentive Data

Data on incentives come from the IncentiveFlow database, developed by Wavteq (a spinoff of Financial Times). The IncentiveFlow database attempts to collect a comprehensive set of project-level incentive deals, alongside detailed information on the deals (amount, tax vs. nontax, jobs/capital expenditure promised by the recipient, et cetera), from a variety of sources (local media, industry periodicals, economic development magazines, etc). Usefully, the database also reports the date (month and year) that the project was announced and the municipality that granted the incentive. While the IncentiveFlow data likely does not include the entire universe of U.S. incentive deals, it is the highest-quality source of data on U.S. incentives that is not reliant on voluntary reporting by local governments themselves. In the conclusion we discuss possible limitation of this data and its implications for this project. We have access to IncentiveFlow data for the calendar years 2015 and 2016.

The key dependent variable is logged total incentive spending, measured at the municipalitymonth level. This is a relatively straightforward measure of cities' spending on new incentive agreements, rather than existing agreements that may not be under the control of the current administration. The sample is restricted to U.S. municipalities with populations of at least 50,000, of which there are 757.

Results

Figure 2 displays the results of the model comparing municipalities' tax and nontax economic development incentive spending before and after GASB 77, with robust standard errors clustered on the municipality. As a reminder, we predict that GASB 77 will lead municipalities to reduce their incentive spending, and therefore we expect a negative treatment effect. We find little evidence in support of this expectation. While there are no significant



Figure 2: GASB 77 did not decrease tax incentive spending relative to non-tax incentive spending.

differences between tax and non-tax spending prior to GASB 77, the same hold true after the legislation was enacted: the average treatment effect on the treated (ATT) is non-significant in sixteen of the seventeen post-treatment periods.

Figure 3 presents the results of the model comparing tax incentive spending in GAAP vs. non-GAAP municipalities pre- and post-GASB 77, again with robust standard errors clustered on the municipality. Again, we predict that GASB 77 should increase transparency and thus reduce tax incentive spending in GAAP municipalities but not non-GAAP municipalities, resulting in a negative treatment effect. However, this is not what we observe in the data. While GAAP mandated municipalities do appear to abate less tax revenue than non-GAAP mandated municipalities in the pre-treatment period, this difference disappears



Figure 3: GASB 77 did not decrease tax incentive spending in GAAP mandated cities relative to non-GAAP mandated cities.

after GASB 77 is enacted.

As a robustness check, we estimate additional models using Imai, Kim and Wang (2020)'s nonparametric difference-in-differences estimator ("PanelMatch").⁷ The PanelMatch estimator, developed for designs with staggered treatment assignment, allows us to adjust for confounders (GDP, population, and partisanship) for which we do not have monthly data. We also test the possibility that the transparency shock of GASB 77 led cities to use tax incentives more effectively, creating more jobs and generating more investment. To do so, we examine additional outcome variables from the IncentiveFlow dataset: job creation (log jobs created from incentives), capital expenditure (log capex generated by incentives), and

⁷More information about PanelMatch can be found in Appendix A.1.

the proportion of incentive deals that fund new (rather than existing) projects. However, the results (presented in Appendix A.2 and A.3) show that GASB 77 had little to no effect on either the quality or the quantity of cities' tax incentives.

Despite the hopes of transparency advocates and economic development reformers, we find no evidence that this major transparency initiative affected economic development policy making. In the following section, we discuss potential explanations for these non-findings and avenues for future research.

Why Didn't GASB 77 Change Incentive Spending?

Why didn't municipal governments adjust their tax incentive spending in response to the transparency requirements imposed by GASB 77? We investigate two (related) potential explanations. First, it is possible that the problem is one of compliance; cities could simply be failing to report their incentives (noncompliance) or reporting strategically/selectively, meaning that the policy change failed to increase transparency in the first place. Second, our original survey of hundreds of policymakers (local elected officials and finance officers) showed that most officials were unfamiliar (or only passingly familiar) with GASB 77. It is possible that the policy change was so minimally salient that elected officials were unaware that their accountants had implemented it, preventing them from changing incentive spending in response.

Delayed Compliance and Noncompliance

To investigate (non)compliance with GASB 77, we examined the documents in which municipalities are required to report on their tax incentive spending: the comprehensive annual financial report (CAFR), which contains accounting information from the previous fiscal year.⁸ We collected CAFRs from the years 2015-2018 for the 757 U.S. municipalities with populations greater than 50,000, primarily by searching the cities' websites. We

⁸For municipal governments, the fiscal year is rarely the same as the calendar year. So, for example, a 2015 CAFR might report on the period of August 01, 2014 through July 30, 2015.



Figure 4: Most states did not report their tax incentives until the 2017 fiscal year.

then searched the CAFRs to determine whether municipalities were reporting on their tax incentives as required by GASB 77.

Figure 4 plots the percentage of municipalities that filed CAFRs and reported on their tax incentives in each year. First, note that the policy change certainly increased transparency: almost half of all municipalities reported their tax incentives publicly by 2018, while virtually none had done so as of 2015. Second, note that the vast majority of municipalities did not update their incentive reporting until the release of their 2017 CAFR. This is not indicative of noncompliance; governments were required to begin reporting in accordance with GASB 77 in fiscal years that began after December 15, 2015, which for almost all municipalities would have been the 2017 fiscal year. However, it raises the possibility that—despite the



Figure 5: Variation in GASB 77 noncompliance across states.

fact that incentive deals that were made in 2016 would need to be reported on the 2017 CAFR—municipal governments did not change their behavior until they first began actually complying with the new rule. To investigate this possibility, we plan to acquire IncentiveFlow data from 2017 and 2018 and use it to extend our analysis. For example, we can examine how incentive spending changes around two new potentially important cutoffs: the beginning of the 2017 fiscal year, and the release of the 2017 CAFR.

It is difficult to precisely estimate the extent of municipal noncompliance with GASB 77. This is largely because the IncentiveFlow data does not capture every single tax abatement that cities would be required to report. We therefore cannot determine whether a city that does not appear in the IncentiveFlow data and did not report tax incentives is compliant (they had nothing to report) or noncompliant (they gave tax incentives that we could not observe). However, we can calculate lower-bound noncompliance estimates by labeling municipalities noncompliant if they both:

- 1. Agreed to tax incentive deals in 2015/2016, as contained in the IncentiveFlow dataset;
- 2. Did not report these incentives on their 2017/2018 CAFRs.

This exercise produces an estimated noncompliance rate of 22% in 2017 and 20% in 2018. As Figure 5 shows, there is substantial state-level heterogeneity, and Californian municipalities (which comprise nearly a quarter of the sample) are particularly noncompliant. Strategic noncompliance—e.g., selection into non-reporting by municipalities who would stand to face the largest public opinion backlash if they were to report their incentive spending—could explain why we fail to see a consistent effect of GASB 77 of tax incentive spending.

Low Salience Among Policymakers

The previous section outlines concerns about compliance with GASB 77. If elected officials aren't subject to these new transparency standards, or they can delay reporting of these costs, incentive use may not change. Yet another plausible alternative is that even in cases where cities are complying with GASB 77, elected officials are either unable of GASB 77 or GASB 77 has no real impact on their offering of incentives.

To address the perceptions of elected officials, we fielded two surveys of local government officials in Fall 2021 through CivicPulse. One survey was administered to local policy makers in U.S. municipal governments with populations exceeding 1,000 residents. The second was administered to heads of finance from U.S. local governments with a population exceeding 1,000 residents. Both surveys were administered online and yielded 651 and 322 responses respectively. Our intention in fielding these surveys was not to test a causal theory, but rather to provide descriptive insights into government officials' perceptions.

Our main questions in both surveys asked respondents about familiarity of GASB 77 as



Figure 6: Local Finance Officers and Elected Officials have limited knowledge of GASB 77.

well as their compliance with the rule. We present the histogram of the responses in Figure 6 of policy makers (top panel) and finance officers (bottom panel). The results were striking from both surveys. Only 12 percent and 4 percent of respondents claimed to be very familiar with GASB 77, and a large number of respondents hadn't heard of this requirement.

We included addition question in surveys directly asking respondents if they included tax abatements in their annual financial reports. We present this data in Figure 7. To our surprise, 43 percent of elected officials indicated "don't know" when asked about tax abatement disclosures in their annual financial reports. Only 20 percent answered their abatements were included in their annual reports. City chief financial officers (bottom panel), were less likely to indicate they didn't know about abatement disclosures in their annual financial reports, but similar to the elected officials survey, only 23 percent indicated that their communith disclosed tax abatements.

These survey results are suggestive a lack of information and effort on GASB 77 and tax transparency compliance. It is important to note that although our tax incentive data ends in 2016, this survey, fielded in Fall 2021, is consistent with our empirical evidence of any



Figure 7: Local Finance Officers and Elected Officials have limited knowledge of their own municipality's GASB 77 reporting.

change in government effort post-GASB 77. Low levels of compliance, and knowledge, seem to limit the potential effectiveness of GASB 77.

Discussion and Conclusion

Economic development transparency continues to be a hard fought battle. In many states, NGOs sued state and local governments to release the details of offers made to Amazon HQ2, and transparency organizations such as Good Jobs First have painstakingly collect data on economic development incentives. GASB 77, the major national transparency change in state and local economic development, was hoped to rein in excessive economic development spending and lead to better policy making.

However, our empirical results from difference-in-differences models show that GASB 77 had no effect on tax incentive spending in affected municipalities, and we found no evidence of any other changes in economic development policy making. We posit three reasons for the lack impact of GASB 77. First, there are concerns that municipalities aren't complying with this rule. Our own analysis of thousands of annual financial reports finds that compliance with GASB 77 is far from universal, with a minimum of $\sim 22\%$ of municipalities failing to publicly report their tax incentive spending. This lack of compliance is in line with the existing work on the subject. Propheter (2021) finds systematic lack of compliance with GASB 77 in Colorado, and descriptive data by NGO Good Jobs first documents systematic noncompliance at city, county and school district with abatements. If the policy did not sufficiently increase cost transparency, this could explain its failure to change tax incentive spending.

Second, our original elite survey data shows a systematic lack of incentive reporting knowledge among local policymakers: familiarity with GASB 77, and with their own city's tax incentive reporting practices, is particularly low among elected officials. Cost transparency may therefore fail to change incentive spending, even in compliant municipalities, because the officials who stand to gain from brokering incentive deals do not know that it has been implemented. Increasing the salience of this accounting standard—informing policymakers that their incentive spending will be scrutinized by the public—could therefore be a low-cost method for improving economic development policy.

Finally, and most importantly for political science research, the unconditional benefits of transparency for improving public policy may have been overstated. It could be that GASB 77 successfully increased transparency in fiscal reporting, but this increased transparency had no meaningful impact on local governments' fiscal policymaking. This could be because transparency is not the same as salience; most citizens don't read their governments' financial accounts, and media outlets may fail to (sufficiently) publicize them. Further, even if transparency does increase salience among the public, the electoral effects may be so limited that policymakers feel no pressure to change their rent-seeking behavior. Future work should examine the conditions under which public policy transparency meaningfully improves the quality of governance.

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A Appendix

A.1 PanelMatch: Details on Estimation

PanelMatch is a nonparametric difference-in-differences estimator that proceeds in three steps. First, each treated observation is matched with a set of counterfactual observations that share its *L*-period treatment history but did *not* receive treatment at time *t*. Then, the matched set of counterfactual observations is adjusted to make sure that treated and counterfactual units are similar on observables. We use propensity score weighting to upweight counterfactuals that are similar to the treated unit in logged GDP, logged population, and the percentage of the population that voted for Obama in 2008.⁹ Finally, the treated observations and their matched sets serve as inputs for the PanelMatch estimator:

$$\hat{\delta}(F,L) = \frac{1}{\sum_{i=1}^{N} \sum_{t=L+1}^{T-F} D_{it}} \sum_{i=1}^{N} \sum_{t=L+1}^{T-F} D_{it} \left\{ (Y_{i,t+F} - Y_{i,t-1}) - \sum_{i' \in M_{it}} w_{it}^{i'} (Y_{i',t+F} - Y_{i',t-1}) \right\}$$
(2)

We set L = 6, and report estimates in each time period F from F = t - 6 to F = t + 16(again omitting t - 1 as the reference category). Standard errors are calculated via block bootstrapping. Additional data on the PanelMatch estimator can be found in Imai, Kim and Wang (2020).

⁹GDP is measured in 2014 at the CSA level, and comes from the U.S. BEA. Population is also measured in 2014 at the municipal level, and comes from the Census. Presidential voting in 2008 is measured at the municipal level, and comes from Elections Atlas.



A.2 PanelMatch: Tax vs. Non-Tax Incentives

Figure 1: **PanelMatch Estimates: Tax vs. Non-Tax**. Outcome variables are logged incentive spending ("Amount") and the proportion of incentives that went towards new projects. Estimates are adjusted for GDP, population, and 2008 presidential voting using propensity score weighting.



Figure 2: **PanelMatch Estimates: Tax vs. Non-Tax**. Outcome variables are logged new jobs created as a result of the incentive(s) and logged capital expenditure as a result of the incentive(s). Estimates are adjusted for GDP, population, and 2008 presidential voting using propensity score weighting.



A.3 PanelMatch: Tax Incentives in GAAP Mandating vs. Non-GAAP Mandating States

Figure 3: **PanelMatch Estimates: GAAP mandating vs. Non-GAAP mandating**. Outcome variables are logged incentive spending ("Amount") and the proportion of incentives that went towards new projects. Estimates are adjusted for GDP, population, and 2008 presidential voting using propensity score weighting.



Figure 4: **PanelMatch Estimates: GAAP mandating vs. Non-GAAP mandating**. Outcome variables are logged new jobs created as a result of the incentive(s) and logged capital expenditure as a result of the incentive(s). Estimates are adjusted for GDP, population, and 2008 presidential voting using propensity score weighting.