Municipal and Economic Consequences of PFAS Contamination Discovery*

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Abstract

Hazardous yet previously unmonitored and unregulated, per- and polyfluoroalkyl substances (PFAS) were detected in 2016 in public drinking water systems of 33 US states during the first-ever PFAS testing. A paired-county difference-in-differences design comparing contaminated counties with bordering, same-state uncontaminated counties shows that the contamination discovery raised municipal bond offering yields by 13 basis points. Municipal revenues, taxes, employment, and expenditures declined, while population out-migration increased. Consistent with rising compensating wage differentials, wages in tradable industries rose, but job creation fell, and firm closures increased. Unemployment and self-employment also increased, emphasizing the multifaceted adverse effects of the PFAS contamination discovery.

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> *For 20 years a farmer in Maine fertilized his fields with treated sewage sludge as part of a state fertilizer program from the 1980's. Sourced from local municipalities, the sludge was contaminated with PFAS. After the EPA's 2016 testing detected PFAS in nearby town's water wells, the farmer found the milk from his farm contained dangerously high levels of PFAS, an entire 15 years after he had stopped using the contaminated sludge in 2004. The century-old farm is now permanently closed.*
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> —EcoWatch October 15, 2019

We examine the effects of discovery of contamination by previously unmonitored and unregulated but hazardous chemicals known as per- and polyfluoroalkyl substances (PFAS) on municipalities and local economic conditions. PFAS are a part of a broader class of chemicals characterized as emerging pollutants, a designation that reflects growing concern of their hazardous effects on both environment and human health. PFAS remained unregulated in the U.S. until 2024. Although manufacturing of two of the most well known PFAS chemicals—perfluorooctanoic acid (PFOA) and perfluorooctane sulfonate (PFOS)—has largely been phased out in the U.S. since around 2008 (ATSDR 2021, p. 654), over 1,100 locations have been found to be contaminated and new locations continue to be identified (PFAS Project Lab 2023a).

We focus on PFAS contamination because of the unique challenges faced by the affected areas. First, remediating the contamination is prohibitively costly and uncertain, if doing so is feasible.[1] Second, while the contamination discovery necessitates immediate remediation, recovering the costs through litigation and regulatory penalties is uncertain and may take a long time, if it happens at all.[2] This is partly due

[1] At least $3.3 billion have already been spent on PFAS remediation as of January 2025 (Safer States 2025). The projected cost of removing PFOA and PFOS nationwide is about $3 to $38 billion, depending on their allowable concentration in drinking water, and of installing technology to remove all PFAS is about $370 billion in investment and over $12 billion in annual maintenance (AWWA 2019). The remediation costs have even constrained budgets of some counties and state governments. For example, New Hampshire postponed enacting limit on PFAS in drinking water anticipating budgetary constraints (Ropeik 2019), and even municipalities from wealthier areas such as Massachusetts have expressed difficulty in budgeting for the cleanup (Beckwith 2020).

[2] First, the government's ability to respond to PFAS contamination under the two main environmental statutes (Safe Drinking Water Act (SDWA) and Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA)) is limited (Johnson 2021). Second, while lawsuits against major PFAS manufacturers have yielded large settlements, more than 15,000 claims nationwide remain ongoing (Kluger 2023) and even their combined assets may not be enough for the cleanup costs. Third, cities and municipalities themselves may face PFAS related lawsuits. For example, BFI Waste Systems (a municipality in Decatur, Alabama) and the City of Decatur of Morgan County are facing a federal lawsuit for improperly disposing PFOS and PFOA (Remkus 2016). Morgan County is one of the contaminated counties in our sample. Unsurprisingly, legal remedies alone do not appear to be a reliable and sufficient response for the affected local governments (Burger 2020).

to the fact that PFAS usage was not restricted in the past, enforceable safe exposure limits were not established, and contamination could result from numerous sources, including production, use, and disposal of products containing PFAS. Identifying the source is further complicated by the extremely long time taken by these chemicals to degrade in natural environment. This is why they are also known as the "Forever Chemicals". Keeping these unique issues surrounding PFAS in mind, this paper examines the effects of PFAS contamination, revealed in 2016 during the first-ever nationwide testing of the public drinking water systems in the U.S., on municipal and economic health of the contaminated areas. In doing so, we aim to draw lessons for how local governments and the broader economy may be influenced in future as more PFAS contamination sites continue to be identified, both in the U.S. and globally. These insights could help inform policy responses for managing future contamination sites.

We identify multifaceted adverse effects of the PFAS contamination discovery. Offering yields of municipal bonds in contaminated counties increased and credit ratings worsened, accompanied by both a reduction in capital supply (by mutual funds) and an increase in long-maturity borrowing demand. In terms of real effects, household out-migration from affected counties increased, contributing to reduced revenues for municipalities, who in turn curtailed public employment and expenditure.[3] Moreover, consistent with workers demanding higher compensating wage differentials to continue working in contaminated counties, tradable industries wages rose, but closure of firms increased and job creation fell. The affected counties also saw heightened unemployment and self-employment.

To lend a causal interpretation to these findings, we use a paired-county difference-in-differences (DD) approach utilizing the discovery of PFAS contamination of drinking water systems in 200 counties across 33 states in August 2016. While an exogenous assignment of contamination to the counties would be ideal for causal inference, we argue that several characteristics of the event mimic an exogenous shift in the

[3] We also find anecdotal evidence that some planned public investments were shelved, such as the redevelopment plan for Willow Grove military base (McDaniel 2019).

information available to the market regarding the contamination status of the counties.[4] First, PFAS had not been tested previously in drinking water. Second, the causes of the contamination are largely unknown and uncertain both ex ante and ex post.[5] Third, in the absence of enforceable safe limits for PFAS concentrations in drinking water prior to the event and with the nationwide tests costing about $87 million (G.A.O. 2014, p. 2), little incentives, if any, existed for private sector to front-run the governmental testing. Further complicating such front-running was the lack of technical procedures to detect PFAS in water until the event (Dorrance et al. 2017, p. 294). In all, is unlikely that the contamination status was known to the market prior to the event, even if it was endogenous to the counties.

In our DD strategy, we classify all local municipalities headquartered in contaminated counties as treated. The control counties are the within-state counties bordering the treated but uncontaminated counties. We then create treated-control county pairs by repeatedly pairing the treated county with each of the bordering control counties, similar to Dube et al. (2010). Our sample period runs from 2013 to 2018. In terms of trends over the pre-event sample period, we find no difference between the contaminated and control counties across a range of municipal bond and county characteristics, including offering yields, credit rating, coupon rate, bond amount, share of firms in PFAS-handling industries, violations of drinking water treatment technique, presence of a facility by the three major PFAS manufacturers (3M, DuPont, and Chemours), and county population and income per capita. Furthermore, since both treated and control counties are from the same state and are geographically contiguous, they are subject to the same fiscal and municipal policies of the state and similar local economic trends. We include county and county-pair×year fixed

[4] Gormley and Matsa (2011) also use this "treatment exposure in the past but treatment disclosure in future" approach by utilizing the carcinogen designation of chemicals to classify the firms using those chemicals as treated. Di Maggio et al. (2017), Fuster and Willen (2017), and Gupta (2019) classify the households who took adjustable rate (instead of fixed rate) mortgage in the past as treated and employ contemporary variations in interest rates to mimic exogenous variations in their monthly repayments.

[5] As previously discussed, the nature of PFAS makes it challenging to determine the source of contamination, and the scientific understanding of PFAS accumulation in environment is still evolving (Abunada et al. 2020; Ahrens and Bundschuh 2014). Hence, we do not attempt to identify the specific reason for the contamination, but we do find a positive association between the share of PFAS-handling industries in a county from 1998 to 2005 and the discovery of contamination in 2016 (see Section 1.3).

effects in all the regression specifications to difference out any effect of contemporary national or state events, such as gubernatorial elections or the surprise result of the 2016 presidential election. In the regression specifications for municipal bonds, we additionally include fixed effects for credit-rating×agency and category of bond's borrowing purpose to adjust for the effect of these factors on the outcome variable.

We estimate that offering yields of municipal bonds issued from the contaminated counties increased by 13 basis points (bps) relative to the bonds issued from control counties. Relative to the bonds in the pre-event sample, this is a 6.2% increase in average yields and is also equivalent to the increase in yields that would have occurred if the S&P credit rating were to drop by three notches from AAA to AA-. These estimates are based on *all* local municipalities, not just those few engaged in drinking water provision. We do so for two reasons. First, in many instances it was the broader city, not the specific water-related municipality, who raised the municipal bond financing to specifically address PFAS contamination.[6] Second, as we discuss later, the event led to wide-ranging effects on real economic activity including population out-migration and firm closures, which reduce revenues and taxes for all municipalities.

We also rule out a series of alternative explanations for the increase in yields. First, the increase in yields is not explainable by the shifts in population alone, since the regressions include control for population. Second, an alternative sample where both treated and control counties are urban yields similar results, indicating that the increase was not driven by rural-urban divide of counties. Third, estimating the effects in a sample of only general-purpose bonds also yields a significant increase in yields of 10 bps, thus the overall increase was not driven by an increase in the proportion of bonds for different purposes. Fourth, including fixed effects for issue-year×maturity-year results in same estimates, indicating that the increase was not driven by the fact that treated and control bonds occupied different regions of the yield curve or had drastically different maturities. Finally, we see a similar increase in yields in an

[6] For example, after the contamination discovery in Hampden County, Massachusetts, the City of Westfield—not a dedicated water municipality—issued $13 million in bonds to address the contamination (Tremblay 2018).

alternate assignment of treatment and control using zip codes, though our preferred strategy is based on county borders.

Regarding credit risk, we find that the credit ratings of bonds issued by municipalities from the affected counties worsened by 0.6 to 1.6 notches after the event relative to the bonds issued by those from the unaffected counties. Moreover, we reaffirm this credit risk channel using a triple difference (DDD) specification that estimates the differential increases in the yields of the bonds associated with factors related to credit risk. In particular, we find that the offering yields increased more for bonds (i) of non-general obligation type; (ii) for bonds from counties where the average bond credit ratings in the pre-event sample period were worse than the median; and (iii) for bonds from states that lack proactive policies to assist distressed municipalities.

Along with the higher yields and credit risk, we document that both equilibrium supply and demand for municipal capital changed. In terms of demand, we find an increase of about 24% to 37% in long-term borrowings by the municipalities after the event, presumably to finance infrastructure necessary to treat and abate PFAS contamination. In terms of supply, we examine mutual funds, who are an important class of institutional municipal capital suppliers (Adelino et al. 2023). The holding of municipal bonds (issued prior to the event) was reduced by active mutual funds for municipalities from the contaminated counties relative to the control, but was not reduced by index and exchange-traded funds (ETFs), who track benchmarks.[7] We then examine heterogeneity in the yields by county-aggregated characteristics of households, the largest municipal investors who also enjoy non-pecuniary utilities from local municipalities. DDD specifications show that the increase in yields was smaller for counties with above-median home and real estate ownership, local tax

[7] Our finding that mutual funds reduced holdings of municipal bonds issued by contaminated counties does not contradict Cornaggia et al. (2021, section 5.4.2). Comparing offering yields at or around issuance across bonds, the authors find that the yields are *less* sensitive to local (opioid) shocks for the bonds more likely to be held by institutional investors. The authors argue this lack of sensitivity to arise from lower capital constraints of institutional investors and from diversification benefits of portfolio spread across locations and asset classes. In contrast, our analysis focuses on portfolio adjustments by mutual funds only (not other institutional investors) and exploits within-fund×bond changes (not across-bond changes) in their holdings of bonds already issued prior to the event.

advantage, and socio-economic resilience, in line with incentives of households to shield the municipalities from adverse shocks (Cornaggia et al. 2021).

We next investigate how the contamination discovery affected broader economic activities. This allows us to uncover the multifaceted adverse effects of the event and also to emphasize the tight link between local economic activities and financial health of the municipalities.[8] Relative to the control, the contaminated counties lost 2.4% more individuals, 1.8% more households, and 3.8% more gross taxable income to out-migration within the state. This is 26% larger than the 1.9% increase in in-migration enjoyed by counties that experienced municipal credit rating upgrades (Cornaggia et al. 2019, p. 17). The counties also saw a 7% decline in municipal revenues, a 7% cut in municipal expenditure, and a 13% cut in public employment. Due to multiplier effect of fiscal expenditure, such cuts are detrimental to local economic growth (Adelino et al. 2017; Dagostino 2025), particularly since these were more than what shrinking population alone could explain. Finally, health insurance plan participation rose by 3.7% after the event, likely because household awareness of adverse health effects of PFAS increased, which also validates the salience of the contamination discovery event.

The event also adversely affected businesses and employment. In line with the idea that workers would demand higher compensating wage differentials to work in contaminated areas, our DDD estimates show that tradable industries—which largely employ skilled workers who tend to have higher mobility—in contaminated counties experienced a 5% increase in payrolls, but also 3.1 more firm closures (4%), and 246 fewer job creation (7.3%) relative to non-tradable industries, indicating that rising wage bill may have contributed to closure of firms. We also find that both unemployment and self-employment increased by 10 and 1.1 percentage points, respectively.

Contribution and related literature: Our primary contribution lies in documenting the causal adverse effects of PFAS contamination discovery on both local municipalities

[8] Declining economic activity reduces future municipal revenues and may lead to imbalances in current and future operational budgets of municipalities, particularly if they also face unexpected expenditures, such as remediating PFAS contamination. These imbalances influence their credit risk, as they are not only expected to balance their budgets but are also restricted to borrow long term only to finance infrastructure, which typically needs voter approval (Ang et al. 2017; Haughwout et al. 2021).

and real economic outcomes. Our arguments regarding the effect on municipal finances are similar to the arguments in Cornaggia et al. (2021) and Li and Zhu (2019), who study the effects of the opioid crisis in the U.S. However, we diverge from their findings in terms of documenting changes in broader real economic outcomes including public expenditures and employment, population out-migration, firm closures, and unemployment.[9] Furthermore, Adelino et al. (2017) show that municipalities increase public expenditure and employment when their borrowing constraints are relaxed. We also reaffirm this pattern, albeit in an opposite context: the PFAS contamination discovery results in tighter constraints and lower expenditures.

Owing to the unique context of PFAS, our findings on its multifaceted effects on local economy are markedly different from the findings regarding effects of long-run sea-level rise on changes in municipal offering yields in seventeen major U.S. coastal cities (Painter 2020) or on changes in *secondary market yield spreads* of municipal bonds of *only* school districts (Goldsmith-Pinkham et al. 2023), since these studies do not speak to effects on household migration, unemployment, and firm closures. The same differences extend to Seltzer et al. (2022), who show that climate regulation risks raise credit risk of bonds of firms with poor ESG ratings.

We are first to document that the PFAS contamination discovery burdens the local governments and adversely affects the local economic activity. Our findings speak to both the literature studying the effect of real economic outcomes on municipal borrowings (Butler and Yi 2022; Cornaggia et al. 2021; Jerch et al. 2023; Li and Zhu 2019) and vice versa (Agrawal and Kim 2021; Yi 2020), and also to the literature linking features of municipal bond markets with offering yields.[10] Our finding on response of mutual funds to the event relates to the recent literature examining the

[9] The events also differ in important ways. First, whereas the opioid crisis is specific to the U.S., PFAS contamination is a global and emerging issue, as new contamination sites continue to be discovered in the U.S. (PFAS Project Lab 2023a), the European Union (European Chemical Agency 2024), China (Ao et al. 2019), and other countries (Adewuyi and Li 2024). Second, unlike the opioids, since PFAS were unregulated in the U.S. until 2024 and remain so in most countries, the scope of governmental response is limited (see Johnson (2021) and Footnote 2).

[10] Offering yields of municipal bonds have been shown to be affected by information intermediaries (Cornaggia et al. 2018; Cornaggia et al. 2019; Gao et al. 2020), policies of state for distressed municipalities (Gao et al. 2019), underwriters (Butler 2008; Garrett and Ivanov 2022; Garrett 2024), tax treatment of municipal bonds (Garrett et al. 2023), and corruption (Butler et al. 2009).

effect of institutional investor demand on municipal finances (Adelino et al. 2023; Li et al. 2023). Finally, our results on real outcomes contribute to the literature concerning environmental contamination, such as the effect of drinking water PFAS contamination on house prices (Marcus and Mueller 2024) and of lead contamination on consumer credit scores (Gorton and Pinkovskiy 2021), healthcare demand (Danagoulian et al. 2020), and public expenditure (Christensen et al. 2023).

1 Background Information and Empirical Research Design

1.1 The Event: Discovery of PFAS Contamination in Drinking Water Systems

Established under Safe Drinking Water Act, Unregulated Contaminant Monitoring Rule (UCMR) requires Environmental Protection Agency (EPA) to monitor contaminants that are unregulated under National Primary Drinking Water Regulations but are known or anticipated to occur in public water systems (EPA 2017). Every five years EPA prepares a list of candidate contaminants whose occurrence in drinking water poses a public health risk and monitors a maximum of 30 such contaminants in all large water supply systems (that serve more than 10,000 individuals) and a representative sample of small systems. In the third iteration of UCMR program (UCMR 3), six PFAS chemicals—PFOA, PFOS, PFHpA, PFHxS, PFNA, and PFBS—were tested across the U.S. from January 2013 to December 2015 (Federal Register 2012, Exhibit 3: Timeline of UCMR Activities). Relying on the data from the program, Hu et al. (2016) identified 33 states to be contaminated with PFAS and made headlines (Feldscher 2016), an example of which is shown in Panel A of Figure 1.

We use the publication date of Hu et al. (2016), August 9, 2016, as the event date in our DD design. We note that the online search activity arising from the U.S. about the keyword "PFAS" spiked on this date, as Panel B of Figure 1 illustrates. The spike in the search activity around the publication date is objective evidence supporting that the contamination of drinking water systems was a salient event that received the attention of the general U.S. population. The salience of the event for households is particularly relevant for our empirical setting, given that we focus on municipal finances and household responses, and households directly hold almost half of all

outstanding municipal bonds. We present further evidence in Section 5.1 indicating the salience of the event. We show that participation of households in health insurance plans increased after the event in the contaminated counties relative to bordering uncontaminated counties, a natural response to an event revealing that the local public drinking water is contaminated.

[Insert Figure 1 About Here]

1.2 Difference-in-Differences Design and Regression Specification

We employ a paired-county DD design. The treatment group comprises the counties revealed to be contaminated under the UCMR 3 program, and the control group consists of the neighboring counties within the same state that border the contaminated counties but were not identified as having PFAS contamination. We pair the treated counties with the control counties while allowing a treated county to appear as many times in the sample as the number of uncontaminated neighboring counties it shares its border with. This county-pair design is a variation of the design in Dube et al. (2010). Our data sample consists of 200 treated counties across 33 states, 426 control counties, and 592 distinct county pairs. Panel C of Figure 1 shows these counties on the map of the contiguous U.S. In all the regressions about municipal finances, we classify all local governments in a county, that is, municipalities, cities, townships and other public issuers, as treated or control based on the contamination status of the county. We focus exclusively on local sub-county-level municipalities, as they are more likely to experience the economic constraints resulting from the contamination compared to the county-level governments. Our estimation sample runs from 2013 till 2018.

We utilize the following Two-way Fixed Effects (TWFE) regression specification:

$$Y_{imcpt} = \alpha_0 + \beta_1 \text{Treat}_c \times \text{Post}_t + \text{Controls} + \text{Rating} \times \text{Agency} + \text{Purpose} + \alpha_c + \gamma_{pt} + \epsilon_{imcpt}, \tag{1}$$

where Y_{imcpt} is a characteristic of a municipal bond i issued on date t by municipality m from county c of county-pair p. $Treat_c$ equals one if the public drinking water system of county c was detected to have PFAS under the UCMR 3 testing and equals zero otherwise. $Post_t$ is one for $t \geq$ August 9, 2016 and zero otherwise. β_1 is the coefficient of

interest capturing the change in the dependent variable before and after the event in the treated counties relative to the control. *Controls* vary across specifications and consist of a host of bond and county-level socioeconomic characteristics. We defer discussing the purpose, interpretation, and benefits of the four fixed-effects terms in our empirical analysis—Rating×Agency, Purpose, county (α_c) and pair×year (γ_{pt})—to Section 3.1, where we present the results. Finally, the use of single event in our DD design (as opposed to multiple staggered events) alleviates the issue of negative weights in TWFE estimator resulting from aggregating potentially heterogeneous treatment effects across time.

1.3 Could the Contamination be Predicted by Past Industrial Activities and Infrastructure Quality?

As previously discussed, our DD strategy relies on the discovery of the status of contamination, not on the contamination itself. Though the reasons and timing of the contamination are unknown, we examine whether the contamination could be predicted by two factors commonly suspected to contribute to PFAS contamination: presence of industries handling PFAS and quality of local drinking water infrastructure. Measuring the presence of PFAS-handling industries accurately is generally complicated because these chemicals were largely unregulated, allowing them to stay unmonitored and unreported until the year 2020 under otherwise comprehensive monitoring programs such as EPA's Toxic Release Inventory. We thus rely on the list of potential industries expected to be handling PFAS compiled by EPA (2023). We combine this list of industries with the data on the number of establishments from County Business Patterns (CBP) using four digit NAICS codes. We then define a county-level annual measure of PFAS-handling industry share equal to the ratio of the number of establishments belonging to these industries to the total number of establishments in a county. We then examine whether the county-level PFAS-industry share in any given year from 1998 till the pre-event year 2015 could predict the contamination status of the county in 2016 using the following specification:

$$\text{Treat}_{cp} = \alpha_0 + \beta_1 \times \text{PFAS-handling Industry Share}_{cp} + \gamma_p + \epsilon_c. \tag{2}$$

Here $Treat_{cp}$ takes the value of 1 if a county c of county pair p was discovered to have PFAS contamination in 2016, and γ_p is county-pair fixed effects. Panel A of Figure 2 shows for each year, the coefficient β_1 and the 90%, 95% and 99% confidence intervals based on standard errors clustered by county. We see that the share of these industries in early years, from 1998 till 2005, is positively associated with the contamination. This predictability accords well with the gradual phase out of PFOA and PFOS manufacturing in the U.S. by 2008 (ATSDR 2021, p.654). More importantly, this difference in exposure of counties to industries potentially handling PFAS is not a threat to our identification strategy, because it is already evened out several years prior to the start of our DD estimation period that runs from 2013 till 2018. We further confirm the absence of difference in this exposure in the next section examining pre-trends.

We repeat the above analysis by replacing the independent variable in equation 2 to the quality of drinking water infrastructure in a county. We measure drinking water infrastructure quality annually as the number of violations of drinking water treatment technique per million population, with more violations representing less robust drinking water infrastructure. Panel B of Figure 2 shows the coefficients β_1 and the associated confidence intervals for each year from 1998 till 2015. We see that the quality of drinking water infrastructure does not predict the contamination.

[Insert Figure 2 About Here]

In all the subsequent DD regressions that estimate differences in outcomes across counties, we include these two characteristics measured contemporaneously as control variables to account for their potential role in shaping those outcomes.

1.4 Pre-trends

We now focus on confronting the key assumption needed for the DD design. It states that the municipal finances in treated counties would have experienced similar trends relative to the control counties in the absence of the treatment. Though this is unverifiable, if the trends experienced by the treated and control counties over the pre-event sample period (that is, pre-trends) are not diverging, we can have some

assurance that the parallel trends assumption might hold. Thus we examine the pre-trends for a range of municipal bond characteristics using the dynamic version of equation 1 as follows:

$$\begin{aligned} Y_{imcpt} = \alpha_0 + \sum_{k=-3,\, k\neq 0}^{3} \beta_k \text{Treat}_c \times \text{Year}_k + \text{Controls} \\ + \text{Rating} \times \text{Agency} + \text{Purpose} + \alpha_c + \gamma_{pk} + \epsilon_{imcpt}, \end{aligned} \tag{3}$$

where $Year_k$ is one when $t = T + k$ and T is the event date. $Year_k$ is measured relative to the event date (see footnote 15 for details). *Controls* include bond and county characteristics and are defined in Section 3.1 and Table 1. The fixed effects terms—Rating×Agency, Purpose, county (α_c) and pair×year (γ_{pk})—are described in Section 3.1. Panels A through D of Figure 3 plot β_k's and the associated 95% confidence intervals obtained using standard errors clustered by county for offering yields, rating (its specification excludes rating×agency fixed effects), coupon rate, and bond amount. We do not observe any statistical difference between the municipalities from contaminated and control counties prior to the event for any of these four municipal bond characteristics, indicating an absence of pre-trends.

[Insert Figure 3 About Here]

We next examine pre-trends in county characteristics using the specification below:

$$Y_{cpt} = \alpha_0 + \sum_{k=2013,\, k\neq 2015}^{2018} \beta_k \text{Treat}_c \times \text{Year}_k + \alpha_c + \gamma_{pk} + \epsilon_{cpt} \tag{4}$$

Panel E through H of Figure 3 show the results for the following four county characteristics respectively—the share of PFAS industries, the number of drinking water treatment technique violations per million population, presence of a establishment by DuPont, 3M, or Chemours (the erstwhile major manufacturers of PFAS), and income per capita (in natural logarithm). The plots for all but income per capita suggest an absence of pre-trends. The income per capita was lower in treated counties but was rising at a higher rate relative to the control over the pre-event period and plateaued after the event, consistent with a decline in economic activities that we document later. A key reason that this pattern in income per capita does not appear to threaten the exogeneity of the event underlying our DD design is that the timing (of the event) is

shaped by UCMR 3's testing schedule, which is determined at the federal level and is independent of county characteristics.

In all, the absence of pre-trends in a host of municipal bond and all-but-one county characteristics in our DD design does not appear to suggest that the parallel-trends assumption might not hold for municipal finances related outcomes.

2 Data and Summary Statistics

We employ a range of environmental, financial, and socioeconomic datasets in our analysis. The data on PFAS contamination are from EPA's UCMR 3 program. These include information on detection levels for the six PFAS chemicals in drinking water and the zip codes of the water service areas.[11] We aggregate these observations to the county level and classify a county as contaminated if any of the zip codes within its boundaries had a water system contaminated with any of the six PFAS chemicals. This process yields 200 contaminated counties across 33 states. Panel A of Table 1 shows key statistics on the contamination levels for the six PFAS compounds in these counties. Column 1 presents the number of counties where a given PFAS was detected, and columns 2 through 6 show summary statistics of detected concentration. We see that PFOA was detected in 128 counties, with mean, median, and maximum concentration of 48.5, 30, and 349 ng/L. The maximum detected level was five times the advised limit applicable then (a total of 70 ng/L for PFOA and PFOS combined) and almost 90 times the maximum allowable legal limit of 4 ng/L after the 2024 regulation (EPA 2024).

[Insert Table 1 About Here]

We assemble municipal bond issuance data from 2013 till 2018 from Thomson Reuters Eikon. This data contain extensive information on bond offerings, including nine-character CUSIP, offering yields, coupon, amount, maturity, purpose, and the zip code of issuer's headquarter. We use headquarter to link municipalities to counties and exclude those which do not operate within the boundaries of a single county, since

[11] The minimum reporting levels for the six tested PFAS chemicals—PFOS, PFOA, PFHpA, PFHxS, PFNA, and PFBS—in ng/L are respectively 40, 20, 10, 30, 20, and 90. The lowest reporting level achievable with the testing technology available then was 20 ng/L.

our baseline identification utilizes county boundaries.[12] After linking municipalities to counties, we classify municipalities headquartered in zip codes within contaminated counties as treated, and those in bordering, uncontaminated counties within the same state as control (in Section 3.1 and Table C.5, we show an alternative empirical strategy relying on zip codes). We additionally exclude a small fraction of bonds of uncommon characteristics that often result in widely different coupon and yields.[13] Additionally, we exclude all federal and state issuers, as well as county issuers. County issuers are excluded because it is the local smaller-than-county-level municipalities that are likely to be financially and fiscally constrained, not the counties themselves. We do so by excluding SDC issuer type 10 (state), 11 (county), 19 (nation), and 20 (national authority). The resulting bonds constitute our sample for analysis of primary market offering yields. It consists of 64,636 bonds (at nine-character CUSIP level) issued by 1,886 municipalities from 164 counties in the treated group and 52,209 bonds issued by 1,521 municipalities from 258 counties in the control group. Panel B of Table 1 presents key statistics on these bonds. We see that a typical new municipal bond has an offering yield at issuance of 2.32%, coupon of 3.55%, Ln(tenure in years) of 2.09, and Ln(issue amount in million) of 13.62.

We also use data from Center for Research in Security Prices (CRSP) mutual fund database to analyze mutual fund holdings of bonds issued by the municipalities in our sample. We identify municipal issuers by the six-character CUSIPs of bonds issued by them and link them with the six character CUSIPS of bonds held by mutual funds.

Finally, we use a host of socioeconomic variables at the county level. County population and payroll data come respectively from Census Bureau's annual county resident population estimates and county business patterns data. County-level personal income per capita and unemployment rate are respectively from Bureau of

[12] We do it in two steps. First, we exclude municipalities headquartered in zip codes that span both a treated and a control county. Second, we exclude all school district bonds and municipalities, since a school district covers an average of 1.6 counties in the 33 states in our sample. We identify these using purpose code beginning with "E" in Eikon data and issuer type 13 (university) and 14 (district or board of education) in SDC data.

[13] We specifically exclude privately-placed bonds, non-tax-exempt bonds, bonds whose capital type is neither new filing nor refunding, bonds whose coupon frequency is neither semiannually nor at maturity, bonds whose coupon is not fixed plain vanilla type, and bonds with maturity less than a year.

Economic Analysis and Bureau of Labor Statistics' local area unemployment statistics (LAUS). We draw on Inland Revenue Service's (IRS) statistics of income (SOI) data to create county level measures of self-employment, homeownership, and population migration. We also use the Census Bureau's nonemployer statistics to create an additional measure of self-employment. We use EPA' toxic release inventory data to determine whether a DuPont, Chemours, or 3M facility is present in a county. We also utilize the list of potential PFAS-handling industries compiled by EPA (2023) and combine it with county business patterns data to construct our county-level measure of industries potentially handling PFAS. Local government revenue and expenditure data are from the annual survey and census of state and local government finances, compiled by Pierson et al. (2015), and public sector employment data are from the annual survey of public employment & payroll. Data regarding firm and establishment deaths, and job creation and destruction by county and industry are from Census Bureau's business dynamics statistics. We source data on the number of treatment technique violations of the Safe Drinking Water Act from the safe drinking water information system. Panel C of Table 1 reports summary statistics of all county-level characteristics used as covariates in regressions.

We also utilize several health-related datasets. We use data from American Community Survey to measure household participation in health insurance plan, and CDC's WONDER database to measure mortality rate due to kidney cancer. We construct pregnancy-related outcomes from natality data available from the national vital statistics system of the national center for health statistics.

3 Effects of PFAS Contamination Discovery on Municipal Finances

The central hypothesis guiding our empirical analysis is that the discovery of PFAS contamination negatively impacts economic activity in the affected area, and therefore, municipal finances. Three sets of factors contribute to the worsening of municipal finances. The first is a reduction in municipal revenues due to declining economic activities, which we identify in terms of an increase in out-migration, firm closures, and unemployment. The second is a reduction in supply of municipal capital, as

investors may assess the event to affect the municipal credit risk. To corroborate this, we will examine changes in credit risk and a reduction in fund allocation by mutual funds. The third is an unexpected increase in capital demand by municipalities to finance the current and future expenditures to remediate the contamination and install abatement technologies (see footnote 6 for an anecdotal example). This may also lead municipalities to forego planned investments and curtail important expenditures, which we examine using changes in general expenditures and public employment.

Accordingly, we structure the empirical analysis as follows: in this section, we examine the changes in offering yields, credit risk, and (equilibrium) demand and supply of municipal capital and in Section 4, the changes in municipal expenditure and broader economic activity.

3.1 Effect on Offering Yields to Maturity

We use the following specification to evaluate the effect of the event on offering yields to maturities:

$$\begin{aligned} \text{YTM}_{imcpt} = \; & \alpha_0 + \beta_1 \text{Treat}_c \times \text{Post}_t + \text{ Bond and County Controls} \\ & + \text{ Purpose} + \text{Rating} \times \text{Agency} + \alpha_c + \gamma_{pt} + \epsilon_{imcpt}, \end{aligned} \tag{5}$$

where YTM_{imcpt} is the offering yield to maturity of bond i (at the CUSIP level) issued by municipality m from county c of county-pair p in year t. Subscript p in YTM_{imcpt} and γ_{pt} denotes that counties (and hence bonds issued by municipalities operating there) are repeated for all county pairs they are part of. β_1 is the coefficient of interest. *Bond controls* include CUSIP-level bond amount (in natural logarithm), tenure in years (in natural logarithm), inverse of natural logarithm of tenure in years, and indicators for whether the bond is callable, insured, bank qualified, non-GO (versus GO), competitive offering (versus negotiated), and refunding type (versus new filing). *County controls* include the share of PFAS-handling industries (defined in Section 1.3), the number of drinking water treatment technique per million population, an indicator for whether an establishment by 3M, DuPont, and Chemours (the erstwhile major manufacturers of PFAS) is present in the county, and natural logarithm of

population and personal income per capita. *Purpose* indicates fixed effects for bond's purpose, which in Eikon database is classified into 11 major types including general purpose, health, transport, education etc. Our regression includes all purposes except education, as described in footnote 12. Its inclusion therefore allows us to account for any differences in the yields of bonds raised for different purposes. *Rating×Agency* incorporates the fixed effects for each rating category assigned by the three rating agencies, S&P, Moody's, and Fitch.[14] Its inclusion allows us to account for differences in yields arising from credit ratings. α_c and γ_{pt} incorporate the fixed effects for county and pair×year, respectively.[15] Incorporating γ_{pt} allows us to flexibly account for any differences in economic and other conditions experienced by county pairs across years. It therefore accounts for any temporal changes occurring in state (such as gubernatorial elections and fiscal and municipal policies), in broader regions (such as sea-level rise in coastal areas or hurricanes), and in the country (such as the surprise result in 2016 presidential election).

Table 2 shows the results of the above regression. The sample in columns 1 and 2 includes contamination by any of the six PFAS, with the former including only bond controls and the latter including both bond and county controls. The estimates of β_1 in these columns suggest that a new bond issued by a municipality in a contaminated county experienced an increase of 10 to 13 bps in offering yields to maturity relative to a municipality from a neighboring uncontaminated county after the discovery of the contamination. Our preferred estimate is from column 2, as this specification accounts for county-level economic trends.

[Insert Table 2 about here]

[14] For example, bonds rated by Moody's, rating takes the value of 1 for Aaa, 2 for Aa1, ..., and 10 for Baa3. To implement the fixed effects for *Rating×Agency*, all bonds rated below Baa3 (less than 0.1% of the sample) are assigned a separate numerical category and so are the unrated bonds. See Section B.1 in Online Appendix for more details.

[15] To incorporate pair×year fixed effects γ_{pt}, we use synthetic year (as opposed to calendar year) based on 365-day intervals counting from the event date, because the event in our setting occurred mid-year on August 9, 2016 whereas issue dates of municipal bonds are spread throughout the year. Using calendar year instead would render the *Post* indicator temporally misaligned with calendar year fixed effects (in pair×year), leading to spurious β_1 estimates.

A key assumption underlying our empirical design is the idea that even though the contamination itself could have occurred endogenously in an unknown past, the revelation of information on contamination status of the counties was exogenous. We argue that the contamination by the two legacy PFAS, PFOA and PFOS, is more likely to occur deeper in the past, because these had been manufactured the longest, used the most, and had been phased out in the U.S. since around 2008. The assumption thus is more likely to hold for these two PFAS. We thus re-estimate the above regressions for the sample of counties contaminated with only these two chemicals and the bordering counties uncontaminated with any of the six PFAS and show the results in columns 3 and 4 of Table 2. This alternative sample yields essentially the same estimates.

Furthermore, a plausible explanation for the increase in yields is a general trend of increasing shift of population from urban to rural counties for reasons unrelated to PFAS contamination. This shift could be of a particular concern in our context if all contaminated (treated) counties are industrial counties and the counties surrounding them (control) are small residential counties, such that the treated counties lose population to control counties independent of the PFAS contamination discovery, leading to a rise in local municipal bond yields. We argue that this concern is partially mitigated through our design choice of including population as a control in the regressions and including the fixed effects for pair×year. To address it more directly, we estimate the effect in an alternative sample where we replace the control counties from within-state first-order neighbors to *within state second order neighbors* of the treated counties. Columns 5 through 8 of Table 2 show the results. We see that the estimates remain remarkably similar, reassuring that a general shift in population from urban to rural counties cannot explain the increase in yields.

To gauge the importance and consequences of a 13 bps rise in offering yields of municipal bonds following discovery of PFAS contamination, we note that it is equivalent to the change in yield that would occur if the S&P credit rating of a typical municipal bond in the pre-event sample were to drop by three notches, from AAA to AA-. It is also equivalent to a 6.2% increase over the average offering yields in the pre-

event sample. Relative to the municipal finance literature, we note that this increase is (i) similar to the 16.7 bps increase in yields caused by the opioid crisis (Cornaggia et al. 2021) and the 16 bps increase in long-term bond yields caused by one percent higher climate risk (Painter 2020); (ii) slightly larger than the 5 to 11 bps increase in yields caused by newspaper closures (Gao et al. 2020); and (iii) significantly larger than the 5.2 bps increase in yields caused by an absence of state's proactive fiscal policy to support distressed municipalities (Gao et al. 2019).

To reassure that the estimates documented above are robust and also to rule out alternative explanations, we undertake a series of modifications to our baseline estimation strategy and show the results in Section C of Internet Appendix. First, does the inclusion of *Rating*×*Agency* fixed effects in the baseline strategy underestimate the rise in yields, given that rating agencies may have updated the credit ratings of bonds to account for the effect of contamination? While we show later that the ratings do worsen by about 0.6 notches after the event, we document in Table C.1 of Online Appendix that the estimates remain essentially the same when we re-estimate the baseline results without the *Rating*×*Agency* fixed effect.

Second, does the increase in yields simply reflect the differences between urban and rural counties if contamination by PFAS is more likely to occur in urban than rural counties? Using Urban-Rural Classification Scheme developed by National Center for Health Statistics in 2013, the year of start of our estimation period, we find that 436 out of 522 treated-control county pairs (83.5%) are urban. Consequently, we find in Table C.2 of Online Appendix that the estimates remain essentially the same when we re-estimate the baseline results by focusing only on urban treated and urban control counties, indicating that our results are not caused by rural-urban county differences.

Third, does the increase in yields reflect the change in proportion of bonds issued for different purposes such that the substantial increase was mostly limited to some special purpose bonds, leaving general purpose bonds largely unaffected? The inclusion of purpose fixed effects in our baseline results implies that the estimates represent a weighted average of the increase in yields experienced by each purpose type, partially

accounting for this concern. Furthermore, we find in Table C.3 of Online Appendix that the estimates decrease to 10 bps when we re-estimate the baseline results by focusing only on general purpose bonds, indicating that a shift in the proportion of bonds across purposes is not the key driver of the estimated effects.

Fourth, does the estimates capture the difference in yields arising from the bonds occupying different regions of the yield curve owing to their differing maturities? We note that our baseline specification may alleviate this concern through the difference-out effect of DD strategy (since it is the same yield curve that is used to discount the treated and control bonds, any effect of yield curve would be differenced out) and also through the bond-level controls for logarithm maturity and inverse of logarithm maturity. Nonetheless, a clustering of the bonds from treated and control counties in distinct regions of the yield curve could contribute to the rising difference in the bonds yields. We address this issue by adding bond's issuance-year×maturity-year fixed effects in the baseline estimate and show the result in Table C.4 of Online Appendix. The estimates decrease to 10 bps, indicating that yield curve differences are insufficient to explain the increase in yields documented previously.

Finally, we address the concern that using counties—rather than zip codes—as the unit of analysis may lead to an underestimation of the true effects, since only zip codes served by the contaminated public water system (PWS) were affected, not all zip codes or municipalities in the county. Our decision to use counties is driven by institutional factors, the specific economic mechanisms we seek to uncover, and convention in the literature. First, counties have mutually exclusive and exhaustive geographic boundaries, enabling unambiguous identification of both contaminated and uncontaminated areas. Second, even if some zip codes within contaminated counties remain unaffected, the economic consequences—such as household migration, business closures, and unemployment—are likely to extend beyond the specific zip codes served by the PWS. Third, municipal taxes and revenues are not confined to the zip codes served by the PWS, further justifying the use of counties. Finally, practical considerations also play

a role: the key economic variables, such as migration and unemployment, are only available at the county level, precluding more granular analysis.

Notwithstanding the aforementioned reasons for choosing counties as the unit of analysis, we re-estimate the contamination effect on yields using a zip code strategy. We begin by identifying the specific zip codes served by contaminated PWS. These zip codes, along with those within three miles, are classified as contaminated, while zip codes between three and five miles away are considered controls. We then pair each treated zip code with every control zip code and implement the specification in equation 5, incorporating fixed effects for zip codes and zip-code-pair×year, rather than county and county-pair×year. We create three more alternative samples: (i) contamination by PFOA and PFOS only; (ii) expanded treatment of within five miles and control between five and seven miles; and (iii) contamination by PFOA and PFOS only in the extended sample. Table C.5 in the Online Appendix presents the estimation results for all four samples. We find a 4 to 7 bps increase in the sample with smaller distances (columns 1 through 4) and a 13 to 14 bps increase in the sample with larger distances (columns 5 through 8), though they are statistically not significant. The smaller estimates in the first sample likely result from control zip codes also being indirectly treated, as they lie within just three miles of the contaminated zip codes. The 10-12 bps increase in the larger sample is nearly identical to the 10–14 bps increase documented previously using the first- and second-order bordering county strategy. The standard errors however are larger (or estimates are statistically insignificant) in the zip code strategy, likely due to the reduced variation in independent variables representing bond- and county-level institutional characteristics, given the proximity of treated and control bonds. In summary, the estimates from the zip code strategy suggest that using counties as the unit of analysis does not lead to an underestimation of the effects.

In all, this section provides robust evidence that the offering yields of municipalities rose by about 13 bps after the discovery of PFAS contamination. In the rest of the paper, we follow the baseline specification.

3.2 Effect on Municipal Credit Risk

We next examine the changes in credit risk of the municipal bonds following the event employing two strategies. First, we examine changes in bond credit ratings. Second, we evaluate heterogeneity in offering yield increase by three municipal market characteristics literature associates with municipal credit risk (Cornaggia et al. 2021; Cornaggia et al. 2022; Gao et al. 2020). The idea is that the offering yields will see a larger increase for the characteristics associated with a higher risk.

To estimate changes in the ratings, we use the same DD specification from equation 5 but omit *Rating*×*Agency* fixed effects. We focus exclusively on non-general obligation (non-GO) bonds, since the repayments of GO bonds are backed by the taxing power of the issuers, making their credit risk much less likely to be influenced. We create two samples. The first sample consists of bonds whose long-term underlying credit ratings range from 1 till 22, with 1 to 21 for usual rating categories from lowest to highest credit risk and 22 for a small subset of bonds with rare ratings. The second sample further adds the bonds without information on long-term underlying ratings, assigning them the numerical rating of 23.[16] Columns 1 and 2 of Table 3 show the regression results for the two samples. We see that the ratings increased by 0.67 notches conditional on being rated and by 1.6 notches when accounting for issuance of unrated bonds. Recall the previous discussion that the increase in the yields was equivalent to an increase of 3 notches in ratings, much larger than the rise of only 0.6 notches. This difference is in line with the arguments that (i) credit rating agencies use a highly mechanical rating process relying on municipal issuers' financial statements, which lack standardization and are published with significant delay (Cornaggia et al. 2021); and (ii) the ratings are too coarse to fully reflect differences in credit quality across all rated securities and are designed to be stable over time and not to reflect real-time changes in credit quality (Cornaggia et al. 2018, p. 2046).

Noting that the credit ratings are integers, we re-estimate these two specifications using Poisson Pseudo Maximum Likelihood (PPML) regression and show the results

[16] See Section B.2 in Online Appendix for more details.

in Table C.6 of Online Appendix. These estimates suggest that ratings increased by 11 to 16% for treated bonds relative to control.

[Insert Table 3 Here]

To analyze heterogeneity in the offering yield increase by characteristics associated with credit risk, we use the following triple difference (DDD) specification:

$$\text{YTM}_{imcpt} = \alpha_0 + \beta_1 \text{Treat}_c \times \text{Post}_t \times \mathbf{X} + \beta_2 \text{Treat}_c \times \text{Post}_t + \beta_3 \text{Treat}_c \times \mathbf{X} + \beta_4 \text{Post}_t \times \mathbf{X} + \beta_5 \mathbf{X} + \text{Bond and County Controls} + \text{Purpose} + \text{Rating} \times \text{Agency} + \alpha_c + \gamma_{pt} + \epsilon_{imcpt}. \quad (6)$$

Here $\mathbf{X}$ represents a credit risk characteristic, and β_1 is the coefficient of interest estimating the differential change in the yields. Note that in addition to shedding light on the credit risk mechanism, these DDD estimations also aid in the causal interpretation of the results in the sense that the identifying assumption for this estimator is weaker than that for DD. It only requires there to be no contemporaneous shock that affects the relative difference across the groups of observations (formed by the third difference variable) within the treated group in the same manner as the contamination event does.

The first variable that we use in DDD specification is based on the average ex ante credit ratings of the bonds issued in a county. We expect that the yields would increase by more in counties that had worse average ex ante ratings in the pre-event year 2015. In line with Gao et al. (2022, footnote 5), we obtain the mean of credit ratings of all bonds rated 10 or below that were issued in a county in the pre-event year 2015 and then use its cross-sectional median to classify counties into low (above median) and high rating (below median) groups. Column 3 of Table 3 shows that within the contaminated counties, those in the ex ante low credit rating group experienced a 21 bps larger increase in offering yields than those in ex ante high credit rating group.

The second variable that we use in DDD specification is based on the type of cash flows pledged to repay the bonds. General obligation (GO) municipal bonds are backed by the cash flows derived from taxation power of the issuers and carry lower credit risk than other bonds. Thus the effect of the event should be less pronounced for GO bonds than for non-GO bonds. Consistent with this, the estimate in column 4 of

Table 3 shows that the increase in the offering yields of the treated non-GO bonds was 7 bps larger than the treated GO bonds, adjusting for the contemporaneous changes in the yields experienced by the control municipalities.

The third variable that we use in DDD specification reflects the credit risk associated with state assistance policies for financially distressed municipalities, which influence creditor protections and thus the credit risk (Gao et al. 2019). These authors argue that creditors view municipalities from the nine states (ME, MI, NC, NJ, NY, OH, PA, NV, and RI) that have proactive policies to assist distressed municipalities as having less credit risk (and also less default risk) than the municipalities from the rest of the states. We thus conjecture that the affected municipalities in the non-proactive states (all states other than the nine proactive states) will see a higher increase in the offering yields after the event relative to those from the proactive states. Consistent with this prediction, the estimate in column 5 of Table 3 suggests that the offering yields of affected municipalities located in non-proactive states rose on average 10 bps more than the affected municipalities in the proactive states, while adjusting for the contemporaneous changes in the yields experienced by the control municipalities.

To summarize, we find that credit risk of the affected bonds increased after the event and within the affected municipalities, the yields increased more for the bonds with characteristics linked to higher credit risk, consistent with the municipal investors recognizing that credit risk of affected municipalities increased after the event.

3.3 Effect on (Equilibrium) Demand for Municipal Capital

We now evaluate the changes in demand (at equilibrium) for capital by the affected municipalities after the discovery of contamination. Municipalities may adjust their bond-raising activities to balance the overall higher borrowing costs with their elevated needs for capital to remediate the contamination and install treatment technology. Weighing the higher borrowing costs against the urgency of financing the remediation needs and maintaining other expenditures, it is unclear whether the total bond issuance amount would increase or decrease after the event. We aggregate the bond issuance amounts to the county-year level and analyze the changes using the following DD

specification:

$$\text{Ln}(\text{Amount})_{cpt} = \alpha_0 + \beta_1 \text{Treat}_c \times \text{Post}_t + \text{ County Controls} + \alpha_c + \gamma_{pt} + \epsilon_{cpt}. \quad (7)$$

Table 4 presents the results of regressing the natural logarithm of amount issued under all bonds in column 1 and non-GO bonds only in column 2. We see that there was no change in average borrowing amounts after the contamination. Keeping in mind that building infrastructure for PFAS abatement and treatment would require long-term capital, we then examine the compositional changes in amounts borrowed for long and short maturities using a triple difference specification as follows:

$$\begin{aligned}\text{Ln}(\text{Amount})_{kcpt} = \alpha_0 &+ \beta_1 \text{Treat}_c \times \text{Post}_t \times \mathbf{X}_k + \beta_2 \text{Treat}_c \times \text{Post}_t + \beta_3 \text{Treat}_c \times \mathbf{X}_k \quad (8)\\ &+ \beta_4 \text{ Post}_t \times \mathbf{X}_k + \beta_5 \mathbf{X}_k + \text{ County Controls} + \alpha_c + \gamma_{pt} + \epsilon_{kcpt},\end{aligned}$$

where k enumerates maturity type—long and short term—in every county c of pair p, and $\mathbf{X}_k$ is an indicator taking the value of one for long-term borrowing. Bonds with maturities≥15 years are classified as long term. In columns 3 and 4 of Table 4, we see that long-term borrowing amount increased by 24.6% ($[e^{0.22} - 1] \times 100$) through all bonds and by 37.7% ($[e^{0.32} - 1] \times 100$) through non-GO bonds. With the assumption that the composition of borrowing maturity would have remained the same in the absence of the discovery of the contamination, these estimates allow us to infer the amount municipalities raised specifically to address the contamination. Given that the average annual long-term bond issuance was $101 million per county, the estimate in column 3 implies that municipalities in a typical contaminated county raised nearly $24.8 million per year presumably to build infrastructure to address and abate the contamination, an amount roughly sufficient to build four new elementary schools (Cornaggia et al. 2018, footnote 25).

[Insert Table 4 about here]

3.4 Effect on (Equilibrium) Supply of Municipal Capital

Our results so far indicate that the event changed the equilibrium borrowing cost, credit risk, and maturity composition of capital demand by municipalities. It is thus natural to expect that investors would also shift the supply of capital in response

to the contamination discovery. We focus on two important classes of investors to understand changes in capital supply in equilibrium after the event: mutual funds and households. For mutual funds, we employ their portfolios data to examine changes in their holdings of bonds issued by affected and unaffected municipalities. For households, we exploit variation in county-level socio-economic characteristics, which influence their willingness and incentives to supply municipal capital, to assess heterogeneous treatment effects using the triple difference specification.[17]

3.4.A Change in Capital Supply by Mutual Funds

Just as variations in investment mandates and demand from institutional investors influence the pricing of corporate bonds (Bretscher et al. 2022), mutual funds play a pivotal role in the municipal bond markets, with their portfolio allocation decisions influencing both the issuance and pricing of municipal bonds (Adelino et al. 2023; Li et al. 2023). Mutual funds are sophisticated investors and are more likely than households investors to base their capital allocation on fundamental municipal characteristics including credit risk. To estimate the response of mutual funds to the affected municipalities, we estimate changes in their holdings of the bonds of the same municipal issuer as in the main sample that were already issued prior to the event date, August 9, 2016. We choose this approach of analyzing already-issued bonds because it is not feasible to examine the changes in within-bond holdings for bonds that are issued after the event. We begin by extracting the first six characters of the CUSIP codes for new bonds included in our main sample to identify relevant municipalities. Using this identifier, we merge these records with bond holdings data from the CRSP Mutual Fund database and exclude bonds issued after the event (identified by their full eight-character CUSIP) from the merged dataset. This process yields a panel dataset at the fund×CUSIP8×year-quarter level. Among bonds issued prior to the event, we classify those from municipalities in contaminated counties as affected and those from municipalities in control counties as control. We focus specifically on holdings in terms

[17] Owing to a lack of household-level data on investment in municipal bonds, we are unable to examine the changes in investments of households in municipal bonds.

of number of bonds, instead of market value of the bonds, because a change in the former reflects intentional decision of funds regarding exposure whereas a change in the latter could arise simply from repricing of the bonds without any intentional decision of the funds. We estimate a Poisson Pseudo Maximum Likelihood (PPML) regression with the number of bonds i (at CUSIP 8 level) held by mutual fund f in year-quarter q as outcome variable. The regression specification is:

$$Y_{ifq} = \beta_0 + \beta_1 \text{ Affected Bond}_i \times \text{Post}_q + \text{ Controls} + \alpha_{if} + \delta_\tau + \text{Maturity Year}_i + \epsilon_{ifq}. \quad (9)$$

$Post_q$ takes the value of one for year-quarter≥2016Q3 and zero otherwise. τ represents year. *Controls* include management fees, the expense ratio, one quarter lagged bond's yield, and the natural logarithm of one-quarter lagged fund's total net assets and market value of the underlying bond. This specification includes fixed effects for fund×CUSIP8 (α_{if}), year (δ_τ), and bond's year of maturity. These flexibly account for any time-invariant characteristics of funds, bonds (hence, also of the municipality), and any time-varying temporal shocks. The coefficient of interest, β_1, captures the change in holdings of the same bond by the same fund before and after the event for *Affected bonds* relative to *Control bonds*.

[Insert Table 5 about here]

Table 5 presents the results. The estimates in column 1 through 3 indicate that bond holdings decreased by 0.7% across all funds, by 0.6% for retail funds, and by 0.8% for institutional funds. In contrast, from column 4 and 5 we see that the holdings did not change significantly for index funds and ETFs, which track benchmarks. This variation in response across fund types supports the interpretation that active funds reduced their exposure to already-issued bonds of affected municipalities relative to those of unaffected ones. For robustness, we regress the natural logarithm of the number of bonds using OLS regression and find similar results, as reported in Table C.7 of Online Appendix. Relying on these reductions in mutual funds' allocations to already-issued bonds, we further argue that reductions likely also extended to the new bonds, since the funds typically invest early in new municipal bonds and gradually sell their stakes to other investors in the secondary market (Azarmsa 2021). Therefore, the pricing of

new municipal bonds are particularly sensitive to mutual fund allocation decisions, plausibly explaining the increase in yields we previously documented.

3.4.B Role of Socio-Economic Characteristics in Capital Supply

We next examine the role of household investors in supplying capital to the municipal bond market. According to the Federal Reserve's 2018 Flow of Funds data, households directly held approximately half of all outstanding municipal bonds between 2002 and 2018, indicating that they are an important investor class in this market. Assuming that the capital demand function does not systematically differ across counties with different investor clientele (Cornaggia et al. 2021), significant heterogeneity in offering yields across such counties would suggest that household capital supply influences municipal bond pricing. The differences in investor clientele we examine are linked to the nonpecuniary utility households derive from supplying capital to local municipalities, which are responsible for last-mile delivery of public goods. To identify these clientele differences, we use four socio-economic measures.

The first measure is the homeownership rate. Homeownership creates mobility constraints and incentivizes residents to invest in their local communities and amenities, thereby strengthening social capital (DiPasquale and Glaeser 1999). Homeowners derive additional nonpecuniary benefits from supporting their local communities and municipalities, which are responsible for delivering a host of public services to them, increasing their willingness to provide capital to local municipalities. Our measure of the county-level homeownership rate is the fraction of total tax returns in the IRS that have mortgage deductions. For the same reasons, we also utilize a second closely-related measure, the county-level ownership rate of real estate, defined as the fraction of total tax returns that have real estate taxes.

The third measure we utilize is tax advantage of local municipal bonds. Babina et al. (2021) show that the tax advantages associated with in-state municipal bonds predict higher ownership by in-state investors. Extending this argument to household investors, we argue that states with higher tax privilege would have a higher share of ownership by in-state households, implying that the effect of the event would be less

pronounced for such states. Our fourth measure uses CDC's data on a county's social vulnerability index (SVI). This index ranks counties according to the ability of their community to respond to hazardous events. We use the socio-economic theme of the index as a measure of socio-economic resilience such that high resilience reflects low vulnerability.

We utilize the above four socio-economic measures in a triple difference specification by categorizing sample counties into high group when the value of the measure is above the cross-sectional median and low group otherwise for all measures except socio-economic resilience. The classification is reversed for this measure. The year of classification is the pre-event year 2015 for all measures except tax privilege, which is time-invariant owing to its cross-sectional mature. We then regress offering yields using the triple difference specification from equation 6 and present the results in Table 6. The results in columns 1 to 4 indicate that, among affected counties, the increase in offering yields was smaller by 20, 20, 10, and 17 bps respectively, in counties with high ex ante homeownership rates, high ex ante real estate ownership rate, high tax advantage, and high socio-economic resilience. These findings highlight the role of households' stake in their local areas in maintaining the supply of municipal capital after the contamination discovery and mitigating the impact on offering yields. For robustness, we re-estimate the above regressions using cross-sectional means to create the cuts and find similar results, as reported in Table C.8 of Online Appendix.

[Insert Table 6 Here]

Overall, we find that the capital supply by both mutual funds and households shaped the overall rise in offering yields after the contamination discovery in a manner consistent with their expertise and incentives.

4 Effects of PFAS Contamination Discovery on Broader Economic Activity

In Section 3, we reported the effects of discovery of contamination with PFAS on municipal finances. In this section we broaden the analysis to investigate the effect

on a wider range of real economic activity. We begin by examining changes in public spending—municipal revenues, taxes, expenditures, and public employment, since these play a role in local economic growth through their fiscal multiplier effects (Adelino et al. 2017). We hypothesize that the contamination discovery event leads to a curtailment of these spending, which in turn adversely affects the municipal finances documented in the previous section.[18] We next estimate the changes in population out-migration, firm dynamics, and unemployment rates, highlighting the broader economic repercussions of the event, as illustrated by the anecdote presented at the start of this article where a Maine farmer's century-old business was forced to close after PFAS contamination was discovered in the vicinity.

4.1 Effects on Municipal Operations

We examine changes in municipal revenues and expenses after the contamination discovery to understand whether they were also affected in the real economy. We aggregate revenues and expenditures to county-year level for all municipalities at county level and below (identified by type code 1 to 3) from the Government Finance data of Pierson et al. (2015). The regression specification is as follows:

$$\text{Outcome}_{cp\tau} = \alpha_0 + \beta_1 \text{Treat}_c \times \text{Post}_\tau + \text{County Controls} + \alpha_c + \gamma_{p\tau} + \epsilon_{cp\tau}, \tag{10}$$

where county c of pair p in year τ experiences $Outcome_{cp\tau}$. Subscript p in $Outcome_{cp\tau}$ and $\gamma_{p\tau}$ denotes that counties may be repeated for all county pairs they are part of. $Post_\tau$ takes the value of one for year≥ 2016 and zero otherwise. County controls and α_c are the same as in equation 5. $\gamma_{p\tau}$ represents fixed effects for pair×year. Standard errors are clustered by county. The outcome variables of interest include the natural logarithm of revenues, total taxes, and property taxes as revenues, as well as total, general, and general current expenditures as expenditures.[19]

[18] Our context is opposite to that in Adelino et al. (2017), who document an increase in public expenditure and employment following credit rating upgrades of municipalities that enhanced their borrowing capacity.

[19] The corresponding variables in Pierson et al. (2015) are: revenues (*Total_Revenues*), total taxes (*Total_Taxes*), property taxes (*Property_Tax*), total expenditure (*Total_Taxes*), general expenditure (*General_Expenditure*) and general current expenditure (*General_Current_Expend*). They define the construction of these variables from census data codes in Appendix B of their paper. Expenditure

[Insert Table 7 about here]

The regression results are reported in Table 7. The findings indicate that all three sources of cash flows—total revenues, total taxes, and property taxes—declined in the contaminated counties by 7.6%, 7.8%, and 7.3%, respectively, relative to the control counties. Moreover, municipalities reduced total, general, and general current expenditures by 7%, 7.4% and 7.4%, respectively. In terms of the magnitude, the expenditure loss caused by PFAS contamination discovery is slightly larger than the 2.7% to 6.7% increase in expenditure associated with the municipal rating upgrades during Moody's 2010 rating recalibration and more than six times the expenditure increase associated with one-standard-deviation increase in fraction of local municipalities experiencing rating upgrade (Adelino et al. 2017, p. 3245). Note also that these estimates account for contemporaneous population, suggesting that the expenditure reductions exceeded what could be attributed solely to changes in resident population. We infer that these expenditure cuts are a result of municipalities adjusting to the dual shock of diminished revenues and increased borrowing costs in the primary market. Such loss of public expenditure has adverse implications for the local economy, potentially degrading the quality of public goods and creating negative spillovers in the private sector due to the fiscal multiplier effect (Adelino et al. 2017; Dagostino 2025; Yi 2020).

4.2 Effects on Public Employment

We next examine changes in public employment, noting that one important use of municipal funds is to maintain (or enhance) the directly-hired employees of local governments. We look at the effects of discovery of PFAS contamination on full-time-equivalent public employment. We regress the natural logarithm of public employment, share of public employment among total employment, and public employment per 1000 population using the specification in equation 10.

relates to external payments of a government and excludes amounts transferred to funds or agencies of the same government. General expenditure mainly includes expenditure on current operations, capital outlays on construction, and other capital outlays. General current expenditure is the difference between general expenditure and capital outlays, and is often used to pay employees, purchase supplies and hire contractors.

[Insert Table 8 about here]

Table 8 shows the regression results. Columns (1) through (3) indicate that, relative to control counties, the contaminated counties experienced a 13.2% decline in full-time-equivalent public employment, a 0.7 percentage points reduction in the share of public employment, and a decrease of 1.8 public jobs per 1,000 population. Recall that these estimates account for contemporaneous population, indicating that the reduction in public employment exceeded what could be attributed solely to changes in resident population. The magnitude of these employment cuts is substantial, comparable to the employment cuts by municipalities with stressed insurers (Amornsiripanitch 2022), and two to four times larger than the public employment rise associated with municipal credit rating upgrades (Adelino et al. 2017, p. 3249).

We further investigate whether municipalities in more financially distressed areas reduce employment to a greater extent. We classify counties into high and low interest burden groups based on the cross-sectional median in the pre-event year 2015 of the ratio of total interest payments to revenues generated from property taxes. The ratio is calculated at the county level by first aggregating the respective terms across all municipalities identified by type codes 1 to 3 in the data from the Government Finance dataset of Pierson et al. (2015). The regression specification is as follows:

$$\begin{aligned}\text{Outcome}_{cp\tau} = \alpha_0 \; &+ \beta_1 \text{Treat}_c \times \text{Post}_\tau \times \mathbf{X}_c \; + \beta_2 \text{Treat}_c \times \text{Post}_\tau \; + \beta_3 \text{Treat}_c \times \mathbf{X}_c \\ &+ \beta_4 \; \text{Post}_\tau \times \mathbf{X}_c + \beta_5 \mathbf{X}_c + \; \text{County Controls} + \alpha_c + \gamma_{p\tau} + \epsilon_{cp\tau}.\end{aligned} \tag{11}$$

$\mathbf{X}_c$ is one for counties whose interest burden ratio is above the median and zero otherwise. The results are presented in columns 4 through 6 of Table 8. They reveal that the three public employment measures experienced more pronounced declines in counties with higher pre-event interest burdens: a reduction of 12.8%, 0.6 percentage points, and 1.6 public jobs per 1,000 population, respectively. Two alternative measures of financial burden—the ratio of total debt to property taxes and the ratio of total interest to total revenues—produce similar estimates, albeit with slightly lower statistical significance, as reported in Table C.9 of Online Appendix.

All in all, we infer that these are a result of municipalities adjusting to the dual shock of diminished revenues and increased borrowing costs in the primary market following the event. This mechanism is distinct from the financing constraints channel of Adelino et al. (2017), where increase in public spending is solely driven by relaxation of financial constraints following municipal credit rating upgrades. While the spending and employment cuts are likely driven by both higher borrowing costs and declining revenues, our empirical framework is unable to disentangle their relative contributions.

4.3 Effects on Population Out-migration

A key implication of Tiebout's seminal model of spatial sorting is that individuals would sort across local communities to align with their preferences for public goods (Tiebout 1956), though the extent of the sorting (migration) is influenced by mobility costs, which may vary across populations and regions (Rhode and Strumpf 2003). Following the discovery of PFAS contamination, population out-migration from affected counties is likely, particularly among higher-income individuals and within state migration than across state, due to lower migration costs. We thus examine changes in within state out-migration from the affected counties following the contamination discovery. We measure out-migration using the IRS SOI county-to-county migration dataset, focusing exclusively on within-state migration. Our first measure is the natural logarithm of the number of individuals migrating out of a county to any other county within the same state. The second measure is the net out-migration rate, calculated as the outflow of individuals from a focal county to other same-state counties minus the inflow from other same-state counties to the focal county, scaled by the focal county's population and expressed as a percentage. The third measure is the ratio of out-migrating individuals to in-migrating individuals. We additionally also examine overall income loss experienced by the counties in four alternate ways. The regression specification is from equation 10 and includes the same control variables.

[Insert Table 9 about here]

Table 9 presents the regression results. Column (1) to (3) show that, relative to the control counties, the affected counties lost 2.4% more individuals, a 9.2

percentage points larger migration as a percentage of resident individuals, and 2.45 more individuals per incoming individuals to the same-state counties after the event. This increase is significant, almost 26% larger than the rise in in-migration of 1.9% observed after municipal credit rating upgrades (Cornaggia et al. 2019, p. 17). When out-migration is measured in terms of households, instead of individuals, in columns 4 through 6, the respective estimates are 1.8%, 7.3 percentage points, and 1.87 more households per incoming households. Furthermore, column 7 indicates that the taxable income of out-migrating individuals from the affected counties was 3.8% more than that of out-migrating population from control counties; and column 8 and 9 show that the taxable income of in-migrating population as a percentage of out-migrating population and as a percentage of residents was smaller by 5.7% and 17.2% respectively relative to those in control counties. Finally, column 10 indicates that there was no difference in the ratio of taxable income of out-migrating population to that of residents. Taken together, these findings suggest that while the income level of out-migrants matched that of residents, in-migrants had lower wealth, resulting in an overall loss in the aggregate taxable personal income for the contaminated counties. In summary, discovery of PFAS contamination not only resulted in out-migration to other same-state counties but also lowered the overall taxable wealth of contaminated counties. Such out-migration are detrimental for the affected counties due to reduced taxable base and revenues (Hastie 1972; Cornaggia et al. 2019; Yi 2020).

4.4 Effects on Wage and Firm Dynamics

To formulate our hypothesis regarding the effect of discovery of PFAS contamination on wages and firm dynamics, we draw on the seminal theory of equalizing differences. This theory postulates that workers require a wage premium (compensating wage differentials) to accept jobs with undesirable non-wage characteristics, such as higher risks of mortality or injury in hazardous occupations (Rosen 1974; Thaler and Rosen 1976; Brown 1980; Rosen 1986, among others). Accordingly, with the discovery of PFAS contamination exposing a previously unknown health risk of living and working in the affected counties, this theory suggests that workers in the contaminated area are likely

to demand higher wages. This demand is likely to be more pronounced in tradable than non-tradable industries, since the former employs skilled workers, who face lower costs of relocation. Depending on the severity, the rising wages could force firms to cut jobs and even wind up. We test for these predictions about wage and firm dynamics in tradable industries relative to non-tradable, arising from increasing compensating wage differentials, a labor supply mechanism.[20]

We use county×industry level data on firm closures and job creation from Census Bureau's business dynamics statistics and payroll data from Census Bureau's county business patterns data. We then construct a pair×industry-type×year level panel data, where industry type (tradable or non-tradable) is defined at the 4-digit NAICS level following (Mian and Sufi 2014).[21] We follow the DDD specification in equation 8, where k now enumerates industry type—tradable and non-tradable—in every county c of pair p, and $\mathbf{X}$ is an indicator taking the value of one for tradable industries.

[Insert Table 10 about here]

Table 10 shows the results of the above regression. Column (1) through (4) indicate that, within affected counties, the tradable industries experienced a 5% higher payroll per employee, 3.16 more firm deaths, 2.9 more establishment deaths, and a reduction of 246.4 in job creation, while controlling for contemporaneous trends in the two sectors in the control counties. Column (5) shows that these industries also experienced 191 fewer job destruction, a statistically insignificant and smaller effect than the reduction in job creation, implying that on balance, the industry saw loss of jobs. These findings thus align with our hypothesis of rising compensating wage differentials following the discovery of PFAS contamination.

For robustness, we re-estimate the results in columns 2 to 5 using PPML regression, instead of OLS, and report the results in Table C.10 of Online Appendix. The respective

[20] This labor-supply based hypothesis is novel to the literature studying differences in tradable and non-tradable industries, since it predominantly focuses on local demand mechanisms, which impacts non-tradable industries more than the tradable. Examples of local demand mechanisms include declining housing prices (Mian and Sufi 2014; Giroud and Mueller 2017), rising stock market wealth (Chodorow-Reich et al. 2021), and reduced demand following local corporate bankruptcies (Bernstein et al. 2019).

[21] Note that industries are defined using 4-digit NAICS code in both payroll data and tradable classification data from Mian and Sufi (2014), whereas using 2-digit NAICS code in business dynamic statistics data. Hence we aggregate the classification data to 2-digit NAICS to match it with the latter.

estimates are a 4% rise in firm deaths, 4.2% rise in establishment deaths, a 7.3% decline in job creation, each statistically significant at 10% level, and a 3.9% decline in job destruction, statistically insignificant. These patterns in wage and firm dynamics is the same as discussed above. Collectively the results highlight the negative impact of discovery of PFAS contamination on private sector wages, firm dynamics, and employment, particularly within tradable industries.

4.5 Effects on Unemployment

Having shown the adverse effects of the event on firm and wage dynamics, we now turn to examine its impact on both unemployment and disadvantaged self-employment—a form of self-employment that individuals transition into out of necessity rather than opportunity, typically as a response to unemployment risk (Evans and Leighton 1989; Pugsley and Hurst 2011; Hou et al. 2025). County-level unemployment data come from the Local Area Unemployment Statistics provided by the Bureau of Labor Statistics. Disadvantaged self-employment is calculated using three distinct measures. The first measure is the natural logarithm of the number of nonemployer businesses derived from the Census Bureau's Nonemployer Statistics dataset. Nonemployer businesses are those with no paid employees, with annual receipts of $1,000 or more (or $1 or more in the construction industry), and subject to federal income taxes. This provides a relatively clean measure of disadvantaged self-employment. Our second measure is the natural logarithm of the number of tax returns in a county reporting self-employment tax (variable N09400). The third measure is the percentage of total tax returns in a county that report a self-employment tax.

[Insert Table 11 about here]

We use the regression specification from equation 10 and present the results in Table 11. The estimates in columns 1 through 4 indicate that, compared to the control counties, the affected counties experienced a 10 percentage points increase in unemployment rate, a 1% increase in the number of nonemployer businesses, a 1.1% increase in self-employed households, and a 0.135 percentage points rise in the share of households reporting self-employment income. Collectively, these results

align with the idea that the discovery of PFAS contamination increased unemployment and compelled some individuals into disadvantaged self-employment, likely due to elevated unemployment risk (Hou et al. 2025).

5 Supplementary Discussion

In this section, we discuss supplementary results that aid in interpreting previous results and in ruling out some alternative explanations.

5.1 Health-related Outcomes

Given that PFAS contamination is a health risk, in this section we examine changes in two outcomes related to health across contaminated and control counties: household participation in health insurance plans and long-term differences in incidences of some diseases known to be associated with PFAS.

5.1.A Effect on Household Participation in Health Insurance

To examine changes in participation in health insurance plans by households following the event, we use one-year ACS survey data. We regress the percentage of households in a county reporting health insurance coverage, including both any form of health insurance and private health insurance using the DD specification from equation 10. Panel A of Table 12 presents the results. The estimates in columns 1 and 2 suggest that the proportion of households in contaminated counties enrolled in any health insurance plan and in a private plan increased respectively by 0.387 and 0.362 percentage points relative to the control counties. These changes specifically support the interpretation that the general population became aware of the PFAS contamination and its perceived adverse health risks after its discovery under UCMR 3. They also complement the findings on the responses of households discussed previously about increased out-migration and higher compensating wage differentials.

5.1.B Long-Term Difference in Incidence of Health Conditions Related to PFAS

We noted previously that although the contamination status of the drinking water systems with PFAS was discovered in 2016 under UCMR 3 program, when and why

the contamination occurred remain unknown. Assuming that these areas had been contaminated with PFAS over a prolonged period, these areas would experience higher incidence of the diseases associated with PFAS relative to the control counties. While it is the subject of active medical research on which diseases are caused by PFAS, some studies have linked PFAS exposure to kidney cancer (Shearer et al. 2021; NIH 2020), pregnancy complications such as preeclampsia, and congenital anomalies such as heart defects (Szilagyi et al. 2020; Ou et al. 2021). We therefore examine cross-sectional difference (as opposed to difference-in-differences) across the contaminated and control counties over a long time period using incidence of kidney cancer mortality rates from 1999 to 2016 using CDC Wonder data and two natality-related outcomes—eclampsia and congenital anomalies—from 1990 to 2002 using data from Division of Vital Statistics, National Center for Health Statistics.[22] The time periods differ due to the limitations of the public availability of the respective datasets.

Panel B of Table 12 presents the results. Columns (1) to (3) indicate that, relative to the control counties, the contaminated counties had 0.332 more deaths from kidney cancer per 100,000 population, a 0.05 percentage point higher incidence of eclampsia among singleton births, and 0.014 more congenital anomalies among the newborns born with the anomalies. These findings are consistent with a prolonged exposure to PFAS in the affected counties, however we do not claim that other factors unrelated to PFAS do not contribute to these differences. Besides, we did not detect a significant difference in incidence of low birth weight, another PFAS-related health condition.

5.2 Was the increase in yield a behavioral over-reaction?

A purely behavioral explanation, such as household sentiment to contamination, appears unlikely, as it is inconsistent with at least three sets of our findings. First, real economic outcomes were affected, including increased out-migration, unemployment, and closure of firms. Second, mutual funds—expected to be more sophisticated and less influenced by sentiment than retail investors—reduced their allocations in bonds

[22] While the studies referenced earlier associate PFAS exposure with preeclampsia, we examine eclampsia since the natality dataset only reports the latter. Both are pregnancy-related complications characterized by high blood pressure, though the latter also involves seizures and is more severe.

issued by affected municipalities. Third, several anecdotal accounts explicitly link PFAS contamination with legal challenges (footnote 2), issuance of bond specifically aimed at raising capital to fund PFAS cleanup (footnote 6), and budgetary provisions.[23]

6 Conclusion

Discovery of sites contaminated with PFAS has been emerging as a global concern, especially since these chemicals remain largely unregulated across the world, limiting the scope of governmental response to the contamination (Johnson 2021). In this paper we study the effects of discovery of PFAS contamination on local municipalities, firms, and employment. Utilizing a paired-county difference-in-differences approach around the first-ever revelation of PFAS contamination of US drinking water systems in 2016, we find that the offering yields and credit risk of municipal bonds issued by municipalities in the contaminated counties increased relative to those in the bordering same-state uncontaminated counties. On demand side, municipalities responded by tilting their bond borrowings towards long-maturity borrowing, likely to finance infrastructure for treatment and abatement technology. On supply side, mutual funds reduced their bond holdings of the affected municipalities, likely in response to heightened credit risk. The event also resulted in heightened out-migration from the contaminated counties, particularly of higher-income individuals, eroding the local taxable income base from which municipalities derive a significant portion of revenues. Consequently, municipal revenues and tax collections declined.

The event also affected private businesses. The tradable industries in contaminated counties saw higher average wages—consistent with rising compensating wage differentials—but increased firm closures and reduced job creation. Unemployment and disadvantaged self-employment also rose following the event.

[23] For example, Pennsylvania set out $3.8 million in the state's budget to clean up Bucks and Montgomery counties (The General Assembly of Pennsylvania 2019; Park and McDaniel 2019). Arizona set aside funds for PFAS contamination-related expenses and free voluntary blood testing of residents (S.B. 1565 2020). Alaska proposed legislation to provide the affected residents with free safe drinking water and voluntary blood testing for up to three years (S.B. 176 2020). Safer States (2025) maintains a detailed record of PFAS-related costs incurred by several US states.

As PFAS contamination sites continue to be discovered across the world and legislative efforts to regulate PFAS continue to progress in the U.S. (PFAS Project Lab 2023b) and internationally (WQA 2019), these findings may offer insights that may aid in formulating regulatory responses.

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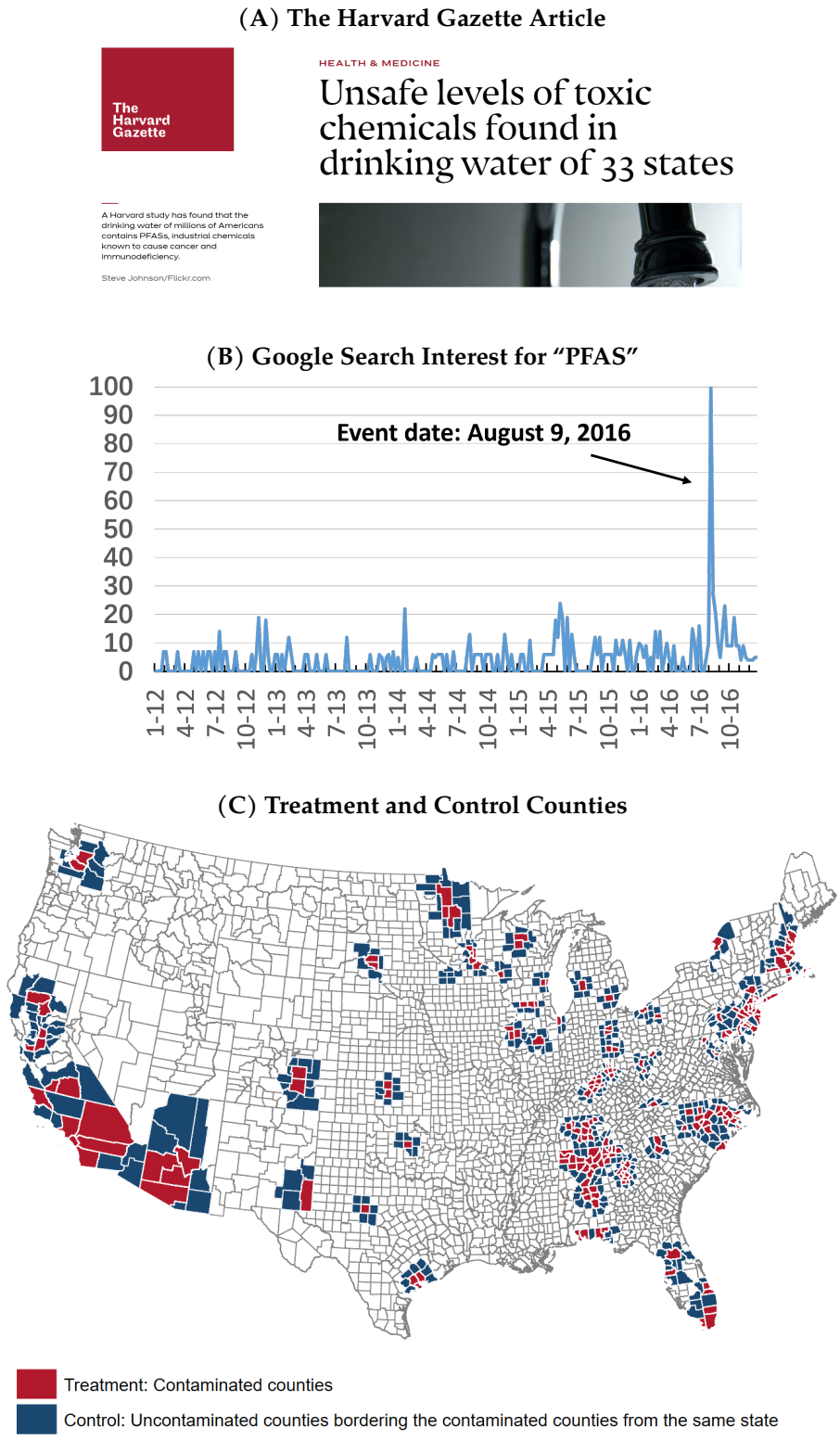
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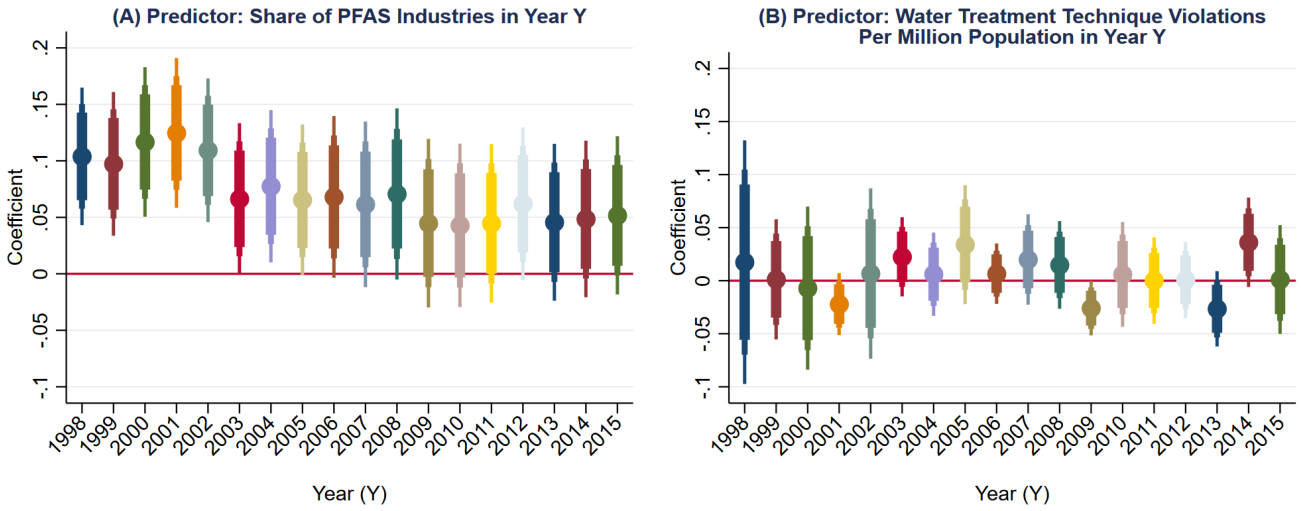
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Figure 1
The Contamination Discovery Event



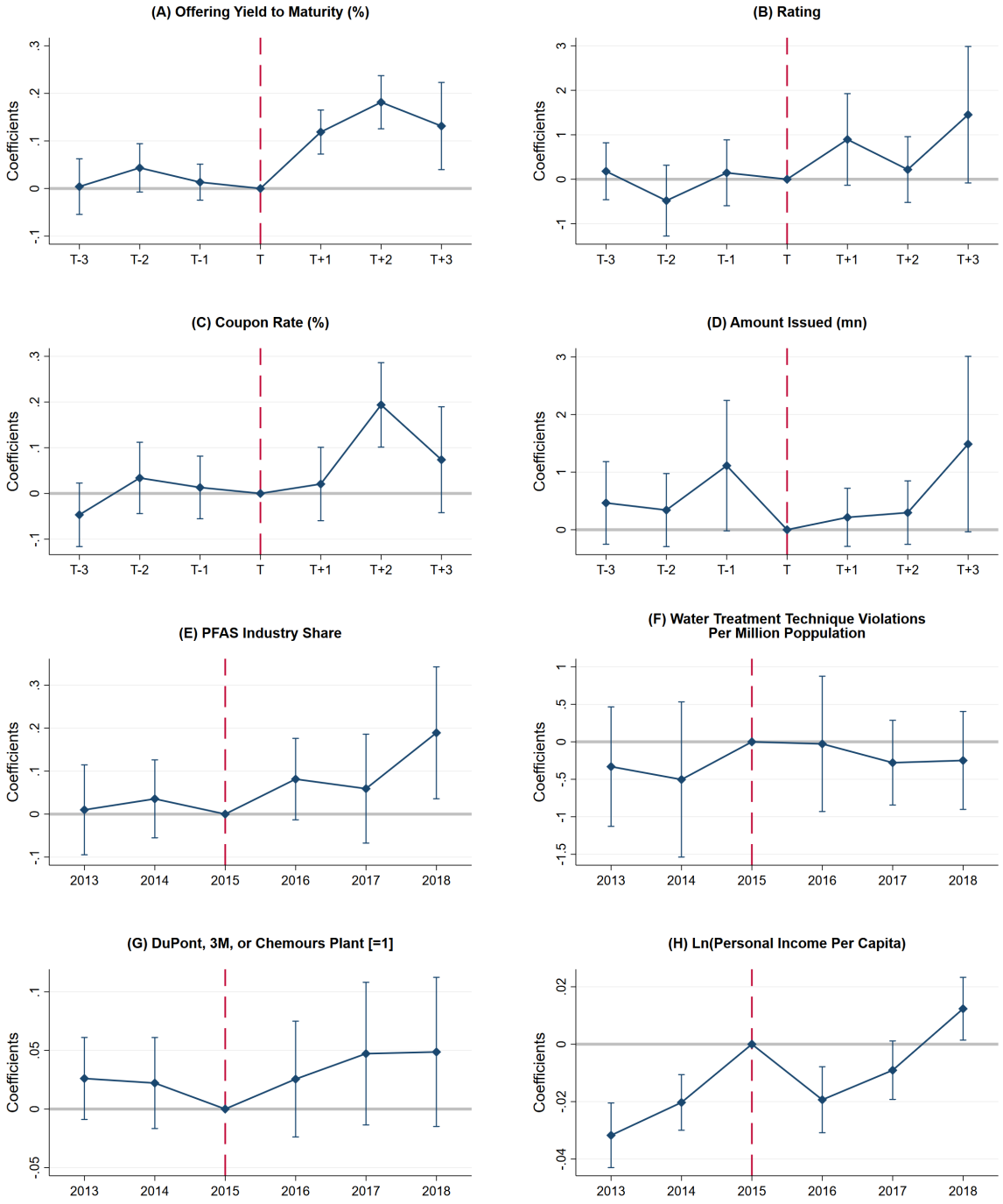
Panel A shows a screenshot of the publication of the findings of Hu et al. (2016) in the Harvard Gazette (Feldscher 2016). Panel B shows the Google Search Interest for the term "PFAS" originating from the U.S. between 2015 and 2017. Google search interest represents the degree of "search interest" for a keyword at any time relative to the highest point during the period of analysis over a given region (U.S.). A value of 100 represents the peak popularity for the term. A value of 50 means that the term is half as popular. Panel C plots on the map of the contiguous U.S. the counties that were revealed under UCMR 3 to have PFAS in drinking water (treated counties) and the bordering same-state uncontaminated counties (control counties).

Figure 2
Predicting the 2016 Discovery of PFAS Contamination of Drinking Water



Panel A of this figure plots the coefficients obtained from regressing the dummy variable Treatment on the percentage of establishments in a county in PFAS-releasing industries following specification in equation 2. Panel B plots the coefficients obtained from regressing the Treatment indicator on the number of annual violations of drinking water treatment technique per million population in a county from 1998 till 2015 using the same specification. The bars indicate confidence intervals at 90%, 95% and 99% in decreasing order of thickness.

Figure 3
Pre-Trends



Panels A through D plot the estimates of β's and associated 95% confidence intervals from regressing four bond variables using the specification in equation 3. Panels E through H mirror panels A through D, but use four county-year variables as dependent variables and follow the regression framework in equation 4.

Table 1
Summary Statistics

	N	Mean	Median	Min.	Max	SD
	(1)	(2)	(3)	(4)	(5)	(6)
A. Contamination Statistics (in ng/L)						
PFOA	128	48.50	30.00	20.00	349.00	58.03
PFOS	103	170.57	70.00	40.00	1800.00	268.36
PFHpA	94	23.77	16.05	10.00	86.91	20.13
PFHxS	64	149.40	82.50	31.67	730.00	164.17
PFNA	15	36.45	32.00	27.00	55.88	10.38
PFBS	14	170.00	150.00	100.00	370.00	86.74
B. Bond-Level Variables						
YTM(%)	116043	2.32	2.30	0.00	12.50	0.98
Coupon(%)	116845	3.55	3.40	0.00	25.22	1.16
Ln(Issue Amt.)	115896	13.62	13.38	8.52	20.80	1.56
Ln(Tenure in years)	116845	2.09	2.20	0.00	3.81	0.73
(Ln(Tenure in years)).−1	114640	0.55	0.46	0.26	1.44	0.27
Callable [= 1]	116845	0.46	0.00	0.00	1.00	0.50
Competitive Offering [= 1]	116637	0.64	1.00	0.00	1.00	0.48
Insured [= 1]	116845	0.11	0.00	0.00	1.00	0.31
Bank Qualified [= 1]	116845	0.35	0.00	0.00	1.00	0.48
Non-GO [= 1]	116845	0.51	1.00	0.00	1.00	0.50
Refunding [= 1]	116845	0.56	1.00	0.00	1.00	0.50
C. County×Year-Level Variables						
PFAS-handling Industry Share (%)	1669	5.49	5.48	0.00	15.84	1.39
Water Treatment Tech. Violations	1669	0.30	0.00	0.00	17.04	1.79
DC3M Plant [= 1]	1669	0.08	0.00	0.00	1.00	0.28
Ln(Population)	1669	12.16	12.23	8.61	16.13	1.39
Ln(Income Per Capita)	1657	10.75	10.71	10.10	12.08	0.26

Panel A reports contamination statistics. *N* indicates the number of counties that detected any of the six PFAS, i.e., PFOA, PFOS, PFHpA, PFHxS, PFNA, or PFBS. Panel B reports the summary statistics of bond variables. *YTM* is the offering yield to maturity at issuance in percentages; *Coupon* is also in percentages; *Ln* (*Issue Amt.*) is the natural logarithm of dollar amount issued; *Ln* (*Tenure in years*) is the natural logarithm of the bond tenure measured in years; and $(Ln\ (Tenure\ in\ years))^{-1}$ is the inverse of *Ln* (*Tenure in years*). *Callable*, *Competitive Offering*, *Insured*, *Bank Qualified*, *Non GO*, and *Refunding* represent whether the bond is callable, offered competitively, insured, bank qualified, a general obligation bond, or issued for refunding, respectively. All preceding variables, except *YTM* and *Coupon*, serve as bond controls in regressions. Panel C reports the summary statistics for county-year control variables. *PFAS Industry Share* is the percentage of industries likely affected by PFAS rulemakings. *Water Treatment Tech. Violations* measures treatment technique violations in drinking water per million population. *DC3M Plant* indicates the presence of a DuPont, Chemours, or 3M facility. *Ln* (*Population*) and *Ln* (*Income Per Capita*) are the natural logarithms of population and personal income, respectively.

Table 2
PFAS Contamination and Municipal Bond Offering Yields

Outcome =	YTM							
Geographic Criterion =	Bordering Counties				Second-order Counties			
Contaminant =	All Six PFAS		PFOA or PFOS		All Six PFAS		PFOA or PFOS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.10***	0.13***	0.11***	0.14***	0.10***	0.14***	0.09***	0.13***
	(5.72)	(6.12)	(6.54)	(6.58)	(6.51)	(6.80)	(5.52)	(5.91)
Controls: (B)ond/(C)ounty	B	B + C	B	B + C	B	B + C	B	B + C
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Purpose, Rating × Agency	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.84	0.84	0.84	0.84	0.84	0.84	0.84	0.84
Observations	257491	256368	252583	252010	468669	467435	410838	410703

This table reports the DD regressions of municipal bond offering yields around PFAS contamination discovery, as specified in equation 5. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Columns 1 and 2 include bonds from counties contaminated by any of the six PFAS, along with bonds from adjacent, uncontaminated counties within the same state. Columns 3 and 4 focus on bonds from counties contaminated specifically by PFOA and PFOS, alongside bonds from neighboring counties within the same state that did not detect any of the six PFAS. Columns 5 through 8 replicate the estimates from columns 1 to 4, while using bonds from same-state counties adjacent to the previous control counties as controls. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 3
Effect on Credit Risk

Specification =	DD		DDD		
	(1)	(2)	(3)	(4)	(5)
Outcome =	Rating	Rating+	YTM	YTM	YTM
Third Difference $\mathbb{X}$ =	–	–	Ex ante Low Rating	Non-GO	Non-Proactive
Treat × Post × $\mathbb{X}$			0.21**	0.07*	0.10**
			(2.40)	(1.83)	(2.37)
Treat × Post	0.67**	1.63**	0.06	0.08***	0.06*
	(2.57)	(2.36)	(1.29)	(3.88)	(1.75)
Bond & County Controls	Y	Y	Y	Y	Y
FE: County, Pair × Year, Purpose	Y	Y	Y	Y	Y
FE: Rating × Agency	N	N	Y	Y	Y
Adjusted R^2	0.57	0.44	0.88	0.84	0.84
Observations	60276	140531	102250	256368	256368

This table reports the effects of contamination discovery on credit risk. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Column 1 reports results of DD regression of the ratings of non-GO bonds that have a credit rating. In Column 2, the sample also includes non-GO unrated bonds by assigning them a numerical rating of 23. Columns 3 to 5 report the heterogeneous effects of contamination discovery on the offering yields of municipal bonds with different credit risk characteristics ($\mathbf{X}$) estimated using DDD specification. $\mathbf{X}$ is one in column 3 for the counties for which the value of mean credit ratings of municipal bonds issued in pre-event year 2015 was above the cross-sectional median (ex ante low mean credit rating); is one in column 4 for non-GO bonds; is one in column 5 for the non-proactive states (consisting of states other than the nine classified proactive in Gao et al. (2019)). The regressions include bond and county controls and fixed effects for county, pair×year, and rating×agency, as indicated. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 4
Effect on Changes in (Equilibrium) Demand for Municipal Capital

Specification =	DD [All Maturity Bonds]		DDD [Long- vs. Short-Term Bonds]	
	(1)	(2)	(3)	(4)
Outcome = Ln (Bond Amt.)	All	Non-GO	All	Non-GO
Treat × Post × Long Term			0.22*	0.32**
			(1.74)	(2.04)
Treat × Post	-0.01	-0.15	-0.03	-0.06
	(-0.09)	(-0.85)	(-0.29)	(-0.33)
Long Term			-0.63***	-0.22**
			(-7.11)	(-2.52)
County Controls	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y
Adjusted R^2	0.80	0.72	0.77	0.79
Observations	2014	1038	6591	4613

This table reports the effects of contamination discovery on the aggregate amount of capital raised through municipal bonds in a county. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Columns 1 and 2 report the DD regressions for aggregate amounts across all bonds and non-GO bonds only, respectively. Columns 3 and 4 report the heterogeneous effects of the contamination discovery on these outcomes by maturity of bonds estimated using DDD specification. A bond is classified as long-term if its maturity is greater than 15 years. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5
Mutual Funds' Allocation to Affected Municipalities

Outcome =	# Bonds (Using PPML Regression)				
	(1)	(2)	(3)	(4)	(5)
Fund Type =	All Funds	Retail Funds	Inst. Funds	Index Funds	ETFs
Affected Bond × Post	-0.007***	-0.006***	-0.008***	0.006	0.003
	(-6.15)	(-4.74)	(-3.88)	(0.49)	(0.13)
Controls	Y	Y	Y	Y	Y
FE: Fund×CUSIP	Y	Y	Y	Y	Y
FE: Year, Maturity Year	Y	Y	Y	Y	Y
Pseudo R^2	0.99	0.99	0.98	0.97	0.96
Observations	2407225	1607976	796190	113503	79158

This table reports the effects of contamination discovery on the allocation decision of mutual funds in the bonds issued by municipalities from the affected and the bordering, unaffected counties. The sample includes all funds in column 1, retail funds in column 2, institutional funds in column 3, index funds in column 4, and exchange traded funds in column 5. The sample period is from 2013 to 2018. The dependent variable is the number of bond i's shares held by fund f in year-quarter q. The regression is Poisson Pseudo Maximum Likelihood (PPML). *Affected Bond* equals 1 if the CUSIP of the bond held by mutual fund is issued by a municipal issuer (identified using 6-digit CUSIP) from the contaminated counties and 0 otherwise. *Post* takes the value of 1 for $q \geq$ Q3, 2016 and 0 for the earlier periods. *Controls* include management fees, the expense ratio, one quarter lagged bond's yield, and the natural logarithm of one-quarter lagged fund's total net assets and market value of the underlying bond. The regressions include fixed effects for fund×CUSIP, year, and bond's year of maturity. Standard errors are clustered by fund, and t-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 6
Treatment Effect Heterogeneity with Socioeconomic Characteristics

Outcome =	YTM			
	(1)	(2)	(3)	(4)
Third Diff. $\mathbb{X}$ =	% HH Homes	% HH Real Est.	Tax Privilege	Socioeco. Resil.
Treat × Post × $\mathbb{1}[\mathbf{X} \geq \text{Median}]$	-0.20**	-0.20**	-0.10***	-0.17***
	(-2.20)	(-2.55)	(-2.96)	(-2.68)
Treat × Post	0.29***	0.30***	0.18***	0.26***
	(3.38)	(3.98)	(7.72)	(4.37)
Bond & County Controls	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y
FE: Purpose, Rating × Agency	Y	Y	Y	Y
Adjusted R^2	0.84	0.84	0.84	0.84
Observations	256368	256368	254991	256368

This table reports the heterogeneous effects of contamination discovery on the offering yields of bonds issued by municipalities from counties with different socioeconomic characteristics **X**. Counties are classified based on whether **X** is above or below the cross-sectional median. **X** is the percentage of households owning real estate and homes in columns 1 and 2; state-level tax privilege in column 3, from Babina et al. (2021); and socioeconomic resilience from the social vulnerability data in column 4. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 7
Effect on Municipal Operations

	Revenues			Expenditures		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome (in Ln) =	Total Rev.	Total Taxes	Property Taxes	Total	General	General Current
Treat × Post	-0.076***	-0.078***	-0.073***	-0.070***	-0.074***	-0.074***
	(-4.11)	(-3.75)	(-3.22)	(-3.72)	(-3.81)	(-3.80)
County Controls	Y	Y	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.97	0.96	0.96	0.96	0.96	0.96
Observations	5558	5544	5498	5556	5556	5556

This table reports the effects of contamination discovery on county revenue and expenditure. Columns 1 to 3 report the regressions of total revenue, total tax, and property tax, while columns 4 to 6 report the regressions of total expenditure, general expenditure, and general current expenditure, all expressed in natural logarithms. General expenditure is expenditures to individuals or agencies outside the government. General current expenditure is the part of general expenditure that is used for current operations. Other revenues and expenditures are self-explanatory. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 8
Effect on Public Employment

Specification =	DD: All Counties			DDD: Third Diff. $\mathbb{X} = \left[\frac{\text{Total Interest}}{\text{Property Taxes}}\right]_{2015}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome =	Ln(PubEmp)	$\frac{\text{PubEmp}}{\text{AllEmp}}$	$\frac{\text{PubEmp}}{\text{Pop (000s)}}$	Ln(PubEmp)	$\frac{\text{PubEmp}}{\text{AllEmp}}$	$\frac{\text{PubEmp}}{\text{Pop (000s)}}$
Treat × Post × $\mathbb{1}[\mathbf{X} \geq \text{Median}]$				-0.128**	-0.006*	-1.623*
				(-2.06)	(-1.81)	(-1.90)
Treat × Post	-0.132***	-0.007***	-2.059***	-0.053	-0.004*	-1.096*
	(-4.77)	(-3.50)	(-5.68)	(-1.19)	(-1.92)	(-1.86)
County Controls	Y	Y	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.95	0.74	0.85	0.95	0.79	0.85
Observations	5264	5332	5332	4488	4544	4544

This table reports the effects of contamination discovery on county public employment in columns 1 through 3 and the differential effects comparing counties with high and low burden of interest payments in columns 3 through 6. A county is classified as having high-interest burden if the ratio of total interest payments (aggregated across all municipalities and county governments) to property taxes (similarly aggregated) is higher than the sample median in the pre-event year 2015. $Ln(PubEmp)$ is the natural logarithm of full-time equivalent public sector employment, $PubEmp/AllEmp$ is the ratio of public to all employment, and $PubEmp/Pop(000s)$ is public employment per 1,000 population. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 9
Effect on Out-migration

Measure Type =	Individuals			Households			Adjusted Gross Income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome =	Ln(Out)	$\frac{\text{(Out – In)}}{\text{L.Res.}}$%	$\frac{\text{Out}}{\text{In}}$%	Ln(Out)	$\frac{\text{(Out – In)}}{\text{L.Res.}}$%	$\frac{\text{Out}}{\text{In}}$%	Ln(AGI Out)	$\frac{\text{AGI In}}{\text{AGI Out}}$%	$\frac{\text{AGI In}}{\text{L.AGI Res.}}$%	$\frac{\text{AGI Out}}{\text{L.AGI Res.}}$%
Treat × Post	0.024***	0.092***	2.448***	0.018***	0.073***	1.865***	0.038***	-5.695***	-0.172***	-0.030
	(3.92)	(2.93)	(3.44)	(3.46)	(2.82)	(3.32)	(3.97)	(-4.43)	(-4.70)	(-0.74)
Ln(In)	0.017	-4.134***	-98.672***	-0.025	-4.225***	-103.702***				
	(0.45)	(-23.63)	(-21.04)	(-0.68)	(-25.25)	(-24.22)				
Ln(AGI In)							-0.097**	135.268***	3.898***	-0.632*
							(-2.39)	(17.46)	(20.73)	(-1.79)
County Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.997	0.797	0.829	0.998	0.815	0.832	0.993	0.742	0.949	0.701
Observations	6958	5796	6958	6958	5796	6958	6952	6952	5794	5794

This table reports the effects of contamination discovery on the out-migration of individuals in columns 1 to 3, on the out-migration of households in columns 4 to 6, and on county adjusted gross income (AGI) in columns 7 to 10. The sample focuses on migration to and from other counties within the same state. The sample period is from 2013 to 2018. *Ln*(*Out*) and *Ln*(*In*) are the number of out-migrants and in-migrants of a county expressed in natural logarithms; (*Out-In*)/*L. Res.* is net out-migration from a county scaled by the county's residents in the previous year; *Out*/*In* is the ratio of out-migrants to in-migrants; *Ln*(*AGI Out*) and *Ln*(*AGI In*) are county-aggregated AGI of out-migrants and in-migrants expressed in natural logarithms; *AGI In*/*AGI Out* is AGI inflows relative to outflows; and *AGI In*/*L. AGI Res.* and *AGI Out*/*L. AGI Res.* are AGI inflows and outflows scaled by the one year lag of residents' AGI, respectively. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 10
Effect on Wage and Firm Dynamics

	(1)	(2)	(3)	(4)	(5)
	Ln(Avg. Payroll)	# Firm Death	# Estab. Death	# Job Creation	# Job Destruction
Treat × Post × Tradable	0.050**	3.165***	2.903**	-246.403**	-191.350
	(1.99)	(2.70)	(2.45)	(-2.30)	(-1.48)
Treat × Post	0.008	-1.730**	-1.423*	131.105**	264.903***
	(0.67)	(-2.10)	(-1.66)	(2.39)	(3.05)
Post × Tradable	-0.006	0.583	0.324	-86.467***	-269.574***
	(-0.36)	(1.09)	(0.58)	(-2.99)	(-4.81)
Treat × Tradable	0.001	-46.268***	-47.809***	-1253.845***	-899.070***
	(0.02)	(-3.08)	(-3.09)	(-3.01)	(-3.40)
Tradable	0.695***	-43.332***	-44.128***	-1119.572***	-803.010***
	(29.45)	(-7.37)	(-7.35)	(-6.34)	(-6.57)
County Controls	Y	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y	Y
Adjusted R^2	0.75	0.73	0.72	0.84	0.87
Observations	12525	14028	14028	14028	14028

This table reports the effect of contamination discovery on wages and firm dynamics in tradable relative to non-tradable sectors. The dependent variable is the natural logarithm of average payroll, the number of firm deaths, the number of establishment deaths, the number of job creation, and the number of job destruction, respectively, from column 1 to 5. *Tradable* equals one for tradable industries and zero for non-tradable industries. These industries are defined following Mian and Sufi (2014). *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 11
Effect on Self-Employment

	(1)	(2)	(3)	(4)
Outcome =	Unemp. Rate	Ln(Non-employer Biz.)	Ln(Self-Emp.)	Self-Emp. HH %
Treat × Post	0.100***	0.010***	0.011***	0.135***
	(2.61)	(3.92)	(4.31)	(3.88)
County Controls	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y
Adjusted R^2	0.97	1.00	1.00	0.99
Observations	6972	6960	6972	6972

This table reports the effects of contamination discovery on unemployment and self-employment. The dependent variable is county unemployment rate, the natural logarithm of nonemployer business, the natural logarithm of self-employment, and the share of self-employed households, respectively, from column 1 to 4. Self-employment is defined by the number of returns with self-employment tax. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 12
Health-Related Outcomes

A: Participation in Health Insurance: DD Estimation over 2013 till 2018		
	Any Hlth. Ins. (%)	Pvt. Hlth. Ins. (%)
	(1)	(2)
Treat × Post	0.369*	0.368**
	(1.93)	(2.14)
County Controls	Y	Y
FE: County, Pair × Year	Y	Y
Adjusted R^2	0.921	0.983
Observations	1488	1488

B: First-Difference in PFAS-related Health Conditions over Long-Term			
	Cancer Mortality over 1999–2016	Pregnancy Outcomes over 1990–2002	
	(1)	(2)	(3)
Condition =	Kidney (/100K)	Eclampsia (%)	# Congenital Anomalies
Treat	0.441***	0.050*	0.014**
	(2.92)	(1.73)	(2.54)
County Controls	Y	Y	Y
FE: Pair, Year	Y	Y	Y
Adjusted R^2	0.72	0.54	0.08
Observations	3403	6049	5917

Panel A reports the effects of contamination discovery on health insurance coverage. The dependent variable is the share of county population with any health insurance coverage in column 1 and the share of county population with private health insurance coverage in column 2. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. Panel B compares the health outcome difference in treated and control counties over the long term. In column 1, the dependent variable is the mortality rate per 100,000 population due to kidney cancer in a county. The sample period is 1999–2016. In columns 2 and 3, the dependent variables are the share of new singleton births in a county where the mother had eclampsia and the average number of congenital anomalies in a county per singleton birth with any congenital anomaly. The sample period is 1990–2002. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for $t \geq 2016$ and 0 for the earlier periods. The regressions include county controls and fixed effects for county and pair×year. County controls included in all columns and panels except for columns 2 and 3 of panel B are defined in Table 1. County controls in columns 2 and 3 of panel B only include *DC3M Plant*, *Ln* (*Population*), and *Ln* (*Income Per Capita*), also defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Internet Appendix to

"Municipal and Economic Consequences of PFAS Contamination Discovery"

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A Background Information on PFAS

PFAS are a family of thousands of synthetic chemicals, about 4,730 currently on record (Abunada et al. 2020). Among them, PFOS and PFOA were the earliest to be developed, have the longest manufacturing history, and are the most thoroughly understood. A wide variety of consumer products and industrial processes have historically made use of these chemicals, e.g., nonstick cookware, grease-resistant food packages, stain- and water-resistant clothes, shaving creams, and fire-fighting foams (Glüge et al. 2020). These are highly toxic and extremely soluble in water. PFAS are currently being researched for adverse developmental, reproductive, and systemic health consequences (Panieri et al. 2022). They have already been linked to cancer, immunosuppression, endocrine disruptions, and cholesterol complications (Barry et al. 2013; Grandjean et al. 2012; Sunderland et al. 2019; C8 Science Panel n.d.), reproductive health (Waterfield et al. 2020) and infant health (Liu et al. 2023; Padula et al. 2023). Table (A.1) summarizes key events related to PFAS in the U.S. Dorrance et al. 2017 discusses manufacturing history, chemical properties and remediation challenges of PFAS. Johnson 2021 details the challenges of regulating them. DeWitt et al. 2015 provides a comprehensive technical discussion of their health effects.

Table A.1
Major Developments Surrounding PFAS in the U.S.

	Before 2000
1940–1950	◦ 3M invented PFOA
	◦ DuPont purchased PFOA to produce Teflon
1999	◦ Lawsuit brought against DuPont (Tennant v. E. I. du Pont de Nemours and Company)
2000	◦ 3M ceased production of PFOA
	◦ DuPont started to manufacture PFOA on its own
	2001–2010
2001	◦ A class-action suit was filed against DuPont by 70,000 people in 6 water districts (Leach, et al v. E. I. DuPont de Nemours and Co.)
	◦ The EPA began investigation
2004	◦ DuPont settled the class-action suit
	◦ The C8 science panel was formed to evaluate if there was a "probable link" between PFOA and any diseases
2005	◦ The EPA fined DuPont $16.5 million
2009	◦ The EPA set a provisional limit of 0.4 ppb for short-term exposure
	◦ GenX, a short-chain PFAS, was introduced to replace PFOA
	2011–2024
2011	◦ The C8 science panel started to publish reports linking PFOA to high cholesterol, ulcerative colitis, thyroid disease, testicular cancer, kidney cancer, and hypertension
	◦ Class members started to file personal injury suits (IN RE: E. I. du Pont de Nemours and Company C-8 Personal Injury Litigation)
2013–2015	◦ DuPont ceased production and use of PFOA
	◦ UCMR 3 testing of 6 PFAS (PFBS, PFHxS, PFHpA, PFOA, PFOS, PFNA) in all 4,064 public water supplies serving >10,000 individuals and 800 public water supplies serving <10,000 individuals
2016	◦ Apr: UCMR 3 data were released
	◦ May 19: For the first time, a non-enforceable Lifetime Health Advisory limit on PFAS was set at 70 ppt by the EPA (EPA 2016).
	◦ Aug 9: The Harvard study was published
2017	◦ Feb: DuPont settled all the personal injury suits
	◦ Aug: The Pentagon tested drinking water of military installations and nearby communities
2018	◦ Mar: The Pentagon report was released
	◦ Jul 29: Michigan declared state of emergency for Kalamazoo county due to high concentrations of PFAS in drinking water
2019–2020	◦ Over 80 bills related to PFAS were introduced in the 116th Congress
2023	◦ 1,938 contamination sites discovered across the U.S. (The PFAS Project Lab).
2024	◦ Apr: EPA established maximum contaminant levels for PFOA (4 ppt), PFOS (4 ppt), PFHxS (10 ppt), PFNA (10 ppt), and GenX (10 ppt) in drinking water and designated PFOA and PFOS as hazardous substances under CERCLA.

This table summarizes major events realted to PFAS in the U.S. from 1940's till 2023. The details are adapted from Rich (2016), Soechtig and Seifert (2018), PFAS Project Lab (2023a), the two court cases, In re E. I. Du Pont De Nemours & Co. C-8 Pers. Injury Litig. (2019) and Leach v. E. I. du Pont de Nemours and Company (2014), and EPA (2024).

B Data Appendix

B.1 Rating Definition for use in *Rating*×*Agency* Fixed Effects

We follow the following steps to code rating variable to implement the fixed effects for *Rating*×*Agency* in the regressions. We begin with all rating types (underlying long-term rating, underlying short-term rating, enhanced long-term rating, enhanced short-term rating and others) assigned to a bond (CUSIP) prior to, or within 60 days, of its dated date. We convert the ratings into numerical values, assigning a value of 1 to AAA (S&P or Fitch) and Aaa (Moody's), 2 to AA+ (S&P or Fitch) and Aa1 (Moody's), and so on, until 10 to BBB- (S&P or Fitch) and Baa3 (Moody's). The rating categories worse than this are very sparsely populated. However, in order to retain these bonds in the regression sample while ensuring that sufficient number of bonds are available within each *Rating*×*Agency* combination necessary for reliable estimation of within FE estimator, we assign these bonds a value of 11. All bonds with no ratings, withdrawn ratings, or those not matched between Eikon and the rating datasets are assigned a value of 12 and a rating agency "none". We then create the FE categories for *Rating*×*Agency*.

B.2 Rating Definition for use as Outcome Variable

We follow the following steps to create the rating variable that is used as outcome variable in columns 1 and 2 of Table 3. We begin with long-term underlying rating assigned to a bond (CUSIP) prior to, or within 60 days, of its dated date. We convert the ratings into numerical values, assigning a value of 1 to AAA (S&P or Fitch) or Aaa (Moody's), 2 to AA+ or Aa1, and so on, and 21 to the worst rating. All bonds, except a few, fall within this range. A few bonds that receive uncommon ratings (PIF, F1, etc. from Fitch; MIG, P-1, etc. from Moody's; and SP-1, SP-2 etc. from S&P), withdrawn rating, and no rating are assigned the numerical value of 22. For bonds in Eikon data that cannot be matched to the rating dataset, we assign a value of 23. If a bond is left with multiple ratings following the above steps, we assign it the worst one.

C Additional Regression Results

Table C.1
PFAS Contamination and Municipal Bond Offering Yields: No Rating× Agency FE

Outcome =	YTM							
Geographic Criterion =	Bordering Counties				Second-order Counties			
Contaminant =	All Six PFAS		PFOA or PFOS		All Six PFAS		PFOA or PFOS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.13***	0.13***	0.11***	0.14***	0.09***	0.13***	0.09***	0.13***
	(6.73)	(6.73)	(6.98)	(7.34)	(5.66)	(6.07)	(5.66)	(6.07)
Controls: (B)ond/(C)ounty	B + C	B + C	B	B + C	B	B + C	B	B + C
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Purpose	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.83	0.83	0.82	0.83	0.83	0.83	0.83	0.83
Observations	256368	256368	252583	252010	410838	410703	410838	410703

This table replicates the results of Table 2 without incorporating *Rating×Agency* fixed effects. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Columns 1 and 2 include bonds from counties contaminated by any of the six PFAS, along with bonds from adjacent, uncontaminated counties within the same state. Columns 3 and 4 focus on bonds from counties contaminated specifically by PFOA and PFOS, alongside bonds from neighboring counties within the same state that did not detect any of the six PFAS. Columns 5 through 8 replicate the estimates from columns 1 to 4, while using bonds from same-state counties adjacent to the previous control counties as controls. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.2
PFAS Contamination and Municipal Bond Offering Yields: Urban Treated and Urban Control Counties

Outcome =	YTM							
Geographic Criterion =	Bordering Counties				Second-order Counties			
Contaminant =	All Six PFAS		PFOA or PFOS		All Six PFAS		PFOA or PFOS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.10***	0.14***	0.10***	0.14***	0.10***	0.15***	0.09***	0.14***
	(5.20)	(5.51)	(5.77)	(5.71)	(5.95)	(6.39)	(4.88)	(5.66)
Controls: (B)ond/(C)ounty	B	B + C	B	B + C	B	B + C	B	B + C
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Purpose, Rating × Agency	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.84	0.84	0.84	0.84	0.84	0.84	0.84	0.84
Observations	243441	242318	239196	238623	437722	436488	391863	391728

This table replicates the results of Table 2 estimated only for county pairs where both treated and control county are urban. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Columns 1 and 2 include bonds from counties contaminated by any of the six PFAS, along with bonds from adjacent, uncontaminated counties within the same state. Columns 3 and 4 focus on bonds from counties contaminated specifically by PFOA and PFOS, alongside bonds from neighboring counties within the same state that did not detect any of the six PFAS. Columns 5 through 8 replicate the estimates from columns 1 to 4, while using bonds from same-state counties adjacent to the previous control counties as controls. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.3
PFAS Contamination and Municipal Bond Offering Yields: Only General Purpose Bonds

Outcome =	YTM							
Geographic Criterion =	Bordering Counties				Second-order Counties			
Contaminant =	All Six PFAS		PFOA or PFOS		All Six PFAS		PFOA or PFOS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.06***	0.10***	0.07***	0.10***	0.09***	0.11***	0.07***	0.10***
	(4.05)	(5.92)	(4.99)	(6.73)	(5.79)	(6.62)	(4.91)	(5.81)
Controls: (B)ond/(C)ounty	B	B + C	B	B + C	B	B + C	B	B + C
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Rating × Agency	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.87	0.87	0.87	0.87	0.87	0.88	0.87	0.88
Observations	151954	151336	149179	148807	274431	273668	244706	244706

This table replicates the results of Table 2 estimated only for bonds whose purpose type is "General Purpose or Public Improvements", identified as type "G" in Eikon dataset. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Columns 1 and 2 include bonds from counties contaminated by any of the six PFAS, along with bonds from adjacent, uncontaminated counties within the same state. Columns 3 and 4 focus on bonds from counties contaminated specifically by PFOA and PFOS, alongside bonds from neighboring counties within the same state that did not detect any of the six PFAS. Columns 5 through 8 replicate the estimates from columns 1 to 4, while using bonds from same-state counties adjacent to the previous control counties as controls. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and t-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.4
PFAS Contamination and Municipal Bond Offering Yields: Issue Year×Mat. Year FE

Outcome =	YTM							
Geographic Criterion =	Bordering Counties				Second-order Counties			
Contaminant =	All Six PFAS		PFOA or PFOS		All Six PFAS		PFOA or PFOS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.09***	0.10***	0.10***	0.11***	0.09***	0.12***	0.08***	0.10***
	(5.50)	(5.50)	(6.12)	(5.85)	(6.07)	(6.08)	(5.06)	(5.24)
Controls: (B)ond/(C)ounty	B	B + C	B	B + C	B	B + C	B	B + C
FE: County, Pair×IssueYear×Mat.Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Purpose, Rating × Agency	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.87
Observations	239873	239120	235212	235158	432592	432166	384226	384201

This table replicates the results of Table 2 with additional fixed effects for "issue year×maturity-year included. The sample period is from 2013 to 2018. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. Columns 1 and 2 include bonds from counties contaminated by any of the six PFAS, along with bonds from adjacent, uncontaminated counties within the same state. Columns 3 and 4 focus on bonds from counties contaminated specifically by PFOA and PFOS, alongside bonds from neighboring counties within the same state that did not detect any of the six PFAS. Columns 5 through 8 replicate the estimates from columns 1 to 4, while using bonds from same-state counties adjacent to the previous control counties as controls. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.5
PFAS Contamination and Municipal Bond Offering Yields: Zip Code Strategy

Outcome =	YTM							
Treat Zipcodes:	Within 3 Miles				Within 5 Miles			
Control Zipcodes:	Within 3–5 Miles				Within 5–7 Miles			
Contaminant =	All Six PFAS		PFOA or PFOS		All Six PFAS		PFOA or PFOS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.02	0.04	0.03	0.07	0.14	0.13	0.14	0.13
	(0.07)	(0.15)	(0.11)	(0.27)	(1.24)	(1.30)	(1.20)	(1.26)
Controls: (B)ond/(C)ounty	B	B + C	B	B + C	B	B + C	B	B + C
FE: (Z)ip code, Z Pair × Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Purpose, Rating × Agency	Y	Y	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.90	0.90	0.91	0.91	0.88	0.88	0.88	0.89
Observations	47727	47706	42201	42201	76089	76089	69273	69273

This table is similar to Table 2 with the distinction that it shows the results of a zip code pair strategy, instead of county pair strategy. The regression follows zip code version of equation 5. In columns 1 through 4, treat zip codes consists of contaminated zip codes and those located within 3 miles of it, and control consists of those located within 3 to 5 miles of the contaminated zip codes. Columns 1 and 2 include bonds from zip codes contaminated by any of the six PFAS, along with bonds from control zip codes. Columns 3 and 4 focus on bonds from zip codes contaminated specifically by PFOA and PFOS, along with bonds from control zip codes that were uncontaminated with any of the six PFAS. Columns 5 through 8 replicate the design from columns 1 to 4 in an alternate sample where treat zip codes consists of contaminated zip codes and those located within 5 miles of it, and control consists of those located within 5 to 7 miles of the contaminated zip codes. The regressions include bond and county controls, and fixed effects for rating×agency, zip code, and zip-code-pair×year. Bond and county control variables are summarized in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.6
Effect on Rating: PPML Regression

	(1)	(2)
Outcome =	Rating	Rating+
Treat × Post	0.16***	0.11**
	(2.68)	(2.37)
Bond & County Controls	Y	Y
FE: County, Pair × Year	Y	Y
FE: Purpose	N	N
Pseudo R^2	0.18	0.27
Observations	60276	140531

This table reports the effects of contamination discovery on credit risk using PPML regression. The sample period is from 2013 to 2018. Columns 1 and 2 report the DD regressions using PPML of the ratings of non-GO bonds. Compared to column 1, column 2 includes bonds without ratings as the lowest rated bonds. Other variables are defined in Table 2. The regressions include bond and county controls, bond purpose fixed effects, county fixed effects, pair-year fixed effects, and rating × agency fixed effects when indicated. Bond and county control variables are summarized in Table 1. Standard errors are clustered by county, and t-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.7
Robustness for Table 5: Using Ln(# Bonds) as Outcome

Outcome =	Ln(# Bonds)				
	(1)	(2)	(3)	(4)	(5)
Fund Type =	All Funds	Retail Funds	Inst. Funds	Index Funds	ETFs
Affected Bond × Post	-0.006***	-0.005***	-0.008***	-0.017	-0.016
	(-5.24)	(-4.28)	(-3.00)	(-1.23)	(-0.83)
Controls	Y	Y	Y	Y	Y
FE: Fund×CUSIP	Y	Y	Y	Y	Y
FE: Year, Maturity Year	Y	Y	Y	Y	Y
Adjusted R^2	0.99	0.99	0.98	0.95	0.95
Observations	2407225	1607976	796190	113503	79158

This table replicates the results of Table 5 where the outcome is specified in terms of the natural logarithm of the number of bonds held by a mutual fund. It reports the effects of contamination discovery on the allocation decision of mutual funds in the bonds issued by municipalities from the affected and the bordering, unaffected counties. The sample includes all funds in column 1, retail funds in column 2, institutional funds in column 3, index funds in column 4, and exchange traded funds in column 5. The sample period is from 2013 to 2018. The dependent variable is the natural logarithm of the number of bond i's shares held by fund f in year-quarter q. *Affected Bond* equals 1 if the CUSIP of the bond held by mutual fund is issued by a municipal issuer (identified using 6-digit CUSIP) from the contaminated counties and 0 otherwise. *Post* takes the value of 1 for $q \geq$ Q3, 2016 and 0 for the earlier periods. *Controls* include management fees, the expense ratio, one quarter lagged bond's yield, and the natural logarithm of one-quarter lagged fund's total net assets and market value of the underlying bond. The regressions include fixed effects for fund×CUSIP, year, and bond's year of maturity. Standard errors are clustered by fund, and t-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.8
Robustness for Table 6 using Mean Cuts, instead of Median

Outcome =	YTM			
	(1)	(2)	(3)	(4)
Third Diff. $\mathbb{X}$ =	% HH Homes	% HH Real Est.	Tax Privilege	Socioeco. Resil.
Treat × Post × $\mathbb{1}[\mathbf{X} \geq \text{Mean}]$	-0.16*	-0.17**	-0.07*	-0.18***
	(-1.95)	(-2.18)	(-1.75)	(-2.70)
Treat × Post	0.25***	0.26***	0.19***	0.26***
	(3.29)	(3.61)	(5.94)	(4.38)
Bond & County Controls	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y
FE: Purpose, Rating × Agency	Y	Y	Y	Y
Adjusted R^2	0.84	0.84	0.84	0.84
Observations	256368	256368	254991	256368

This table replicates the results in Table 6 using cross-sectional mean, instead of median, of the socioeconomic characteristics **X** to divide the baseline sample into respective low and high groups. **X** is the percentage of households owning real estate and homes in columns 1 and 2; state-level tax privilege in column 3, from Babina et al. (2021); and socioeconomic resilience from the social vulnerability data in column 4. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for bonds with sale date ≥ August 9, 2016 and zero otherwise. The regressions include bond and county controls and fixed effects for bond purpose, rating×agency, county, and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.9
Effect on Public Employment

Third Diff. $\mathbf{X}$ =	$\left[\frac{\text{Total Debt}}{\text{Property Taxes}}\right]_{2015}$			$\left[\frac{\text{Total Interest}}{\text{Total Revenue}}\right]_{2015}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome =	Ln(PubEmp)	$\frac{\text{PubEmp}}{\text{AllEmp}}$	$\frac{\text{PubEmp}}{\text{Pop (000s)}}$	Ln(PubEmp)	$\frac{\text{PubEmp}}{\text{AllEmp}}$	$\frac{\text{PubEmp}}{\text{Pop (000s)}}$
Treat × Post × $\mathbb{1}[\mathbf{X} \geq \text{Median}]$	-0.105*	-0.005*	-1.448*	-0.162**	-0.005	-0.811
	(-1.74)	(-1.73)	(-1.75)	(-2.31)	(-1.59)	(-0.96)
Treat × Post	-0.069*	-0.005**	-1.243**	-0.003	-0.003	-1.090*
	(-1.70)	(-2.32)	(-2.06)	(-0.05)	(-1.10)	(-1.67)
County Controls	Y	Y	Y	Y	Y	Y
FE: County, Pair × Year	Y	Y	Y	Y	Y	Y
Adjusted R^2	0.95	0.79	0.85	0.95	0.79	0.85
Observations	4488	4544	4544	4544	4600	4600

This table replicates columns 3 through 6 of Table 8 using alternate definitions for financial distress variable as the third-difference variable **X** in the DDD specification. In columns 1 through 3, **X** is the ratio of total outstanding debt to property taxes. In columns 4 through 6, **X** is the ratio of total interest to total revenue. A county is classified as having high financial burden if the variable **X** is higher than the sample median in the pre-event year 2015. $Ln(PubEmp)$ is the natural logarithm of full-time equivalent public sector employment, $PubEmp/AllEmp$ is the ratio of public to all employment, and $PubEmp/Pop(000s)$ is public employment per 1,000 population. *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table C.10
Effect on Wage and Firm Dynamics: PPML Regression

	(1)	(2)	(3)	(4)	(5)
	Ln(Avg. Payroll)	# Firm Death	# Estab. Death	# Job Creation	# Job Destruction
Treat × Post × Tradable	0.050**	0.040*	0.042*	-0.073*	0.039
	(1.99)	(1.66)	(1.72)	(-1.93)	(0.78)
Treat × Post	0.008	-0.022***	-0.023***	0.002	-0.034**
	(0.67)	(-2.78)	(-2.92)	(0.20)	(-2.02)
Post × Tradable	-0.006	0.052***	0.047***	-0.015	-0.123***
	(-0.36)	(3.02)	(2.67)	(-0.70)	(-4.27)
Treat × Tradable	0.001	0.168	0.163	0.070	0.056
	(0.02)	(1.25)	(1.22)	(0.74)	(0.67)
Tradable	0.695***	-1.650***	-1.661***	-0.827***	-0.718***
	(29.45)	(-26.45)	(-26.81)	(-12.90)	(-14.15)
County Controls	Y	Y	Y	Y	Y
Regression	OLS	PPML	PPML	PPML	PPML
FE: County, Pair × Year	Y	Y	Y	Y	Y
(Adjusted/Pseudo*) R^2	0.75	0.95*	0.96*	0.97*	0.97*
Observations	12525	13492	13492	14028	14028

This table replicates results of Table 10 using PPML regression in column 2 through 5, instead of OLS. The dependent variable is the natural logarithm of average payroll, the number of firm deaths, the number of establishment deaths, the number of job creation, and the number of job destruction, respectively, from column 1 to 5. *Tradable* equals one for tradable industries and zero for non-tradable industries. These industries are defined following Mian and Sufi (2014). *Treat* equals one if the drinking water supply of a county was contaminated with PFAS and zero otherwise. *Post* equals one for year ≥ 2016 and zero otherwise. The regressions include county controls and fixed effects for county and pair×year. The controls are defined in Table 1. Standard errors are clustered by county, and *t*-statistics are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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