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

Calvin Thrall* Nathan M. Jensen†

Abstract

We examine the effect of increased transparency on the quality of 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. We leverage an exogenous increase in the transparency 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 two potential interpretations of the null result: first, it could be explained by imperfect compliance with the policy change. Second, it is possible that there are limits to transparency's ability to improve the quality of policymaking.

*PhD Student, University of Texas at Austin. cthrall@utexas.edu.

†Professor, University of Texas at Austin. natemjensen@austin.utexas.edu. The authors would like to thank the Smith Richardson Foundation for providing financial support for this project. Marilia Sampaio and Venus Shaghaghi provided excellent research assistance. Thanks also to Stephen Jessee and students in his Spring 2020 causal inference seminar for helpful comments. All remaining errors are Calvin's fault, except typos, which are probably Nate's fault.

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 less corruption, to better public policy, to increased 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 tax breaks in 2015. The economic logic of the corporate tax break 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).

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.

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

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.

Many of these economic incentive deals, including for Amazon HQ2 are shrouded in secrecy. We take advantage of a unique policy change - the Governmental Accounting Standards Board (GASB)'s Statement 77 - to provide an answer to the above question. 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.

Transparency and Tax Incentives

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.

There are several literatures that seek to explain the effects of various forms of governmental transparency. 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 higher levels of overall spending (Alt, Lassen and Skilling, 2002; Alt and Lowry, 2010). There are two potential interpretations of this result: first, it could be that fiscal transparency induces politicians to be more fiscally responsible, as Alt, Lassen and Skilling (2002) suggest. However, it could also be the case that fiscally responsible politicians are more likely to be fiscally transparent because they know that transparency will benefit them. Either way, transparent governments appear to be better fiscal stewards on average than their non-transparent counterparts.

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.

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. Likewise, tax incentives and electoral budget cycles only woo voters under conditions of nontransparency; when viewed in isolation, voters may view tax incentives as a rational exchange of future tax revenue for new jobs and investment today. If voters were made aware of their government's total spending on tax incentives, however, they may reevaluate the favorability of the exchange. It follows that tax incentives are only electorally valuable to incumbents when detailed information about incentive spending is not easily available to the public. 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

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

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

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 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 difference-in-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

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

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.

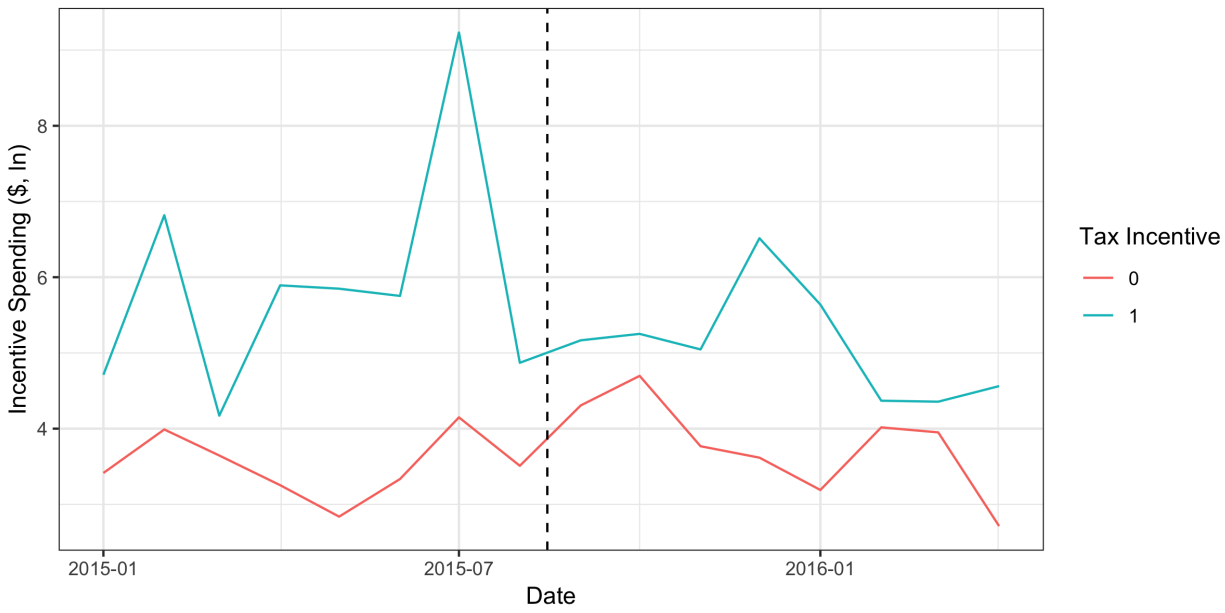


Figure 1: **Tax vs. Nontax Incentive Spending, pre- and post-GASB 77**

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.

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. Since the early 2000s, the SEC has allowed states to move away from GAAP standards towards the International Financial Reporting System (IFRS) standards.[5] As a result, 13 states no longer require their local governments to issue financial reports in accordance with GAAP standards, and thus they face no 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.

Using Angrist and Pischke (2009)'s notation, the general equation for both specifications can be written:

$$ln(Incentive)_{ist} = \lambda_t + \gamma_s + \delta D_{st} + \epsilon_{ist}$$

Time is indexed by t, treatment group is indexed by s, and municipality is indexed by i. In both specifications, λ_t is the time effect (effect of being post-GASB 77) and ϵ_{ist} is the municipality-group-time level error term. In the tax vs. nontax model, γ_s is the coefficient

[5] See https://www.sec.gov/spotlight/globalaccountingstandards.shtml

[6] The states are Alabama, Alaska, Idaho, Illinois, Indiana, Kansas, Michigan, Missouri, New York, North Dakota, Oklahoma, Washington, and West Virginia.

on the tax incentive dummy, and δ is the treatment effect (e.g. the effect of being a tax incentive post-GASB 77). In the GAAP vs. non-GAAP model, γ_s is the coefficient on the GAAP dummy, and δ is the treatment effect (e.g. the effect of being a GAAP state post-GASB 77).

Data and Sample

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 accessed IncentiveFlow data from the time range of January 2015 through December 2016 and prepared it in three way: first, we limit the data to incentives announced 8 months before and after GASB 77 was issued on August 15, 2015, to ensure equivalency of pre- and post-treatment time spans. Second, we aggregate the project-level incentive data to the city-month level. Finally, we create a dummy variable for incentives that were offered post-GASB 77. This allows me to use the most basic D-in-D setup: one treatment group, one control group, two time periods, binary treatment.

The key dependent variable is logged total incentive spending, measured at the municipality-month 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

Table 1: **Transparency and tax vs. non-tax incentive spending**

	Dependent variable:	
	Incentive spending (log)	
	(1)	(2)
Tax incentive	0.154***	0.154***
	(0.036)	(0.037)
Post-GASB 77	−0.008	−0.008
	(0.036)	(0.023)
Tax*Post-GASB	−0.018	−0.018
	(0.051)	(0.051)
Constant	0.127***	0.127***
	(0.025)	(0.017)
Observations	2,400	2,400
R^2	0.014	0.014
Adjusted R^2	0.012	-
CRSE	N	Y
Note:	*p<0.1; **p<0.05; ***p<0.01	

administration. The sample is restricted to U.S. municipalities with populations of at least 50,000, of which there are 757. For the GAAP vs. non-GAAP specification, we are comparing the pre-GASB 77 and post-GASB 77 incentives for each municipality, yielding 757*2= 1,514 observations. For the tax vs. nontax specification, we are comparing pre-GASB 77 and post-GASB 77 tax and nontax incentives for all municipalities in GAAP-compliant states (of which there are 700), yielding 700*2*2=2,400 observations.

Results

Table 1 displays the results of the model comparing municipalities' tax and nontax economic development incentive spending before and after GASB 77, with and without robust

Table 2: **Transparency and GAAP vs. non-GAAP tax incentive spending**

	Dependent variable:	
	Incentive spending (log)	
	(1)	(2)
GAAP	−0.195**	−0.195*
	(0.082)	(0.105)
Post-GASB 77	−0.067	−0.067
	(0.103)	(0.127)
GAAP*Post-GASB	0.044	0.044
	(0.115)	(0.136)
Constant	0.568***	0.568***
	(0.073)	(0.099)
Observations	1,514	1,514
R^2	0.006	0.006
Adjusted R^2	0.004	-
CRSE	N	Y
Note:	*p<0.1; **p<0.05; ***p<0.01	

standard errors clustered on the municipality.[7] As a reminder, we predict that GASB 77 will lead municipalities to reduce their incentive spending, and we expect a negative and significant coefficient on the interaction between the `Tax incentive` and `Post-GASB 77` variables. While the interaction term is negatively signed, it is small and not statistically significant. Thus, we cannot reject the null hypothesis that GASB 77 had no differential effect on tax vs. nontax incentive spending. In fact, the tiny and insignificant coefficient on the `Post-GASB 77` variable suggests that the issuance of GASB 77 may not have affected any incentives at all, at least in the time span covered by this sample.

Table 2 presents the results of the model comparing tax incentive spending in GAAP vs. non-GAAP municipalities pre- and post-GASB 77, again with and without robust standard

[7] Bertand, Duflo and Mullainathan (2004) note that this strategy helps mitigate the false positive problem with difference-in-differences estimation.

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 sign on the interaction between `GAAP` and `Post-GASB 77` variables. However, the difference-in-differences estimate is small, positive, and statistically indistinguisable from zero. Interestingly, municipalities in GAAP states do appear to spend less on tax incentives than municipalities in non-GAAP states, suggesting that fiscal reporting requirements may have an effect on incentive spending; however, the difference between GAAP and non-GAAP states does not widen following the issuance of GASB 77.

Neither of the difference-in-differences comparisons provide any evidence that GASB 77 reduced local governments' tax incentive spending. 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, I discuss potential explanations for these non-findings and avenues for future research.

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.

Unfortunately, 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, the timing and implementation of GASB 77 is complex. While GASB 77 was formally issued on August 15th, 2015 this ruling wasn't completely unexpected. For example,

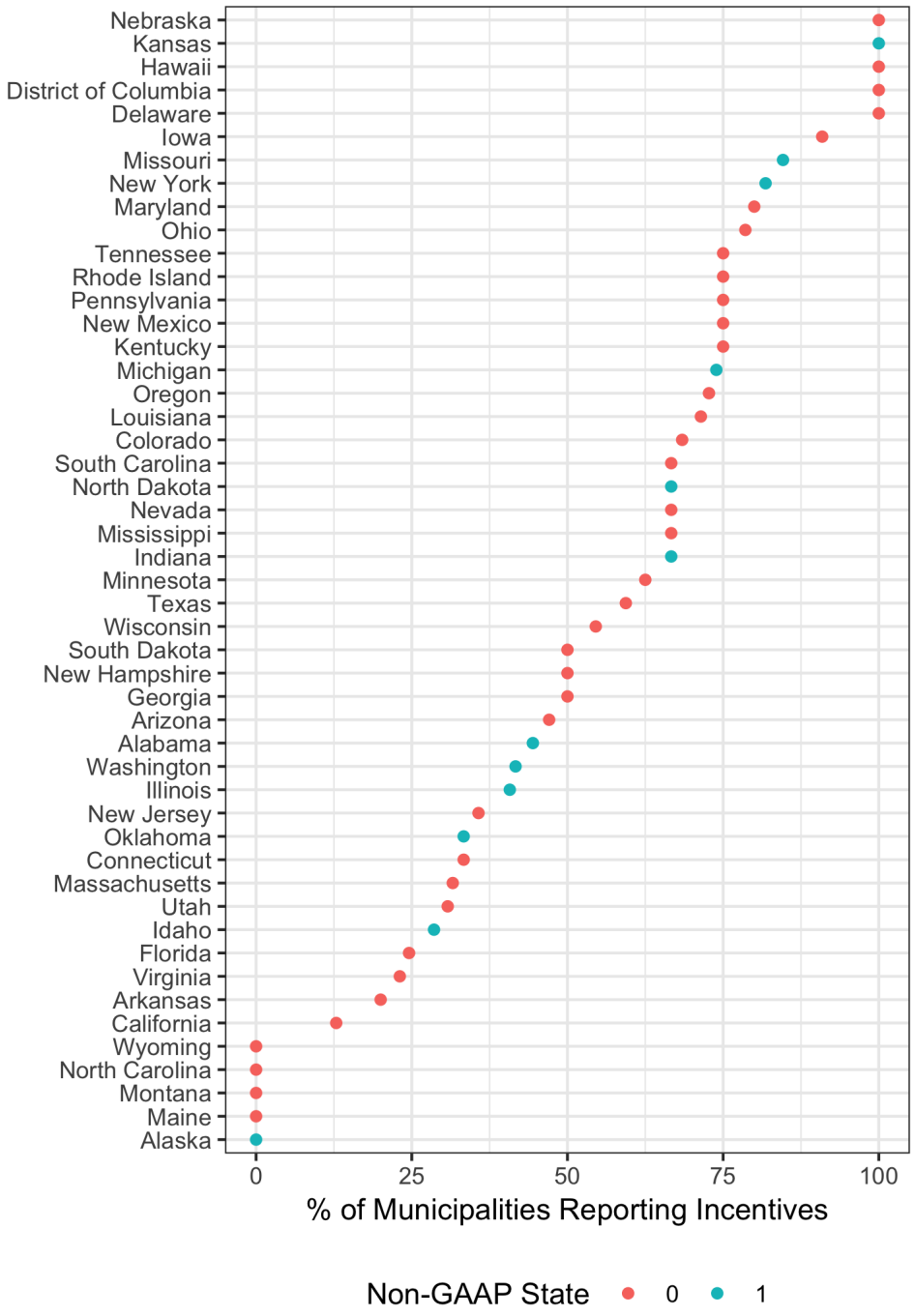


Figure 2: **Percentage of municipalities in compliance with GASB 77 as of 2017, by state**

the first draft of the statement was released for comments in October 2014; forward-looking local governments may have begun adjusting their incentive spending immediately upon the release of the draft statement.

Second there are concerns municipalities aren't complying with this rule. Our own analysis based on comprehensive annual financial reports (CAFRs) for reporting on incentives finds that compliance with GASB 77 is far from universal. Figure 2 displays the percentage of municipalities in each U.S. state (plus D.C.) that reported tax incentives in their 2017 CAFRs. First, note the striking amount of state-level variation: 75% of Pennsylvanian municipalities were compliant, compared to 60% of Texan municipalities and only 13% of Californian municipalities. Second, there does not appear to be a strong relationship between states' GAAP reporting requirements and their municipalities' incentive disclosure. The blue dots on Figure 2 indicate states that do not require their cities to follow GAAP standards. Note that three of these states are among the 10 most compliant, while only two of them are among the 10 least compliant states.

Unfortunately, we do not know if these cities didn't offer any incentives, or if they offered incentives and aren't complying. But for many cities that didn't offer incentives, they made clear that they were complying with GASB 77 and directly state they have no abatements to report. But for roughly half of cities in our data set, they make no mention of GASB 77.

Finally, and most important for political science research, the benefits of transparency in improving 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.

We can not fully distinguish between these three causes for the lack of any meaningful relationship between transparency and economic development policy. What is clear, at least to date, that GASB 77 failed to change government officials use of economic development incentives.

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