Rivers, Lakes and Revenue Streams: The Heterogeneous Effects of Clean Water Act Grants on Local Spending

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June 24, 2022

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Abstract

The Clean Water Act (CWA) funded $167 billion (2020$) in grants to municipal governments for wastewater treatment upgrades. We leverage variation in the timing of grant receipt with a difference-in-differences design to estimate the effect of CWA grants on local spending. On average, each dollar of grant revenue caused a $0.45 increase in sewerage capital spending. Dividing previously estimated benefit-to-cost ratios of CWA grants by this estimate suggests that each CWA grant dollar that municipalities spent on sewerage capital generated an average return of $1.01. In addition to funding grants, the Act set new capital standards for all wastewater treatment facilities in the United States. We show that CWA grants caused a dollar-for-dollar increase in sewerage capital spending up to the amount needed to cover the costs of capital upgrades newly mandated by the CWA, but after municipalities met these capital requirements, or if the capital mandate was not binding, they reduced their own spending on sewerage capital in response to grant receipt. Municipalities then redistributed grant money to local residents by reducing water bills.

JEL Classification: H72, H77, Q51, Q53, Q58, R53

Keywords: Local Government Expenditures, Intergovernmental Grants, Cost Benefit, Water Pollution, Environmental Regulation, Local Public Infrastructure

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From 1972 to 1988, the Clean Water Act funded \$167 billion (in 2020 dollars) in grants to municipal governments for capital upgrades to wastewater treatment facilities. While CWA grants caused significant reductions in pollution and increases in housing prices (Keiser and Shapiro, 2019), as well as improvements in infant health (Flynn and Marcus, 2021), analyses of the CWA consistently estimate benefit-to-cost ratios below one (Keiser et al., 2019) with each grant dollar generating an average return of \$0.45 (Keiser and Shapiro, 2019; Flynn and Marcus, 2021). Intergovernmental transfers like CWA grants account for around one-fifth of all municipal government revenues (Urban-Brookings Tax Policy Center, 2017), but donor governments often have little control over how receiving governments adjust spending in response to grant receipt. In most cases, having a high rate of "grant pass-through", the increase in targeted expenditure induced by a dollar of grant revenue, is a necessary condition for grant programs to accomplish their policy goals.

This paper uses recently developed difference-in-differences methods (Deshpande and Li, 2019; Callaway and Sant'Anna, 2020b; Callaway et al., 2021) to show that, after accounting for incomplete pass-through, the previously quantified benefits of CWA-funded capital upgrades approximately equal the costs of these upgrades. Our analysis combines data on every CWA grant with spending data from 112 municipal governments and compares changes in spending in newly treated municipalities to changes in spending in municipalities that will be treated in future periods to show that, on average, each dollar of grant revenue is associated with a \$0.45 increase in sewerage capital spending. We divide previously estimated benefit-to-cost ratios of CWA grants from Keiser and Shapiro (2019) and Flynn and Marcus (2021) by this pass-through estimate to calculate the benefit-to-cost ratio of capital investments funded by the CWA. Taking the benefit-to-cost ratios in Keiser and Shapiro (2019) and Flynn and Marcus (2021) as given, this back-of-the-envelope calculation suggests that each CWA grant dollar that municipalities spent on sewerage capital generated an average return of $1.01.

Along with funding grants, the Act set new capital standards for all wastewater treatment facilities in the United States. Municipalities could use grant funds to offset the new costs that the CWA's capital mandate imposed on them, but many CWA grants were substantially larger than the cost of coming into compliance with the mandate. This motivates an examination of how pass-through relates to mandated costs. Using data from an EPA engineering assessment of wastewater capital upgrade costs, we divide our sample of municipalities into those that received grants that were approximately equal to mandated costs and those that received grants that were larger than mandated costs. We show that grants caused a dollar-for-dollar increase in sewerage capital spending in municipalities that received grants that were approximately equal to mandated costs, but in municipalities that received grants that were larger than mandated costs, sewerage capital spending only increased by 27 cents for every dollar of grant funding. The EPA also distributed many CWA grants to municipalities with wastewater treatment facilities that were already in compliance with the CWA's capital mandate. The mandate imposed no costs on these "compliant" municipalities, and they only increased sewerage capital spending by three cents for every dollar of grant funding.

The heterogeneous effects of CWA grants on local spending inform the design of intergovernmental grant programs and our understanding of the Clean Water Act. By providing evidence of full pass-through in municipalities where the CWA's capital mandate was binding, our results suggest that, by pairing grants with new regulation, the CWA increased the likelihood of full pass-through and the desired policy outcome. This result may be generalizable to other intergovernmental transfer programs, such as grants for capital expenditure on highways and other infrastructure. This method of enacting policy is limited by the ability of donor governments to identify which receiving governments need fiscal assistance and estimate the costs of grant-funded programs. Both of these challenges to implementation hindered the CWA, leading to low grant pass-through.

Heterogeneity in pass-through across receiving municipalities can explain why our estimates are lower than previous estimates of CWA grant pass-through. In particular, Keiser and Shapiro (2019) leverages continuous variation in grant size in a difference-in-differences framework to show evidence of nearly full pass-through of CWA grants. This type of estimate is identified in part off of comparisons of municipalities that receive large grants to municipalities that receive small grants before and after grant receipt, but in order to identify an economically meaningful result using this type of comparison, we must assume that the effect of a grant of a given size is homogeneous across units (Callaway et al., 2021). Because compliant and non-compliant municipalities respond to grant receipt differently, this assumption does not hold, and observations of compliant municipalities after grant receipt will not make valid counterfactuals for non-compliant municipalities (and vice-versa) in pass-through estimations. We show that this causes pass-through estimates identified off of variation in grant size without accounting for heterogeneity in pass-through to over-estimate the effect of CWA grant funding on targeted spending.

Similar problems can arise whenever there is heterogeneity in the relationship between grant amount and targeted spending across receiving municipalities. Evaluations of intergovernmental grant programs often estimate pass-through using difference-in-differences designs that leverage continuous variation in grant size, but pass-through is likely correlated with grant size for many grant programs. For example, grant size is often positively correlated with receiving governments' preferences for public goods (Knight, 2002), so in many cases, the effect of a dollar of grant funding on targeted spending is increasing in grant size. This selection into grant size will bias pass-through estimates identified off of comparisons of units that receive large grants to units that receive small grants.

Finally, low pass-through in compliant municipalities motivates an examination of how these municipalities adjusted spending in response to grant receipt. CWA grants differ from many other intergovernmental transfers in that they support a function of local government that has a direct "fee-for-service" revenue stream where water and sewerage utilities collect charges from local residents. We show that compliant municipalities redistributed grant revenue to residents by reducing water and sewerage charges. This suggests that grant pass-through may be lower in settings where receiving governments control a channel through which they can redistribute money with relatively little friction. Municipalities receiving federal grants for electrical utilities (USDA, 2019) or waste management (USDA, 2020), as well as those receiving modern wastewater treatment grants (Travis et al., 2004) may be able to use the same crowd-out mechanism. Because downstream communities are the primary beneficiaries of increased sewerage capital spending, the low pass-through of unrestricted grant funds also suggests that crowd-out may be more severe when the costs of grant-funded public goods are more salient to local residents than their benefits.

Since annual historical spending data only exists for municipalities with populations over 75,000, we estimate pass-through for a relatively small number of municipalities. The municipalities in our sample are large, so our results represent the pass-through of $28 billion (2020$) of 167 billion total CWA grant dollars. While we cannot directly estimate the pass-through rate of CWA grants that donor governments distributed to smaller municipalities', we provide evidence that municipal population is not correlated with pass-through for municipalities in our sample, which suggests that CWA grants outside of our sample may have a similar pass-through rate to the grants in our sample.

1 Background

1.1 The Clean Water Act

Congress originally passed The Clean Water Act in 1948 as the Federal Water Pollution Control Act. Facing pressure to enact policy to reduce surface water pollution after a series of high profile river fires, Congress significantly expanded the CWA in 1972. The strengthened Act utilized both subsidies and new regulation to combat water pollution, distributing an estimated $167 billion (in 2020 dollars) in grants to fund upgrades to municipal wastewater treatment capital and imposing new capital standards on all municipal wastewater treatment plants in the United States.

The CWA funded wastewater treatment grants as follows. First, the EPA distributed grant money to states according to a congressionally mandated formula that was based on each state's wastewater treatment needs, as well as its total population and forecast population (Rubin, 1985). States then issued grants to municipalities according to priority lists that were based on the severity of nearby surface water pollution, the size of the population affected, the need for conservation of the affected waterway and that waterway's specific category of need.[1] States wrote their own priority lists, but these lists had to be approved

[1]We show that this did not lead to selection on treated potential outcomes in Appendix Section A.1

by the EPA annually. The Clean Water Act explicitly prohibited states from considering the geographic location of a receiving municipality, future population growth projections and any of a municipality's development needs that were not directly related to pollution abatement when writing priority lists (USEPA, 1980). Notably, federal and state governments did not explicitly allocate CWA grants according to the finances or spending preferences of receiving governments.

CWA grants funded capital upgrades to municipal wastewater treatment facilities. Municipal sewerage utilities collect wastewater from homes, businesses, and industries, as well as surface runoff, through a system of sewers, which deliver this wastewater to a treatment facility for processing and discharge into local waterways (USEPA, 2004). Ninety-eight percent of of these treatment plants are owned and operated by local governments (USEPA, 2002), and local governments fund these facilities through fees paid by local residents that consume water and sewerage services. Governments receiving CWA grants include cities, towns, and sewage districts (Keiser and Shapiro, 2019). Since historical spending data only exists annually for municipalities with large populations, this paper focuses on the effect of CWA grants on cities.[2]

Donor governments specifically designated many CWA grants for upgrades to wastewater treatment technology. When the CWA came into effect, more than a quarter of all wastewater treatment facilities in the US were using relatively inexpensive primary treatment (USEPA, 2000), which filters out large detritus and improves the aesthetics of surface water, but discharges all but the heaviest organic material into rivers and lakes (USEPA, 1998). The CWA required all wastewater treatment facilities to upgrade to more effective

[2]We discuss how generalizable our results are to other types of governments in Section 3.6.

secondary treatment technology, which removes about 85 percent of organic material before discharge, by 1977.[3] Additionally, many states required facilities to satisfy treatment technology requirements that were more stringent than the CWA's mandate (USEPA, 2000). These state requirements included mandates to upgrade to tertiary treatment, which aims at removing ammonium, nitrates, and phosphates (USEPA, 2000).

While the benefits of upgrading a facility's treatment technology were well understood, large upfront capital costs often made these upgrades prohibitively expensive without federal support. Upgrading could cost as much as 30 percent of the initial cost of the facility (National Environmental Research Center, 1972), so in general, municipalities only invested in secondary treatment or higher before the CWA came into effect because of either state level regulation or pressure from nearby communities to reduce the flow of harmful pollutants downstream (Jerch, 2018).[4]

These mandates were binding for most wastewater treatment facilities, and eighty-eight percent of grant dollars were distributed to facilities bound by state or federal treatment technology mandates. This set of facilities, which we refer to as non-compliant facilities, includes any facilities that were using less than secondary treatment technology, as well as facilities already using secondary treatment technology in states that required a level of treatment higher than secondary.[5] Municipalities could use grants for these facilities

[3]The EPA enforced the secondary treatment standard through the National Pollutant Discharge Elimination System (NPDES), the CWA's new permitting system, which required more than 65,000 industrial and municipal dischargers to obtain permits from the EPA or state governments. Permits required municipal treatment plants to employ secondary treatment and had to be renewed every five years. Violating the terms of a permit resulted in a compliance order or civil suit by the EPA, and violators could be fined up to $25,000 per day (Copeland, 2016).

[4]Facilities that needed to make upgrades to come into compliance with court orders could drive additional spending after grant receipt.

[5]Facilities that were in compliance with the CWA's capital mandate but were not in compliance with more

to upgrade from raw or primary treatment to secondary treatment, or to upgrade from secondary treatment to tertiary treatment. They could also make non-mandated upgrades.

Many CWA grants were larger than the cost of mandated treatment technology upgrades.[6] In an assessment of the wastewater treatment upgrades mandated by the CWA, the EPA estimated the cost of upgrading to secondary treatment as a function of the amount of wastewater flowing through a facility (USEPA, 1973). We use these estimates to calculate the cost of upgrading for each non-compliant facility that we have wastewater flow data. Figure 1 plots the total costs that each municipality faced against its total grant amount. This figure shows that, while grants were similar to costs for municipalities that received up to around $730 per capita in total grant aid, grants to municipalities that received more than this amount were substantially larger than the estimated cost of upgrading.[7]

The remaining twelve percent of CWA grant dollars went to facilities that were already in compliance with both state and federal treatment technology standards, which we refer to as compliant facilities. These grants funded non-mandated capital expenditures at wastewater facilities, such as capacity expansions. Grants to compliant municipalities were not bound by the CWA's capital mandate.[8]

To illustrate variation in compliance, grant purpose, and local spending, consider the example of two cities along the Willamette River in Oregon, historically one of the most

stringent state standards still faced incentives to spend CWA grants on upgrades, which is why we classify them as non-compliant. We show that our results are robust to dropping municipalities bound by state level regulations that were more stringent than the CWA's in Appendix Section D.2.

[6]States distributed grant dollars above the cost of upgrading to fund non-mandated capital expenditures, such as expansions of facility capacity.

[7]Figure A1 applies quadratic and linear fits to this data.

[8]The CWA allowed for grants to reimburse local governments that upgraded treatment technology prior to 1972 at a maximum 55% federal matching rate (USGAO, 1994), however, our grant data indicates that none of the complaint municipalities in our sample received grants for the purpose of reimbursement.

polluted rivers in the Pacific Northwest. Eugene, OR is located at the top of the Willamette, while Portland, OR lies at its base. To address municipal and industrial waste flowing downstream to Portland, the Oregon State Sanitary Authority (est. 1939) required Eugene and other municipalities discharging waste into the river to upgrade to secondary treatment in 1960. Because Eugene was bound by this state-level regulation, the city already met CWA treatment technology standards when they went into effect in 1972. Since Portland is located at the base of the Willamette, the city's discharge did not pollute other localities further downstream. Consequently, Portland was not subject to the 1960 treatment technology requirement and did not satisfy CWA treatment technology standards in 1972. Portland received CWA grant funding to upgrade its treatment facility to meet new federal standards in 1974. Although their facility already satisfied the CWA's capital mandate, Eugene received federal funding to expand facility capacity and upgrade one of its pump stations. Sewerage capital expenditure approximately doubled over the five years following grant receipt in Portland relative to the previous five as it upgraded its treatment technology to meet the new mandate, but sewerage capital expenditure did not increase at all in Eugene.

1.2 Grant Pass-Through

Despite its importance to policy design, researchers and policymakers alike remain unsure of when to expect grant programs to have higher or lower pass-through rates.[9] Economic theory predicts that local governments should treat inframarginal grant revenue (i.e. grant

[9] In many cases, grant programs do not affect reduced form outcomes of interest unless they first cause an increase in spending. Gordon (2004) demonstrates this principle in the context of K-12 education by showing that previous findings of null effects of Title I transfers on student achievement can be explained by local governments reducing their own contribution to education spending in response to grant receipt.

revenue smaller than existing local expenditure) as if it is an unconditional lump sum transfer (Oates, 1999), but empirical results suggest that this is not often the case, and that, even in the absence of binding constraints, inframarginal transfer revenue "sticks where it hits" (Henderson, 1968; Gramlich, 1969).[10] Known as the "flypaper effect," this phenomenon appears in a range of settings.[11] Proposed explanations for the flypaper effect include misperception by voters (Courant et al., 1979), mis-specification by researchers (Megdal, 1987), uncertainty (Vegh and Vuletin, 2015) and a focus by researchers on the short- rather than long-term effects of revenue changes on local spending (Helm and Stuhler, 2021), but a consensus has yet to emerge (Inman, 2008).

CWA grants did not place any explicit restrictions on local spending. For this reason, although CWA grants were allocated for specific projects, a municipality could redistribute these conditional grant funds toward other purposes if they were inframarginal to that municipality's budget. The median baseline sewerage expenditure in our sample ($125 per capita) was larger than the median yearly grant ($94 per capita), so a municipality could effectively disguise the continuation of existing spending as the use of grant funds while allocating grant dollars according to local preferences. Depending on local preferences, municipalities could spend inframarginal funds entirely on sewerage capital (full pass-through), entirely on

[10] While this pattern often holds, estimates of grant pass-through are not consistent across settings (Hines and Thaler, 1995), and pass-through estimates can vary widely among similar grant programs. For example, Card and Payne (2002) studies the effects of school finance reforms between 1970 and 1992 on education spending and finds that a one dollar increase in state aid leads to a 60 cent increase in district education spending. Lutz (2010) examines a similar policy where state governments distributed unrestricted lump sum transfers for education to municipalities based on per-pupil property wealth and finds that these transfers are almost completely crowded out by changes in local spending.

[11] For example, researchers have documented the flypaper effect in intergovernmental grants for education (McGuire, 1978; Craig and Inman, 1986; Fisher and Papke, 2000; Card and Payne, 2002; Brunner et al., 2020), infrastructure (Gamkhar, 2000, 2003; Leduc and Wilson, 2017), welfare programs (Grossman and Roberts, 1989; Gamkhar and Oates, 1996) and police funding (Mello, 2019).

other goods and services (full crowd-out), or a mix of sewerage capital and other goods and services.

CWA grants were matching grants. Grants covered 75% of wastewater facility project costs, while municipalities covered the remaining 25% themselves.[12] This local cost-sharing does not guarantee full pass-through, or even at least 25% pass-through, but rather increases the pre-grant spending necessary for grant funds to be considered inframarginal.[13] In particular, a 1:3 local-to-federal cost-sharing ratio implies that existing spending would need to be larger than $\frac{4}{3}$ times the grant amount for funds to be completely inframarginal.[14]

The interaction between the Clean Water Act's grant program and its wastewater treatment technology mandate yields several predictions for our grant pass-through estimates. The Act's mandate on wastewater treatment technology has the potential to ensure full pass-through when grant amount is set equal to the cost of mandated upgrades, but inaccurate grant sizing allows for crowd-out (when grants are too big) or imposes a fiscal burden on grant recipients (when grants are too small). The mandate imposes no constraint on how municipalities use inframarginal grant funds for compliant facilities.[15] Consequently, for cities already in compliance with the mandate, we predict pass-through near zero; for

[12]In 1981, the federal share of costs was reduced to 55%.

[13]Cost-sharing yields two different possible interpretations of pass-through. We define full pass-through as $1 in grant funding leading to a $1 increase in sewerage capital spending. Alternatively, since municipalities are required to pay 25% of project costs, one could define full pass-through as $1 in grant funding leading to a $1.33 increase in sewerage capital spending (the actual $1 in grant aid plus the $0.33 in local obligations). Our estimates are based on the first definition, appealing to the notion of federal funding itself being spent as intended by the donor government. We present pass-through estimates using the second definition in Appendix Section A.2.

[14]The median baseline spending in our sample ($125 per capita) is approximately $\frac{4}{3}$ times the median yearly grant size ($94 per capita).

[15]Compliant municipalities could still spend grant dollars on sewerage capital, but if they used this money to pay for upgrades that they already planned on making under their pre-grant budget constraint, CWA grants would not necessarily cause an increase in spending.

cities constrained by the mandate, we expect pass-through to decline in grant size relative to estimated costs.[16]

The contemporary political climate toward public utilities informs how municipalities may have redistributed grant funds that were not bound by the mandate. When the CWA came into effect, public utilities were facing pressure to reduce user fees and become more competitive (Daigger, 1998). Since local governments had the means to redistribute grants to residents with little friction by adjusting utility prices, CWA grants to compliant municipalities could crowd out sewerage capital spending already in place relatively easily.[17] Compliant municipalities could then use this crowded out money to offset water and sewerage utility costs, which are usually funded through water bills.

1.2.1 Target Parameter

The term "grant pass-through" can describe a number of target parameters. We focus on the increase in targeted spending induced by the average grant dollar.[18] This parameter, which we refer to as the "total pass-through rate", is equivalent to the increase in *total* targeted spending in *all* receiving municipalities relative to the *total* grant dollars distributed to *all* receiving municipalities. This average allows for an accounting of the effectiveness of an entire grant program. It also has an interpretation that is convenient for conducting benefit-

[16]Table A1 shows that compliant municipalities, non-compliant municipalities that received large grants and non-compliant municipalities that received small grants were observably similar before treatment. Table A2 shows that observable characteristics from 1971 do not predict grant size or compliance with the CWA's treatment technology mandate.

[17]The CWA instructed the EPA to issue guidelines to local governments on how to charge industrial and non-industrial users for waste treatment services, but did not place restrictions on adjusting rates.

[18]This is often the most policy-relevant parameter, but we may be interested in other parameters. For example, we may be interested in the increase in spending in the *average municipality* rather than the increase in expenditure induced by the *average dollar*. We estimate this "average pass-through rate" in Appendix Section A.3.

to-cost analyses; dividing the benefit-to-cost ratio of all grant dollars by the total pass-through rate will return an estimate of the cost-effectiveness of grant dollars that receiving municipalities actually spent on the targeted category of expenditure. Our research design aims to recover the total pass-through rate of CWA grants.

2 Data

2.1 Clean Water Act Data

We obtain data on every CWA grant from the EPA's Grant Information Control System. This data contains information on the year that the EPA distributed each grant, which municipality received the grant, the specific wastewater treatment facility the grant was designated for and the amount distributed.[19]

Using a unique facility code, we merge our grant data to the 1972 Clean Watershed Needs Survey (CWNS), an assessment of the capital investment that publicly-owned wastewater treatment facilities required to come into compliance with the Clean Water Act. Importantly, the EPA conducted the 1972 CWNS before distributing any CWA grants, so the CWNS provides information on the treatment technology that each facility had in place before grant receipt.

The CWNS also includes information on the total amount of wastewater flowing through many facilities. We use these flow rates to calculate the costs that each non-compliant facility faced to upgrade its wastewater treatment technology using cost estimates from

[19] Our analysis does not include grants distributed through predecessor programs similar to the CWA. See Appendix Section D.1 for further discussion.

USEPA (1973), a separate EPA analysis of upgrade costs.[20]

2.2 Municipal Finance Data

We merge this linked facility level data to spending data from the municipality that operates each facility, dropping facilities operated by municipalities that we do not have spending data for. We refer to municipalities with only compliant facilities as compliant municipalities and those with only non-compliant facilities as non-compliant municipalities. Since we cannot observe exactly how local governments spend grants for specific treatment plants, we drop the 45 municipalities in our finance data with both compliant and non-compliant facilities from our sample.[21] Many municipalities receive multiple grants during our study period, so we define treatment as an absorbing state that begins when a municipality receives its first CWA grant.

Our data on municipal finances comes from the Census Bureau's Historical Database on Individual Government Finances. This database includes annual financial data starting in 1951 sourced from the *Compendium of City Government Finances* and *City Government Finances* surveys. Initially the Census Bureau surveyed the universe of municipalities with a population of at least 25,000. This limit rose to 50,000 in 1960 and 75,000 in 1987. As a result, we observe spending in 216 municipalities every year from 1951 until 1999.[22]

[20] We do not calculate cost estimates for the 23 non-compliant municipalities in our sample that operate facilities that are missing information on total wastewater flow.

[21] Some facilities in the CWNS are missing information on compliance with treatment technology standards, however, no facilities operated by municipalities in our spending data are missing this information.

[22] Starting in 1967, the Census also collected spending data from a larger sample of municipalities once every five years. Since the data reports capital expenditure but not capital stock, missing years of data could cause us to under-estimate the effect of grant receipt on targeted spending, so we cannot use this data to estimate pass-through. We re-estimate the crowd-out results from Section 4 using this data in Appendix B.

Variables concerning municipal spending on water and sewerage utilities are available beginning in 1956. The data separates expenditure toward sewerage services into capital outlays and non-capital expenditure. We also observe revenue raised through a city's water and sewerage utility services (i.e. water bills). Because timing of grant receipt varies, we adjust both grant amount and all municipal spending outcomes to 2020 dollars.

States had some discretion about where they distributed grants, so grant receipt might be correlated with trends in municipal spending. Rather than leveraging variation in grant *receipt*, we focus our analysis on the municipalities in our finance data that received at least one CWA grant and estimate pass-through by leveraging variation in grant *timing*.[23] Table 1 presents summary statistics for these 112 municipalities from 1970, two years before Congress passed the CWA, split based on whether or not the city's wastewater treatment facilities were in compliance with the CWA's capital mandate when the Act came into effect.[24] All variables besides population are in per capita terms. Compliant and non-compliant municipalities demonstrate no statistically distinguishable differences across any of the observable characteristics presented in Table 1, though this is largely due to statistical imprecision from the small sample size. Comparisons of means suggest that, on average, non-compliant municipalities in our sample had larger populations and greater total revenues and expenditures than compliant municipalities. Taken at face value, transfer revenue from other governments to non-compliant municipalities and more lucrative property taxes explain this difference. Figure 2 shows the location of each municipality in our sample.[25]

[23]We obtain similar results when we use the municipalities in our finance data that never received a CWA grant as a control group in Appendix Section A.4.

[24]We show that observable spending characteristics do not predict grant timing in Appendix Section A.5.

[25]See Appendix D for more discussion of our data.

3 Grant Pass-Through

3.1 Pass-Through Methods

We want to select a design that recovers the total pass-through rate. To do this, we need an estimate of the average treatment effect on the treated (ATT), which describes the average increase in sewerage capital expenditure after grant receipt. Dividing the ATT by average grant amount returns the pass-through rate for the average CWA grant dollar in our sample.[26]

We follow Callaway and Sant'Anna (2020b) and leverage variation in the timing of CWA grant receipt to estimate "group-time average treatment effects". At least one municipality receives its first CWA grant in every year from 1972 to 1981, as well as in 1985 and 1988. This variation in treatment timing yields 12 "timing groups", which we index $g = 1972, ..., 1981, 1985, 1988$. We estimate the group-time average treatment effect for each group g in each time period t by comparing units in g to units that were not yet treated in time t. We then summarize the reduced form effect of grant receipt on sewerage capital

[26]Two-way fixed effects (TWFE) estimators are one of the most common ways to leverage variation in treatment timing. This estimator regresses targeted spending on a treatment dummy and unit and time fixed effects. The resulting estimate is a weighted average of comparisons between (1) spending in newly treated municipalities relative to spending in municipalities that have not yet been treated and (2) spending in newly treated municipalities relative to spending in already-treated municipalities. While the first type of comparison only requires a parallel trends assumption to recover an estimate of the ATT, to use the second type of comparisons, we must assume that treatment effects are constant over time. If treatment effects are dynamic, already-treated units, which are still actively responding to treatment, will not make valid counterfactuals for what would have happened in the absence of treatment, so the second type of comparison does not return a meaningful result and we cannot interpret a TWFE coefficient as the ATT (Goodman-Bacon, 2021a). We have an a priori reason to expect the effect of grant receipt on spending to change over time; states distributed CWA grants to fund specific projects, so we would expect grant receipt to cause an increase in sewerage capital spending that only lasts until the project is completed. When spending returns to pre-treatment levels, this decrease is subtracted from a TWFE estimate, biasing the estimate upwards. Since this estimator does not return the ATT, we cannot recover the total pass-through rate from a TWFE estimate, which motivates us to use a different estimator.

spending by aggregating these group-time average treatment effects together.[27]

Under a parallel trends assumption, the group-time average treatment effect for group g at time t is

$$ATT(g,t) = E[C_t - C_{g-1}|G_g = 1] - E[C_t - C_{g-1}|D_t = 0]$$

where C_t is average sewerage capital spending at time t, C_{g-1} is average sewerage capital spending the year before treatment, G_g is a dummy variable that equals one for units in timing group g and D_t is a dummy that equals zero for units not-yet-treated at time t. We estimate each $ATT(g,t)$ with its sample analogue, $\widehat{ATT(g,t)}$.This process yields many $\widehat{ATT(g,t)}$, most of which are identified off of relatively few observations, so instead of interpreting individual $\widehat{ATT(g,t)}$, we summarize the effect of treatment by aggregating the $\widehat{ATT(g,t)}$ together.

We aggregate group-time treatment effects into summary measures in several ways. First, we examine how the effect of CWA grant receipt on sewerage capital expenditure evolves over time with an event study specification. For each time relative to treatment e, we estimate the effect of treatment for units that have been treated for e periods with $\widehat{\theta}_D(e)$.

$$\widehat{\theta}_D(e) = \sum_{g=1972}^{1985} \sum_{t=1957}^{1987} 1\{t - g = e\} \widehat{ATT(g,t)} P(G = g|t - g = e) \tag{1}$$

Taking an average of the $\widehat{\theta}_D(e)$ for e such that $e \geqslant 0$ provides a summary measure of the

[27] Since this estimator only relies on comparisons between newly treated units and not-yet-treated units, we do not need to place any restrictions on treated potential outcomes and our estimates will not be biased by dynamic treatment effects.

reduced form effect of grant receipt on sewerage capital spending.

Another way to summarize this effect is to take a weighted average of all of the $\widehat{ATT(g,t)}$ such that $g \leqslant t$ where the weights are based on timing group size. The formula for this weighted average is

$$\frac{1}{\kappa} \sum_{g=1972}^{1985} \sum_{t=1972}^{1987} 1\{g \leqslant t\} \widehat{ATT(g,t)} P(G = g) \tag{2}$$

where $\kappa = \sum_{g=1972}^{1985} \sum_{t=1972}^{1987} 1\{g \leqslant t\} P(G = g)$.

We can use these estimates to recover the total pass-through rate. Under a parallel trends assumption, our summary measures of the effect of grant receipt on sewerage capital spending represent estimates of the average increase in targeted spending after grant receipt. We can repeat our estimation process with grant amount as the dependent variable to estimate the associated first stage. Dividing the reduced form effect of grant receipt on sewerage capital spending by the first stage relationship between grant receipt and grant amount returns the average increase in sewerage capital spending induced by a dollar of grant funding, which is the total pass-through rate.

To identify a given $ATT(g,t)$, we must assume that, in absence of treatment, the average outcomes in timing group g would have followed parallel trends with not-yet-treated groups in period t. Since we are primarily interested in summary measures, we do not need to interpret individual $\widehat{ATT(g,t)}$ and we do not need this assumption to hold for all group-time pairs. Instead, we must assume that, on average, parallel trends holds for all groups g in all periods t such that $g \leqslant t$.

Callaway and Sant'Anna (2020a) constructs standard errors with a multiplier bootstrap procedure. Instead of re-sampling observations as in a pair-bootstrap, each multiplier bootstrap draw perturbs the influence function of the estimate (which measures the dependence of the estimate on each cluster in the sample). Our pass-through estimator is a function of two Callaway and Sant'Anna (2020a) objects (specifically, the reduced for divided by the first stage). Rather than derive the influence function for this estimator, we present pair-bootstrap standard errors clustered at the municipality level for our pass-through estimates.[28]

3.2 Full Sample Pass-Through Results

We begin with a flexible specification that explores how the effect of grants evolves over time. Figure 3 examines the first stage relationship between grant receipt and grant amount by presenting the $\widehat{\theta}_D(e)$ from equation 1 with grant amount as the dependent variable. Each $\widehat{\theta}_D(e)$ in Figure 3 is equal to average grant amount e years after treatment. Figure 4 then presents the associated reduced form estimates of the effect of grant receipt on sewerage capital spending. The null effects in the 16 years before grant receipt support a research design that leverages variation in grant timing by showing common trends in sewerage capital spending in treated and not-yet-treated municipalities before treatment, which suggests that parallel trends would have continued in absence of treatment. The estimates increase after a municipality receives its first CWA grant and remain high for seven years after grant receipt. Sewerage capital spending decreases to near pre-treatment levels by eight years after grant receipt. The EPA estimates that upgrades paid for with CWA grants could take up to 10 years from initial grant receipt to project completion (USEPA, 2002), so the shapes of

[28] We discuss standard errors and our choice of estimators in greater detail in Appendix C.

Figures 3 and 4 are consistent with grant funding and sewerage capital spending returning to pre-treatment levels as municipalities complete grant funded projects.[29]

Table 2 summarizes the results in Figures 3 and 4. First, Panel A presents averages of the $\widehat{\theta}_D(e)$ from Figures 3 and 4. Since the effect of grant receipt on both sewerage capital spending and grant amount return to near pre-treatment levels by seven years after treatment, Panel B re-calculates the results in Panel A for the fist seven post-treatment periods.

Since treatment timing varies, we do not observe all of the municipalities in our sample for the same number of post-treatment periods. For this reason, the composition of the groups that contribute to each event study coefficient in Figures 3 and 4 changes when event time changes. This causes our dynamic aggregations in Panels A and B of Table 2 to weight up observations of municipalities that are treated earlier, which can produce misleading results if the effect of grant receipt varies across timing groups. We address this in Panel C by re-calculating the results in Panel B on the sub-sample of municipalities that we observe for at least seven post-treatment periods. The estimates are similar to those in Panel B.[30]

Finally, Panel D of Table 2 presents a weighted average of all post-treatment group-time-treatment effects calculated with equation 2.

Dividing the reduced form estimates in Table 2 by their respective first stage estimates suggests that CWA grants have a total pass-through rate between 0.446 and 0.480. Across aggregations, the 95% confidence intervals of the first stage and reduced form estimates do not overlap and we can reject a test of the hypothesis that our pass-through estimate equals one, so we reject full grant pass-through for the full sample.

[29] Table A3 presents the results in Figures 3 and 4 in tabular form.

[30] Figure A2 presents the associated event study.

3.3 Evaluating CWA Investments

We can use estimates of the total pass-through rate for the full sample to find the benefit-to-cost ratio of the CWA grant dollars that municipalities spent on sewerage capital (i.e., the benefit-to-cost ratio of the wastewater capital upgrades themselves). Using increased housing prices to quantify the benefits of CWA grants, Keiser and Shapiro (2019) estimates a benefit-to-cost ratio of 0.26. Flynn and Marcus (2021) finds that this ratio may be as high as 0.45 after incorporating improvements to infant health.[31] Dividing this benefit-to-cost ratio by the total pass-through rate yields the benefit-to-cost ratio of CWA grant dollars that municipalities spent on sewerage capital. Our preferred specification in Panel B of Table 2 shows that, in total, receiving governments in our sample spent 44.6% of CWA grant dollars on sewerage capital upgrades. Dividing these results implies that the CWA grant dollars that municipalities spent on sewerage capital have a benefit-to-cost ratio of 1.01.[32] This suggests that, after accounting for incomplete pass-through, the previously quantified benefits of CWA-funded capital upgrades approximately equal the costs of these upgrades.[33]

The standard error on our preferred specification suggests a 95% confidence interval of 0.165 to 0.727. Dividing previously estimated benefits of CWA grants by the upper bound lets us reject a benefit-to-cost ratio less than 0.62.

We examine which municipalities are driving this low pass-through in the next section.

[31] This assumes that hedonic estimates do not capture any health benefits. It is unlikely that this historical population fully understood the relationship between surface water quality and the health of infants in utero, so this assumption is plausible.

[32] The other results in Table 2 suggest that the CWA grant dollars that municipalities spent on sewerage capital had a benefit-to-cost ratio of between 0.937 and 1.01. This benefit-to-cost ratio is even higher if we use the estimates from our stacked difference-in-difference in Table A15, which suggest a benefit-to-cost ratio of 1.32.

[33] We discuss potential shortcomings of this back-of-the-envelope calculation in Appendix Section A.7.

3.4 Heterogeneity in Pass-Through

We explore heterogeneity in pass-through by mandated costs. Compliant municipalities face no mandated costs, and mandated costs vary within our sample of non-compliant municipalities. Since, in theory, setting grant amount equal to mandated costs increases the likelihood of full pass-through, we estimate pass-through separately for compliant municipalities, non-compliant municipalities for which grant amount is approximately equal to mandated costs, and non-compliant municipalities for which grant amount is greater than mandated costs.

Splitting the sample along compliance is straightforward, but it is not immediately obvious how to divide our sample of non-compliant municipalities based on mandated costs. We first need to identify which grants are approximately equal to mandated costs and which grants are larger than mandated costs. Once we define these groups, we can separately estimate pass-through for (1) non-compliant municipalities whose grant funding is completely bound by the capital mandate and (2) non-compliant municipalities whose grants include funding that is not bound by the capital mandate. Figure 1 shows that, while grants are similar to costs for municipalities that receive relatively small grants, grants to municipalities that receive large grants are substantially larger than the estimated cost of upgrading. For this reason, we want to divide our sample of non-compliant municipalities into those that received large and small grants.

We divide our sample at the point on the distribution of grant amount where costs are no longer increasing in grant size.[34] To estimate this point, we keep one observation from each

[34] This is the point where the quadratic fit in Figure A1 flattens out.

non-compliant municipality that we have cost data for and estimate the following equation with non-linear least squares

$$
\begin{aligned}
cost_i =&(a_1 + b_1 * TotalGrant_i) * 1\{TotalGrant_i < split\} \\
&+ (a_1 + b_1 * split + b_2 * (TotalGrant_i - split)) * 1\{TotalGrant_i >= split\}
\end{aligned}
\tag{3}
$$

where $cost_i$ is our estimate of municipality i's total per capita cost of upgrading and $TotalGrant_i$ is municipality i's total per capita grant amount. Column 1 of Table 3 presents our estimate of *split*. This result shows that costs are increasing in grant size up to \$730 of total per capita in grants and flatten out for grants above \$730. We estimate pass-through separately for non-compliant municipalities above and below this threshold.[35]

3.4.1 Semi-Parametric Methods

Rather than imposing parametric restrictions on the relationship between grant amount and targeted spending, we explore heterogeneity in pass-through using an approximation of the dose-response function relating grant amount and sewerage capital spending. As shown in Figure 1, there is substantial variation in grant amount, and plotting out each municipality's total grant amount will give us the x-axis of this dose-response function. We can then find the y-axis by estimating each municipality's response to grant receipt.

To do this, we follow Deshpande and Li (2019) and re-organize our data into "stacks".

[35] The EPA did not include facilities with a total flow rate of less than 5 million gallons per day in their sample when estimating the cost functions that we use to calculate costs. These cost functions over-estimate costs for smaller facilities (USEPA, 1973). Most municipalities in the bottom decile of grant aid had facilities of this size, so we drop these municipalities when we estimate the results in column 1 of Table 3. We re-estimate equation 3 on the full sample in column 2, which yields an estimated cutoff of \$805 per capita. Table A4 shows that our pass-through estimates are similar if we use either of the cutoffs in Table 3.

Each stack S is defined by a single treated municipality, called municipality s, which is labeled as treated in that stack. We then add municipalities that received grants at least seven years after the treated municipality to the stack, which are labeled as controls. We can estimate the reduced form effect of grant receipt on sewerage capital spending for municipality s by comparing spending between municipality s and the control municipalities in stack S before and after the year in which municipality s becomes treated.[36] Estimating equation 4 on observations in stack S returns the result from this comparison.

$$C_{it} = \alpha_0 + \beta_s^{2X2} post_t * treat_i + \delta post_t + \omega treat_i + \epsilon_{it} \tag{4}$$

Municipalities are indexed by i, stacks by s, and years by t. C_{it} is per capita sewerage capital spending, and $treat_i$ is a dummy variable that equals one for municipality s (the treated unit). Plotting β_s^{2X2} against the total amount of grant dollars distributed to municipality s for all stacks yields a semi-parametric approximation of the dose-response relationship between grant amount and sewerage capital spending.[37]

We summarize these figures by appending groups of stacks into one dataset and estimating

[36]By estimating the effect of grant receipt on *individual units* across *all post treatment periods*, a stacked estimator allows us to semi-parametrically examine the relationship between grant amount and sewerage capital spending before constructing summary measures of pass-through. Since timing groups contain both compliant and non-compliant municipalities, as well as municipalities that received different sized grants, we cannot repeat this process with Callaway and Sant'Anna (2020a) without making a priori assumptions about the relationship between grant amount and sewerage capital spending, as well as the relationship between compliance and sewerage capital spending.

[37]To identify the β_s^{2X2}, we must assume that sewerage capital spending in municipality s and the control municipalities in stack S would have followed parallel trends in absence of treatment. This is stronger than the assumption we need to identify $\beta_{rf}^{stacked}$ in equation 6, which only requires us to assume that parallel trends holds on average (we derive this identifying assumption in Appendix Section C.4.2). For this reason, we should interpret individual β_s^{2X2} with caution. Note that, under parallel trends, each β_s^{2X2} represents the effect of going from zero grant dollars to the grant dollars distributed to municipality s *on municipality* s. This does not tell us anything about the effect of going from municipality $s-1$'s grant amount to municipality s's grant amount, or the effect of grant receipt on any other municipality.

a stacked difference-in-difference. Equation 5 estimates first stage relationship between grant receipt and grant amount, denoted g_{it},

$$g_{it} = \alpha_0 + \beta_{fs}^{stacked} D_{it} + \alpha_{is} + \alpha_{ts} + \epsilon_{its} \tag{5}$$

and equation 6 estimates the reduced form effect of grant receipt on sewerage capital spending,

$$C_{it} = \alpha_0 + \beta_{rf}^{stacked} D_{it} + \alpha_{is} + \alpha_{ts} + \epsilon_{its} \tag{6}$$

where D_{it} is a dummy variable that equals one after grant receipt, and α_{is} and α_{ts} are stack-by-municipality and stack-by-year fixed effects. In tandem, theses fixed effects force identification to come from within-stack variation, ensuring that our estimates will not reflect any comparisons between newly treated municipalities relative and already-treated municipalities. Since it is only identified off of within-stack comparisons, $\beta_{rf}^{stacked}$ is an unweighted average of all the β_s^{2X2}. We present pair-bootstrap standard errors clustered at the municipality level.[38]

3.4.2 Pass-Through Results for Sub-Groups of CWA Grants

Figure 5 examines the relationship between grant amount and sewerage capital spending in non-compliant municipalities. This figure plots out the β_s^{2X2} from estimating equation 4

[38] To construct standard errors, we take a random sample (with replacement) of municipalities in the unstacked data, re-form stacks, then perform our estimates. We repeat this process 1000 times and use the results to calculate bootstrap standard errors. See Appendix Section C.4 for further discussion of stack construction and inference with a stacked difference-in-differences estimator.

on units in each non-compliant stack against municipality s's total grant amount.[39] Sewerage capital spending is increasing in grant amount for municipalities that received grants totaling up to \$730 per capita, but this relationship is flat for municipalities receiving more than \$730 per capita. Importantly, Figure 5 shows a relationship between the β_s^{2X2} and total grant amount that is very similar to the relationship between cost and total grant amount shown in Figure 1.

Figure 6 repeats this process for compliant municipalities. This figure shows that grants to compliant municipalities had low pass-through regardless of size.

Table 4 summarizes Figures 5 and 6 by estimating equations 5 and 6 on sub-samples of stacks. Panel A presents pass-through estimates for non-compliant municipalities that received grants totaling up to \$730 per capita. The pass-through estimate in column 3 suggests that CWA grants led to a dollar-for-dollar increase in sewerage capital expenditure in these municipalities. Panel B presents estimates for all other stacks defined by non-compliant municipalities, which have much lower pass-through. Panel C presents estimates for all stacks defined by compliant municipalities, which also have low pass-through.[40]

Interpreting the results in Table 4 as the total pass-through rate suggests that non-compliant municipalities in our sample that received grants totalling less than \$730 dollars per capita increased sewerage capital spending by 104.9% of all grant dollars distributed to them, while all other non-compliant municipalities in our sample increased sewerage capital spending by 26.7% of all grant dollars distributed to them. Compliant municipalities in our

[39] Because they reflect averages across seven years, we multiply the β_s^{2X2} in Figures 5 and 6 by seven. This makes these figures more comparable in scale to Figure 1 and with one another.

[40] We show that differences in pass-through between sub-groups are not driven by differences in baseline sewerage capital spending in Appendix Section A.8.

sample increased sewerage capital spending by 3.37% of grant dollars distributed to them.[41]

Based on the above results, we reject full pass-through for compliant municipalities and non-compliant municipalities receiving more than $730 per capita in grant aid, but cannot reject full pass-through for non-compliant municipalities receiving grants totaling less than $730 per capita, as expected.[42] While we cannot reject complete crowd-out in non-compliant municipalities that received grants totaling up to $730 per capita or equality of the pass-through estimates across groups, these results, along with the shapes of Figures 5 and 6, are consistent with municipalities only increasing sewerage capital spending up to the point where they are in compliance with the CWA's capital mandate.[43]

3.5 Does Variation in Grant Size Recover Total Pass-Through?

Researchers often estimate pass-through by leveraging variation in grant size with a dose-response two-way fixed effects estimator. In this section, we follow Callaway et al. (2021) to show that, when the effect of a grant of a given size is not constant across receiving municipalities, or when the effect of grant receipt changes over time, this type of estimator may not return the total pass-through rate.

Consider, as an example, the pass-through estimates in Keiser and Shapiro (2019), which

[41] Table A5 shows that pass-through was low for compliant municipalities that received large and small grants alike, which suggests that the heterogeneity in pass-through by grant size documented in Panels A and B of Table 4 is driven by differences in costs rather than other confounding factors associated with grant size that are similar across compliant and non-compliant municipalities.

[42] We obtain similar results when we re-estimate the results in Table 4 with Callaway and Sant'Anna (2020a). We present these results in Appendix Section A.6, and discuss our choice of estimators in Appendix C.

[43] Since we define municipalities that already met the CWAs capital requirement as non-compliant, this result does not mean that municipalities only spent CWA grant dollars on upgrading to secondary treatment. It instead suggests that municipalities only spent CWA grant money on mandated upgrades, which could include installing secondary treatment or higher.

suggest that municipalities that received any CWA grant spent \$0.94 of every federal dollar on sewerage capital by estimating

$$C_{it} = \alpha_0 + \beta_{dr}^{TWFE} g_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{7}$$

where C_{it} is cumulative sewerage capital spending from 1970 to 2002 and g_{it} is cumulative grant amount.[44] β_{dr}^{TWFE} is a weighted average of (1) comparisons of newly treated municipalities relative to municipalities that have not yet been treated, (2) comparisons of newly treated municipalities relative to already-treated municipalities, and (3) comparisons of municipalities that received large grants relative to municipalities that received smaller grants.

To interpret comparisons of newly treated municipalities relative to already-treated municipalities as the ATT, we need to make additional assumptions along with parallel trends. When treatment effects are dynamic, already-treated units, which are still actively responding to treatment, will not make valid counterfactuals for what would have happened in the absence of treatment. For this reason, we must assume that treatment effects do not change over time to use this type of variation (Goodman-Bacon, 2021a).

Identifying a meaningful effect using comparisons of municipalities that received large grants relative to municipalities that received smaller grants requires additional assumptions as well. In particular, we must assume that a grant of a given size will have the same effect on sewerage capital spending in all receiving municipalities in our sample (Callaway et al., 2021). If this assumption does not hold, municipalities that received small grants will not be

[44] Note that our highest pass-through estimate (0.48 with a standard error of 0.207) could encompass the lowest estimate in Keiser and Shapiro (2019) (0.84 with a standard error of 0.19).

valid counterfactuals for what would have happened if a municipality that received a large grant had received a small grant instead, and this type of comparison can either over- or under-estimate the total pass-through rate.

The results in Sections 3.2 and 3.4.2 suggest that the effect of CWA grants on sewerage capital spending is not homogeneous across time relative to treatment, or across receiving municipalities. As shown in Figure 4, treatment effects change over time as grant funding runs out. Additionally, Figure 5 suggests that the effect of grants to non-compliant municipalities is increasing in grant size (up to the point where the capital mandate is satisfied), while Figure 6 suggests that grants to compliant municipalities had little to no effect on sewerage capital spending regardless of grant size. For this reason, a grant of a given size would likely affect spending differently in a compliant municipality than in a non-compliant municipality, which violates the homogeneity assumption.[45]

The reliability of an estimate of β_{dr}^{TWFE} from equation 7 depends on how much of the estimate is identified off of comparisons of early treated units to not-yet-treated units. Table 5 decomposes β_{dr}^{TWFE} using Goodman-Bacon (2021b), which shows how much of β_{dr}^{TWFE} is identified off of comparisons of newly treated units to not-yet-treated units, comparisons of newly treated units to already treated units, and comparisons of units that received large grants to units that received smaller grants.[46]

Row 1 of Table 5 describes the part of β_{dr}^{TWFE} identified off of comparisons of newly

[45]Estimating β_{dr}^{TWFE} on compliant and non-compliant municipalities separately would solve the problem introduced by heterogeneity in treatment effects by compliance, but would not address problems caused by dynamic treatment effects. For this reason, estimating β_{dr}^{TWFE} on sub-groups of municipalities will not necessarily return the total pass-through rate for these sub-groups. See Appendix Section C.2 for further discussion of the assumptions required to use this type of variation.

[46]This program is the analogue of Goodman-Bacon et al. (2019) for difference-in-difference estimates identified off of continuous variation.

treated units to not-yet-treated municipalities. This variation suggests a pass-through rate of 0.428, which is similar to the pass-through estimates in Table 2.[47] This represents an estimate of the total pass-through rate, but less than two percent of β_{dr}^{TWFE} comes from this type of comparison.

Row 2 of Table 5 shows that more than half of β_{dr}^{TWFE} comes from comparisons of newly treated municipalities to already-treated municipalities. Consistent with bias from treatment effects that decrease over time, comparisons of newly treated municipalities to already-treated municipalities yield a result that is more than twice as large as the result from comparing newly treated municipalities to not-yet-treated municipalities.

Finally, Row 3 of Table 5 shows that a large portion of β_{dr}^{TWFE} comes from comparisons of municipalities that received large grants to those that received smaller grants. Many of these comparisons are between compliant and non-compliant municipalities, which do not represent causal estimates.

The decomposition in Table 5 shows that, when the effect of a grant of a given size is heterogeneous across receiving municipalities, dose-response TWFE estimators will produce estimates that may not equal the total pass-through rate.

[47] The part of the pass-through estimate in Keiser and Shapiro (2019) that is identified off of comparisons of newly-treated units to not-yet-treated units is identified off of the same type of variation as the pass-through estimates in Table 2. The similarity of these results suggests that, even though we make several sample restrictions that Keiser and Shapiro (2019) do not (we drop both municipalities that are missing information on compliance and municipalities in our finance data with both compliant and non-compliant facilities), the differences between our estimates and those in Keiser and Shapiro (2019) come from differences in the source of variation we use rather than differences between our samples.

3.6 Can We Generalize These Results to All CWA Grants?

Our results represent the pass-through of $28 billion (2020$) of the $167 billion in grants funded by the CWA. In this section, we discuss how generalizable our results are to CWA grants outside of our sample.

The Census Bureau collects annual finance data from the universe of municipalities with populations over 75,000, so our sample only includes grants distributed to relatively large cities. If population is correlated with preferences for sewerage capital spending, grants to municipalities outside of our sample might have a different pass-trough rate than grants to municipalities within our sample. We adjust for heterogeneity in pass-through by population in Table 6 by weighting our pass-through results by population.[48] This does not change our results in a meaningful way, which suggests that, within our sample, treatment effects are not correlated with population.[49] Since the Census data that we use is the only comprehensive source of local government finance data collected annually on a nationwide scale (US Census Bureau, 2021), we cannot test if this homogeneity holds *outside of our sample*, but the similar pass-through rates across municipality size *within our sample* suggests that CWA grant pass-through is not strongly correlated with population.

[48] Figures A3 and A4 present the associated event studies. Since weighting is inefficient when treatment effects are homogeneous across population, the coefficients are less precisely estimated than those in Figures 3 and 4.

[49] We test for this in the Appendix in three additional ways. First, Table A6 re-estimates our pass-through results on sub-samples of municipalities divided by tercile of population. While less precise, these estimates are relatively similar to our full sample estimates. Second, we show in Appendix Section A.3 that pass-through in the average municipality is similar to the pass-through of the average grant dollar, which suggests that our results are not being meaningfully driven by a few large municipalities. Third, starting in 1967, the Census began collecting spending data from a larger sample of municipalities once every five years, and we incorporate this data into our analysis in Appendix Section B.4. We use this data to show that, for municipalities in this larger sample, sewerage capital spending increased significantly more after grant receipt in non-compliant municipalities than in compliant municipalities. This result suggests that the heterogeneity in pass-through by compliance in Section 3.4 holds outside of our sample.

Since pass-through is correlated with mandated costs, our pass-through estimates might not be generalizable to all CWA grants if municipalities outside of our sample faced different mandated costs than municipalities in our sample. We compare municipalities in our sample to municipalities outside of our sample in Table 7. Municipalities outside of our sample received smaller grants than those in our sample, but they also faced lower mandated costs, so the ratio of grant size to mandated costs in municipalities within and outside of our sample is relatively similar. It is not unreasonable to expect non-compliant municipalities outside of our sample to increase sewerage capital spending up until the point where they are in compliance with the CWA's capital mandate, then reduce their own contribution to sewerage capital spending. If this is the case, then the similar relationship between grant size and mandated costs in non-compliant municipalities within and outside of our sample suggests that non-compliant municipalities outside of our sample should have a similar pass-through rate to non-compliant municipalities within our sample. Similarly, since compliant municipalities outside of our sample did not face any mandated costs, we would expect them to have pass-through close to zero.[50]

For the full sample of CWA grants, receiving governments include cities, towns, and sewage districts (Keiser and Shapiro, 2019), but our sample of municipal spending data only includes cities. Focusing on cities should not affect the generalizability of our results to CWA grants outside of our sample. Towns could use the same crowd-out mechanism as cities, and in cases where grants are distributed to special water utility districts that handle wastewater

[50]In total, twelve percent of CWA grant dollars were distributed to facilities that were already in compliance with both state and federal treatment technology standards. Assuming that our estimates of the total pass-through rate for compliant and non-compliant municipalities are accurate, we can take a weighted average of pass-through estimates for compliant and non-compliant municipalities to approximate pass-through for all CWA grants. We present the results of this exercise in Table A7. This suggests a pass-through rate of 0.353, and we can reject full grant pass-through.

treatment for multiple cities, no single city has authority to divert grant funds received by the utility district to other purposes, so the only diversion possible is for the utility district to pass on grant funds to customers via lower fees, which is consistent with the crowd-out mechanism that we explore in the next section.

4 How did Municipalities Spend Crowded-Out Funds?

4.1 Redistribution Methods

Evidence of low pass-through in compliant municipalities motivates an examination of how these municipalities adjusted spending in response to grant receipt. Both Callaway and Sant'Anna (2020a) and our stacked difference-in-differences estimator rely on comparisons of newly treated units to not-yet-treated units, but since we observe a relatively small number of compliant municipalities, we leverage a different source of variation to estimate the effect of CWA grants on local spending in compliant municipalities. Since pass-through is generally lower in compliant municipalities than in non-compliant municipalities, we can estimate the effect of grant receipt on spending in compliant municipalities by comparing outcomes between compliant municipalities and non-compliant municipalities before and after grant receipt with equation 8.

$$R_{it} = \alpha_0 + \theta grant_{it} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it} \tag{8}$$

Our dependent variable of interest, R_{it}, is water utility revenue, which we expect to decrease as compliant municipalities redistribute money to residents by lowering utility bills.[51] $grant_{it}$ is a dummy variable that equals one after a municipality receives a grant, $compliant_i$ is a dummy equaling one for compliant municipalities and α_i is a municipality fixed effect. g indexes timing groups, which are defined by the year in which a municipality receives its first CWA grant and α_{gt} is a timing-group-by-year fixed effect.

Including α_{gt} in equation 8 lets us estimate the effect of grants to compliant municipalities on spending without using any variation in treatment timing.[52] The result from equation 8 is equivalent to estimating the difference-in-difference in equation 9 on all observations in timing group g, which compares water revenues between compliant and non-compliant municipalities that receive their first grant in year g in periods before and after g, then averaging together the θ_g^{2X2} from all timing groups.

$$R_{it} = \alpha_0 + \theta_g^{2X2} 1\{t \geqslant g\} * compliant_i + \delta 1\{t \geqslant g\} + \omega compliant_i + \epsilon_{it} \tag{9}$$

We first examine the relationship between compliance and water revenue with an event study that compares water revenue in compliant municipalities to water revenue in non-

[51] This variable does not include revenue from CWA grants.

[52] As with our grant pass-through results, comparisons of early treated units to late treated units will be unbiased under parallel trends, but comparisons of late treated units to early treated units may be wrong signed. While there is nothing wrong with the first type of comparison, it is likely that variation in compliance will identify a different treatment effect than timing variation, so combining these sources of variation in one equation will reduce the precision of our estimates. Instead, we leverage these two sources of variation with different designs. We present results from a complementary design that identifies off of variation in treatment timing in Appendix Section B.1.

compliant municipalities before and after grant receipt with equation 10,

$$
\begin{aligned}
R_{it} = \alpha_0 &+ \sum_{y=-16}^{-2} \pi_y 1\{t - t_i^* = y\} * compliant_i \\
&+ \sum_{y=0}^{11} \gamma_y 1\{t - t_i^* = y\} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it}
\end{aligned}
\tag{10}
$$

then summarize this event study with equation 8.[53]

Since we only rely on comparisons between compliant and non-compliant municipalities, the identifying assumption of this design is that, if not for the differences in compliance, average water revenues would have trended similarly in compliant and non-compliant municipalities after grant receipt. Table 8 supports comparing spending outcomes between compliant and non-compliant municipalities. Column 1 estimates equation 8 with grant amount as the dependent variable. The result is small and insignificant, which shows that compliant and non-compliant municipalities receive similarly sized grants. Column 2 re-estimates equation 8 with sewerage capital spending as the dependent variable. Even though grants to compliant and non-compliant municipalities are similar in size, per capita sewerage capital spending is \$4.79 lower in compliant municipalities than in non-compliant municipalities after grant receipt. In column 3, we check if changes in revenue are driven by falling costs in compliant municipalities by estimating equation 8 with water operations costs as the dependent variable. The coefficient is small and insignificant, indicating that grants are not associated with changes in the cost of operating water utilities between compliant and non-compliant

[53] In our crowd-out event studies, we report coefficients for 16 years before and 11 years after grant receipt, which allows us to report only balanced coefficients. These specifications also includes bins for 17 or more years before grant receipt and 12 or more years after grant receipt, but our results are not sensitive to this choice of binning. We examine how effects evolve in later years in Appendix Section B.3.

municipalities.

4.2 Redistribution Results

Figure 7 plots the π_y and γ_y from estimating equation 10 with per capita water revenue as the dependent variable. The null estimates in the pre-treatment period provide evidence of parallel trends in water revenues in compliant and non-compliant municipalities prior to grant receipt. The estimates begin to decrease after grant receipt and continue to fall for 11 years after treatment. The gradual decrease in water revenue is consistent with municipalities receiving multiple grants that they could spend over several years.

Column 1 of Table 9 summarizes this effect by estimating equation 8 with water revenue as the dependent variable. This estimate shows that per capita water revenue decreases by \$13.55 per person in compliant municipalities after grant receipt relative to non-compliant municipalities that receive grants in the same year. Compared to our sample's median per capita water revenue, this estimate represents a 10 percent decline in water revenues raised by compliant municipalities in response to grant receipt.[54]

Columns 2-4 of Table 9 present alternative specifications. The shape of the event study in Figure 7 suggests that this figure might be better summarized with a trend-break specification, so column 2 estimates a version of equation 9 where we interact the treatment dummy with year relative to treatment. This estimate suggests that water revenues decrease by an

[54]Figure B1 checks if this effect is consistent across timing groups by presenting the θ_g^{2X2} from estimating equation 9 on each timing group separately. All of the estimates in this figure are negative and relatively close to the result in column 1 of Table 9, so no particular within-timing group comparison is driving this result. This figure does not include coefficients for the 1980, 1985 or 1988 treatment cohorts because either no compliant or no non-compliant units were treated in those years. We omit the coefficient for the 1978 cohort because only one compliant and one non-compliant municipality became treated in that year.

additional \$2.12 each year after treatment. The map in Figure 2 shows that the estimates in columns 1 and 2 of Table 9 rely on comparisons across large geographic areas, so in column 3, we re-estimate the specification in column 2 with region-by-year fixed effects.[55] This estimate, which is identified off of within-region variation in compliance, is not meaningfully different than the estimate in column 2, and our result is still strongly significant despite losing a substantial amount of variation. Finally, column 4 shows that our results are robust to including year fixed effects in place of cohort-by-year fixed effects.[56]

These results show that water revenues decreased in compliant municipalities relative to non-compliant municipalities after grant receipt. This suggests that compliant municipalities redistributed grant money to residents by lowering water utility prices, consistent with public utility providers offsetting water and sewerage utility costs with CWA grant money in order to lower prices to consumers.

5 Discussion & Conclusion

There was substantial heterogeneity in the effect of CWA grants on sewerage capital spending. Municipalities spent grant money on capital upgrades up to the point where they were in compliance with the CWA's new treatment technology requirements, but after municipalities met these requirements, or if none were in place, grants crowded out money that municipalities were already spending on sewerage capital. Per capita water revenues

[55] We divide the United States into four regions; the Northeast, consisting of CT, MA, NH, NJ, NY, PA and RI, the Midwest, consisting of IA, IL, IN, KS, MI, MN, MO, NE, OH and WI, the South, consisting of AR, AL, DE, FL, GA, LA, MD, MS, NC, OK, SC, TN, TX, VA and WV and the West consisting of AZ, CA, CO, MT, NM, OR, UT and WA. States not listed do not appear in our data.

[56] Figures B2 and B3 present the event studies associated with columns 3 and 4 of Table 9.

decreased by ten percent from the median after grant receipt in municipalities with low pass-through relative to municipalities with higher pass-through, suggesting that municipalities redistributed crowded out funds to residents through reductions in water bills. In total, receiving governments in our sample spent 44.8% of all CWA grant dollars on sewerage capital.

Dividing previously estimated benefit-to-cost ratios of CWA grants by our pass-through estimates suggests that the CWA grant dollars that municipalities spent on sewerage capital have a benefit-to-cost ratio of 1.01. This result comes from an analysis of $28 billion (2020$) of the $167 billion in grants funded by the CWA. While our sample does not include grants distributed to relatively small municipalities, pass-through is not strongly correlated with population in our sample, which suggests that CWA grants distributed to smaller municipalities may have a similar pass-through rate to municipalities in our sample.

Heterogeneity in pass-through informs which types of variation researchers can use to recover the effect of the average grant dollar on spending. Many evaluations of intergovernmental grant programs estimate pass-through using difference-in-differences designs with continuous treatment, but when the effect of a grant of a given size is not constant across receiving governments, estimates identified off of continuous variation in grant size that do not account for heterogeneity in pass-through across units can either over- or under-estimate the effect of the average grant dollar on targeted spending. In the case of the Clean Water Act, estimates of grant pass-through identified off of variation in grant size over-estimate the total pass-through rate.

The relationship between grant amount and targeted spending is likely heterogeneous for many grant programs. As an example, the American Recovery and Reinvestment Act of 2009 distributed almost $50 billion to state departments of education to offset budget shortfalls caused by the Great Recession through the State Fiscal Stabilization Fund. The costs that state governments faced depended on the severity of the local economic downturn, but the federal government determined grant amount based on state population (Superfine, 2011). Budget shortfalls varied widely across states, so if pass-through depended on the costs that receiving governments faced to enact policy, it is unlikely that all states would have responded to transfers of a given size in the same way. More generally, grant size is often positively correlated with receiving governments' preferences for public goods (Knight, 2002), so the effect of a dollar of grant funding on targeted spending is likely increasing in grant size in many settings.

We cannot know for sure if heterogeneity in the relationship between grant amount and targeted spending is the cause of the inconsistent estimates of grant pass-through found in other settings, but future research that accounts for heterogeneity in pass-through across receiving governments (as well as heterogeneity across time relative to treatment in cases where the timing of grant receipt varies) may provide further evidence that this is the case. That being said, researchers are usually limited in the types of variation they can leverage to estimate pass-through; donor governments often distribute grants in such a way that there is no never-treated group and no variation in treatment timing, making it difficult to test for homogeneity in pass-through across receiving governments.

We conclude by noting several implications of this study with respect to the design of intergovernmental grant programs. First, crowd-out may be high in our setting because the CWA allotted grants toward a function of local government that had a direct revenue stream, enabling grant dollars to "drip" from where they hit down to local residents through the low-friction adjustment of water rates. This suggests that crowd-out may be more severe in settings where local governments have a low-friction channel through which to redistribute grants. Crowd-out may also be high in this setting because the costs of environmental spending are more salient to constituents than the benefits; since downstream communities are the primary beneficiaries of environmental investment (e.g. communities downstream from a wastewater treatment plant or communities downwind from air polluters), a city's environmental spending is not salient to constituents, whereas fees (in this case, water bills) are. In many other settings, taxpayers observe their state or local government receiving federal grants for a specific purpose and would notice if targeted spending did not increase, so in most cases both spending and fees are salient to constituents.

Second, pairing grants with new regulation creates a binding constraint, which (weakly) increases the likelihood of full pass-through and the desired policy outcome. Many grant programs use "maintenance of effort" requirements to achieve a similar end, but these requirements are notoriously difficult to enforce. In essence, new regulation can act like a binding maintenance of effort requirement that is relatively easy to enforce, since a donor government can check if a receiving government satisfies a mandate rather than auditing that receiving government's spending. For this method of enacting policy to be effective, donor governments must be able to identify which receiving governments need fiscal assistance and accurately estimate the costs of grant-funded programs for each receiving government.

Similarly, our results suggest that supplementing new regulation with grants can allow local governments to comply with new requirements without reducing spending in other categories (Baicker, 2001; Baicker and Gordon, 2006) or raising additional revenue (Jerch, 2018).

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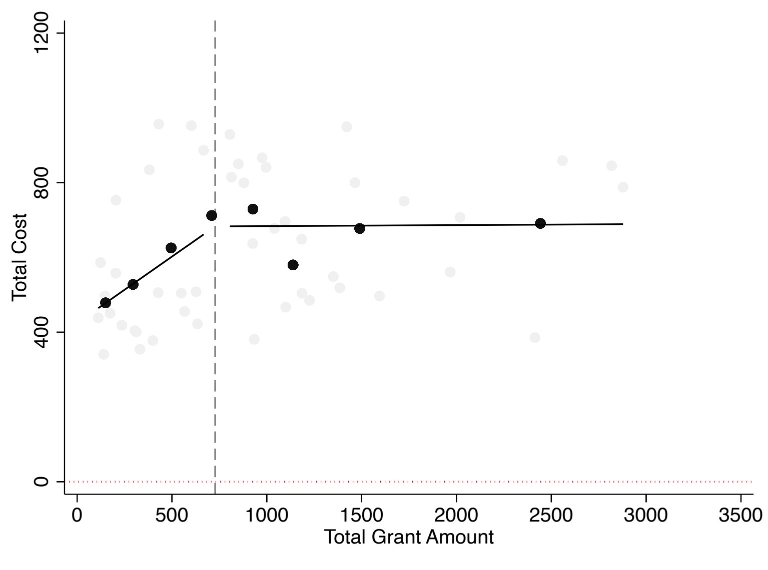


Figure 1: Costs Are Increasing in Grant Size Up to $730 and Constant Above $730

Notes: This figure plots estimates of non-compliant municipalities' total costs of upgrading to secondary treatment technology per capita against the total per capita grant dollars distributed to each municipality. The light gray dots are the true data and the black dots represent bins of total grant amount. Our cost estimates under-estimate costs for small grants, so we drop municipalities in the bottom decile of total grant aid. We divide the data at $730 per capita of total grant dollars, which is the point where costs are no longer increasing in grant size that we estimate in Table 3, and fit lines to the true data in each group.

Figure 2: Map of Municipalities

Notes: This figure plots the locations of the 112 municipalities in our sample. Non-compliant municipalities are shown as Xs and compliant municipalities are shown as points.

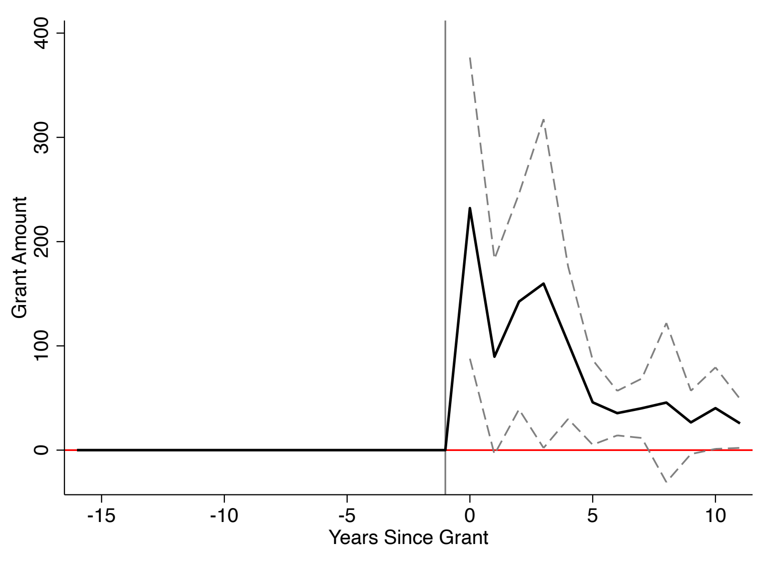


Figure 3: First Stage Relationship Between Grant Receipt and Grant Amount

Notes: This figure shows average per capita grant amount for each year relative to treatment.

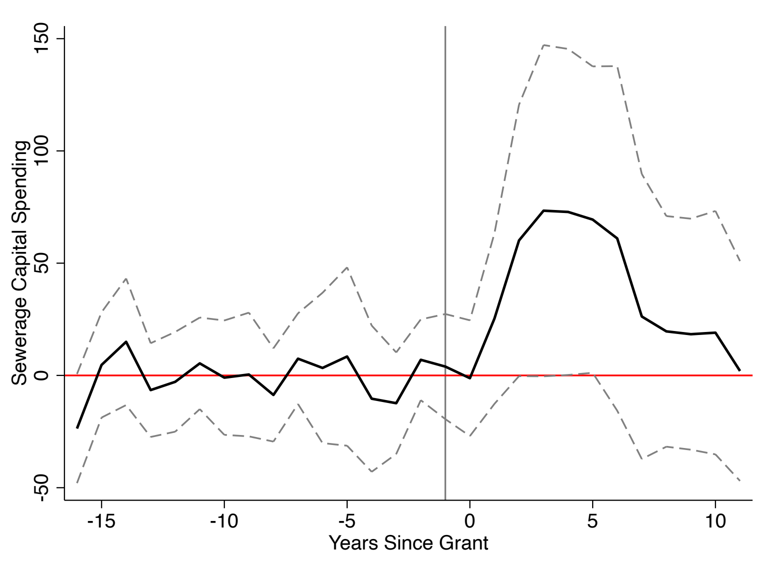


Figure 4: Sewerage Capital Spending Increased After Grant Receipt

Notes: This figure shows the reduced form relationship between grant receipt and per capita sewerage capital spending by presenting the $\widehat{\theta}_D(e)$ for each year e relative to treatment, where $\widehat{\theta}_D(e) = \sum_{g=1972}^{1985} \sum_{t=1957}^{1987} 1\{t - g = e\} \widehat{ATT(g,t)} P(G = g | t - g = e)$. g indexes timing groups and t indexes years. $\widehat{ATT(g,t)}$ is the difference between changes in spending in units in group g in periods t and $g-1$ and changes in spending in units not yet treated at time t in periods t and $g-1$.

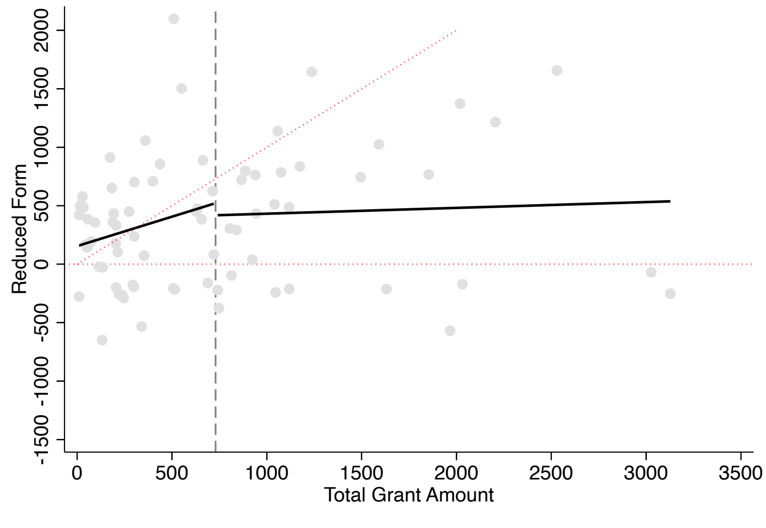


Figure 5: 2X2 DD Coefficients vs Grant Amount for Non-Compliant Municipalities

Notes: This figure presents the β_s^{2X2} from estimating $C_{it} = \alpha_0 + \beta_s^{2X2} post_t * treat_i + \delta post_t + \omega treat_i + \epsilon_{it}$ on observations in each stack s (for stacks defined by non-compliant municipalities). Per capita sewerage capital spending is the dependent variable. We plot each stack's β_s^{2X2} against the total per capita grant dollars distributed to the treated municipality in that stack. We separate the stacks at $730 per capita of total grant dollars and fit lines to the β_s^{2X2} in each group. We multiply the β_s^{2X2} by seven to reflect that each β_s^{2X2} represents an average across seven post-treatment years. The dotted red lines represent full crowd-out and full pass-through.

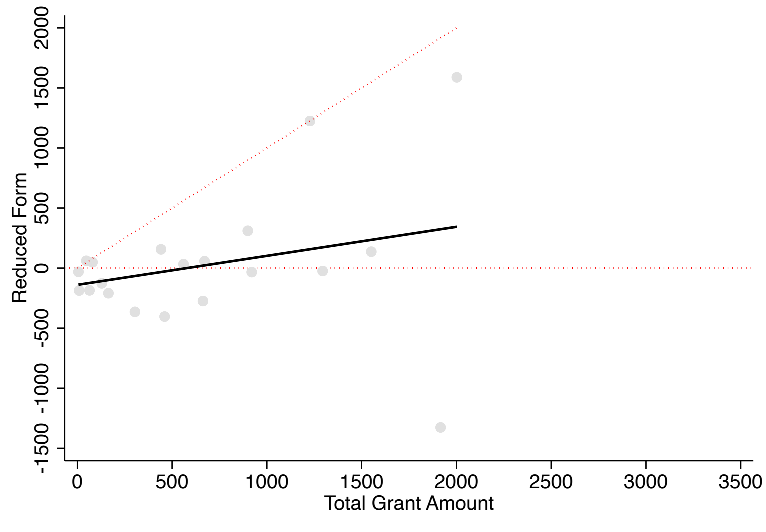


Figure 6: 2X2 DD Coefficients vs Grant Amount for Compliant Municipalities

Notes: This figure re-estimates the results in Figure 5 for stacks defined by compliant municipalities.

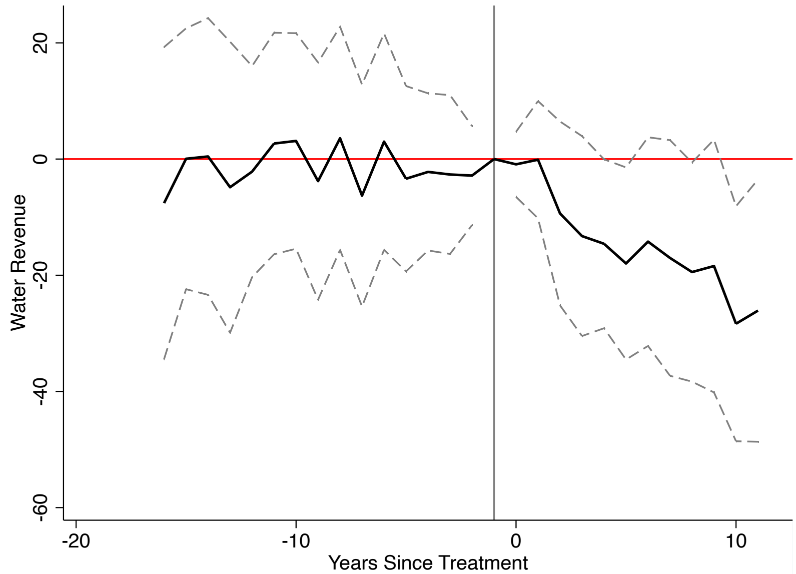


Figure 7: Water Revenue Decreased After Grant Receipt in Compliant Municipalities

Notes: This figure plots the π_y and γ_y from estimating $R_{it} = \alpha_0 + \sum_{y=-16}^{-2} \pi_y 1\{t - t_i^* = y\} * compliant_i + \sum_{y=0}^{11} \gamma_y 1\{t - t_i^* = y\} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it}$ with per capita water revenue as the dependent variable. $compliant_i$ equals one for observations of compliant municipalities. We include municipality and timing-group-by-year fixed effects, α_i and α_{gt}, where g indexes the year in which a municipality received its first CWA grant.

Table 1: 1970 Summary Statistics by Facility Compliance

	(1) Compliant	(2) Non-Compliant	(3) Difference
Population	115,501	396,525	-281,023
	(73,126)	(954,559)	(204,352)
Total Revenue	1475.74	1715.26	-239.51
	(805.53)	(904.53)	(210.84)
Total IGR	308.08	375.40	-67.32
	(366.90)	(410.74)	(95.79)
Federal IGR	66.63	63.25	3.38
	(91.20)	(67.42)	(17.26)
Nonwater Utility Rev	123.50	105.83	17.68
	(261.15)	(263.06)	(62.48)
Property Tax Revenue	479.29	573.08	-93.79
	(424.10)	(461.05)	(108.03)
Water Utility Revenue	134.65	133.56	1.08
	(53.04)	(50.02)	(12.04)
Total Expenditure	1616.44	1796.60	-180.16
	(901.08)	(971.44)	(227.94)
Total IG Exp	11.24	26.43	-15.18
	(26.48)	(58.21)	(12.75)
Capital Outlays	462.61	372.40	90.22
	(334.21)	(219.50)	(58.41)
Highway Exp	123.37	127.96	-4.59
	(48.83)	(54.84)	(12.78)
Sewerage Capital Outlay	52.10	35.98	16.12
	(56.01)	(39.63)	(10.28)
Sewerage Non-capital Exp	32.09	29.38	2.71
	(18.23)	(18.13)	(4.32)
Nonwater Utiltiy Exp	130.94	109.70	21.24
	(287.14)	(273.17)	(65.62)
Water IG Exp	11.57	13.92	-2.35
	(11.12)	(15.10)	(3.43)
Water Non-capital Exp	77.78	74.94	2.84
	(27.64)	(34.40)	(7.90)
Water Capital Outlay	78.43	48.80	29.63
	(110.83)	(67.22)	(18.42)
Observations	22	90	112

Notes: This table presents summary statistics for municipalities with compliant and non-compliant facilities in 1970, two years prior to the CWA. All covariates aside from the facility compliance dummy and population represent per capita 2020 values.

Table 2: CWA Grants had Low Pass-Through

Panel A: Dynamic Aggregation	(1) Grant Amount	(2) Sewerage Capital	(3)
Treat * Post	68.33***	32.81***	
	(8.39)	(10.775)	
Pass-through			0.480**
			(0.207)
p-value: Pass-through = 0			0.020
p-value: Pass-through = 1			0.012
Observations	3630	3630	3630
Panel B: Dynamic Aggregation ($e \leqslant 6$)			
Treat * Post	115.48***	51.56***	
	(17.675)	(14.868)	
Pass-through			0.446***
			(0.142)
p-value: Pass-through = 0			0.002
p-value: Pass-through = 1			0.000
Observations	3630	3630	3630
Panel C: Balanced Dynamic Aggregation			
Treat * Post	115.82***	51.97***	
	(16.953)	(12.809)	
Pass-through			0.448***
			(0.137)
p-value: Pass-through = 0			0.001
p-value: Pass-through = 1			0.000
Observations	3564	3564	3564
Panel D: Simple Aggregation			
Treat * Post	77.31***	35.65***	
	(9.463)	(10.821)	
Pass-through			0.461***
			(0.156)
p-value: Pass-through = 0			0.003
p-value: Pass-through = 1			0.001
Observations	3630	3630	3630

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the first stage relationship between grant receipt and grant amount, the reduced form effect of grant receipt on sewerage capital spending and the pass-through rate for all CWA grants. Panel A presents averages of the $\widehat{\theta}_D(e)$ for each time relative to treatment e for $e \geqslant 0$ calculated with $\widehat{\theta}_D(e) = \sum_{g=1972}^{1985} \sum_{t=1957}^{1987} 1\{t-g=e\}\widehat{ATT(g,t)}P(G=g|t-g=e)$. Panel B re-estimates the results in Panel A for e such that $0 \leqslant e \leqslant 6$. Panel C re-estimates the results in Panel B for municipalities that we observe for at least seven post-treatment periods. Panel D presents averages of group-time treatment effects based on group size calculated with the following equation $\frac{1}{\kappa}\sum_{g=1972}^{1985} \sum_{t=1972}^{1987} 1\{g \leqslant t\}\widehat{ATT(g,t)}P(G=g)$ where $\kappa = \sum_{g=1972}^{1985} \sum_{t=1972}^{1987} 1\{g \leqslant t\}P(G=g)$. Per capita grant amount is the dependent variable in column 1, and per capita sewerage capital spending is the dependent variable in column 2. Column 3 shows the implied pass-through rate from dividing the reduced form in column 2 by the first stage in column 1.

Table 3: Non-Linear Least Squares Estimates of Break in Cost

	(1) Total Cost	(2) Total Cost
Split	727.8*	805.5
	(297.6)	(623.0)
Observations	41	58

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents *split* from estimating $cost_i = (a_1 + b_1 * grant_i) * 1\{grant_i < split\} + (a_1 + b_1 * split + b_2 * (grant_i - split)) * 1\{grant_i >= split\}$ on a sample of non-compliant municipalities with non-linear least squares. Column 1 presents results from estimating this equation with total cost per capita calculated from estimates in USEPA (1973) as the dependent variable. Column 2 re-estimates the result in column 1 without dropping municipalities in the bottom decile of total grant amount per capita.

Table 4: Pass-Through is Heterogeneous in Grant Size and Compliance

	(1)	(2)	(3)
Panel A: Non-Compliant and < $730	Grant Amount	Sewerage Capital	
Treat * Post	40.42***	42.44	
	(4.421)	(43.826)	
Pass-through			1.049
			(1.138)
p-value: Pass-through = 0			0.357
p-value: Pass-through = 1			0.965
Observations	2162	2162	2162
Panel B: Non-Compliant and >= $730			
Treat * Post	268.25***	68.85	
	(35.465)	(41.358)	
Pass-through			0.267
			(0.156)
p-value: Pass-through = 0			0.087
p-value: Pass-through = 1			0.000
Observations	1518	1518	1518
Panel C: Compliant			
Treat * Post	95.80***	3.22	
	(22.851)	(27.133)	
Pass-through			0.0337
			(0.345)
p-value: Pass-through = 0			0.922
p-value: Pass-through = 1			0.009
Observations	920	920	920

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of $Y_{it} = \alpha_0 + \beta D_{it} + \alpha_{is} + \alpha_{ts} + \epsilon_{its}$. Municipalities are indexed by i, stacks by s, and years by t. D_{it} is a dummy variable that equals one after grant receipt, and α_{is} and α_{ts} are stack-by-municipality and stack-by-year fixed effects. Grant amount per capita is the dependent variable in column 1, and sewerage capital spending per capita is the dependent variable in columns 2 and 3. Column 3 uses D_{it} as a instrument for grant amount, which returns a coefficient equivalent to dividing the reduced form in column 2 by the first stage in column 1. Panel A presents estimates for stacks defined by non-compliant municipalities who received grants totalling less than $730 per capita, Panel B for all other non-compliant stacks, and Panel C for all stacks defined by compliant municipalities. We present p values from testing for a pass-through rate of one at the bottom of each panel.

Table 5: Decomposition of Keiser and Shapiro (2019) Pass-Through Estimates

	(1) Weight	(2) Average Estimate
Newly Treated vs Not-Yet-Treated	0.017	0.428
Newly Treated vs Already-Treated	0.553	1.002
Large Grant vs Small Grant	0.430	0.835

Notes: This table presents the average difference-in-difference estimates and their weights from decomposing β_{dr}^{TWFE} from $C_{it} = \alpha_0 + \beta_{dr}^{TWFE} + \alpha_i + \alpha_t + \epsilon_{it}$ using Goodman-Bacon (2021b). C_{it} is cumulative sewerage capital spending, g_{it} is cumulative grant amount, and α_i and α_t are municipality and year fixed effects. Source: Keiser and Shapiro (2019)

Table 6: Pass-Through Results are Robust to Population Weighting

	(1) Grant Amount	(2) Sewerage Capital	(3)
Treat * Post	84.84***	32.81***	
	(20.468)	(9.504)	
Pass-through			0.387**
			(0.162)
p-value: Pass-through = 0			0.017
p-value: Pass-through = 1			.000
Observations	3630	3630	3630

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results from Panel B of Table 2 weighting by population.

Table 7: Full Sample of Grants

Panel A: All Grants	(1) All	(2) Non-compliant	(3) Compliant
Total Grant Amount	15,983,497	27,998,956	18,181,962
Total Cost		13,916,509	
Observations	10,200	3778	1065
Panel B: Grants with Spending Data			
Total Grant Amount	273,331,584	315,972,992	102,766,048
Total Cost		167,992,672	
Observations	112	90	22

Notes: Panel A presents average total costs and average total grant amount for all municipalities that received CWA grants, and Panel B presents average total costs and average total grant amount the municipalities in our sample.

Table 8: Non-Compliant Cities are Valid Counterfactuals for Compliant Cities

	(1)	(2)	(3)
	Grant Amount	Sewerage Capital	Water Operations Cost
Grant X Compliant	-2.924	-27.90*	-2.991
	(17.30)	(14.28)	(5.286)
Observations	4752	4752	4752

Standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of θ from $Y_{it} = \alpha_0 + \theta grant_{it} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it}$. The dependent variable is grant amount per capita in column 1, sewerage capital spending per capita in column 2, and water operations costs per capita in column 3.

Table 9: Compliant Municipalities Spent Crowded Out Funds on Lowering Water Bills

	(1)	(2)	(3)	(4)
	Water Revenue	Water Revenue	Water Revenue	Water Revenue
Grant X Compliant	-13.55**			
	(6.743)			
Grant X Compliant X e		-2.122***	-2.306***	-2.570***
		(0.804)	(0.872)	(0.768)
Timing Group X Year FE	X	X	X	
Region X Year FE			X	
Observations	4752	4752	4752	4840

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table estimates the reduced form effect of grants to compliant facilities on per capita water revenues. Column 1 presents estimates of θ from $R_{it} = \alpha_0 + \theta grant_{it} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it}$. Column 2 presents estimates of θ from $R_{it} = \alpha_0 + \theta grant_{it} * compliant_i * e_{it} + \alpha_i + \alpha_{gt} + \epsilon_{it}$ where e_{it} indicates year relative to treatment. Column 3 re-estimates the result in column 2 with region-by-year fixed effects. Column 4 re-estimates the result in column 2 with year fixed effects instead of timing-group-by-year fixed effects.

A Additional Pass-Through Results

A.1 Selection on Treated Potential Outcomes

In Section 3.4.2, we document heterogeneity in grant pass-through by grant size and compliance with the CWA's treatment technology mandate, but these are not necessarily the only sources of heterogeneity in pass-through. Since states distribute CWA grants to municipalities according to priority lists, we might expect treatment timing to be positively correlated with treated potential outcomes. This is not a threat to identification (we do not place any restrictions on the evolution of outcomes after treatment and our identifying assumption allows for selection into treatment on the basis of unobserved time-invariant characteristics), or the interpretation of the heterogeneity we document in Section 3.4.2 (as shown in Figure A6, the distribution of treatment timing is similar across compliant and non-compliant municipalities), but it does have implications for the best way to summarize treatment effects. Callaway and Sant'Anna (2020b) suggests that when treatment effects vary across timing groups, we should first aggregate the $ATT(g,t)$ for each timing group g, then combine these timing group-specific effects into a summary measure by taking a weighted average of the effect in each timing group. Specifically, we first calculate

$$\tilde{\theta}(g) = \frac{1}{1985 - g + 1} \sum_{t=1972}^{1987} 1\{g \leqslant t\} ATT(g,t)$$

for each group, then summarize the effect with

$$\theta_S = \sum_{g=1972}^{1985} \tilde{\theta}(g) P(G = g)$$

θ_S is similar to the simple aggregation based on group-size given by equation 2. The difference is that the weights in θ_S do not depend on how long a unit has been treated for, while the weights in equation 2 depend on how many periods we observe a timing group for after treatment. This weights up groups that are treated early, so in the presence of selection on gains, equation 2 weights up units that experience the largest effects. In contrast, the weights in θ_S only depend on group size.

Table A8 presents averages of the $\tilde{\theta}(g)$ for all municipalities in the first three treatment groups, the second three treatment groups and the last five treatment groups in columns 1-3. Column 4 presents θ_S for the entire sample. θ_S is similar to the dynamic and simple aggregations in Panels A and D of Table 2, which suggests that treatment timing is not correlated with treated potential outcomes.

Column 5 calculates θ_S with grant amount as the dependant variable. Dividing the result in column 4 by this first stage estimate implies a pass-through rate of 0.46, which is consistent with the results in Table 2.

A.2 Alternative Definitions of Full Pass-Through

Cost-sharing yields two different possible interpretations of pass-through. We define full pass-through as \$1 in grant funding leading to a \$1 increase in sewerage capital spending. Alternatively, since municipalities are required to pay 25% of project costs, one could define full pass-through as \$1 in grant funding leading to a \$1.33 increase in sewerage capital spending (the actual \$1 in grant aid plus the \$0.33 in local obligations). Our estimates in the main text are based on the first definition, appealing to the notion of federal funding itself being spent as intended. We present pass-through estimates using the second definition in Tables A9 and A10. These tables re-estimate the results in Tables 2 and 4 after multiplying grant amount by 1.33 in years before 1981, and by 1.82 in 1981 and beyond. Using this definition of full pass-through, we still reject full pass-through for the full sample, non-compliant municipalities that received large grants, and compliant municipalities. We cannot reject full pass-trough or complete crowd-out for non-compliant municipalities that received small grants, but the point estimate for these municipalities suggests that, while CWA grants led to an increase in targeted spending equal to grant amount, they did not cause an increase in local contributions to sewerage capital spending.

A.3 Average Pass-Through Rate

In the main text, we attempt to recover the total pass-through rate of CWA grants, but that is not the only parameter of potential interest. Figure A7 presents the distribution of per capita grant amount for grants in our sample and shows that grant amount is distributed approximately lognormally; given this distribution, it is possible that a few larger grants are meaningfully driving our estimates of the total pass-through rate. Since we want to place the largest weight on the most expensive grants when analyzing the effectiveness of a grant program, this is a desirable feature. That being said, we may instead be interested in the "average pass-through rate", the percent of grant dollars spent on the targeted category in the average receiving municipality.

We can estimate the average pass-through rate by estimating pass-through in each unit separately, then taking an average across municipalities. To do this, we first estimate equations 4 and A1 on observations in stack each S.

$$g_{it} = \alpha_0 + \beta_{s_{fs}} post_t * treat_i + \delta post_t + \omega treat_i + \epsilon_{it} \tag{A1}$$

We then divide β_s^{2X2} by $\beta_{s_{fs}}$ to obtain an estimate of pass-through for municipality s, then average the resulting within-stack pass-through rates across municipalities.

We present estimates of the average pass-through rate in Table A11. While we cannot reject full pass-through or full crowd-out, the point estimate is similar to our estimates of the total pass-through rate.

A.4 Excluding Municipalities that did not Receive Grants

Since grant timing is determined by a municipality's placement on a state priority list and priority lists are explicitly based on factors that are unlikely to be correlated with trends in sewerage capital spending, we identify our main results off of variation in the *timing* of grant receipt. It is less clear if grant *receipt* is uncorrelated with trends in local spending, but if we are willing to assume that whether or not a municipality received a grant is uncorrelated with trends in local spending, we can use municipalities in our finance data that never receive a CWA grant as an additional control group.

Using never-treated units as a control group only requires minor changes to our estimators. The Callaway and Sant'Anna (2020a) estimator works in exactly the same way, except we now estimate group-time average treatment effects with

$$\widehat{ATT(g,t)} = E_n[\widehat{C}_t - \widehat{C}_{g-1} | G_g = 1] - E_n[\widehat{C}_t - \widehat{C}_{g-1} | T_i = 0]$$

where T_i is a dummy variable that equals zero for never treated units. We can then aggregate these group-time treatment effects together the same way we did in Section 3. We present these results in Figures A8 and A9 and Table A12. These estimates are similar to our main results.

For our stacked estimator, we construct stacks so that, instead of using units treated at least seven years after the treated unit as controls, we use never-treated units as controls. We present these results in Table A13.

A.5 Observable Characteristics do not Predict Grant Timing

The identifying variation of our grant pass-through research design comes from *when* each municipality receives its first CWA grant. To identify causal effects with this variation in a difference-in-difference framework, we must assume that the timing of grant receipt is uncorrelated with trends in sewerage capital spending. We perform more explicit tests of our parallel trends assumption in the main text, but in this section, we provide suggestive evidence that early and late treated units would have followed parallel trends in absence of treatment by showing that early and late treated municipalities are observably similar before treatment. We test if observable characteristics of municipalities predict the timing of grant receipt with equation A2.

$$GrantYear_i = \alpha_0 + \alpha_1 compliant_i + \alpha_2 X_{ip} + \epsilon_{ip} \tag{A2}$$

We regress the year municipality i receives its first CWA grant on observable municipality characteristics X from a baseline year p, conditional on the municipality not yet receiving a grant in year p. Table A14 presents results from estimating equation A2 for baseline years 1971, 1974 and 1977.[A1] X_{ip} includes city population and per-capita measures of both holistic (e.g. total revenue, total intergovernmental revenue, total capital outlays) and specific (e.g. sewerage capital outlays, water utility revenue) categories of government revenue and expenditure.

None of these observable characteristics consistently predict grant timing. All of the point estimates are small and precisely estimated and the only covariates that have a significant effect on year of grant receipt are water capital spending and log population using 1971 as a baseline year. The coefficients on these variables do not represent economically meaningful changes; a ten percent difference in 1971 population is associated with a municipality receiving its first grant 0.08 years earlier and a one standard deviation increase in water capital outlay is associated with a less than one year change in grant timing. Both of these characteristics are no longer significant and change signs when we use other baseline years. While the covariates are jointly significant when we use baseline years of 1971 and 1974, a standard deviation change in any spending variable in either year represents a less than one year change in grant timing. This shows that grant timing is not strongly correlated with any municipal spending characteristics, which motivates a design that leverages variation in treatment timing to estimate pass-through.

[A1] Roughly one third of grants to municipalities in our sample were distributed from 1972 to 1973, 1974 to 1976 and post-1976.

A.6 Alternative Pass-Through Specifications

In Section 3, we summarize pass-through for the full sample using Callaway and Sant'Anna (2020a) and explore heterogeneity in pass-through using a stacked difference-in-differences estimator. We discuss our choice of estimators in greater detail in Appendix C, and in this section, we present additional results to show that our results not sensitive to this choice.

A.6.1 Full Sample Stacked Estimates

Figure A10 presents the π_y and γ_y from estimating the following stacked event study on a pooled sample of all stacks.

$$C_{it} = \alpha_0 + \sum_{y=-16}^{-2} \pi_y 1\{t - t_i^* = y\} * treat_{is} + \sum_{y=0}^{6} \gamma_y 1\{t - t_i^* = y\} * treat_{is} + \alpha_{is} + \alpha_{ts} + \epsilon_{its} \tag{A3}$$

Time relative to treatment is defined by $1\{t - t_i^* = y\}$, which is a dummy variable that equals 1 for observations of municipality i y years before or after t_i^*, the year municipality i receives its first CWA grant and $treat_{is}$ is a dummy variable that equals one for the treated unit in each stack. Similar to the results in Figure 4, we see null effects before treatment. The estimates increase after the arrival of a municipality's first CWA grant and remain high for up to 6 years after grant receipt.

Table A15 summarizes the results in Figure A10. Column 1 estimates the first stage relationship between grant receipt and per-capita grant amount by estimating equation 5 on a pooled sample of all stacks. Column 2 estimates the associated reduced form effect of grant receipt on sewerage capital spending by estimating equation 6 on all stacks. Column 3 estimates equation 6 using D_{cy} as an instrument for grant amount, which returns an estimate equivalent to dividing the reduced form by the first stage. This implies a pass-through rate of 0.342. Using this result, we can reject full pass-through in the full sample (i.e. a test of the hypothesis that the IV estimate in column 3 equals one).

A.6.2 Sub-Sample Callaway and Sant'Anna Estimates

Table A16 re-estimates the results in Panel B of Table 2 on sub-samples of municipalities. These results are averages of the $\widehat{\theta}_D(e)$ from equation 1 for e s.t. $0 \leqslant e \leqslant 6$. Overall, these results are similar to those from our stacked difference-in-difference; we can reject full pass-through in non-compliant municipalities that received large grants, but cannot reject full pass-through for non-compliant municipalities receiving small grants. Since the effect of grant receipt on sewerage capital spending is marginally significant for non-compliant municipalities receiving small grants, we can also marginally reject complete crowd-out in these municipalities.

We cannot say anything about compliant municipalities using this estimator. Since there are relatively few compliant municipalities in our sample, the simultaneous critical value is arguably 'too large' to be reliable when we estimate the effect of grant receipt on sewerage capital spending in these municipalities, so we default to our stacked results.

A.7 Measuring the Benefits of CWA Grants

In this section, we discuss several potential issues with using previously estimated benefits of CWA grants to find the benefit-to-cost ratio of CWA grants that municipalities spent on sewerage capital.

First incorporating other currently unmeasured benefits could further increase the benefit-to-cost ratio CWA grants, and a full welfare analysis would include these unmeasured benefits.

Second, a more formal analysis would take the confidence intervals and potential bias of estimates of the benefits of CWA grants into account. In particular, both the hedonic estimates in Keiser and Shapiro (2019) and the infant health estimates in Flynn and Marcus (2021) come from TWFE estimators with continuous treatment, but we show in Section 3.5 that this type of estimator can produce misleading results in the presence of heterogeneity in treatment effects across compliance or heterogeneity in treatment effects across time. The main estimates in Flynn and Marcus (2021) account for heterogeneity across compliance. Flynn and Marcus (2021) also obtains similar results when using estimators that account for dynamic treatment effects. While it does not explicitly account for heterogeneity across time, the event study graphs in Keiser and Shapiro (2019) do not show evidence of dynamic treatment effects, and instead show that CWA grant receipt led to a mean shift in housing prices. Keiser and Shapiro (2019) does not account for heterogeneity across compliance either, but since there is evidence that similar demographic groups sorted into communities downstream from compliant and non-compliant municipalities (see Table 2 in Flynn and Marcus (2021)), it is not clear that the effect of CWA grants on housing prices is heterogeneous across compliance. That being said, if this heterogeneity is present, it could bias estimates of the effect of CWA grants on housing prices. It is not clear a priori which direction this bias would move in.

Finally, the benefits of CWA grants may be driven in part by spending on categories besides sewerage capital. In Section 4, we provide evidence that compliant municipalities redistributed grant dollars to residents through reduced water bills. A hedonic model suggests that these savings should be capitalized into housing prices. Since Keiser and Shapiro (2019) estimates the effect of CWA grant receipt on housing prices by comparing all housing units within 25 miles of treated plants against all housing units within 25 miles of untreated plants, the estimated benefit-to-cost ratio of 0.26 in Keiser and Shapiro (2019) may be driven in part by the value that residents place on reduced water bills. Consequently, our estimated benefits likely include the value that residents place on lowered water rates in addition to the benefits of improved infant health and the value that residents place on increased water quality.

Reductions in water rates are driven by grant dollars that municipalities redistributed to residents. For this reason, we do not want to include these benefits when we are trying to find the benefit-to-cost ratio of CWA grant dollars spent on sewerage capital, but we cannot disentangle the portion of the effect of CWA grants on housing prices driven by lower utility prices and the portion driven by improved water quality. We can instead find the benefit-to-cost ratio of CWA grant dollars spent on sewerage capital *and* CWA grant dollars that municipalities redistributed to residents through lowered water bills. Table B6 suggests that 48.5% of grant dollars for compliant facilities went to reducing water bills (though this estimate is imprecise). Twelve percent of CWA grant dollars went to compliant municipalities, so this represents 5.82% of all CWA grant dollars. Since the benefit-to-cost ratio of 0.45 from Keiser and Shapiro (2019) and Flynn and Marcus (2021) comes from both the 44.6% of CWA grant dollars that municipalities spent on sewerage capital and the 5.82% of all CWA grant dollars went to reducing water bills, we can obtain the benefit-to-cost ratio of CWA grant dollars that were spent on sewerage capital or redistributed to residents through reduced water bills by dividing previously estimated benefit-to-cost ratios of CWA grants, 0.45, by the portion of CWA grant dollars spent on sewerage capital and reducing water bills combined, 0.504. This suggests a benefit-to-cost ratio of 0.89, however this is not exactly the number that we are interested in.

We want the benefit-to-cost ratio of CWA grant dollars spent on sewerage capital. We can use the results in Table B6 to estimate a range for this number. To generate a lower bound, we can assume that reductions in water rates were completely capitalized into housing prices (i.e. a one dollar reduction in water bills leads to a one dollar increase in housing prices). If this were the case, then, of the \$0.26 of the average dollar of CWA grant aid capitalized into housing prices, 0.058 would come from reduced water bills and the other 0.202 would come from improvements in water quality. Combining this number with the infant health benefits from Flynn and Marcus (2021) suggests that the CWA grant dollars spent on everything except reducing water bills would have a benefit-to-cost ratio of 0.392. We can divide this number by the pass-through rate to calculate the benefit-to-cost ratio of CWA grant dollars spent on sewerage capital. This suggests a benefit-to-cost ratio of about 0.879. If instead reductions in water bills were not capitalized into housing prices at all, all of the benefits of the CWA in Keiser and Shapiro (2019) and Flynn and Marcus (2021) would come from sewerage capital spending, and the benefit-to-cost ratio of CWA grant dollars spent on sewerage capital would be $\frac{0.45}{0.446}$, or about 1.01. Where in this range the actual benefit-to-cost ratio of CWA grant dollars spent on sewerage capital falls depends on the capitalization rate of reductions in water bills into housing prices, but the existing literature has not reached a consensus on the effect of changes in fees on housing prices (Yinger et al., 2016). Recent results suggest that there should be near full capitalization if the housing supply is inelastic (Lutz, 2015), but determining the elasticity of housing around municipal wastewater treatment facilities is outside the scope of this paper.

Similarly, the portion of CWA grant dollars that municipalities spent on categories besides sewerage capital spending and lowering water bills may have caused increases in housing prices or improvements in infant health, which would similarly decrease the benefit-to-cost ratio of CWA grants spent on sewerage capital.

A.8 Baseline Sewerage Spending by Compliance and Grant Size

If municipalities that received small grants had relatively low pre-treatment sewerage capital spending, it could be the case that a relatively small portion of the grants distributed to these municipalities are inframarginal. If less of these grants dollars are inframarginal, then the higher pas-through that we document in municipalities that receive small grants could be mechanical; municipalities that received small grants would not have had much spending to to crowd-out, while municipalities that received larger grants would have had more baseline spending to crowd out. Table A17 shows that this is not the case, and that pre-treatment spending is highest in non-compliant municipalities that receive small grants.

B Additional Water Revenue Results

B.1 Estimating Water Revenue Results Using Timing Variation

In Section 4, we leverage variation in pre-CWA compliance with the CWA's treatment technology mandate to examine how compliant municipalities adjust spending in response to grant receipt, but we obtain similar results when we estimate these effects using variation in treatment timing.

In Figure B4, we estimate separate event studies for compliant and non-compliant municipalities. These event studies compare water revenue after treatment to water revenue in not-yet-treated municipalities using Callaway and Sant'Anna (2020a). Sub-Figure B4a shows that grant receipt has no significant effect on water revenues in non-compliant municipalities. Sub-Figure B4b shows that water revenues are stable before grant receipt and decrease after grant receipt in compliant municipalities. Table B1 summarizes these figures.

Note that the estimates in column 1 of Table B1, which are identified off of comparisons between early and late treated compliant municipalities, are larger than the estimates in Table 9, which are identified off of comparisons between compliant and non-compliant municipalities. Similarly, the event studies in Figure B4 have a somewhat different shape than the event study in Figure 7. This does not reflect a problem with either design. Rather, it demonstrates that these two sources of variation identify different treatment effects.

This difference is caused in part by the way in which OLS combines the effects in different treatment cohorts into summary measures. OLS weights based on the size of timing groups and within-group variance of the treatment dummy, which artificially weights up units treated near the middle of the panel (Goodman-Bacon, 2021a). To avoid weighting based on within-group variance, we can take a weighted average of the effect in each timing group where the weights are based only on the size of each timing group (i.e. a weighted average of the effects in Figure B1). This average is -3.76, which is closer to the estimate in column 1 of Table B1.

B.2 Why was there No Effect in Non-Compliant Municipalities?

Table B1 shows that grant receipt had no average effect on water revenue in non-compliant municipalities. This result provides support for the identifying assumption of the design we use in Section 4, but it is somewhat counter-intuitive; since we do not estimate full pass-through in all non-compliant municipalities in our sample, we might expect grants to cause a decrease in water revenues in non-compliant municipalities.

Figure 1 suggests one explanation for this result. Note that while, on average, small grants are similar to costs and large grants are greater than costs, there are non-compliant municipalities that receive grants that are too small or too large along the entire distribution of grant size. If this leads some municipalities redistribute grant funds by decreasing their water bills while other municipalities increase water bills to cover costs greater than grant amount, these effects will cancel out.

Figure B5 provides suggestive evidence that this is the case by re-estimating the results in Sub-figure B4a on sub-samples of municipalities where grants are larger or smaller than estimated costs. Sub-figure B5a suggests that municipalities that receive grants that are smaller than costs increase their water bills in order to raise the revenue needed to come into compliance with the CWAs treatment technology mandate. Sub-figure B5b does not provide clear evidence of an increase or decrease in water revenues in non-compliant municipalities that receive grants larger than estimated costs.

Table B2 summarizes the results in Figure B5. The estimate for municipalities that receive grants smaller than costs is positive, while the estimate for municipalities that receive grants larger than costs is negative, as expected, though neither of these effects are statistically significant. This suggests that grants did not cause a decrease in water revenues in non-compliant municipalities because these countervailing effects cancel out.

The results in Table B1 also motivate a discussion of how our results relate to Jerch (2018). Jerch (2018) uses an instrumental variables approach to estimate the effect of the CWA's capital mandate on local spending and finds that the capital mandate imposed a large financial burden on non-compliant municipalities and that non-compliant municipalities then increased water bills in response to the capital mandate. Two important differences between this paper and Jerch (2018) can explain why we obtain different results: first, the research design in Jerch (2018) intentionally does not capture the effect of CWA grants, and second, the sample of municipalities in Jerch (2018) includes municipalities that did not receive any CWA grants. In contrast, we explicitly attempt to capture the effect of CWA grants, and we only look at the effect of the CWA's capital mandate on water revenues in municipalities that received CWA grants. We find that non-compliant municipalities that received CWA grants did not adjust their water rates, which suggests that the increase in water revenues in non-compliant municipalities documented in Jerch (2018) is driven by municipalities that did not receive CWA grants. This is consistent with other studies of unfunded federal mandates (Baicker, 2001; Baicker and Gordon, 2006).

B.3 How Long Do Effects Persist?

Figures 7 and B4 both show that the effect of grants to compliant municipalities on water revenue grow over time, which motivates an examination of how long these effects persist. Our spending data includes observations from 1956 to 1999 and the latest treated municipality receives its first CWA grant in 1988. This allows us to estimate event study coefficients for 12 balanced post-treatment periods in our main specifications, but if we drop the compliant municipalities treated after 1981, we can estimate event study coefficients for 19 balanced post-treatment periods.

Figure B6 re-estimates the results in Figure 7 on this sample. The estimates for the 16 years before treatment and the first 12 years after treatment are very similar to those in Figure 7. The effect of grant receipt on water revenue reaches its lowest point 13 years after treatment, then begins to return to pre-treatment levels.

This pattern of effects is consistent with municipalities receiving multiple grants that were fungible across years. Figure B7 shows the distribution of grants that a municipality received each year relative to the year the municipality receives its first grant. This figure shows that some municipalities receive CWA grants up to 16 years after initial grant receipt, which can explain why the effect of grant receipt on water revenues persists well into the post-treatment period.

B.4 Alternative Data

Starting in 1967, the Census began collecting spending data from a larger sample of municipalities once every five years. Since it only contains one pre-treatment period, we do not use this data in our main crowd-out specifications, however, we obtain similar results when we re-estimate the results in Sections 4 and B.1 using this dataset.

Figure B8 re-estimates the results in Figure 7 on this data. The pattern of effects is similar to those in Figures 7 and B6. Water revenue decreases in compliant municipalities relative to non-compliant municipalities for 15 years after grant receipt, then begin to return to pre-treatment levels.

Table B3 summarizes Figure B8. Column 1 estimates equation 8 on our alternative dataset. Column 2 re-estimates the specification in column 1 with region-by-year fixed effects, and column 3 re-estimates the specification in column 1 with a year fixed effect in place of a timing-group-by-year fixed effect. The results are similar to the estimate of equation 8 in column 1 of Table 9.

Figure B9 and Table B4 then re-estimate the results in Figure B4 and Table B1 respectively. The results are again similar to those we obtain with our main dataset.

Since this data reports capital expenditure but not capital stock, missing years of data could cause us to under-estimate the effect of grant receipt on targeted spending, so we cannot use this data to estimate pass-through, however, we can use it to test if the broad patterns in pass-through in the main text hold in a larger dataset. Figure B10 and Table B5 re-estimate the results in Figure B8 and Table B3 with sewerage capital spending as the dependent variable. This shows that sewerage capital spending increased significantly more after grant receipt in non-compliant municipalities than in compliant municipalities.

B.5 How Much Crowd-Out can Redistribution Account For?

In Section 4, we show that, relative to non-compliant municipalities that received grants in the same year, compliant municipalities reduced water bills by \$2.32 per capita after grant receipt. In this section, we examine how much crowded-out spending reductions in water revenue account for.

Column 1 of Table B6 presents estimates of equation 8 with sewerage capital spending as the dependent variable. Panel A estimates this equation with a dummy treatment variable and Panel B re-estimates this specification interacting the treatment dummy with year relative to treatment. These results suggest that, after grant receipt, compliant municipalities increased sewerage capital spending by \$27.90 per capita (or by an additional \$2.98 per year) less than non-compliant municipalities that were treated in the same year. Since, as shown in column 1 of Table 8, compliant and non-compliant municipalities that were treated in the same year received grants of a similar size, this difference in sewerage capital spending is likely due to differences in pass-through. For this reason, dividing our reduced form estimates of the effect of grant receipt on water revenues in compliant municipalities from Table 9 (also presented in column 3 of Table B6) by this difference will show how much crowded-out spending reductions in water revenue can account for. We present these results in column 3 of Table B6. While they are imprecise and we cannot reject that the coefficients equal 1 (which would suggest that reductions in water revenue can account for all crowded-out spending) or 0 (which would suggest that they do not account for any crowded out spending), taken at face value, these results suggest that reductions in water revenue can account for between half and three-quarters of crowded-out spending in compliant municipalities.

Since we do not find evidence of an increase in targeted spending in compliant municipalities, compliant municipalities likely redistributed any grant funding that this does not account for to other functions of local government according to local preferences.

C Choice of Estimators

There are many ways to estimate treatment effects in a difference-in-differences framework, including two way fixed effects estimators with binary and continuous treatment, Callaway and Sant'Anna (2020a), and stacked difference-in-difference estimators. Each of these methods identifies off of different comparisons and combines these comparisons into summary measures according to different weights. The decision of which method to use is context specific, so we rely on different estimators for different portions of our analysis. In this section, we discuss our choice of estimators in detail.

C.1 Two Way Fixed Effects with Binary Treatment

We could leverage variation in grant timing with a two way fixed effects (TWFE) estimator. To do this, we would first estimate the reduced form effect of grant receipt on sewerage capital spending with the following equation

$$C_{it} = \alpha_0 + \beta_{rf}^{TWFE} D_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{C1}$$

where C_{it} is per capita sewerage capital spending, D_{it} is a dummy variable that equals one for observations of municipalities after they receive their first grant and α_i and α_t are municipality and year fixed effects. We would then divide our estimate of β_{rf}^{TWFE} by an estimate of the first stage relationship between grant receipt and per capita grant amount,

g_{it}, from estimating

$$g_{it} = \alpha_0 + \beta_{fs}^{TWFE} D_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{C2}$$

Since every unit in our sample is eventually treated, estimating β_{rf}^{TWFE} returns an average of comparisons between (1) newly treated municipalities relative to municipalities that have not yet been treated and (2) newly treated municipalities relative to already-treated municipalities. When treatment effects are dynamic, municipalities still *actively responding to treatment* are not a valid counterfactual to represent potential outcomes in the *absence of treatment*, so the second type of comparison can produce results that do not have a causal interpretation. Mechanically, changes in the treatment effects of already-treated units over time are subtracted from a TWFE estimate and we cannot interpret the resulting coefficient as the average treatment effect of the treated (ATT) (Goodman-Bacon, 2021a).

We have an a priori reason to expect the effect of CWA grants on sewerage capital spending to change over time. States distributed CWA grants to fund specific projects, so we would expect grant receipt to cause an increase in sewerage capital spending that only lasts until the project is completed. When spending returns to pre-treatment levels, this decrease is subtracted from a TWFE estimate, biasing the estimate upwards.

We illustrate this problem with a simple example (shown graphically in Figure C1a). Consider a setting where there are two municipalities, A and B, and three time periods, $t = \{0,1,2\}$. Treatment turns on for municipality A in period $t = 1$ and for municipality B in period $t = 2$. After treatment turns on, the outcome variable y increases by one, then returns to pre-treatment levels in the next period. Summarizing this effect with TWFE will return an average of (1) a comparison of municipality A to municipality B in periods 0 and 1 where municipality A is the treated unit and (2) a comparison of municipality B to municipality A in periods 1 and 2 where municipality B is the treated unit. If we denote y_i^t as the outcome variable in municipality i in period t, the first comparison will be

$$(y_A^1 - y_A^0) - (y_B^1 - y_B^0) = (4-3) - (1-1) = 1$$

and the second will be

$$(y_B^2 - y_B^1) - (y_A^2 - y_A^1) = (2-1) - (3-4) = 2$$

TWFE will return a weighted average of these two comparisons, which is strictly greater than the true effect, and our result will be biased upwards. This sort of upward bias could be one reason that estimates of grant pass-through are higher than economic theory would predict in situations where grants roll out over time.

To test the validity of a TWFE estimator in this context, we check how much of our estimate of β_{rf}^{TWFE} comes from comparisons of newly treated units relative to already-treated units. We first estimate equation C1 on the full sample, then decompose the resulting estimate of β_{rf}^{TWFE} into every possible comparison between timing groups using Goodman-Bacon et al. (2019). Figure C2 presents a scatterplot of these comparisons and their associated weights. Table C1 summarizes Figure C2 and shows that more than half of our TWFE estimate comes from comparisons of newly treated units to already-treated units, raising concerns over its interpretation and motivating a different estimator.

C.2 Dose-Response Two Way Fixed Effects

Researchers often estimate pass-through by leveraging continuous variation in grant size. Callaway et al. (2021) documents several potential problems that arise when using this type of variation in a difference-in-differences framework. In this section, we discuss how the results from Callaway et al. (2021) apply to estimating grant pass-through.

Consider the following equation, which regresses targeted spending on a continuous measure of grant size and unit and time fixed effects:

$$C_{it} = \alpha_0 + \beta_{dr}^{TWFE} g_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{C3}$$

When there is no never treated group, then, similar to a TWFE estimate with binary treatment, estimating β_{dr}^{TWFE} returns a weighted average of (1) comparisons of newly treated municipalities relative to municipalities that have not yet been treated, (2) comparisons of newly treated municipalities relative to already-treated municipalities, *and* (3) comparisons of municipalities that received larger grants relative to municipalities that received smaller grants. As in Section C.1, the first type of comparison produces meaningful estimates under parallel trends. In the presence of dynamic treatment effects, the second type of comparison can produce results that do not have a causal interpretation.

To identify meaningful effects with the third type of comparison, we must assume that the effect of a grant of a given size is homogeneous across receiving municipalities. To see why, consider two groups of municipalities treated in the same period. Municipalities in one group receive grants of size d and municipalities in the the other group receive grants of size d'. Express the effect of d on the group that received grants of size d as $ATT(d|d)$. Under parallel trends, comparing the effects of grants in these groups of municipalities yields the following result:

$$ATT(d|d) - ATT(d'|d') = \underbrace{ATT(d|d) - ATT(d'|d)}_{\text{effect of going from d' to d}} + \underbrace{ATT(d'|d) - ATT(d'|d')}_{\text{selection bias}}$$

We can interpret the first term as the effect of increasing grant size from d' to d on units that actually received d, which is a causal parameter. The problem is that this type of comparison also includes a selection bias term, which will be averaged into our estimate of β_{dr}^{TWFE}.

Under parallel trends, we can write the selection term as

$$E[Y_t(d') - Y_{t-1}(0)|D = d] - E[Y_t(d') - Y_{t-1}(0)|D = d']$$

which is equal to the effect of a grant of size d' on units that actually received d minus the effect of a grant of size d' on units that actually received d'. If we assume that the effect of d' is homogeneous, then these terms cancel.

Since grants of a given size often have different effects in compliant and non-compliant municipalities, this assumption is unlikely to hold in the context of CWA grants. The following example illustrates why heterogeneity in grant pass-through by compliance presents a problem for estimation. Consider what happens when we compare a non-compliant municipality that received a grant of \$210 and increased spending by \$75 to a compliant municipality that received a grant of \$200 and increased spending by \$25. The total pass-through rate of the grants to these two municipalities is 0.244, but comparing these two municipalities implies that each dollar of grant money led to a $\frac{75-25}{210-200} = 5$ dollar increase in targeted expenditure. Comparisons of compliant municipalities to non-compliant municipalities will face the opposite problem; this type of comparison suggests that grants led to a *decrease* in spending, which will bias β_{dr}^{TWFE} downward.

We can address problems caused by heterogeneity across compliant and non-compliant municipalities by estimating β_{dr}^{TWFE} on each of these groups separately, (we present these results in Table C2) however, these results may not return the total pass-through rate if treatment effects are dynamic. We illustrate how comparisons across timing groups complicate estimation with a modified version of the example in Figure C1a (shown graphically in Figure C1b). There are two municipalities, A and B, and three time periods, $t = \{0,1,2\}$. Treatment turns on for municipality A in period $t = 1$ and for municipality B in period $t = 2$. Municipality A receives a dose of size 2 and municipality B receives a dose of size 1. After treatment turns on, the outcome variable y increases by dose size before returning to pre-treatment levels in the next period. A TWFE estimate will include (1) a comparison of municipality A to municipality B in periods 0 and 1 where municipality A is the treated unit, divided by the dose size in municipality A, (2) a comparison of municipality B to municipality A in periods 1 and 2 where municipality B is the treated unit, divided by the dose size in municipality B and (3) a comparison of municipality A to municipality B in periods 0 and 3 where municipality A is the treated unit, divided by the the difference in doses between municipality A and municipality B. If we denote the dose size in municipality i as d_i, the first comparison will be

$$\frac{(y_A^1 - y_A^0) - (y_B^1 - y_B^0)}{d_A} = \frac{(5-3)-(1-1)}{2} = 1$$

the second will be

$$\frac{(y_B^2 - y_B^1) - (y_A^2 - y_A^1)}{d_B} = \frac{(2-1)-(3-5)}{1} = 3$$

and the third will be

$$\frac{(y_A^2 - y_A^0) - (y_B^2 - y_B^0)}{d_A - d_B} = \frac{(3-3)-(2-1)}{2-1} = -1$$

Only the comparison of early treated units to not-yet-treated units accurately describe the relationship between dose size and the outcome variable. Note that dose-response variation returns a result that is wrong signed *even though the effect of grants is homogeneous in grant size.*[C1]

To interpret results identified off of dose-response variation as the ATT when treatment timing varies, we must assume that the response to a given does in a given time period is constant across groups (Callaway et al., 2021). Since treatment effects are heterogeneous across both time relative to treatment (as shown in Figure 4) and compliance (as shown in Figures 5 and 6), this assumption does not hold. For this reason, our research design does not leverage variation in grant size.

C.3 Callaway and Sant'Anna

Since it focuses on combinations of $ATT(g,t)$, Callaway and Sant'Anna (2020a) is far simpler than a TWFE estimator. In practice, $\widehat{ATT(g},t)$, the sample analogue of $ATT(g,t)$,

[C1]See Theorem 5 in Callaway et al. (2021) for a formal discussion.

is equivalent to the OLS estimate from a two group, two time period difference-in-difference that compares observations from group g and observations from groups not yet treated at time t in periods t and $g-1$. That is, $\widehat{ATT(g,t)}$ is identical to an estimate of β from equation C4.

$$C_{it} = \alpha_0 + \alpha_1 G_g + \alpha_2 1\{T = t\} + \beta(G_g * 1\{T = t\}) + e \tag{C4}$$

Our 12 timing groups, observed across many post-treatment periods, yield 96 different $\widehat{ATT(g,t)}$. Most of the $\widehat{ATT(g,t)}$ are identified off of relatively few observations, so instead of interpreting individual $\widehat{ATT(g,t)}$, we summarize the effect of treatment by aggregating the $\widehat{ATT(g,t)}$ together. Unlike a TWFE estimator, where comparisons are always weighted together based on group size and within-group variance of the treatment dummy (Goodman-Bacon, 2021a), researchers can select weights appropriate to the setting. We report the dynamic aggregation given by equation 1 and the simple aggregation given by equation 2 in the main text, as well as an aggregation that allows for selective treatment timing in Appendix Section A.1.

Callaway and Sant'Anna (2020a) constructs standard errors with a multiplier bootstrap procedure.[C2] Instead of re-sampling observations, each bootstrap draw perturbs the influence function of the estimate (which measures the dependence of the estimate on each cluster in the sample). We report multiplier bootstrap standard errors for our summary measures of the first stage relationship between grant receipt and grant amount and the reduced form effect of grant receipt on sewerage capital expenditure.

[C2] See Algorithm 1 in Callaway and Sant'Anna (2020b).

Rather than derive the influence function for our pass-through estimator, we construct standard errors for our pass-through estimates with a pair-bootstrap procedure. To do this, we take a random sample (with replacement) of municipalities and jointly estimate our first stage and reduced form estimates. We then divide our reduced form estimate by the associated first stage estimate to obtain β_b^{boot}, where b indexes bootstrap iterations. We repeat this process 1000 times and use the results to calculate bootstrap standard errors with the following equation

$$\sqrt{\frac{1}{999}\sum_{b=1}^{1000}(\beta_b^{boot} - \overline{\beta^{boot}})^2} \tag{C5}$$

where $\overline{\beta^{boot}} = \frac{1}{1000}\sum_{b=1}^{1000}\beta_b^{boot}$.

Because pair-bootstrap draws do not preserve the relative size of each timing group, the standard errors for our pass-through estimates will not be efficient if the effect of treatment varies across treatment groups. The results in Section A.1 suggest that this is not the case, so changes in the size of timing groups across bootstrap samples should not add variance to our pair-bootstrap estimates.[C3]

C.4 Stacked Difference-in-Differences

When examining heterogeneity in the relationship between grant amount and targeted spending, we rely on estimates from a stacked difference-in-differences estimator. While most of our results are robust to using Callaway and Sant'Anna (2020a) instead, a stacked esti-

[C3]Note that we do not go so far as to assume that treatment timing is truly random. When that is the case, difference-in-differences designs do not provide the most efficient estimates (Roth and Sant'Anna, 2021).

mator allows us to define our treatment and control groups more flexibly. While Callaway and Sant'Anna (2020a) estimates the average effect of grant receipt on *all units* in a *given timing group* in *different periods*, we construct our stacks so that we estimate the average effect of grant receipt on *individual units* across *all post treatment periods*. This allows us to semi-parametrically examine the relationship between grant amount and sewerage capital spending before constructing summary measures of pass-through. Since timing groups contain both compliant and non-compliant municipalities and municipalities that received different sized grants, we cannot repeat this process with individual $ATT(g,t)$ without making a priori assumptions about the relationship between grant amount and sewerage capital spending, as well as the relationship between compliance and sewerage capital spending. That being said, Callaway and Sant'Anna (2020a) is much more careful about aggregating these results together and conducting inference, which is why we rely on Callaway and Sant'Anna (2020a) for our main results.[C4]

A stacked estimator can be more complicated than Callaway and Sant'Anna (2020a), so in this section, we discuss how we construct stacks, as well as inference and identification with a stacked estimator.

C.4.1 Constructing Stacks and Obtaining Estimates

Each stack S is defined by a single municipality, called municipality s, which is labeled as treated in stack S. Stack S also includes municipalities that receive grants at least seven years after the treated municipality, which are labeled as controls. Municipalities that receive

[C4]Callaway and Sant'Anna (2020a) is also more careful about controlling for confounders, though our analysis does not have any controls.

grants early (specifically, those treated before 1979) are only in stacks in which they are the treated unit. Municipalities that receive grants too late to have any controls of their own (those treated after 1981) are only included as controls in other stacks. Municipalities that receive grants in the middle of our study period (from 1979 to 1981) can be a treated unit in one stack, while observations of that municipality from before it received a CWA grant can be controls in other stacks.

We can estimate the reduced form effect of grant receipt on sewerage capital spending for municipality s, denoted β_s^{2X2}, by comparing spending in municipality s to spending in the control municipalities in stack S before and after t_s^*, the year in which municipality s becomes treated. For a concrete example, consider a municipality treated in 1972. In the stack defined by that municipality, we can estimate a simple two group difference-in-difference where treatment turns on for the treated group in 1972. This compares sewerage capital spending in the treated municipality to spending in the control municipalities before and after 1972. The control group consists of every municipality treated in 1979 or later. This stack will include observations of both the treated and control units in each year from 1956 to 1978. Since there are no observations from 1979 or later in this stack, treatment never turns on for the control group.

Using units treated at least seven years after municipality s as controls is not an arbitrary choice. As shown in Figures 3 and 4, both grant amount and sewerage capital spending return to near pre-treatment levels by 6 years after treatment. If we only used units treated more than seven years in the future as controls, we could look at a longer post period, but our control group would be very small. We could have a larger control group if we used a shorter post period, but the resulting estimates would not capture increases in sewerage capital spending caused by CWA grants that took several years to appear in the data. Using units treated seven years in the future as controls is a compromise between these two options that likely captures the majority of the effects of CWA grants.

Our estimates of the β_s^{2X2} are noisy, so instead of interpreting individual β_s^{2X2}, we create summary measures of the effect of grant receipt on sewerage capital spending by aggregating the β_s^{2X2} together. Before doing so, we examine heterogeneity in pass-through by both grant size and compliance in Figures 5 and 6. On the bias/variance trade-off, these figures are very low bias, and we can use them to determine the best way to obtain lower variance summary measures of pass-through. The shapes of these figures motivate us to separately summarize pass-through for compliant municipalities, non-compliant municipalities that received small grants and non-compliant municipalities that received large grants.

We summarize the effect of grants on sewerage capital spending for each of these groups with $\beta_{rf}^{stacked}$ from equation 6. In practice, $\beta_{rf}^{stacked}$ is a weighted average of the β_s^{2X2} from each stack. The weights on each β_s^{2X2} in our estimate of $\beta_{rf}^{stacked}$ are based on stack size and within-stack variance of D_{it} (Goodman-Bacon, 2021a). We construct our stacks in such a way that each β_s^{2X2} is weighted equally by our regression. To do this, we collapse our control observations in each stack to yearly means. In the collapsed data, each stack contains two observations for each year (one treated observation and one control observation) for 16 years of pre-treatment data and 7 years of post-treatment data. Since each stack has the same number of observations and the treated unit in each stack is treated for the same percent of the time, each stack has the same weight.

We cannot construct standard errors clustered at the municipality level using this collapsed data. Instead, we construct pair-bootstrap standard errors. To do this, we take a random sample (with replacement) of observations in the unstacked data, clustering at the municipality level to preserve any dependence of error terms across time. We then re-form our stacks and perform our estimates. We repeat this process 1000 times and use the results to calculate bootstrap standard errors with equation C5.

C.4.2 Identifying Assumption of a Stacked Difference in Differences

In the main text, we assume that treatment timing is uncorrelated with trends in local spending, but we can state the assumption needed to interpret our stacked estimates as the ATT more precisely. Remember that the coefficient from our stacked estimator is an unweighted average of the difference in sewerage capital spending between treated and control municipalities in each stack before and after treatment. Estimating this simple difference-in-difference in a given stack S (defined by municipality s) yields β_s^{2X2}, which we can express in terms of means as

$$\beta_s^{2X2} = (\bar{y}_s^{post} - \bar{y}_s^{pre}) - (\bar{y}_{\sim s}^{post} - \bar{y}_{\sim s}^{pre})$$

This is the difference in the outcome variable in municipality s before and after t_s^* minus the difference in the outcome variable in the control municipalities in stack S before and after t_s^*. If we define $y_{it}(1)$ as the potential outcome if municipality i is treated in year t, and $y_{it}(0)$ as the potential outcome if municipality i is not treated in year t, then we can express

β_s^{2X2} in terms of potential outcomes as

$$\begin{aligned}
\beta_s^{2X2} =& (E[y_{it}(1)|i = s, post] - E[y_{it}(0)|i = s, pre]) \\
& - (E[y_{it}(0)|i \neq s, post] - E[y_{it}(1)|i \neq s, pre]) \\
=& E[y_{it}(1) - y_{it}(0)|i = s, post] + E[\Delta y_i(0)|i = s] - E[\Delta y_i(0)|i \neq s] \\
=& ATT_s + E[\Delta y_i(0)|i = s] - E[\Delta y_i(0)|i \neq s]
\end{aligned}$$

ATT_s is the effect of treatment in municipality s, so β_s^{2X2} consists of a causal parameter and an identifying assumption. In this case, to interpret β_s^{2X2} as the effect of treatment in municipality s, we must assume that, on average, municipality s would have followed parallel trends with the control municipalities in stack S in absence of treatment.

Since $\beta^{stacked}$ (the result from a stacked difference in difference) is an unweighted average of the β_s^{2X2} from each stack, we can express $\beta^{stacked}$ as

$$\begin{aligned}
\beta^{stacked} &= \frac{1}{S}\sum_{s=1}^{S}(ATT_s + E[\Delta y_i(0)|i = s] - E[\Delta y_i(0)|i \neq s]) \\
&= \frac{1}{S}\sum_{s=1}^{S} ATT_s + \frac{1}{S}\sum_{s=1}^{S}(E[\Delta y_i(0)|i = s] - E[\Delta y_i(0)|i \neq s])
\end{aligned}$$

so our identifying assumption is that $\sum_{s=1}^{S}(E[\Delta y_i(0)|i = s] - E[\Delta y_i(0)|i \neq s]) = 0$. Note that this does not require us to assume that parallel trends holds in *every* stack, only that any violations of parallel trends cancel out when we take an average of the effect in each stack.

D Data Details

D.1 Grant Data

We begin with grant data from the EPA's Grant Information Control System, which we obtained through a Freedom of Information Act request. This data contains information on the year that the EPA distributed each grant, which municipality received the grant, the specific wastewater treatment facility the grant was designated for and the amount distributed. Keiser and Shapiro (2019) uses the same data, and Appendix Section B.4 of Keiser and Shapiro (2019) demonstrates its accuracy.

Some grants are explicitly for construction (and include a plant code), while others do not have a plant code. It is unclear to what extent these grants were precisely for upgrading wastewater treatment plants, so we drop grants that did not have a specific facility code. This restricts our sample to 33,429 grants. We also drop grant records that are missing the year in which grant funds were distributed, which drops another 3533 observations, as well as the 475 grants distributed to municipalities outside the contiguous U.S.

Some municipalities received grants in 1960-72 through predecessor programs similar to the CWA. At least some of these precursor grants are included in our data from the EPA, but we did not assess the quality or completeness of information of grants from before 1972, so we exclude the 5248 observations of precursor grants from our analysis. This leaves us with records of 25,997 grants to 12,291 facilities.

If municipalities in our sample received any of the grants that we drop, our pass-through results could be biased upwards in two ways. First, when units act as a treated group, any increases in spending that these grants induce will appear in the reduced form estimate of the effect of grant receipt on targeted spending, but the the associated first stage estimates will not capture increases in grant funding; second, the treatment effects of these grants will change over time (in particular, spending will return to pre-treatment levels as grant funding runs out), and when units act as controls, these decreases in treatment effects will be subtracted out of our reduced form estimates.

It is also possible that, when units act as a treated group, receipt of one of these grants will lead to an increase in targeted spending in the pre-treatment period, biasing our reduced form estimates downward. If this was a large problem, it would appear in the pre-trend of our event studies.

D.2 Clean Watershed Needs Survey Data

We define whether a facility was in compliance with the CWA's capital mandate using the 1972 Clean Watershed Needs Survey, which we merge to our grant data with a unique facility code. The CWNS is an assessment of the capital investment that publicly-owned wastewater treatment facilities needed to make in order to come into compliance with the Clean Water Act and contains information on which community the facility serves, the total wastewater flowing through the facility, the treatment technology currently in place, whether the facility needs to meet standards higher than the EPA's secondary treatment requirement and whether they are currently in compliance with these requirements. This data comes from the EPA's CWNS team, and is the same data that Jerch (2018) uses to define compliance with the CWA's capital mandate.

We use a facility's answer to Question 21 on the CWNS questionnaire (reproduced in

Figure D1) to define compliance. Question 21b asks if a facility needs to meet treatment technology requirements that are more stringent than the EPA's secondary treatment requirement. Question 21c then asks whether a facility is currently in compliance with both the EPAs secondary treatment mandate and any higher mandates. We define facilities that answered "yes" on question 21c as "compliant", and those that answer "no" as "non-compliant". Table D1 shows cross-tabs of these variables. Our sample includes facilities that were in compliance with the CWA's capital mandate when the CWA came into effect but were not in compliance with more stringent state standards. These facilities still faced incentives to spend CWA grants on sewerage capital, so we classify them as non-compliant.

Note that many facilities installed tertiary treatment after the CWA came into effect (USEPA, 2000). This increase was likely driven by municipalities bound by state standards or compelled by lawsuits to make upgrades beyond secondary treatment. Since we define these municipalities as non-compliant and we document an increase in sewerage capital spending in response to grant receipt in non-compliant municipalities, it is possible that municipalities used CWA grants to upgrade to tertiary treatment.

Our results are robust to dropping municipalities with facilities that were in compliance with the CWA's capital mandate but were not in compliance with more stringent state standards. Panels A and B of Table D2 re-estimate the results in Panels A and B of Table 4 after dropping non-compliant municipalities that had to satisfy state wastewater capital requirements greater than the EPA's secondary treatment requirement. There is still evidence of full pass-through in municipalities that received grants smaller than $730 per capita and substantial crowd-out in municipalities that received grants larger than this amount.

We re-estimate the point where costs were no longer increasing in grant size using this sample in Table D3. This produces an estimate that is similar to the results in Table 3. We re-estimate pass-through for non-compliant facilities that were not required to satisfy state wastewater capital requirements greater than the EPA's secondary treatment requirement that received grants above and below this point in panels C and D of Table D2. Again, the results are similar to those in Table 4.

Since our data on compliance is nearly 50 years old, we assessed its quality by comparing it to information on treatment technology from Table 2.4 of USEPA (2000). This table shows that, of 19,355 publicly owned wastewater treatment facilities surveyed, 9,887 were using secondary treatment or greater in 1972. This represents 51% of facilities. While we only observe whether a facility was in compliance with *both* EPA and more stringent state standards (as opposed to knowing if they satisfied just the secondary treatment technology standard), we can use our data to calculate the percent of facilities that had at least secondary treatment among facilities that were only required to meet the EPA's standard. Column 1 of Table D1 shows that 56% of these facilities were using secondary treatment or greater in 1972. Given the different sample, this is relatively close to the 51% of facilities using secondary treatment or greater in 1972 in USEPA (2000).

Our definition of compliance does not directly enter into our pass-through equations. Instead, we use compliance to define sub-groups for which we expect to find heterogeneous treatment effects. If we mis-classify non-compliant municipalities as compliant, our estimates of pass-through for non-compliant municipalities will not be biased, but our estimates of pass-through for compliant municipalities will be biased upwards. This is because our compliant estimates will include the effect of grant dollars that were bound by the CWA's capital mandate, and thus theoretically more likely to be spent on sewerage capital.

Conversely, if we mis-classify compliant municipalities as non-compliant, our estimates of pass-through for compliant municipalities will not be biased, but our estimates of pass-through for non-compliant municipalities will be biased downwards since they will include the effect of more grant dollars that were not bound by the CWA's capital mandate.

Our definition of compliance does directly enter into our estimates of the effect of grant receipt on water revenue in Section 4. Mis-classifying municipalities as compliant or non-compliant will attenuate these results.

D.3 Estimating Costs

We define compliance in such a way that compliant municipalities never face any mandated costs, but compliance is not the only source of heterogeneity in mandated costs; there is also variation in mandated costs within non-compliant municipalities. The costs of upgrading depend largely on facility size (USEPA, 1973), and we use engineering estimates of the costs of upgrading from USEPA (1973) to approximate these costs.

USEPA (1973) uses data from the observed costs of upgrading plants to secondary treatment and fits this data to the number of gallons of wastewater flowing through a facility. This yields the following equation:

$$TotalCost = exp(.13732 + .77872 * ln(TotalFlow))$$

We use this equation to calculate mandated costs for each facility, then sum these costs across the facilities operated by a municipality to approximate the total costs that each municipality faces. While this measure of mandated costs is noisy, since it is based on an engineering formula, it is unlikely to be correlated with local preferences.[D1]

These cost estimates never enter into any of our equations directly. Instead, we use them to determine if grants are larger or smaller than mandated costs. Using these cost estimates, we find that, while grants are similar to costs for municipalities that receive up to around \$730 per capita in total grant aid, grants to municipalities that receive more than this amount are substantially larger than the estimated cost of upgrading. We then separately estimate pass-through for municipalities that received grants above and below \$730 per capita in total grant aid. Noise in our cost estimates will cause us to mis-classify non-compliant municipalities between these two groups. This will bias our pass-trough estimates for non-complaint municipalities that received small grants downwards, since it will cause these estimates to include non-compliant municipalities that received grants that included funding that was not bound by the capital mandate. It will also bias our pass-trough estimates for non-complaint municipalities that received large grants upwards, since it will include municipalities whose grants were completely bound by the capital mandate. Noise in our cost estimates will also cause our pass-through estimates for both groups to lose precision.

D.4 Merging Facility Data to Municipal Spending Data

The CWNS contains data on the authority that operates each facility, and we use this information to merge our facility-level data to municipality-level spending data from the Census Bureau's Historical Database on Individual Government Finances. The CWNS data does not contain a unique identifier of municipalities that is consistent with our local gov-

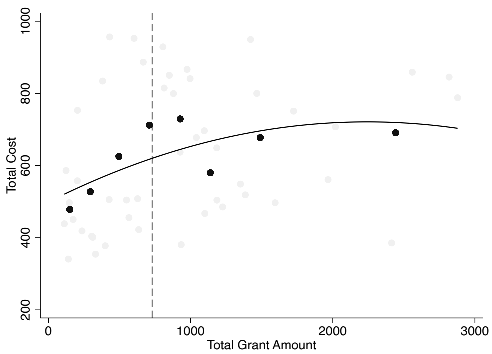
[D1]If we used an assessment conducted by municipalities instead, we might be concerned that self-reported total costs are correlated with preferences for spending.

ernment finance data, but since we only have a balanced panel of spending data for 216 municipalities, it is relatively straightforward to use the state and municipality name in both detests to match our municipality level data to the facilities that they operated.

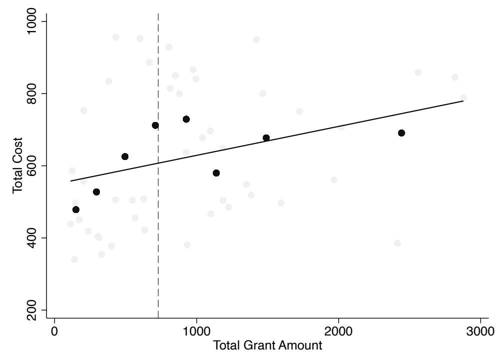
D.5 Measuring Spending in Real Per Capita Dollars

We follow the previous pass-through literature by measuring grant amount and all spending in per capita terms. (Strumpf, 1998; Card and Payne, 2002; Vegh and Vuletin, 2015). All of our variables of interest are largely influenced by population, but dividing these variables by population ensures that our estimates are not directly biased by population growth. Figure D2 re-estimates the results in Figure 4 with population as the dependent variable, and shows that population did not change after grant receipt.

We deflate grant amount and all spending variables to 2020 dollars with the Consumer Price Index. As shown in Figures D3, D4 and D5, and Tables D4, D5 and D6, our main results are robust to measuring spending and grant amount in nominal terms.



(a) Quadratic Fit



(b) Linear Fit

Figure A1: Alternative Fits

Notes: This figure re-creates Figure 1 with quadratic and linear fits.

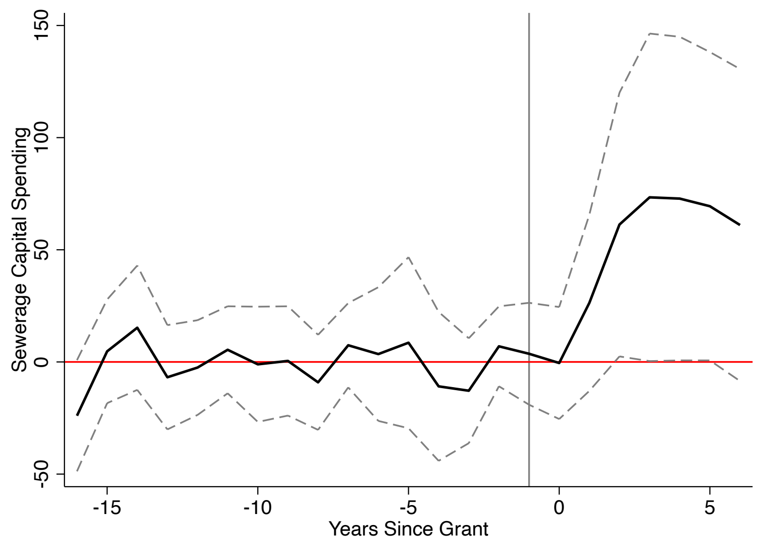


Figure A2: Balanced Sewerage Capital Event Study

Notes: This figure re-estimates the results in Figure 4 on a sample of municipalities that we observe for at least seven post-treatment periods.

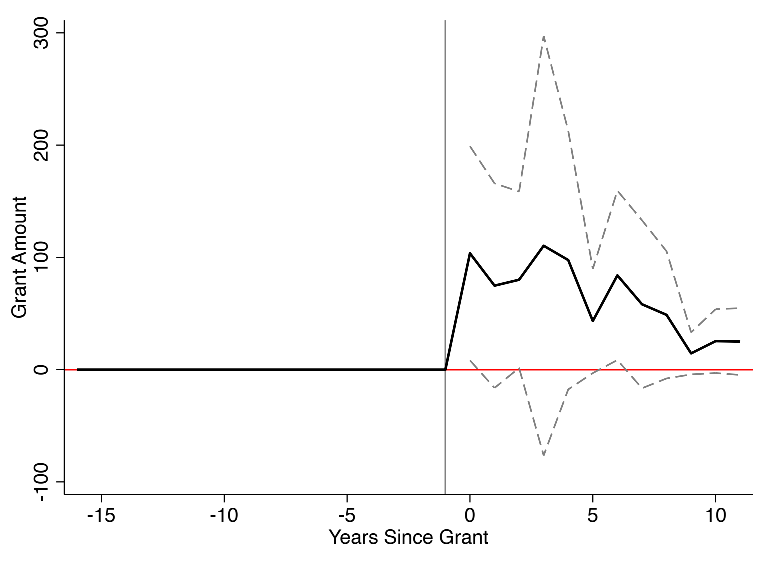


Figure A3: Weighted Grant Amount Event Study

Notes: This figure re-estimates the results in Figure 3 weighting by population.

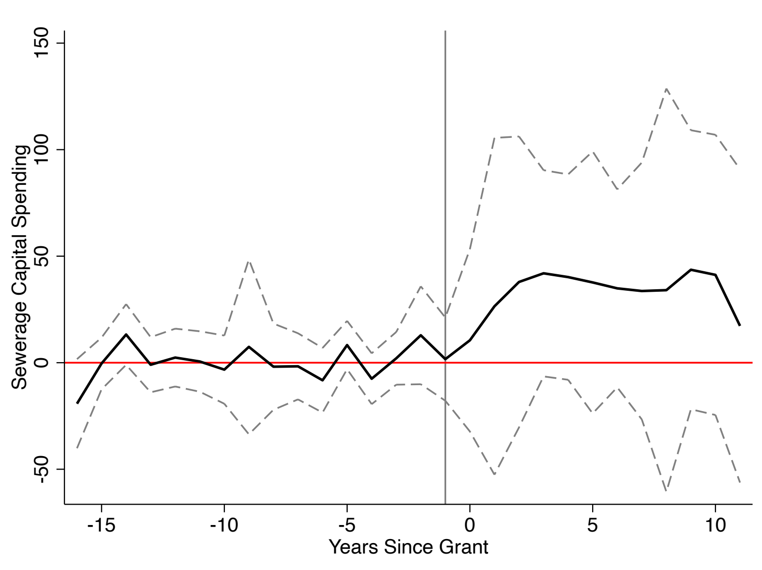


Figure A4: Weighted Sewerage Capital Spending event study

Notes: This figure re-estimates the results in Figure 4 weighting by population.

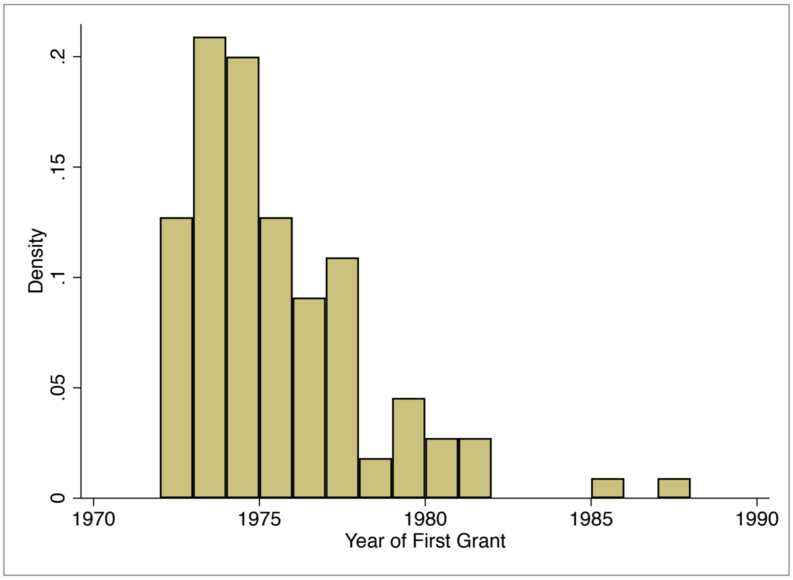
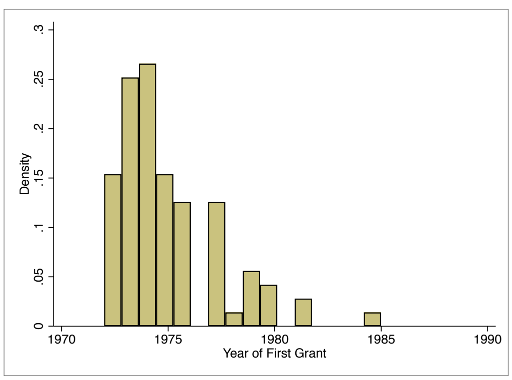
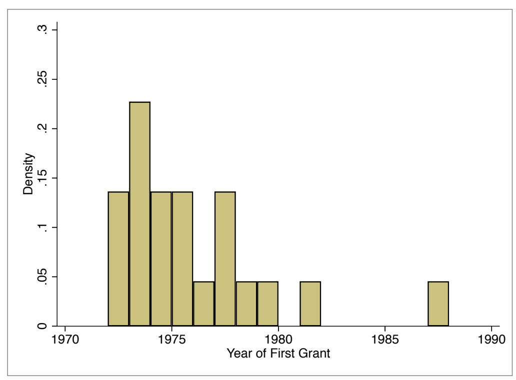


Figure A5: Timing Groups

Notes: This figure shows how many municipalities received their first CWA grant in each year from 1972 to 1988.



(a) Non-Compliant



(b) Compliant

Figure A6: Timing Groups by Compliance

Notes: This figure re-creates Figure A5 for sub-samples of compliant and non-compliant municipalities.

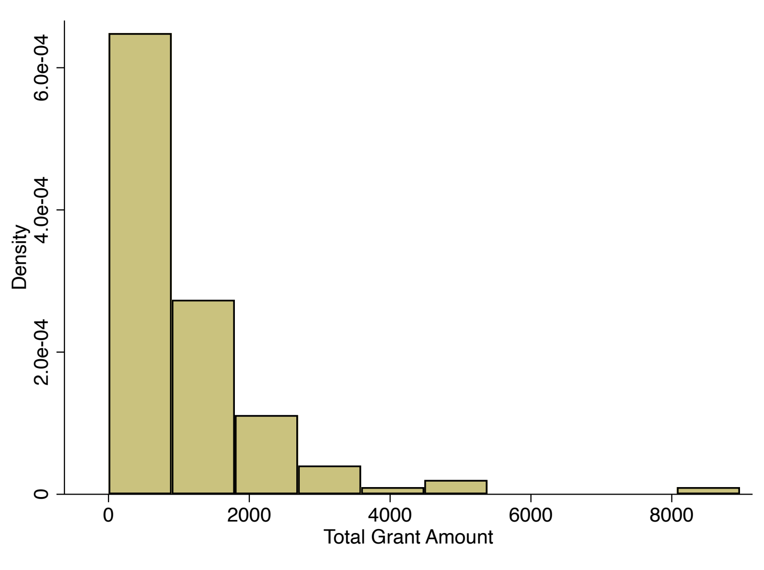


Figure A7: Distribution of Grant Size

Notes: This figure shows the distribution of total per capita grant dollars.

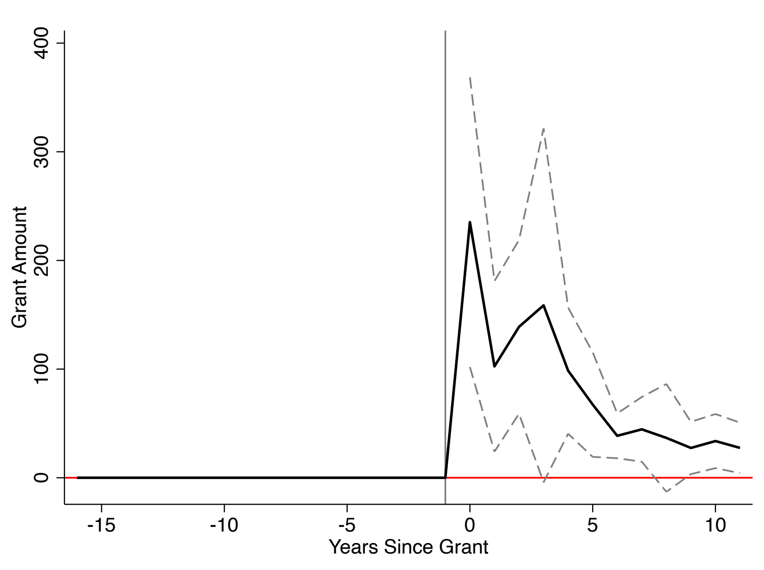


Figure A8: Grant Amount Event Study Using Never-Treated Controls

Notes: This figure re-creates Figure 3 using never-treated units as a control group.

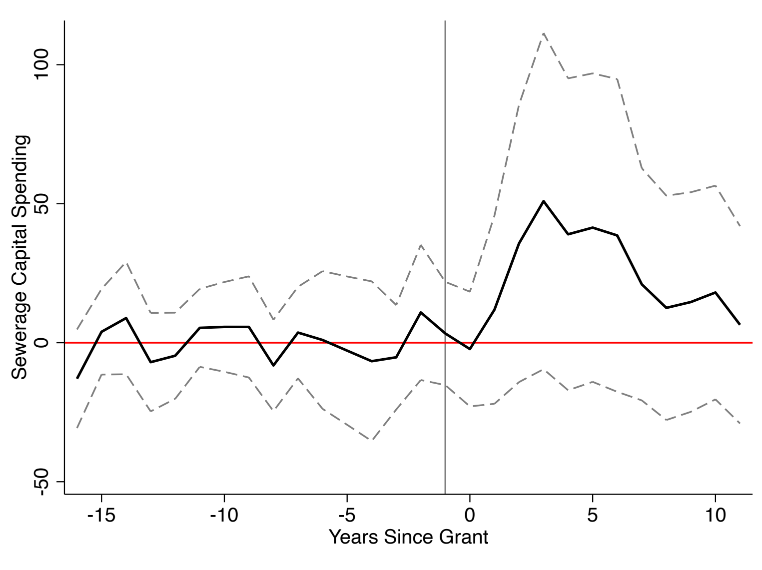


Figure A9: Sewerage Capital Spending Event Study Using Never-Treated Controls

Notes: This figure re-creates Figure 4 using never-treated units as a control group.

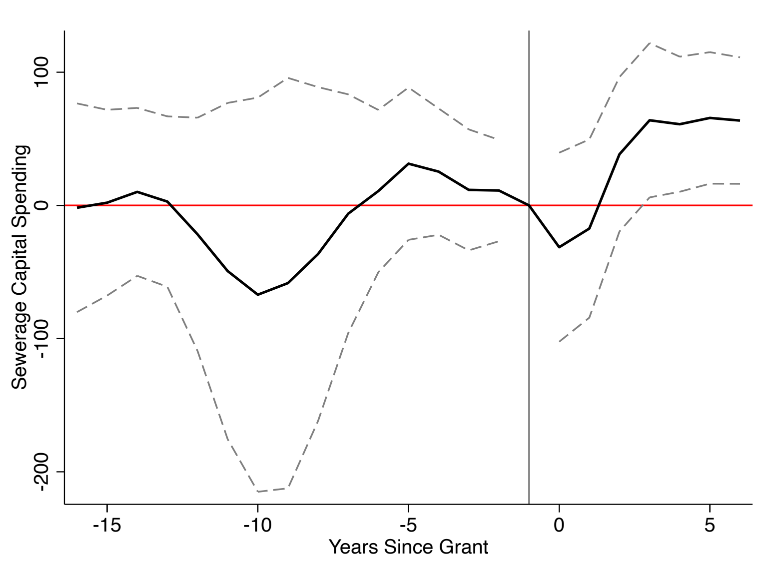


Figure A10: Stacked Sewerage Capital Event Study

Notes: This figure plots the π_y and γ_y from estimating $C_{it} = \alpha_0 + \sum_{y=-16}^{-2} \pi_y 1\{t - t_i^* = y\} * treat_{is} + \sum_{y=0}^{6} \gamma_y 1\{t - t_i^* = y\} * treat_{is} + \alpha_{is} + \alpha_{ts} + \epsilon_{its}$. i indexes municipalities, t indexes years and s indexes stacks. $treat_{is}$ equals one for observations of the treated municipality in each stack after that municipality received its first CWA grant. The dependent variable is per capita sewerage capital spending and α_{is} and α_{ts} are stack by municipality and stack by year fixed effects.

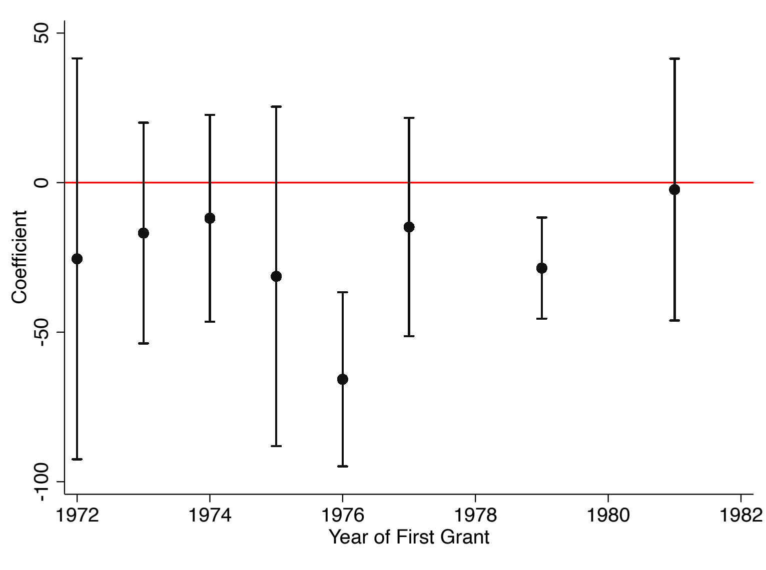


Figure B1: Decomposition of Main Water Revenue Estimate

Notes: This figure presents estimates of the θ_g^{2X2} from estimating $R_{it} = \alpha_0 + \theta_g^{2X2} 1\{t \geqslant g\} * compliant_i + \delta 1\{t \geqslant g\} + \omega compliant_i + \epsilon_{it}$ on observations from each timing group g along with the 95% confidence interval of each θ_g^{2X2}. We plot each θ_g^{2X2} against the year timing group g became treated.

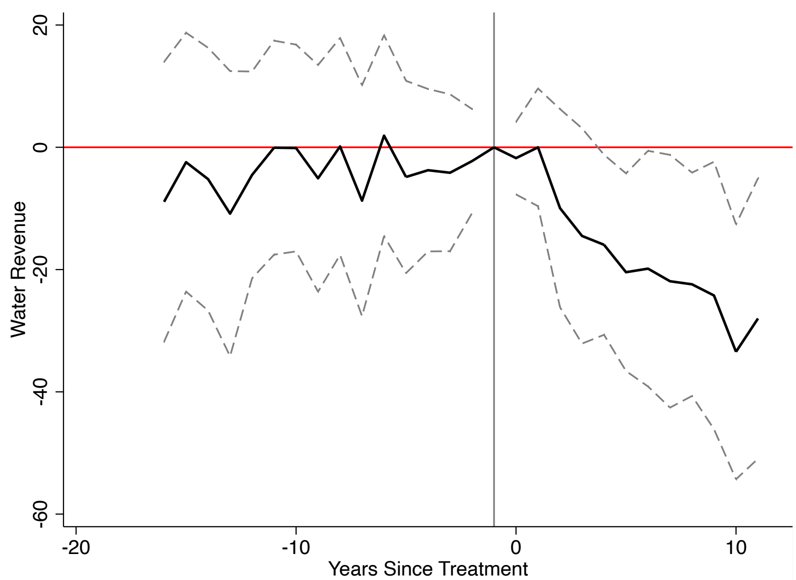


Figure B2: Water Revenue Event Study with Region-by-Year Fixed Effects

Notes: This figure plots the π_y and γ_y from estimating $R_{it} = \alpha_0 + \sum_{y=-16}^{-2} \pi_y 1\{t - t_i^* = y\} * compliant_i + \sum_{y=0}^{11} \gamma_y 1\{t - t_i^* = y\} * compliant_i + \alpha_i + \alpha_{gt} + \alpha_{rt} + \epsilon_{it}$ with per capita water revenue as the dependent variable. $compliant_i$ equals one for observations of compliant municipalities. We include municipality, timing-group-by-year, and region-by-year fixed effects, α_i, α_{gt}, and α_{rt}, where g indexes the year in which a municipality received its first CWA grant and r indexes region.

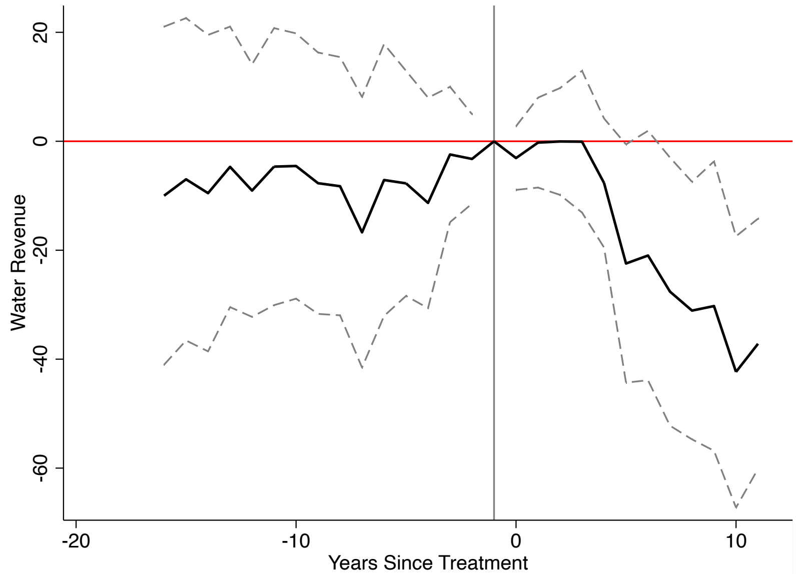
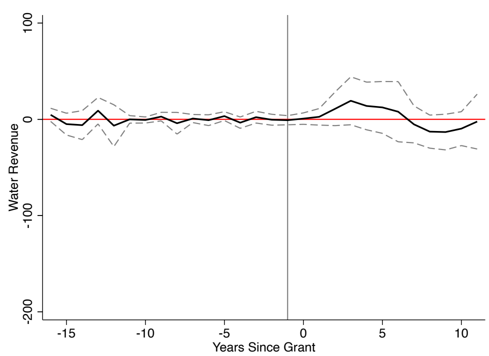
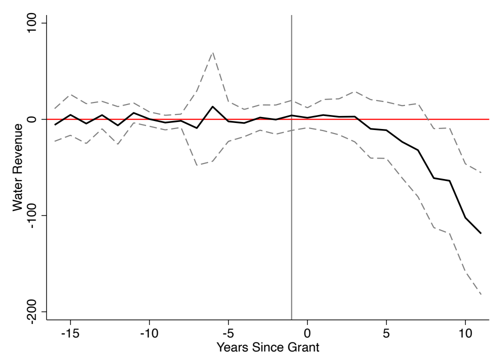


Figure B3: TWFE Water Revenue Event Study

Notes: This figure plots the π_y and γ_y from estimating $R_{it} = \alpha_0 + \sum_{y=-16}^{-2} \pi_y 1\{t - t_i^* = y\} * compliant_i + \sum_{y=0}^{11} \gamma_y 1\{t - t_i^* = y\} * compliant_i + \alpha_i + \alpha_t + \epsilon_{it}$. α_i and α_t are municipality and year fixed effects.



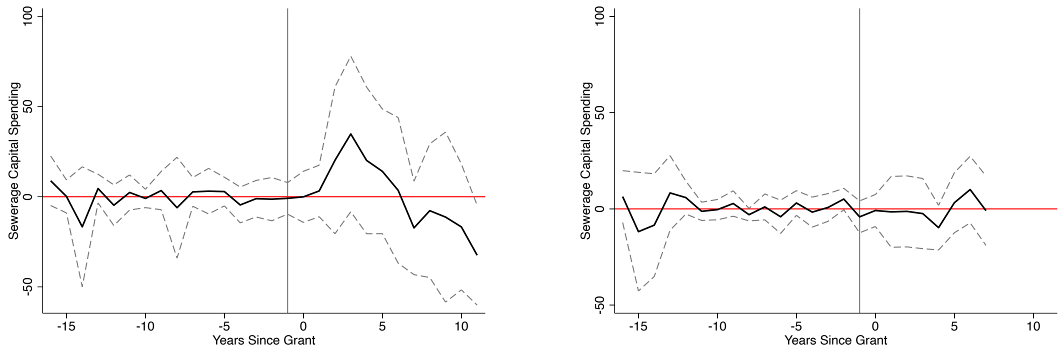
(a) Non-Compliant



(b) Compliant

Figure B4: Water Revenue Decreased After Grant Receipt in Compliant Municipalities

Notes: This figure presents $\widehat{\theta}_D(e)$ for each year e relative to treatment, where $\widehat{\theta}_D(e) = \sum_{g=1972}^{1985} \sum_{t=1957}^{1987} 1\{t - g = e\} \widehat{ATT(g,t)} P(G = g | t - g = e)$. Per capita water revenue is the dependent variable.



(a) Grants Less Than Costs (b) Grants Greater Than Costs

Figure B5: Municipalities Raised Water Bills When Grants Were Too Small

Notes: This figure re-estimates the results in Sub-Figure B4a on sub-samples of municipalities where grants are larger or smaller than estimated costs.

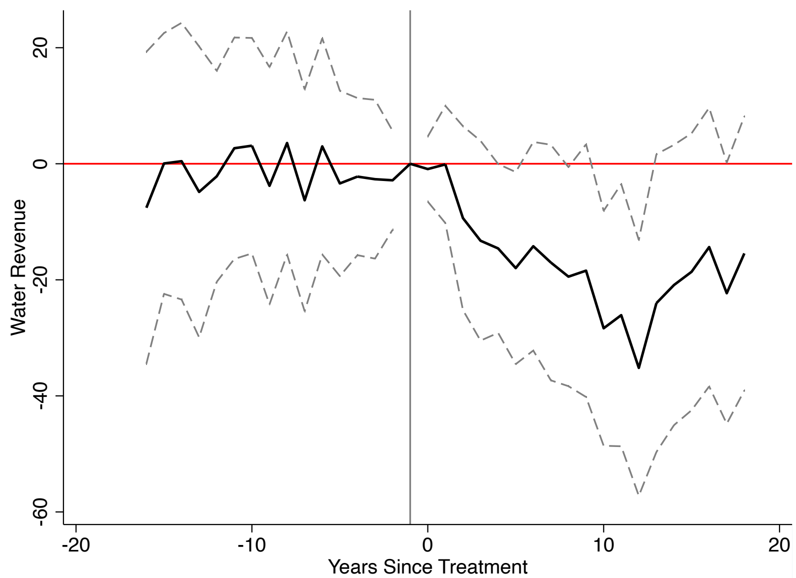


Figure B6: The Effect of Grants on Water Revenue Flattens Out

Notes: This figure re-estimates the results in Figure 7 on municipalities treated before 1982.

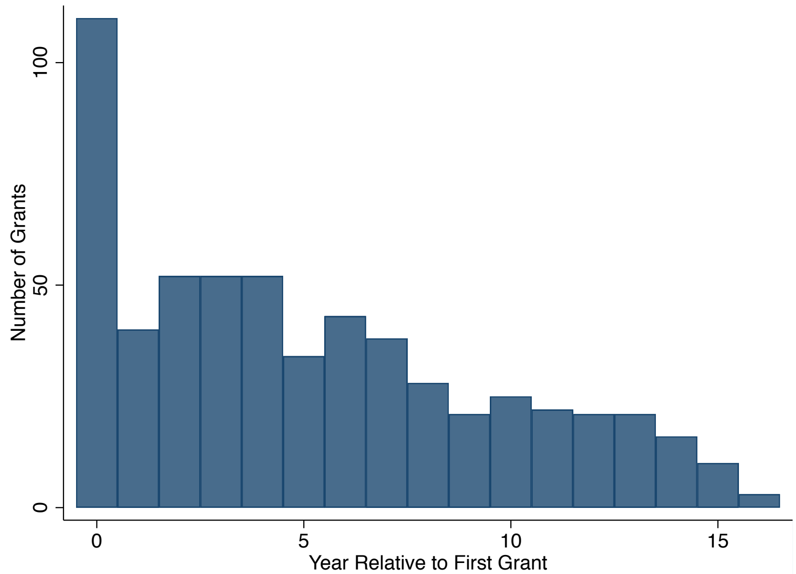


Figure B7: Distribution of Grants Over Time

Notes: This figure shows the number of grants that municipalities received each year relative to the year of the municipality's first grant.

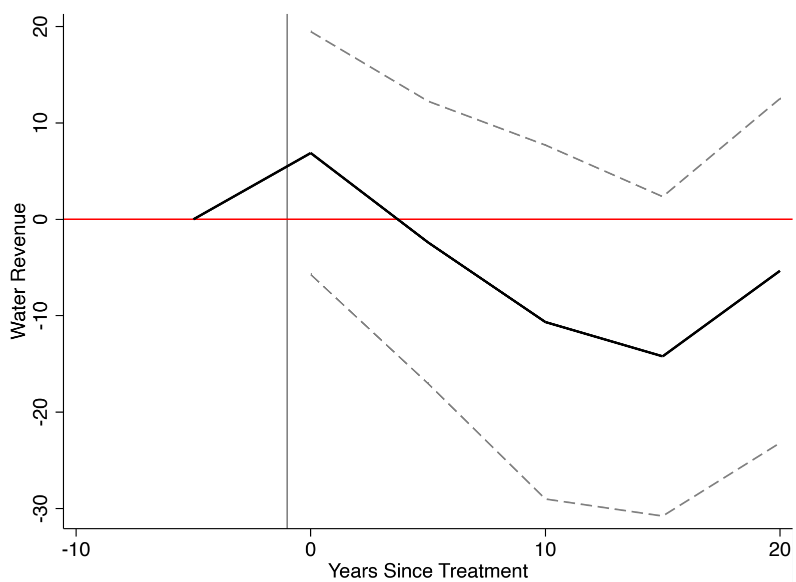
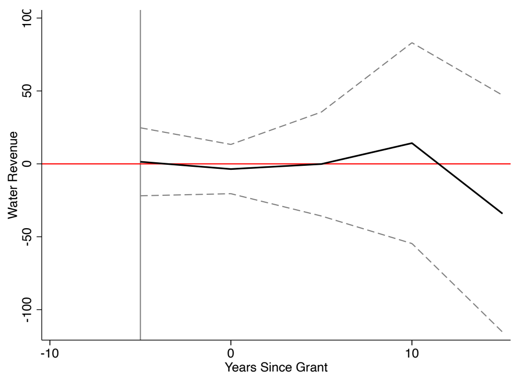
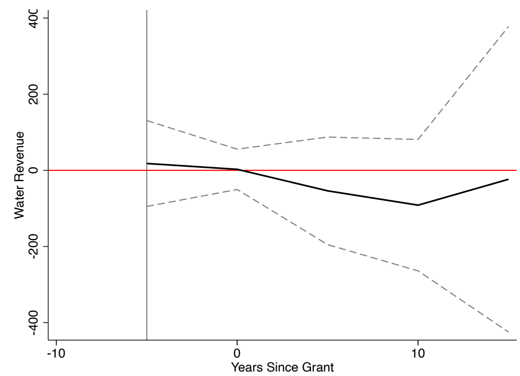


Figure B8: Water Revenue Event Study with Alternative Data

Notes: This figure re-estimates the results from Figure 7 on an alternative dataset.



(a) Non-Compliant



(b) Compliant

Figure B9: Callaway and Sant'Anna (2020a) Water Revenue Event Study with Alternative Data

Notes: This figure re-estimates the results from Figure B4 on an alternative dataset.

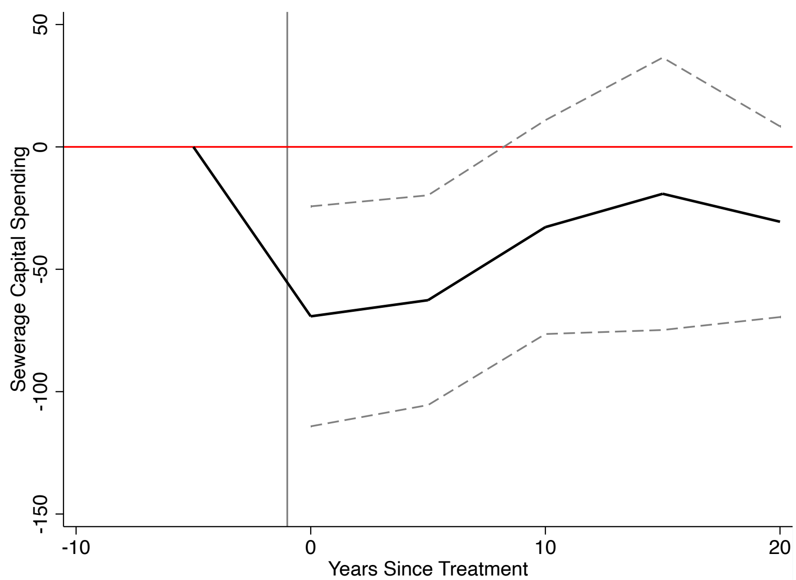
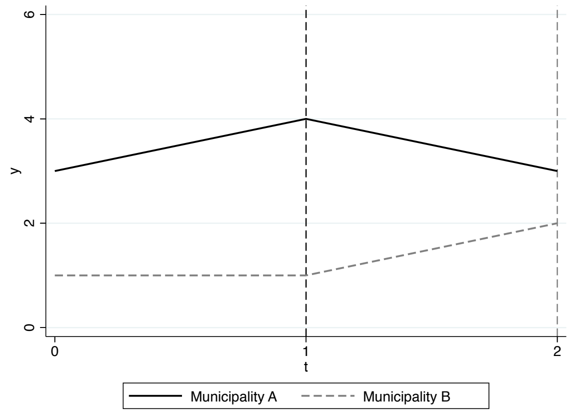
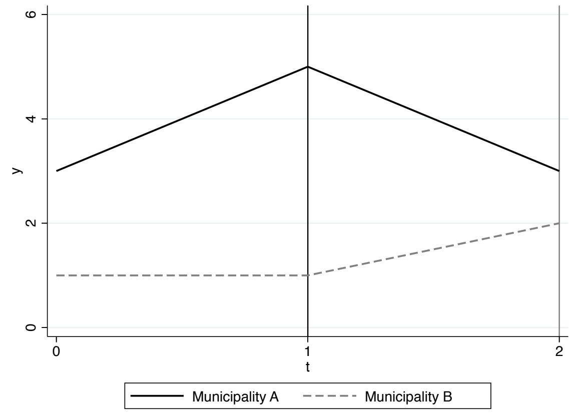


Figure B10: Sewerage Capital Event Study with Alternative Data

Notes: This figure re-estimates the results from Figure B8 with sewerage capital spending as the dependant variable.



(a) Binary Treatment



(b) Continuous Treatment

Figure C1: Example of Problems with TWFE

Notes: This figure shows two hypothetical situations where two municipalities are treated at different times. Municipality A is treated in period 1 and Municipality B is treated in period 2.

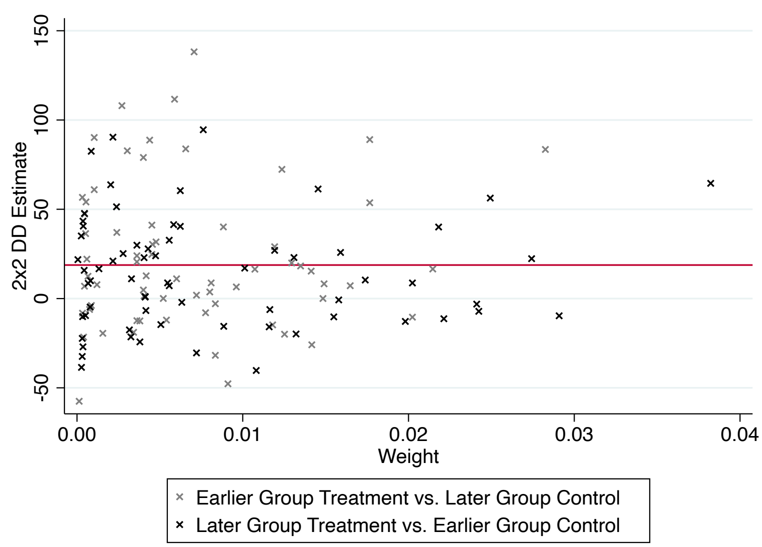


Figure C2: Bacon Decomposition of Sewerage Capital Spending TWFE Estimate

Notes: This figure presents a scatterplot of all possible two-group/two period DD estimators and their associated weights. These estimates and weights come from a decomposition of β_{rf}^{TWFE} from $C_{it} = \alpha_0 + \beta_{rf}^{TWFE} D_{it} + \alpha_i + \alpha_t + \epsilon_{it}$ made using Goodman-Bacon et al. (2019). C_{it} is per capita sewerage capital expenditure, D_{it} is a dummy variable that equals one after grant receipt and α_i and α_t are municipality and year fixed effects.

Authority and Facility No. ▶ __ __ - __ __ __ __ - __ __ __

Section III – CATEGORY I – ASSESSMENT OF NEEDS BY TYPE AND COST

ASSESSMENT OF NEEDS TO ACHIEVE REQUIRED LEVEL OF SECONDARY TREATMENT	*Mark appropriate box for each item*	**EPA USE ONLY**
21a. Can this plant meet water quality standards applicable to the stream segment to which it discharges by a level of treatment LESS than defined as secondary treatment by EPA?	1 ☐ Yes 2 ☐ No 3 ☐ Not known 4 ☐ Not applicable; no discharge to waters	
b. What level of secondary treatment must the discharge from this plant meet by July 1, 1977?	1 ☐ Secondary treatment level as defined by EPA, **OR** 2 ☐ Higher level of secondary treatment required by State – *Specify* Higher level secondary treatment Nature of State action	
c. Does the discharge from this plant **NOW** meet the level of secondary treatment identified in 21b?	1 ☐ Yes 2 ☐ No	
d. Will the discharge from this plant meet on July 1, 1977, the level of secondary treatment identified in 21b? *(Give consideration to changes in flow and concentration of influents and to the changes in treatment capability now under construction or provided for in approved grants.)*	1 ☐ Yes – *SKIP to item 25* 2 ☐ No	
e. Which approaches will be used to enable this existing or proposed plant to meet the secondary treatment level identified in 21b? (1) Addition of land disposal as a means of treatment	1 ☐ Yes 2 ☐ No	
(2) New plant – no replacement of existing plant	1 ☐ Yes 2 ☐ No	
(3) Replacement plant	1 ☐ Yes 2 ☐ No	
(4) Modification – no change in capacity or treatment level	1 ☐ Yes 2 ☐ No	
(5) Modification – increase in capacity	1 ☐ Yes 2 ☐ No	
(6) Modification – increase in treatment level	1 ☐ Yes 2 ☐ No	
(7) Improved operation and maintenance, increase staffing	1 ☐ Yes 2 ☐ No	
(8) Reduce infiltration	1 ☐ Yes 2 ☐ No	

Page 15

Figure D1: CWNS Compliance Question

Notes: This figure reproduces the question from the 1972 CWNS that we use to define compliance.

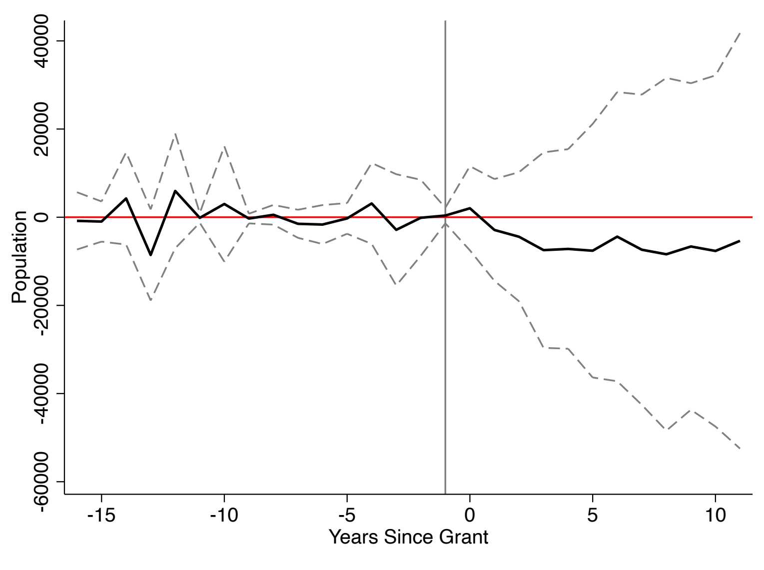


Figure D2: Population Did Not Change After Grant Receipt

Notes: This figure shows the reduced form relationship between grant receipt and population by presenting the $\widehat{\theta}_D(e)$ for each year e relative to treatment, where $\widehat{\theta}_D(e) = \sum_{g=1972}^{1985} \sum_{t=1957}^{1987} 1\{t-g=e\}\widehat{ATT(g,t)}P(G = g|t-g=e)$. g indexes timing groups and t indexes years. $\widehat{ATT(g,t)}$ is the difference between changes in population in units in group g in periods t and $g-1$ and changes in population in units not yet treated at time t in periods t and $g-1$.

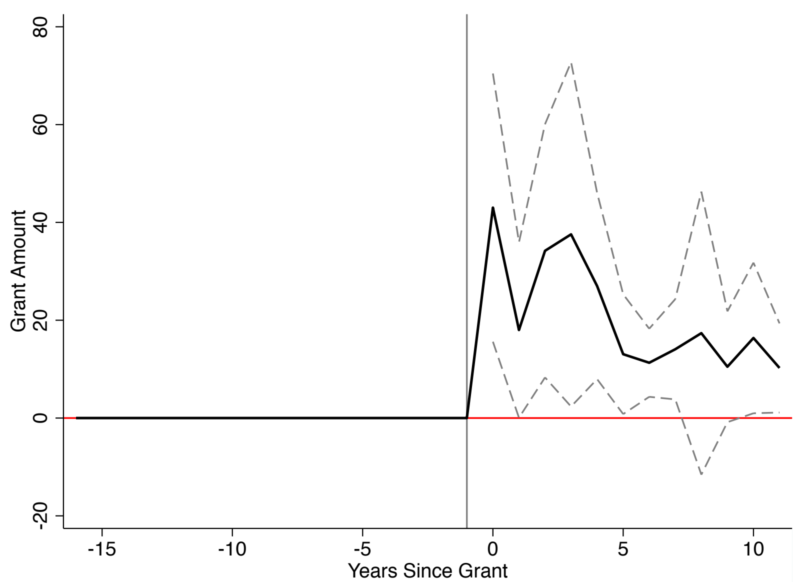


Figure D3: Grant Amount Event Study (Nominal Dollars)

Notes: This figure re-creates Figure 3 measuring grant amount in nominal dollars.

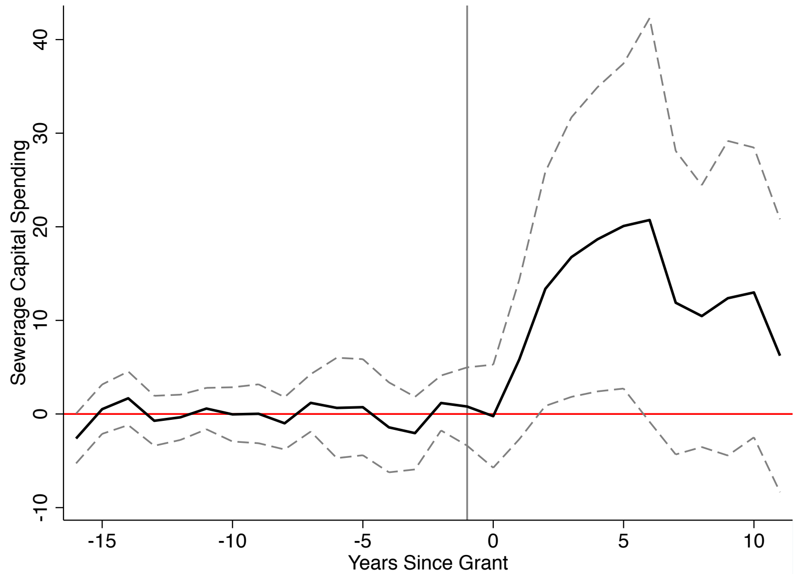


Figure D4: Sewerage Capital Spending Event Study (Nominal Dollars)

Notes: This figure re-creates Figure 4 measuring sewerage capital spending in nominal dollars

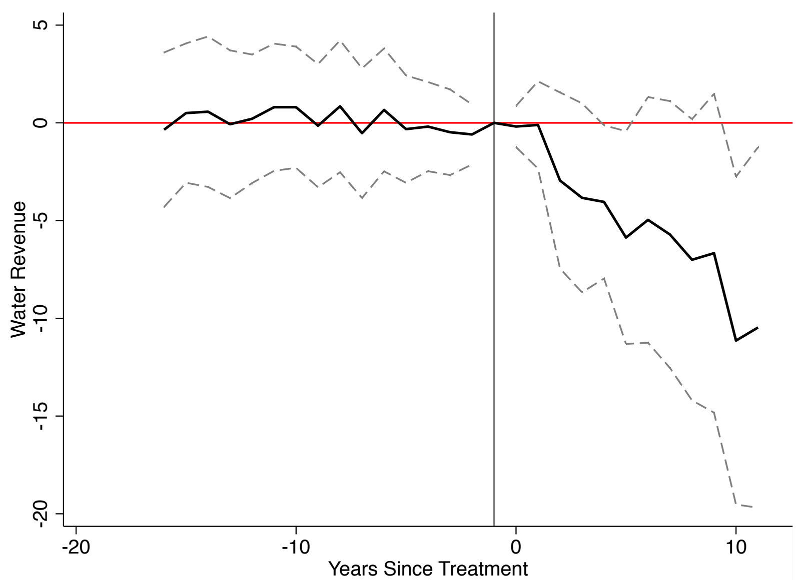


Figure D5: Water Revenue Event Study (Nominal Dollars)

Notes: This figure re-creates Figure 7 measuring water revenue in nominal dollars.

Table A1: 1970 Summary Statistics by Facility Compliance and Grant Size

	(1) Compliant	(2) NC & < $730	(3) NC & >= $730
Population	115,501	504,942	278,022
	(73,126)	(1264899)	(388,540)
Total Revenue	1475.74	1773.20	1651.92
	(805.53)	(1035.63)	(742.44)
Total IGR	308.08	392.16	357.08
	(366.90)	(441.76)	(378.31)
Federal IGR	66.63	67.91	58.16
	(91.20)	(69.69)	(65.29)
Nonwater Utility Rev	123.50	156.46	50.48
	(261.15)	(314.10)	(180.49)
Property Tax Revenue	479.29	589.57	555.04
	(424.10)	(513.41)	(401.37)
Water Utility Revenue	134.65	130.19	137.25
	(53.04)	(48.43)	(52.03)
Total Expenditure	1616.44	1863.64	1723.32
	(901.08)	(1092.85)	(825.31)
Total IG Exp	11.24	24.90	28.09
	(26.48)	(51.04)	(65.73)
Capital Outlays	462.61	399.30	342.99
	(334.21)	(221.95)	(215.51)
Highway Exp	123.37	113.85	143.38
	(48.83)	(52.75)	(53.47)
Sewerage Capital Outlay	52.10	43.52	27.74
	(56.01)	(45.30)	(30.78)
Sewerage Non-capital Exp	32.09	27.89	31.02
	(18.23)	(18.42)	(17.87)
Nonwater Utiltiy Exp	130.94	155.10	60.07
	(287.14)	(303.38)	(229.11)
Water IG Exp	11.57	15.20	12.53
	(11.12)	(16.90)	(12.91)
Water Non-capital Exp	77.78	69.89	80.46
	(27.64)	(38.94)	(28.06)
Water Capital Outlay	78.43	58.29	38.43
	(110.83)	(82.49)	(43.73)
Observations	22	47	43

Notes: This table presents summary statistics for municipalities with compliant facilities, non-compliant facilities that received small grants, and non-complaint facilities that received large grants in 1970. All covariates aside from the facility compliance dummy and population represent per capita values.

Table A2: Observable Characteristics Do Not Predict Grant Size or Compliance

	(1) Grant >= 730	(2) Compliant
ln Population	0.0315	-0.0662*
	(0.0595)	(0.0370)
Total Revenue	-0.0000335	-0.000474
	(0.000507)	(0.000330)
Total IGR	0.000372	-0.0000244
	(0.000344)	(0.000240)
Federal IGR	-0.0000125	0.0000617
	(0.000486)	(0.000332)
Nonwater Utility Rev	-0.000561	-0.000654
	(0.000509)	(0.000406)
Property Tax Revenue	-0.000170	0.000130
	(0.000202)	(0.000154)
Total Expenditure	0.000292	0.000277
	(0.000542)	(0.000361)
Total IG Exp	0.000804	-0.000598
	(0.000783)	(0.000615)
Capital Outlays	-0.000634	0.000163
	(0.000446)	(0.000355)
Highway Exp	0.00105	0.000304
	(0.000943)	(0.000733)
Sewerage Capital Outlay	0.000293	-0.000504
	(0.000854)	(0.000602)
Sewerage Non-capital Exp	-0.000716	0.00123
	(0.00263)	(0.00228)
Nonwater Utiltiy Exp	0.0000405	0.000858*
	(0.000493)	(0.000473)
Water IG Exp	-0.00156	-0.00322
	(0.00440)	(0.00394)
Water Non-capital Exp	0.00415	-0.000646
	(0.00279)	(0.00165)
Water Capital Outlay	0.000542	0.00139
	(0.000954)	(0.000978)
Water Utility Revenue	-0.000993	-0.000266
	(0.00170)	(0.00134)
Observations	112	112

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents results from regressing dummies indicating municipalities that received grants totaling more than $730 per capita (in column 1) and compliant municipalities (in column 2) on potential predictive covariates from 1971. All covariates aside from the natural log of population represent per capita 2020 values.

Table A3: Event Study Coefficients

	Grant Amount	Sewerage Capital
e = -16	0.0000	-23.6529
		(7.5663)
e = -15	0.0000	4.6855
		(7.2978)
e = -14	0.0000	14.9915
		(8.7752)
e = -13	0.0000	-6.5164
		(6.5003)
e = -12	0.0000	-2.8340
		(6.9121)
e = -11	0.0000	5.3855
		(6.3477)
e = -10	0.0000	-0.9785
		(7.9286)
e = -9	0.0000	0.4192
		(8.5758)
e = -8	0.0000	-8.6760
		(6.4669)
e = -7	0.0000	7.4720
		(6.2840)
e = -6	0.0000	3.3479
		(10.4214)
e = -5	0.0000	8.4300
		(12.3644)
e = -4	0.0000	-10.3522
		(10.1364)
e = -3	0.0000	-12.3568
		(7.0395)
e = -2	0.0000	6.9730
		(5.6019)
e = -1	0.0000	3.9491
		(7.2852)
e = 0	232.1391	-1.2140
	(52.2567)	(8.0241)
e = 1	89.6106	25.3382
	(33.8825)	(11.8245)
e = 2	142.5056	60.1366
	(37.4014)	(18.8165)
e = 3	159.7510	73.3865
	(57.0400)	(22.9644)
e = 4	103.0060	72.8221
	(26.4315)	(22.6019)

e = 5	45.8575	69.4205
	(14.7120)	(21.2404)
e = 6	35.4843	61.0448
	(7.7271)	(23.8857)
e = 7	40.1637	26.2520
	(10.3128)	(19.7588)
e = 8	45.6045	19.6322
	(27.6117)	(15.9867)
e = 9	26.5908	18.3762
	(10.9811)	(16.0120)
e = 10	40.2673	19.0513
	(14.1382)	(16.8738)
e = 11	25.6392	1.9856
	(8.4919)	(15.2403)

Bootstrap standard errors in parentheses, clustered at municipality level

Notes: This table presents the $\widehat{\theta}_D(e)$ from Figures 3 and 4.

Table A4: Pass-Through Results are Robust to Different Cutoffs

	(1)	(2)	(3)
Panel A: Non-Compliant and < \$805	Grant Amount	Sewerage Capital	
Treat * Post	43.12***	28.96	
	(4.524)	(44.574)	
Pass-through			.904
			(1.047)
p-value: Pass-through = 0			0.388
p-value: Pass-through = 1			0.924
Observations	2254	2254	2254
Panel B: Non-Compliant and >= \$805			
Treat * Post	278.69***	76.04	
	(37.374)	(40.954)	
Pass-through			0.273
			(0.152)
p-value: Pass-through = 0			0.073
p-value: Pass-through = 1			0.000
Observations	1426	1426	1426

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panels B and C of Table 4 using a different cutoff. Panel A summarizes pass-through for stacks defined by non-compliant municipalities who received grants totalling less than \$805, and Panel B summarizes pass-through for all other stacks defined by non-compliant municipalities.

Table A5: Compliant Pass-Through is Low for Large and Small Grants

	(1)	(2)	(3)
Panel A: Compliant and < \$730	Grant Amount	Sewerage Capital	
Treat * Post	39.58***	-15.66	
	(11.090)	(9.544)	
Pass-through			-0.395
			(0.275)
p-value: Pass-through = 0			0.151
p-value: Pass-through = 1			0.000
Observations	598	598	598
Panel B: Compliant and >= \$730			
Treat * Post	200.21***	38.29	
	(23.144)	(53.359)	
Pass-through			0.191
			(0.277)
p-value: Pass-through = 0			0.491
p-value: Pass-through = 1			0.004
Observations	322	322	322

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panels B and C of Table 4 on compliant municipalities.

Table A6: Pass-Through by Population Tercile

	(1)	(2)	(3)
Panel A: Tercile 1	Grant Amount	Sewerage Capital	
Treat * Post	139.70***	49.85*	
	(45.101)	(428.370)	
Pass-through			0.357
			(0.240)
p-value: Pass-through = 0			0.137
p-value: Pass-through = 1			0.007
Observations	1584	1584	1584
Panel B: Tercile 2			
Treat * Post	105.39***	63.71***	
	(18.162)	(22.529)	
Pass-through			0.605**
			(0.279)
p-value: Pass-through = 0			0.030
p-value: Pass-through = 1			0.156
Observations	1628	1628	1628
Panel C: Tercile 3			
Treat * Post	119.98***	31.39**	
	(27.519)	(12.933)	
Pass-through			0.262
			(0.169)
p-value: Pass-through = 0			0.119
p-value: Pass-through = 1			0.000
Observations	1628	1628	1628

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results from Panel B of Table 2 by terciles of population. Tercile 1 has the lowest population in our sample, and Tercile 3 has the highest.

Table A7: Re-weight

	(1) Non-Compliant	(2) Compliant	(3) Full Sample
Pass-Through	0.397	0.0337	0.353
	(0.310)	(0.363)	(0.278)
p value: Pass-through = 0	0.200	0.926	0.204
p value: Pass-through = 1	0.052	0.008	0.020
Observations	3680	920	4600

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table approximates pass-through of all CWA grants by weighting together pass-through estimates for compliant and non-compliant municipalities. Column 1 estimates $C_{it} = \alpha_0 + \beta D_{it} + \alpha_{is} + \alpha_{ts} + \epsilon_{its}$ on stacks defined by non-compliant municipalities. D_{it} is a dummy variable that equals one after grant receipt, we use D_{it} as a instrument for grant amount. Column 2 repeats this process for compliant municipalities. Column 3 presents a weighted average of these two coefficients where compliant municipalities represent 12 percent of the estimate and non-municipalities municipalities represent the other 88 percent.

Table A8: Selection on Gains

	(1) 1972-1974	(2) 1975-1977	(3) 1978-1985	(4) Full Sample	(5) Full Sample
	Sewerage Capital Spending				Grant Amount
Treat * Post	37.08	35.74	24.74	34.18***	73.55***
				(11.882)	(9.753)
Observations	3630	1656	495	3630	3630

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of $\theta_S = \sum_{g=1972}^{1985} \tilde{\theta}(g) P(G = g)$ where $\tilde{\theta}(g) = \frac{1}{1985-g+1} \sum_{t=1972}^{1985} 1\{g \leqslant t\} ATT(g,t)$. Column 1 presents the average of the $\tilde{\theta}(g)$ for municipalities treated in 1972-1974. Column 2 presents the average of the $\tilde{\theta}(g)$ for municipalities treated in 1975-1977, and column 3 presents the average of the $\tilde{\theta}(g)$ for municipalities treated in 1978-1981. Column 4 estimates θ_S for the entire sample and column 5 estimates the associated first stage.

Table A9: Full Sample Results (Adjusted for Local Matching)

	Grant Amount	Sewerage Capital	
Treat * Post	155.48***	51.56***	
	(22.366)	(14.185)	
Pass-through			0.331***
			(0.106)
p-value: Pass-through = 0			0.002
p-value: Pass-through = 1			0.000
Observations	3630	3630	3630

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panel B of Table 2 after adjusting grants for local matching.

Table A10: Grant Pass-Through for Sub-Groups (Adjusted for Local Matching))

	(1)	(2)	(3)
Panel A: Non-Compliant and < $730	Grant Amount	Sewerage Capital	
Treat * Post	54.29***	42.44	
	(5.716)	(42.004)	
Pass-through			0.782
			(0.853)
p-value: Pass-through = 0			0.359
p-value: Pass-through = 1			0.798
Observations	2162	2162	2162
Panel B:Non-Compliant and >= $730			
Treat * Post	360.55***	68.85	
	(48.283)	(41.913)	
Pass-through			0.191
			(0.120)
p-value: Pass-through = 0			0.112
p-value: Pass-through = 1			0.000
Observations	1518	1518	1518
Panel C: Compliant			
Treat * Post	129.45***	3.22	
	(32.762)	(28.258)	
Pass-through			0.025
			(0.286)
p-value: Pass-through = 0			0.930
p-value: Pass-through = 1			0.001
Observations	920	920	920

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Table 4 after adjusting grants for local matching.

Table A11: Average Pass-Through

	(1) Grant Amount	(2) Sewerage Capital	(3)
Treat * Post	126.68***	43.31	
	(20.356)	(34.562)	
Average Pass-through			0.212
			(1.070)
p-value: Pass-through = 0			0.843
p-value: Pass-through = 1			0.462
Observations	4600	4600	4600

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table estimates $Y_{it} = \alpha_0 + \beta D_{it} + \alpha_{is} + \alpha_{is} + \epsilon_{its}$ on a pooled sample of all stacks. Grant amount is the dependent variable in column 1, and sewerage capital spending is the dependent variable in columns 2 and 3. Column 3 shows the average pass-through rate obtained from dividing the each stack's reduced form by its first stage.

Table A12: Full Sample Results (Never-Treated)

	Grant Amount	Sewerage Capital	
Treat * Post	119.98***	30.75***	
	(14.401)	(10.389)	
Pass-through			0.256***
			(0.079)
p-value: Pass-through = 0			0.001
p-value: Pass-through = 1			0.000
Observations	8492	8492	8492

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panel B of Table 2 using never-treated units as a control group

Table A13: Grant Pass-Through for Sub-Groups (Never-Treated)

	(1)	(2)	(3)
Panel A: Non-Compliant and < $730	Grant Amount	Sewerage Capital	
Treat * Post	20.59***	19.51***	
	(1.343)	(6.339)	
Pass-through			0.947***
			(0.310)
p-value: Pass-through = 0			0.002
p-value: Pass-through = 1			0.865
Observations	4494	4494	4494
Panel B:Non-Compliant and >= $730			
Treat * Post	132.44***	47.40***	
	(10.240)	(8.270)	
Pass-through			0.358***
			(0.058)
p-value: Pass-through = 0			0.000
p-value: Pass-through = 1			0.000
Observations	3940	3940	3940
Panel C: Compliant			
Treat * Post	68.12***	7.15	
	(9.313)	(10.221)	
Pass-through			0.105
			(0.160)
p-value: Pass-through = 0			0.512
p-value: Pass-through = 1			0.000
Observations	3476	3476	3476

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Table 4 using never-treated units as a control group.

Table A14: Observable Characteristics Do Not Predict Timing of Grant Receipt

	(1)	(2)	(3)
Baseline Year:	1971	1974	1977
Compliant Facility	0.408	1.211	0.544
	(0.777)	(1.804)	(2.901)
ln Population	-0.823**	0.0214	0.104
	(0.342)	(0.395)	(1.501)
Total Revenue	-0.00140	0.00223	0.00527
	(0.00318)	(0.00229)	(0.00578)
Total IGR	-0.00108	0.000766	0.00168
	(0.00180)	(0.00239)	(0.00669)
Federal IGR	0.000826	-0.00242	0.00284
	(0.00203)	(0.00277)	(0.0112)
Nonwater Utility Rev	0.00696	0.00200	0.000278
	(0.00492)	(0.00428)	(0.0147)
Property Tax Revenue	0.00141	0.00509	-0.00158
	(0.00137)	(0.00389)	(0.00605)
Total Expenditure	0.00150	-0.00198	-0.0108
	(0.00303)	(0.00219)	(0.00731)
Total IG Exp	-0.00319	-0.0187*	0.0154
	(0.00520)	(0.00950)	(0.0205)
Capital Outlays	0.00170	0.000438	0.0101
	(0.00249)	(0.00211)	(0.0117)
Highway Exp	-0.00242	0.00130	-0.0142
	(0.00495)	(0.0116)	(0.0224)
Sewerage Capital Outlay	-0.00206	0.00474	-0.00791
	(0.00400)	(0.00411)	(0.0156)
Sewerage Non-capital Exp	0.00221	-0.00196	-0.00621
	(0.0172)	(0.0301)	(0.0272)
Nonwater Utiltiy Exp	-0.00616	-0.000942	0.00362
	(0.00453)	(0.00356)	(0.0137)
Water IG Exp	0.0273	-0.0338	-0.155
	(0.0188)	(0.0348)	(0.197)
Water Non-capital Exp	-0.00635	-0.00696	0.0300
	(0.0130)	(0.0157)	(0.0572)
Water Capital Outlay	-0.0153***	-0.00332	0.00663
	(0.00577)	(0.00619)	(0.0297)
Water Utility Revenue	-0.00536	0.00817	0.00609
	(0.00656)	(0.00962)	(0.0521)
p value	0	0	.979
Observations	112	52	27

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents results from regressing the year a municipality receives its first CWA grant on potential predictive covariates. Column 1 estimates $GrantYear_i = \alpha_0 + compliant_i + X_{ip} + \epsilon_{ip}$ for all treated municipalities using covariate data from 1971. Columns 2 and 3 use data from 1974 and 1977, respectively, and limit the data to municipalities that do not receive a grant until after those years. All covariates aside from the facility compliance dummy and natural log of population represent per capita values. We present p values from a test of joint significance of all variables at the bottom of the table.

Table A15: Full Sample Stacked Pass-Through Estimates

	(1) Grant Amount	(2) Sewerage Capital	(3)
Treat * Post	126.68***	43.31	
	(20.356)	(34.562)	
Pass-through			0.342
			(0.267)
p-value: Pass-through = 0			0.200
p-value: Pass-through = 1			0.014
Observations	4600	4600	4600

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table estimates $Y_{it} = \alpha_0 + \beta D_{it} + \alpha_{is} + \alpha_{is} + \epsilon_{its}$ on a pooled sample of all stacks. Grant amount is the dependent variable in column 1, and sewerage capital spending is the dependent variable in columns 2 and 3. Column 3 shows the implied pass-through rate from dividing the first stage in column 1 and the reduced form in column 2.

Table A16: Grant Pass-Through for Sub-Groups (Dynamic CS Estimates)

	(1)	(2)	(3)
Panel A: Non-Compliant and < $730	Grant Amount	Sewerage Capital	
Treat * Post	28.10***	34.824*	
	(4.029)	(17.686)	
Pass-through			1.239
			(0.774)
p-value: Pass-through = 0			.110
p-value: Pass-through = 1			.758
Observations	1410	1410	1410
Panel B: Non-Compliant and >= $730			
Treat * Post	309.39***	53.94***	
	(80.028)	(19.420)	
Pass-through			0.174*
			(0.092)
p-value: Pass-through = 0			.059
p-value: Pass-through = 1			0.000
Observations	1353	1353	1353
Panel C: Compliant			
Treat * Post	94.27***	45.82	
	(19.507)	(36.060)	
Pass-through			0.486
			(0.415)
p-value: Pass-through = 0			.242
p-value: Pass-through = 1			0.215
Observations	726	726	726

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panel B of Table 2 on sub-samples of municipalities. Panel A summarizes pass through for non-compliant municipalities that received grants totaling less than $730, Panel B for all other non-compliant municipalities, and Panel C for all compliant municipalities.

Table A17: Pre-CWA Sewerage Capital Spending

	(1) Full Sample	(2) Small Non-compliant	(3) Large Non-compliant	(4) Compliant
Total Sewerage Capital Spending	14,439,038	19,880,522	13,283,985	4,0966,648
Observations	1760	752	656	352

Notes: This table shows average total sewerage capital spending from 1955 to 1971 for the full sample of municipalities, non-compliant municipalities that received grants totaling less than $730 per capita, non-compliant municipalities that received grants totaling more than $730 per capita, and compliant municipalities.

Table B1: Water Revenue Only Decreased in Compliant Municipalities

	(1) Compliant	(2) Non-Compliant
Panel A: Simple Aggregation	Water Revenue	
Treat * Post	-51.45***	3.81
	(6.583)	(5.863)
Observations	726	2904
Panel B: Dynamic Aggregation		
Treat * Post	-34.24***	2.059
	(6.859)	(5.789)
Observations	726	2904

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents estimates of the effect of grant receipt on water revenue. Panel A presents averages of group-time treatment effects based on group size calculated with the following equation $\frac{1}{\kappa}\sum_{g=1972}^{1985}\sum_{t=1972}^{1987} 1\{g \leqslant t\}\widehat{ATT(g,t)}P(G = g)$ where $\kappa = \sum_{g=1972}^{1985}\sum_{t=1972}^{1987} 1\{g \leqslant t\}P(G = g)$. Panel B presents averages of the $\widehat{\theta}_D(e)$ for each time relative to treatment e for $e \geqslant 0$ calculated with $\widehat{\theta}_D(e) = \sum_{g=1972}^{1985}\sum_{t=1957}^{1987} 1\{t-g=e\}\widehat{ATT(g,t)}P(G = g|t-g=e)$. Column 1 presents results for compliant municipalities with not-yet-treated compliant municipalities acting as controls, and column 2 presents results for non-compliant municipalities with not-yet-treated non-compliant municipalities acting as controls.

Table B2: Municipalities Raised Water Bills When Grants Were Too Small

	(1) Less than Costs	(2) Greater than Costs
	Water Revenue	
Treat * Post	6.878	-1.173
	(10.422)	(4.377)
Observations	960	825

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table re-estimates the results in column 2 of Table B1 on sub-samples of municipalities where grants were larger or smaller than estimated costs. Column 1 shows results for non-compliant municipalities where costs were greater than grant amount, and column 2 shows results for non-compliant municipalities where costs were less than grant amount.

Table B3: Water Revenue Estimates with Alternative Data

	(1) Water Revenue	(2) Water Revenue	(3) Water Revenue
Grant X Compliant	-14.57*	-14.51*	-13.50*
	(7.443)	(7.510)	(6.934)
Timing Group X Year FE	X	X	
Region X Year FE		X	
Observations	6412	6412	6412

Standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Table 9 on an alternative dataset. Column 1 presents estimates of θ from $R_{it} = \alpha_0 + \theta grant_{it} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it}$. Column 2 re-estimates the result in column 1 with region-by-year fixed effects. Column 3 re-estimates the result in column 1 with year fixed effects instead of timing-group-by-year fixed effects.

Table B4: CS Water Revenue Estimates with Alternative Data

	(1) Compliant	(2) Non-Compliant
Panel A: Simple Aggregation	Water Revenue	
Treat * Post	-42.04	2.132
	(32.338)	(15.453)
Observations	1881	6363
Panel B: Dynamic Aggregation		
Treat * Post	-41.52	-5.879
	(53.587)	(16.815)
Observations	1881	6363

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table re-estimates the results in Table B1 on an alternative dataset.

Table B5: Sewerage Capital Estimates with Alternative Data

	(1) Sewerage Capital	(2) Sewerage Capital	(3) Sewerage Capital
Grant X Compliant	-34.78**	-34.37**	-19.96
	(15.14)	(15.06)	(13.27)
Timing Group X Year FE	X	X	
Region X Year FE		X	
Observations	6412	6412	6412

Standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Table B3 with sewerage capital spending as the dependent variable.

Table B6: How Much Crowd-Out can Redistribution Account For?

	(1) Sewerage Capital	(2) Water Revenue	(3) Water Revenue
Grant X Compliant	-27.90*	-13.55**	
	(14.28)	(6.743)	
Sewerage Capital			0.485
			(0.358)
Grant X Compliant X e	-2.982**	-2.122***	
	(1.428)	(0.804)	
Sewerage Capital			0.712
			(0.433)
Observations	4752	4752	4752

Standard errors in parentheses, clustered at the municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: Panel A presents estimates of θ from $Y_{it} = \alpha_0 + \theta grant_{it} * compliant_i + \alpha_i + \alpha_{gt} + \epsilon_{it}$. Panel B presents estimates of θ from $Y_{it} = \alpha_0 + \theta grant_{it} * compliant_i * e_{it} + \alpha_i + \alpha_{gt} + \epsilon_{it}$ where e_{it} indicates year relative to treatment. The dependent variable is sewerage capital spending in column 1 and water revenue in columns 2 and 3. In column 3, we use $grant_{it} * compliant_i$ as an instrument for sewerage capital spending

Table C1: Bacon Decomposition of Sewerage Capital Spending TWFE Estimate

	(1) Weight	(2) Average Estimate
Newly Treated vs Not-Yet-Treated	0.454	23.734
Newly Treated vs Already-Treated	0.546	14.643

Notes: This table presents the average difference-in-difference estimates and their weights from decomposing β_{rf}^{TWFE} from $C_{it} = \alpha_0 + \beta_{rf}^{TWFE} D_{it} + \alpha_i + \alpha_t + \epsilon_{it}$ using Goodman-Bacon et al. (2019).

Table C2: Dose-Response TWFE Estimates

	(1) Small Non-Compliant	(2) Large Non-Compliant	(3) Compliant
	Cumulative Sewerage Capital Spending		
Cumulative Grant Dollars	1.045**	0.133	0.410*
	(0.517)	(0.100)	(0.214)
p value: Pass-through = 0	0.043	0.184	0.056
p value: Pass-through = 1	0.931	0.000	0.059
Observations	2068	1804	968

Standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of $C_{it} = C_{it} = \alpha_0 + \beta_{dr}^{TWFE} g_{it} + \alpha_i + \alpha_t + \epsilon_{it}$ on sub-samples of municipalities. C_{it} is per capita sewerage capital expenditure, g_{it} is per capita grant amount, and α_i and α_t are municipality and year fixed effects. Column 1 summarizes pass through for non-compliant municipalities that received grants totaling less than $730, column 2 for all other non-compliant municipalities, and column 3 for all compliant municipalities.

Table D1: Definition of Compliance

	EPA Standard Only	Higher Standard	Full Sample
At or Above Standard	5,872	1,113	6,985
Below Standard	4,613	3,334	7,947
Total	10,485	4,447	14,932

Notes: This table presents cross-tabs of facilities answers to questions 21b and 21c on the 1972 CWNS questionnaire.

Table D2: Stacked Difference-in-Difference Pass-Through Estimates (EPA Mandate Only)

	(1)	(2)	(3)
Panel A: Non-Compliant and < $730	Grant Amount	Sewerage Capital	
Treat * Post	48.16***	54.27	
	(5.866)	(44.248)	
Pass-through			1.176
			(0.997)
p-value: Pass-through = 0			0.238
p-value: Pass-through = 1			0.865
Observations	1656	1656	1656
Panel B: Non-Compliant and >= $730			
Treat * Post	330.60***	116.30**	
	(68.390)	(45.004)	
Pass-through			0.352**
			(0.145)
p-value: Pass-through = 0			0.015
p-value: Pass-through = 1			0.000
Observations	1150	1150	1150
Panel C: Non-Compliant and < $975			
Treat * Post	51.35***	54.86	
	(6.626)	(44.380)	
Pass-through			1.068
			(0.971)
p-value: Pass-through = 0			0.272
p-value: Pass-through = 1			0.936
Observations	1932	1932	1932
Panel D: Non-Compliant and >= $975			
Treat * Post	359.18***	123.49***	
	(67.352)	(46.719)	
Pass-through			0.344***
			(0.141)
p-value: Pass-through = 0			0.015
p-value: Pass-through = 1			0.000
Observations	874	874	874

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panels B and C of Table 4 on the sample of municipalities that did not have to satisfy state wastewater capital requirements greater than the EPA's secondary treatment requirement. Panel A summarizes pass-through for stacks defined by non-compliant municipalities who received grants totalling less than $730, and Panel B summarizes pass-through for all other stacks defined by non-compliant municipalities. Panel C summarizes pass-through for stacks defined by non-compliant municipalities that received grants totalling less than $975, and Panel D repeats this for all other stacks defined by non-compliant municipalities

Table D3: Alternative Estimates of Break in Cost (EPA Mandate Only)

	(1)
	Total Cost
Split	975.2
	(1385.7)
Observations	29

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table re-estimates the results in Table 3 on a sample of non-compliant municipalities that did not face state level treatment technology requirements greater than the EPA's secondary treatment standard.

Table D4: Full Sample Results (Nominal)

	Grant Amount	Sewerage Capital	
Treat * Post	26.31***	13.60***	
	(3.589)	(3.200)	
Pass-through			0.517***
			(0.126)
p-value: Pass-through = 0			0.000
p-value: Pass-through = 1			0.000
Observations	3630	3630	3630

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Panel B of Table 2 measuring spending and grant amount in nominal terms.

Table D5: Grant Pass-Through for Sub-Groups (Nominal)

	(1)	(2)	(3)
Panel A: Non-Compliant and < \$730 (2020\$)	Grant Amount	Sewerage Capital	
Treat * Post	6.72***	9.37	
	(0.747)	(7.740)	
Pass-through			1.39
			(1.21)
p-value: Pass-through = 0			0.251
p-value: Pass-through = 1			0.745
Observations	2162	2162	2162
Panel B:Non-Compliant and >= \$730 (2020\$)			
Treat * Post	50.75***	18.55**	
	(6.32)	(9.456)	
Pass-through			0.365*
			(0.188)
p-value: Pass-through = 0			0.052
p-value: Pass-through = 1			0.001
Observations	1518	1518	1518
Panel C: Compliant			
Treat * Post	21.76***	5.27	
	(5.404)	(5.237)	
Pass-through			0.242
			(0.418)
p-value: Pass-through = 0			0.070
p-value: Pass-through = 1			0.563
Observations	920	920	920

Bootstrap standard errors in parentheses, clustered at municipality level

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Table 4 measuring dollars in nominal terms.

Table D6: Water Revenue Results (Nominal)

	(1) Water Revenue	(2) Water Revenue	(3) Water Revenue	(4) Water Revenue
Grant X Compliant	-5.353**			
	(2.587)			
Grant X Compliant X e		-0.850**	-0.956**	-1.233***
		(0.376)	(0.424)	(0.442)
Timing Group X Year FE	X	X	X	
Region X Year FE			X	
Observations	4752	4752	4752	4840

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table re-estimates the results in Table 9 measuring water revenues in nominal terms.