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TWO ESSAYS ON THE ROLE OF DEMAND-SIDE FACTORS IN MUNICIPAL BOND RISK PREMIA AND ENVIRONMENTAL REGULATION STRINGENCY IN THE U.S.

By

Abinash Pati
Doctor of Philosophy

Business

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An abstract of
A dissertation submitted to the Faculty of the
James T. Laney School of Graduate Studies of Emory University
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy
in Business
2023

Abstract

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The dissertation consists of two essays on demand-side factors, showing why and how considering demand-side factors is important in answering fundamental asset pricing questions, as well as understanding the breadth of factors that explain environmental regulation stringency in the US.

The first essay (Heterogeneous Investors and Risk Premiums in the Municipal Bond Market) studies how asset prices vary with the risk exposures of heterogeneous financial intermediaries in the municipal bond market. Banks with local bank branches, as marginal investors, price their interest rate risk exposure into offering yield spreads of bank-qualified bonds. The pricing of interest rate risk exhibits strong intertemporal heterogeneity, varying with the level of the federal funds rate. Banks with higher deposit market power charge lower risk premiums for bearing interest rate risk. Apart from banks, mutual funds provide liquidity services while investing in illiquid assets like municipal bonds. Funds with higher cash holdings, indicative of greater liquidity management needs, pay lower prices for more illiquid municipal bonds. The results highlight that in segmented markets, the risk exposures of heterogeneous intermediaries who are the marginal investors determine asset prices. The cost of financing for issuers has direct consequences for real investment and local infrastructure.

The second essay (Heterogeneity in Enforcement Stringency and Environmental Pollution in the U.S.) examines the relationship between local housing wealth and the stringency of environmental regulation enforcement in the United States. Using county-level variation in median home values driven by exogenous housing supply elasticity and mortgage rate shocks, the analysis shows that increases in local housing wealth lead regulators to significantly strengthen enforcement of clean air standards under the Clean Air Act. The effect is stronger in counties with higher social capital and in states with Democratic governors. Heightened enforcement compels local polluting plants to reduce future toxic releases by 3-6% and increase investments in abatement technologies like recycling. The findings highlight that decentralized environmental policy enforcement can become fragmented when local communities differ in their willingness to pay for environmental quality.

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Heterogeneous Investors and Risk Premiums in the Municipal Bond Market*

Abinash Pati †

March 1, 2023

Abstract

I study how asset prices vary with the risk exposures of heterogeneous financial intermediaries in the municipal bond market. Banks with local bank-branches, as marginal investors, price their interest rate risk exposure into offering yield spreads of bank-qualified bonds. The pricing of interest rate risk exhibits strong intertemporal heterogeneity, varying with the level of the federal funds rate. Similarly, consistent with their role in liquidity transformation, I find that the liquidity management need of municipal bond funds, as measured by their cash holdings, is positively associated with offering yield spreads of bonds, held by these funds at the time of their issuance. The results highlight that the risk exposures of intermediaries matter for asset prices in imperfectly competitive and segmented asset markets.

Keywords— Intermediary Asset Pricing, Municipal Bonds, Banks, Interest Rate Risk, Bond Mutual Funds, Liquidity Risk

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1. Introduction

A growing literature in finance has studied the important role of intermediaries as the marginal investors who set asset prices across asset classes.[1] In sufficiently segmented asset markets, the price of risk for otherwise similar assets may differ when their marginal investors differ in their risk-adjusted discount rates. This discount rate may vary across intermediaries, depending on the exposure of the intermediary to aggregate risks. Indeed, He and Krishnamurthy (2018) emphasize understanding which intermediaries matter for asset prices in which asset markets and in which states-of-the-world. The industrial organization of the relevant intermediaries, and hence the risk exposure they carry on their balance sheets, should therefore, in equilibrium, decide the quantities and prices of securities held by these intermediaries.

In this paper, I use the U.S. municipal bond market as a laboratory to study how heterogeneous marginal investors set risk prices in an imperfectly competitive capital market. Municipal bonds provide a low cost of capital to the public sector for the purpose of public infrastructure and development, while providing tax-exempt income to investors. The market for municipal bonds is characterized by scarce liquidity (Harris and Piwowar (2006)) and strong search frictions with persistent issuer-underwriter-investor relationships (Brancaccio and Kang (2021)); thus offering a nice setting to test the role of heterogeneous intermediaries in the price formation process.[2] Specifically, I hypothesize that the price of interest rate risk for municipal bonds issued in a given county is determined by the interest rate risk exposure of banks with a physical branch presence in the county. Later, I also test the hypothesis that municipal bond mutual funds' liquidity risk exposure matters for the price of liquidity risk for bonds held at issuance by these bond mutual funds.

How large is the share of bank holdings in the municipal bond market? U.S. chartered depository institutions held about 13% of all municipal debt in 2021. More importantly, bank holdings of munis are concentrated in a subset of bonds that are designated as bank-qualified (Dagostino (2018)).[3] This is because banks receive the federal tax exemption only when investing in the

[1] In the corporate bond market, Friewald and Nagler (2019) and He et al. (2022a) show that the balance sheet health of dealers, along with search and bargaining frictions explain a large fraction of the variation in yield spreads. In the treasury bond market, Haddad and Sraer (2020) show that an increase in banks' average exposure to interest rate risk forecasts an increase in treasury bond risk premia. He et al. (2022b) show that dealer balance sheet constraints were pivotal in explaining the stress in the Treasury bond market during the COVID-19 crisis. Gabaix et al. (2007) study the mortgage-backed securities market, and present evidence that the marginal investor pricing these assets is a specialized intermediary rather than a CAPM-type representative household. Siriwardane (2019) demonstrates that changes in intermediary capital explain as much variation in CDS spreads as standard credit factors.

[2] In 2019, there were about one million bonds outstanding from $\sim 50,000$ issuers in the \$4 trillion municipal bond market. In comparison, there were only about 40,000 different securities from $\sim 6,000$ issuers in the \$9.6 trillion corporate bond market.

[3] A bank-qualified bond is a tax-exempt obligation that is issued after August 7, 1986, by a qualified small issuer, which are local governments that raise a maximum of \$10M within the calendar year. Banks may deduct 80% of the

bank-qualified bonds. These bonds are usually placed locally, with banks that have physical bank branches in the county of issuance (Bergstresser and Orr (2014)).[4] This quasi-natural ownership segmentation motivates the following hypothesis - local banks'[5] balance sheet exposure to fluctuations in interest rates determines the price of interest rate risk for locally issued bank-qualified bonds. Intuitively, if banks' exposure to interest rate risk rises, placing fixed-rate long maturity bonds with these banks should become more expensive.

To construct a measure of bank exposure to interest rate risk, I use the income gap at the bank level, which is a popular measure of interest rate risk in the academic literature (Flannery and James (1984); Purnanandam (2007); Gomez et al. (2021)), and is extensively used by practitioners. I define the income gap as the difference between the dollar amount of a bank's assets that reprice or mature within a year and the dollar amount of liabilities that reprice or mature within a year, normalized by total assets. To construct the income gap measure, I use information from quarterly FR Y-9C filings of Bank Holding Corporations (BHC) that is reported to the Federal Reserve and quarterly FFIEC Call Reports, which all regulated commercial banks in the U.S. file with their primary regulator. To capture bank-qualified bonds' exposure to interest rate risk borne by banks, I construct my main explanatory variable, the local income gap, at the county level. I take a weighted average of the income gap of all banks that have a physical bank presence in the county, by using their deposit share (of all deposits raised in that particular county) as weights. This construction rests on the assumption that bank-qualified municipal bonds are placed locally with banks that have a local presence in that geography rather than with banks with no local bank-branches, and that banks' holdings of locally issued bank-qualified bonds are in proportion to their share of deposits in the county. [6]

In a sample of municipal bond offerings spanning from the second quarter of 1998 to the first quarter of 2020, I run regressions of the offering yields and tax-adjusted yield spreads of bank-qualified bonds on the four-quarter lagged local income gap measure.[7] The estimated coefficient is statistically significant, and suggests that a one standard deviation decrease in the local income gap is associated with an increase of about two basis points (bps) in offering yields. The effect

carrying cost of a "qualified tax-exempt obligation".

[4] Using California's detailed bond placement data, Yi (2021) finds that about 70% of bank-financed bonds are placed with banks have a physical branch in the county.

[5] Although these are national banks with business operations across state lines, I use the term "local" to denote banks with physical bank-branches in a county.

[6] To verify whether my results are sensitive to the deposit-share weighting assumption, I construct an equal-weighted local income gap measure, by assigning the same weight to all banks which have a branch-presence in the county.

[7] Multiple bonds are brought to the market in the same issue, and the issue price is usually decided months in advance to the offering date. To account for this staleness in pricing, I lag my income gap measure by four quarters. I obtain very similar results when I use the one or two quarters lagged local income gap measure.

is stronger when the local income gap measure is constructed using the income gap of only large banks. This suggests that large banks with physical bank branch presence in the county are the likely price setting investors for bank-qualified bonds. Next, towards a stronger claim for causality, I show that the local income gap doesn't affect offering yields of non-bank qualified bonds, where banks are much less likely to be the marginal investors. I also show that the interest rate risk effect doesn't vary by the credit rating of the bond, suggesting that omitted credit risk factors are unlikely to be correlated with the local income gap.[8] Finally, I also show that the results are unaffected by the inclusion of macroeconomic controls - the inflation rate, the quarterly growth in industrial production, and the current output gap.

The full sample analysis masks the strong inter-temporal heterogeneity in interest rate risk pricing. Banks' concerns over their IR risk exposure varies with the level and path of the federal funds rate. This can be understood through the effect of monetary policy on banks' profitability. Upon an interest rate hike or cut, two forces affect banks' net worth. Banks make capital gains (incur losses) on assets with long-term fixed-rate coupon payments, when interest rates go down (up). For banks with a higher income gap, rate cuts (hikes) decrease (increase) banks' net interest income going forward. Thus, upon a decrease in the fed funds rate, the negative impact on net interest income is more than offset by the positive impact on loan loss provisions and non-interest income (Altavilla et al. (2018)). But upon an interest rate hike, banks on average, see a decline in their equity values. This is exacerbated for banks with more long-term assets relative to short-term liabilities (lower income gap), which then experience more negative stock returns, as shown in Flannery and James (1984). Thus IR risk management is a greater concern for banks in periods of rising interest rates. This suggests an asymmetry in IR risk pricing over the interest rate cycle. I find that the pricing of IR risk is strongly associated with the path of the federal funds rate. I split my sample into five different time periods based on the path of policy rates at the start of every period. I find that local income gap has a strong effect on offering yield spreads in the 2005-2008 period (when the Fed returned to higher rates after a prolonged period of low interest rates), and in the preceding years between 1998 to 2000.[9] During the 2005-2008 period, a one std. deviation decrease in local income gap would have raised tax-adjusted yield spreads by 13 bps. Similarly during the 1998-2000 period, a one std. deviation decrease in local income gap raised tax-adjusted yield spreads by 6 bps. In contrast, the results suggest that the sensitivity of offering yield spreads

[8] Also, the within county variation in local income gap is as large as the between county income gap, suggesting that endogenous bank-county matching is unlikely.

[9] The Federal Reserve raised interest rates six times between June 1999 and May 2000 in an effort to cool the economy to achieve a "soft landing". In the wake of the dot-com crash and the subsequent 2001–2002 recession the Federal Reserve dramatically lowered interest rates to historically low levels, from about 6.5% to just 1%. Between 2004 and 2006, the Fed raised interest rates 17 times, increasing them from 1% to 5.25%, before pausing.

to the IR risk exposure of banks is much lower in periods of decreasing interest rates.

The income gap measure excludes core deposits from its construction. The reasoning is that deposit rates are usually sticky and do not change one-for-one with the fed funds rate. Yet, the "stickiness" of deposit rates and thus the interest rate sensitivity of deposits could exhibit substantial heterogeneity across banks. Indeed, Drechsler et al. (2017) argue that banks with higher deposit market power pay deposit rates that are low and relatively insensitive to interest rate changes. On the extensive margin, as argued in Drechsler et al. (2021), banks with more deposit market power hold longer duration assets. On the intensive margin, this should then imply that banks with higher deposit market power (and thus with lower IR risk sensitivity of future cash flows), should charge a lower compensation for bearing interest rate risk. To test this hypothesis, I construct the average bank Herfindahl-Hirschman index (HHI), which captures the exposure of a given county to funding conditions of its banks (i.e. banks with branches in the county) across all their deposit markets. Indeed, I find that a higher average bank HHI significantly lowers the negative correlation between the local income gap and offering yield spreads. Banks with higher deposit market power are thus relatively insulated to interest rate changes, and charge lower IR risk premiums.

The pricing of interest rate risk may also depend on the underwriting method. Intuitively, the IR risk of local banks is more likely to priced if the underwriting bank is situated locally, since local underwriting banks are more likely to know about demand from local bank branches, and hence would adjust the price of the bond accordingly. In a sample of school district bonds, Cestau (2019) shows that local underwriter banks specialize in negotiated method of sale, as it requires building relationships with local investors and issuers. Since local underwriters have a cost advantage in the specialization investments required for negotiated sales, they often dominate this market. I find that the sensitivity of offering yield spreads to IR risk exposure is higher for bonds that are placed through negotiated offerings.

A potential concern with the intermediary asset pricing channel may be that the risk exposure on the intermediaries' balance sheet reflects or correlates with households' risk exposures (Haddad and Muir (2021)). Indeed, when intermediaries simply take exposure to risk factors on behalf of households, as if they were merely a pass-through, it is the households who are the ultimate marginal investors. This is less of a concern in the municipal bond market setting, since whereas the cross-state variation in tax privilege incentivizes retail investors to hold within state issued munis, the federal tax exemption for banks for bank-qualified bonds doesn't vary across states. Since these banks operate across state lines and asset classes, it is unlikely that omitted variables pertaining to local households' risk exposures correlate with the interest rate risk exposure of banks.

My findings suggest that banks actively manage their interest rate risk exposure, especially during high interest rate regimes, and hence as marginal investors, price this risk exposure into asset prices. Banks have maturity-mismatched balance sheets, with long-duration nominal assets (e.g., fixed-rate mortgages) and short-duration nominal liabilities (e.g., deposits). This exposes them to both duration mismatch (interest rate risk) and funding risk (illiquidity). This raises a follow up question. If banks price their IR risk exposure, should they also be pricing their liquidity risk exposure? This is not clear ex-ante, as banks have a natural hedge against market-wide liquidity shocks, as shown in Gatev and Strahan (2006). Gatev and Strahan (2006) argue that unlike other intermediaries, only banks have funding inflows that co-vary negatively with market liquidity, and hence banks can insure firms against systematic declines in liquidity[10] at lower costs than other institutions. To test the liquidity risk hypothesis, I follow Acharya and Mora (2015), and construct various measures of liquidity risk exposure at the bank level. These include the share of illiquid & liquid assets, the undrawn commitments ratio, the wholesale funding ratio and the core deposits ratio. Similar to the local income gap measure, I construct my county level weighted bank liquidity risk ratios by weighting each bank measure by its share of the county's deposits. Regressing yield spreads at issuance on these county level liquidity risk ratios, I find no relationship between banks' liquidity risk exposures and municipal bond yield spreads. The hypothesis that banks do not price their liquidity risk exposure, thus, cannot be rejected.

The story so far has focused on offering yield spreads as the outcome variable; given a rise in the quantity of risk borne by the intermediary, the price must rise as well, to compensate the intermediary for bearing the risk. Now, the price of the asset need not necessarily be the sole margin of adjustment; as in equilibrium, there can be adjustments in other non-price terms like the quantity of asset issuance (e.g., in commercial bank lending), or terms specific to certain asset markets (e.g., covenants in the syndicated loan market). I find little evidence of changes on the extensive margin. In the full sample regressions, local income gap doesn't affect the probability of issuance, the bond size, the maturity or the callability of the bond.

Apart from banks, mutual funds provide liquidity services to their investors by allowing them to redeem their investment or part thereof at the fund's end-of-day net asset value (NAV).[11] Through their investments in illiquid assets such as corporate or municipal bonds, open-end mutual funds engage in substantial liquidity transformation. To accommodate inflows and outflows, rather than transacting in the underlying portfolio assets immediately and to minimize price impact, mu-

[10] Banks have traditionally provided backup liquidity in the form of loan commitments to many classes of borrowers

[11] They do so by pooling liquidation costs across their investors. If investors were to directly hold the underlying investments, they would have to bear their own liquidation costs when selling those assets.

tual funds hold cash buffers. Relying on a revealed preference argument, Chernenko and Sunderam (2016) argue that the cash-holdings of mutual funds can be used to measure their liquidity transformation needs.[12] This implies that bond funds with higher liquidity management needs, either would tend to hold more liquid securities, or on the intensive margin, would pay relatively lower prices for investing in more illiquid municipal bonds.

To test the above hypothesis, I gather bond mutual funds' holdings of municipal bonds, their cash holdings and other fund characteristics from the Center for Research in Security Prices (CRSP) database, starting from 2010Q1. I focus my analysis on only municipal bond mutual funds, defined as funds whose total holdings of municipal bonds is $> 50\%$ of its total net assets. I classify a bond mutual fund as the marginal investor in a bond if the fund reports holding the bond in the same quarter as the bond offering date. Next, I construct an equal weighted bond-level fund cash-holding measure (EWFCH) by aggregating the one quarter lagged cash-holdings of all bond mutual funds that hold the bond at issuance. Note that, this measure is zero for bonds that are not held by any mutual funds in the first quarter of issuance.

Since higher rated bonds tend to be more liquid, I use the credit rating of the bond as a proxy for liquidity (Li et al. (2021)), and interact it with the EWFCH measure. The assumption here is that any differential pricing of bonds conditional on their credit rating (default risk), by funds with varying levels of cash holdings, must originate from their liquidity transformation need. In other words, there is no other plausible reason why two bond funds with similar liquidity management needs would pay a different price for the same bond. Thus it is unlikely that a differential default risk premium channel could confound the use of credit ratings as a proxy for perceived liquidity. In all my regression specifications, I use the bond's credit rating interacted with the month of issuance to control for any time variation in the pricing of default risk. I use county-year fixed effects to control for any changes in local economic conditions that might be impacting the credit risk of bonds. I also use issuer fixed effects to control for any endogenous matching between issuers and bond funds, based on their credit risk profile or through sticky issuer-underwriter-investor ties.

Regressing offering yield spreads on EWFCH and its interaction with credit rating, I find a statistically significant positive correlation between EWFCH and offering yields. If bond funds with more cash holdings invest less in more liquid bonds (& thus bonds with lower yield spreads), the coefficient on EWFCH would be biased downward. The coefficient on the interaction term is negative, implying that *ceteris paribus*, funds with higher cash holdings pay more for bonds

[12] Consequently, Chernenko and Sunderam (2020) show that in illiquid asset markets, the strength of the cross-sectional relationship between mutual fund cash holdings and fund flow volatility can be used as a measure of bond market liquidity.

with higher credit ratings. In the next set of tests, I regress offering yields on other bond-investor characteristics such as the number of distinct bond funds that hold the bond at issuance, and the presence of back-end load fees. Given the limited number of investors in a bond and the limited ability of dealers to intermediate [13], the price impact of any given trade can be large. Bond funds are less likely to be concerned about bond liquidity, if there are other investors who can absorb the selling pressure from mutual funds. As long as bond funds do not face correlated liquidity shocks, having a wider investor base would be perceived as liquidity enhancing.[14] Similarly the presence of a rear-end load fee should also be associated with lower offering yield spreads. A rear-end load fee is charged when an investor redeems the mutual fund shares, and thus is associated with lower potential fund outflows and is indicative of better liquidity management. I find evidence consistent with the above hypotheses.

While the risk-based channel that I propose originates from the liquidity management need of bond mutual funds, resulting in a price discount for more illiquid bonds, the analysis could be potentially plagued by endogeneity concerns. Reverse causality could be a concern if bond mutual funds select into holding bonds with higher yields with a higher credit risk, and as a result hold larger cash buffers to mitigate against the risk of future default driven fund outflows.[15] To address this issue, I use the instrumental variable (IV) approach of Koijen and Yogo (2019), which is motivated by the idea that an investment mandate of a mutual fund is pre-determined and should be exogenous to contemporaneous shocks to issuers' credit risk. The IV exploits exogenous variation in mutual funds' demand for municipal bonds, which is driven by the cross-sectional composition of mutual funds that include these bonds in their mandates. The instrumental variable approach shows a stronger effect of liquidity management of bond funds on offering yield spreads. Conditional on mutual fund ownership, a one std. deviation increase in the EWFCH measure is associated with a ~ 10 bps increase in tax-adjusted offering yield spreads for unrated bonds whereas there is almost no effect on bonds in the highest credit-rating category (AAA bonds). Accounting for the mutual fund ownership effect, AAA offering yield spreads are reduced by ~ 6 bps when placed with bond mutual funds.

The liquidity management channel that I document, is different from the run-risk channel documented in Li et al. (2021). The authors show that after the Covid-19 crisis and price dislo-

[13] Li et al. (2021) show that dealers' willingness to intermediate trading is likely to decline in bonds facing larger mutual fund redemption risks. Not only is it challenging for muni dealers to locate potential buyers for mutual funds' bulk sales in a retail dominated market, but also mutual fund fire sales can subject dealers to losses if dealers keep mutual-fund-held bonds in their inventories.

[14] Li and Yu (2022) find that bonds with more investors (less concentration) have better liquidity and lower yield.

[15] Evidence of reaching for yield behavior has been documented among insurance firms, who are more likely to choose high yielding securities conditional on minimizing their risk adjusted capital ratios (Becker and Ivashina (2015)), and also among corporate bond mutual funds (Choi and Kronlund (2018)).

cations in the muni market, market participants learned about the destabilizing effect caused by mutual fund runs, and thus required higher compensation for holding bonds whose mutual fund owners were more susceptible to runs. In contrast to their study where the marginal investors pricing this run-risk are non-bond fund investors, in my channel, it is the bond funds. who anticipating the future liquidity of their bond holdings, adjust their liquidity management needs and demand a higher compensation for holding bonds with higher "perceived" illiquidity.

Overall my results highlight the key role of heterogeneous investors in the price formation process in segmented markets. The industrial organization of intermediaries drives the price of risk that borrowers in these markets pay to place their debt with these intermediaries. Risk pricing has direct consequences on real investment decisions and in the case of municipal bonds, an effect on the local public infrastructure and quality of living.[16] I focus on the pricing of two important risk factors, interest rate risk and liquidity risk. These risks have been made salient especially in the current market environment,[17] and my study underscores the importance of intermediaries who take on these risk exposures on their balance sheets, for asset prices.

2. Related Literature

My findings contribute to the now well established literature on intermediary asset pricing, which offer a new perspective for understanding risk premia. The seminal papers in this literature, Adrian et al. (2014) explore the pricing power of broker-dealer leverage for equities and US government bonds, while He et al. (2017) propose an empirical measure of the intermediary SDF based on broker/dealer capital ratio and expand the approach to include corporate bonds, foreign sovereign bonds, options, credit default swaps (CDS), commodities, and foreign exchange (FX). Changes in intermediary balance sheets have been linked to fluctuations in asset prices across asset markets.[18]

[16] Agrawal and Kim (2022) find that the collapse of the municipal bond insurance industry detrimentally affected municipalities that had relied more heavily on these insurers for water infrastructure financing, and led to a deterioration in drinking water quality.

[17] In the bid to control inflation, the Federal Reserve has continued to raise interest rates, raising rates from 0% at the onset of Covid-19 to 3.25% in September 2022. The rapid growth of shadow banking, including that of the asset management sector, has raised concerns about financial fragility. As experienced during the financial crisis of 2008, when market conditions unexpectedly deteriorated, investors ran on open-end funds, causing fire sales and market dislocations. Although the Fed intervened and stabilized markets across asset classes during the recent Covid-19 crisis, liquidity risk remains a major concern esp. with bond mutual funds. Further, Acharya and Rajan (2022) show that domestic banks even when they are flush with liquidity might shy away from intermediating this liquidity to non-bank financial institutions.

[18] Gabaix et al. (2007) study the pricing of prepayment risk in mortgage-backed securities (MBS), providing evidence to support intermediary asset pricing models. Ivashina et al. (2015) document that following the Eurozone sovereign crisis, U.S. money-market funds sharply reduced their exposure to European banks. Entering into FX swaps became costly since there was limited capital to take the other side of the swap trade. Consequently, dollar lending by Eurozone banks fell relative to their euro lending. Pan and Zeng (2017) show that large bond flow shocks to

Relative to this literature which has mainly focused on balance sheet constraints of intermediaries, my contribution focuses on intermediaries' actual underlying risk exposure as an important determinant of risk prices. The key takeaway is that given the segmentation in asset markets, the heterogeneous industrial organization of intermediaries matters for asset prices.

The findings in the paper also contribute to the nascent but growing literature on municipal finance. Although much of the literature has focused on the pricing of credit risk determinants [19], there is much less we know about the pricing of other risk factors. A notable exception is the study by Babina et al. (2021), who show that that the tax induced ownership segmentation of the muni bond market leads to local (state-specific) idiosyncratic risk being priced in offering yields of within-state bonds. Their findings are similar in spirit with Gabaix et al. (2007), who show that prepayment risk which is a wash in the aggregate is priced because the marginal investor is a specialized arbitrageur rather than a well diversified representative agent. My findings complement Babina et al. (2021) and show that the municipal bond market exhibits a stronger degree of market segmentation even at the county and the bond level, and that the price of risk for each asset is determined by intermediaries specific to the asset, not the asset class. More recently, there have been other papers focusing on the pass-through of capital supply shocks through different intermediaries to asset prices and issuance decisions.[20] In contrast to these papers which focus on how demand shocks from intermediaries impact muni bond prices, my study focuses on how risk management decisions of heterogeneous intermediaries impact bond prices.

Finally, this paper adds to the literature on optimal risk management in banking. The topic of why banks bear interest rate risk (or why do intermediaries in general take on exposures to aggregate risk), has received much attention in the theoretical literature.[21] Kirti (2020) shows that banks with more floating-rate liabilities make more floating-rate loans, hold more floating-rate

authorized participants balance sheets limits ETF arbitrage, leading to persistent relative mispricings. Du et al. (2018) provide sharp evidence tying the movements in CIP deviations to capital frictions in intermediation.

[19]Such default risk determinants include underfunded pensions of state governments (Novy-Marx and Rauh (2012)), newspaper closures (Gao et al. (2020)), opioid epidemics (Cornaggia et al. (2021), mass shootings (Chordia et al. (2022))), sea-level rise risk (Goldsmith-Pinkham et al. (2021)).

[20](Adelino et al. (2021) show causal effects of capital supply from mutual funds on municipal financing. Li et al. (2021) study the fragility in the muni bond market owing to outflows from bond mutual funds during COVID-19. Rossi et al. (2021)) show that insurance companies affected by Hurricane Katrina transmitted liquidity shocks to the real economy, resulting in an increase in borrowing costs in the primary market which led to lower investment in muni-reliant sectors.

[21]Diamond and Rajan (2001) show that non-contingent demand deposits emerge as the optimal contract when ex-ante bankers cannot credibly commit to employing their specialized human capital so that assets yield their highest payoffs. Diamond and Rajan (2012) introduce aggregate uncertainty in a model where the lack of commitment by managers gives rise to non-contingent deposit contracts. English et al. (2018) use high-frequency data around FOMC announcements to study how bank stock prices react to unexpected changes in the level and slope of the yield curve and find that bank stocks fall after interest rate increases. Paul (2020) extends this analysis by decomposing the slope of the yield curve into an expectations term and a term premium term, and shows that banks with a larger maturity mismatch benefit from a term premia increase.

securities, and quote lower prices for floating-rate loans. Thus as compensation for taking on interest rate risk on their balance sheets, banks charge a risk premium for investing in assets that increase their interest rate risk mismatch. The closest study to this paper is Haddad and Sraer (2020) who show that banks' balance sheet exposure to fluctuations in interest rates strongly forecasts excess treasury bond returns. Following their work, a question to be answered is how variation in bank's income gap transmits to bond risk premia, given that banks are fairly small investors in the treasury bond market relative to other institutional investors.[22] The other important difference is that their paper shows that it is only the aggregate income gap that matters for bond risk premia, suggesting that interest rate risk is shared among banks. My results instead show that as marginal investors in bank-qualified local bonds, banks price their own income gap into offering yields, which suggests that IR risk may not be shared amongst banks as suggested by Haddad and Sraer (2020).

3. DATA and Summary Statistics

3.1. Bank Variables

3.1.1. Income Gap

I use the income gap as banks' net exposure to interest rate risk.[23] The income gap is defined as,

$$\text{Income Gap } = A^{IR} - L^{IR}$$

where A^{IR} and L^{IR} denote interest rate-sensitive assets and liabilities that mature or reprice within the next year. A negative value of the income gap means that the bank holds more interest rate-sensitive liabilities than assets. If so, a increase in interest rate in the next year reduces its interest income, leaving the bank exposed to refinancing risk. In contrast, a positive value of the income gap leaves the bank vulnerable to decreases in the interest rate next year, and thus to reinvestment risk. As a measure of interest rate risk, the income gap has the appealing property that, in first approximation, changes in one year ahead net interest income are proportional to it, i.e.,

$$\Delta\text{Net Interest Income} \approx IncomeGap * \Delta r$$

[22] In 2014, private depository institutions held just 3.2% of all outstanding Treasuries. Their holdings are four times as much in the municipal bond market, and especially in the bank-qualified segment, banks have $\sim$ 80% market share.

[23] I follow Haddad and Sraer (2020) and Gomez et al. (2021) here.

where ΔNet Interest Income is the change in one year ahead net interest income arising from assets and liabilities and Δr the change in the level of the one-year ahead short-term interest rate. Thus the one-year income gap can be interpreted as the interest rate sensitivity of a bank's one-year ahead net interest margin (NIM), which is a natural forecasting horizon. In this sense, the income gap is considered a "cash flow measure" of interest rate risk. The NIM only captures cash flow shocks and not discount rate shocks, which is ideally what an interest rate risk measure should capture. There is sufficient evidence that the income gap is a satisfactory measure of interest rate risk. Gomez et al. (2021) show that the sensitivity of bank profits to interest rates increases significantly with their income gap. Flannery and James (1984) show that the income gap correlates with the sensitivity of common stock returns to interest rates. Banks with more long-term assets relative to short-term liabilities experience more negative stock returns following an increase in interest rates.

I use BHC (bank holding company) level information from FR Y-9C reports to construct the income gap measure.[24] However, the FR Y-9C data doesn't include several standalone and smaller banks. Therefore, I construct an equivalent measure based on the Call Report Data, which includes all commercial banking institutions in the US. For the sample of matched banks (banks which have both FR Y-9C and Call reports data), I verify that the income gap measures match closely. For all other bank level variables defined in Appendix table A.1, I build my panel data set from the quarterly FFIEC Quarterly Call Reports, which all regulated commercial banks file with their primary regulator. Following Williams (2020), I use data at the holding company level since financing decisions and asset allocation decisions are made at the holding company level. Because some banks are owned by a common holding company, I aggregate the bank-level data for banks with common ownership because these ownership ties could foster liquidity sharing across subsidiaries (see Houston et al. (1997)). Note that in calculating all the banking ratios, the numerator and denominator are separately summed for all the commercial banks within the holding company (for standalone banks, there is no summing involved). All the deposit-weight aggregated bank variables are defined at the county quarter level starting from 1997Q2. **Figure IA.3** shows the time-series evolution of the equal-weighted income gap. **Figure IA.5** and **Figure IA.6** show the frequency distribution of the local income gap and the bank level income gap. The map in **Figure 3.2** plots the local income gap across counties in the U.S. in the year 2005. As is evident from the map, there is considerable variation across in the local income gap measure. **Panel A of table 1** presents the summary statistics for the local income gap (LIG) variable. The average county LIG is 12.7% with a standard deviation of 12%. **Panel C of table 1** shows that the within county and between county standard deviation in the LIG measure is rather similar, suggesting that local income gap

[24] The exact construction is detailed in appendix table A.1

exhibits strong within county variation.

A potential concern with the income gap variable might be in the treatment of core deposits. Time and saving deposits account for over 60% of total funding. Following English et al. (2018), I assume all transaction deposits to have zero maturity, in effect excluding them interest rate risk calculations.[25] Time deposits, which usually have a lock-in period, are the ones included in interest risk calculation. However, if these core deposits adjust slightly to changes in the federal funds rate, the average income gap will overestimate the real income gap. To account for deposits, I construct a local income gap (LIG) with deposits measure, assuming that all non interest-bearing deposits have short maturity. The LIG with deposits variable has a mean of -0.2% versus 12.7% for the main LIG variable.

3.1.2. Other Bank Variables

I construct measures of bank deposit market concentration using the branch-level Summary of Deposits (SOD) data, which are available at the Federal Deposit Insurance Corporation (FDIC) website over the full period of my sample, 1997-2020. Since the FDIC collects total deposits in the SOD each June, I merge variables based on these data into the following four quarters of the Call Reports and FR Y-9C data. Following Drechsler et al. (2017) I build my measures of deposit market concentration starting from the bank-branch level and then aggregate it upto the bank and county-bank-level. The branch Herfindahl-Hirschman Index of deposit concentration for the local markets varies at the level of the county-year (c,t) and equals the sum of squared deposit market shares for all bank branches operating in a given county. The construction of the County HHI variable is detailed in **appendix table A.1**. The County HHI variable captures the competitive conditions in the county, but not the aggregate funding conditions of a given bank operating in the county. Gilje et al. (2016) show that most banks have branches in multiple counties and move funds across local markets to accommodate differential lending conditions at the local level. To capture a given bank's deposit funding condition, I build Bank HHI that varies by bank (b) and year (t) and which captures a given bank's average market power in raising deposits across all of the markets in which it has branches, weighted by the share of deposits the bank raises in each market. Finally, I build the Average Bank HHI that varies at the county-year level and captures the exposure of a given county to funding conditions across all banks operating within it. Average bank HHI and County HHI are correlated, as one is a weighted average of the other (the correlation coefficient is

[25] Drechsler et al. (2021) show that maturity transformation does not expose banks to interest rate risk— it hedges it. The reason is the deposit franchise, which allows banks to pay deposit rates that are low and insensitive to market interest rates. But since the income gap measure excludes core deposits from its calculation of interest rate sensitive liabilities, the income gap measure encapsulates Drechsler et al. (2021)'s findings.

0.72). The local income gap is negatively correlated with the average bank HHI variable, suggesting that banks raising deposits in concentrated markets have a lower income gap or take on a higher interest rate risk exposure.

To calculate the repricing maturity of bank assets and liabilities, I follow the same procedure as in Drechsler et al. (2021) who adapt their methodology from English et al. (2018). Banks report their holdings of five asset categories (residential mortgage loans, all other loans, Treasuries and agency debt, mortgage-backed securities (MBS) secured by residential mortgages, and other MBS) separated into six bins by repricing maturity interval (0 to 3 months, 3 to 12 months, 1 to 3 years, 3 to 5 years, 5 to 15 years, and over 15 years). To calculate the overall repricing maturity of a given asset category, I assign the interval midpoint to each bin (and 20 years to the last bin) and take a weighted average using the amounts in each bin as weights. Banks report the repricing maturity of their small and large time deposits by four intervals (0 to 3 months, 3 to 9 months, 1 to 3 years, and over 3 years). I assign the midpoint to each interval and five years to the last one. I assign zero repricing maturity to demandable deposits such as transaction and savings deposits. I also assign zero repricing maturity to wholesale funding such as repo and Fed funds purchased. I assume a repricing maturity of five years for subordinated debt. I compute the repricing maturity of liabilities as the weighted average of the repricing maturities of all of these categories. The local repricing gap is 4.2 years on average with a standard deviation of 1.5 years. Bank liquidity ratios are constructed following Acharya and Mora (2015), and their definition is provided in **Appendix table A.1 Panel A of table 1** presents the summary statistics for relevant bank level explanatory variables at the county year quarter level from 1997Q2 to 2019Q1. **Panel D of table 1** presents the summary statistics for variables at the bank-quarter level from 1997Q2 to 2019Q1.

3.2. Municipal Bonds

The offering yield and other attributes of each bond are collected from the Mergent Municipal Bond Securities database. The attributes of individual bonds include the state of issuance, issue series, issuance date, type of issue sale, maturity date, coupon rate, bond size, as well as bond ratings from Moody's, Standard and Poor's, and Fitch. Following Cornaggia et al. (2021), I convert character ratings into numeric ratings with 21 corresponding to the highest credit quality and 1 to the lowest. The Mergent database also provides information about whether the bond is general obligation, insured, and callable. I collect the county location of the municipal issuers from Bloomberg and SDC Platinum. This is done by geo-locating each bond to a county using the first six digits of the bond's CUSIP, which uniquely identifies the issuer. We collect from Bloomberg the 6-digit CUSIPs for all issuers that can be linked to a county. These issuers cover various forms of local governments, such as counties, cities, school districts, and special purpose districts. The County FIPS (Federal Information Processing Standards) code is the matching variable we use to merge the municipal bond data with data from local government finances and other county demographics. Following Chordia et al. (2022), I also gather the type of municipal issuers from the Electronic Municipal Market Access (EMMA) system to classify issuers into state and local governments. Following Gao et al. (2020), I exclude municipal bonds with a maturity of more than 100 years, a coupon rate greater than 20 percentage points, or a variable coupon rate.

As my primary outcome variable, aside from the raw offering yield, I use the tax-adjusted spread over an identical coupon synthetic treasury bond to proxy for the financing cost of municipal bonds. Since bonds are issued at different times and the offering yields of bonds change with interest rate and other macroeconomic factors, I cannot directly compare the raw yield of bonds. To get the bond yield spread, I first use the yield of a coupon-equivalent synthetic treasury bond by calculating the present value of its future coupon and principal payments using the U.S. Treasury yield curve from Gürkaynak et al. (2007). This present value calculation gives us the price of a synthetic treasury bond with the same payoff structure as the municipal bond, which is then used to calculate the yield-to-maturity on this synthetic treasury bond. Next, to account for the tax effect, I follow Schwert (2017) wherein the marginal tax rate impounded in the tax-exempt bond yields is assumed to be the top statutory income tax rate in each state. I obtain top income tax rates by state and year from the TAXSIM model provided by the NBER. Precisely, I compute the tax-adjustment factor as follows,

$$1 - \tau_{s,t} = (1 - \tau_t^{fed})(1 - \tau_{s,t}^{state})$$

where τ_t^{fed} is the top federal income tax rate and $\tau_{s,t}^{state}$ is the top income tax rate in state s in year t. After accounting for this tax adjustment factor, we calculate the municipal bond tax-adjusted yield spread as the difference between raw yield of municipal bond (divided by $1 - \tau_{s,t}$) and yield-to-maturity of the synthetic risk-free bond.

$$\textit{Tax-adjusted Yield Spread} = \frac{\textit{Raw Yield}}{1 - \tau_{s,t}} - \textit{Yield on Synthetic Treasury}$$

Panel A of Table 2 presents the summary statistics for bank-qualified bonds. There are 1,099,694 bank-qualified bonds in the entire sample period. These bonds have an average bond size of $357,054, issue size of $4.67 million, and maturity of 8.98 years. 37% of the bonds are insured, 71% are general obligation, in that they are backed by the tax revenue of the issuing municipality, and 46% are callable. Finally, 26% of the bonds are sold through negotiated method of sale. The average coupon rate is 3.52%, the raw yield (tax-adjusted yield spread) of the average bond is 3.096% (1.81%). For comparison, as reported in **Panel B of Table 2**, there are 1,154,939 non-bank qualified bonds in the entire sample period. These bonds have an average bond size of $3.01 million, issue size of $55 million, and maturity of 10.61 years. 40% of these bonds are insured, 52% are general obligation, 49% are callable, and 37% are sold through negotiated method of sale. The average coupon rate is 4.15%, the raw yield (tax-adjusted yield spread) of the bond in the control sample is 3.30% (2.05%). Overall, bank-qualified bonds have $\sim$ 20 bps lower offering yields than non-bank qualified bonds, and are more likely to be general obligation bonds with a lower coupon rate, and more likely to be offered through a negotiated method of sale.

3.2.1. Institutional Details of Bank-Qualified Bonds

Banks, like other investors, purchase municipal bonds in order to obtain the benefit of earning interest that is exempt from Federal income taxation. Historically, commercial banks were the major purchasers of tax-exempt bonds. Banks' demand for municipal bonds changed in 1986 with the passage of the Tax Reform Act of 1986, now under section 265(b) of the Internal Revenue Code of 1986, as amended. Under the Code, banks may not deduct the carrying cost (the interest expense incurred to purchase or carry an inventory of securities) of tax-exempt municipal bonds. For banks, this provision has the effect of eliminating the tax-exempt benefit of municipal bonds. An exception is included in the Code that allows banks to deduct 80% of the carrying cost of a "qualified tax-exempt obligation." In order for bonds to be qualified tax-exempt obligations the bonds must be (i) issued by a "qualified small issuer," (ii) issued for public purposes, and (iii) designated as qualified tax-exempt obligations. A "qualified small issuer" is (with respect to bonds issued during any calendar year) an issuer that issues no more than $10 million of tax-exempt bonds during the calendar year. Qualified tax-exempt obligations are commonly referred to as "bank qualified bonds." Effectively two types of municipal bonds were created under the Act; bank qualified and non-bank qualified. Although banks may purchase non-bank qualified bonds they seldom do so. The rate they would require in order for the investment to be profitable would approach the rate of taxable bonds. In contrast, banks have a strong appetite for bank qualified bonds that are in limited supply. As a result, bank qualified bonds carry a lower rate than non-bank qualified bonds. Any rate differential between bank qualified and non-bank qualified bonds only impacts the maturities purchased by banks. Generally banks purchased shorter maturities of bonds, maturing in ten or fewer years (Bergstresser and Orr (2014)).

3.3. Bond Fund Holdings

I obtain municipal bond mutual funds' holdings information from the Center for Research in Security Prices database (CRSP) for the years between 2000 and 2019. Since the data coverage is less comprehensive before 2009 and CRSP does not provide the overall values of municipal bond holding before 2010, I primarily work with the quarter-end holding data of the funds between 2010 and 2020. I follow a sequence of steps to construct my final data set. First, I restrict my sample to just municipal bond funds, funds with percentage of municipal holdings > 50%.[26] Next, in the data, multiple funds can be managed under a single portfolio. These funds are usually managed by the same manager, and invest in similar securities, but are marketed to different investor types (retail vs institutional), and therefore have different load shares & expense ratios. The analysis is done at the portfolio level as all the holdings are reported at the portfolio level as a percentage of the total net assets (TNA). I match these funds to their respective portfolios by using the fundno-portno match from CRSP.[27] The mutual fund holdings data-set also provides the CUSIP of every holding, which enables me to link it to the bond issuance data from Mergent. Since each share represents a fixed amount of investment at par value, one could infer the total par value of each holding from the corresponding number of shares. Aggregating all municipal bond holdings within a given portfolio, I present the summary statistics[28] for the universe of the mutual funds examined in this study in **Panel A of Table 11**. We see that their market share has increased drastically over the years of the sample, indicating their increasing importance in the municipal bond market. At the end of 2019, there were 630 distinct muni bond mutual fund portfolios and the total value of municipal bonds held by the mutual funds was 860.37 billion dollars. These portfolios held an average of 434 muni bonds from 188 issuers. Only one percent of the portfolio TNA was invested in cash, suggesting that muni bond funds hold small cash buffers and invest almost entirely in municipal bonds. In **Panel B**, we see that the cash holdings variable exhibits little correlation with most other portfolio level variables - income yield, average maturity, turnover ratio, and the rear load indicator.

To focus on the set of municipal bond funds which "might" have potentially held the municipal bond at issuance, I classify a muni bond as held by a municipal bond mutual fund (MBMF)

[26]This restricts the sample of funds to funds with CRSP objective codes as IU/IUS/IUI, which are either state-specific or national municipal bond mutual funds.

[27]I sum the TNAs of all funds that come under a single portfolio. The cash holdings measure at the portfolio level is calculated by taking a value-weighted average (by total net assets) of all funds within the portfolio (there is actually very little variation in the percentage of muni holdings or cash holdings within a portfolio). For variables like income yield, average maturity, rear load fees, and expense ratios, I match the portfolio to the fund with the maximum total net assets. I define the variable rear load indicator as 1 if the fund imposes a rear load fee on its investors.

[28]The summary data is reported for the first quarter of every year starting from 2010Q1 to 2020Q1.

at issuance if the mutual fund portfolio reports holding the bond in the same quarter as the offering date of the bond. My main explanatory variable, the equal-weighted fund cash flow holdings (EWFCH) measure, is constructed at the individual bond level by taking an equal–weighted average of the cash-flow holdings of the fund portfolios that hold the bond at issuance. In my main analysis, I lag the portfolio level variables (cash holdings, rear load indicator, income yield, turnover ratio, weighted average maturity) by a quarter. **Panel A of table 12** reports the summary statistics for these bonds. There are 77,609 bonds in the entire sample period. These bonds have an average bond size of $10.85 million, issue size of $123 million, and maturity of 12.9 years. Only 13% of the bonds are insured, 32% are general obligation, 59% are callable. Finally, 31% of the bonds are sold through competitive bidding. The average coupon rate is 4.56%, the raw yield (tax-adjusted yield spread) of the average bond is 2.796% (2.509%). On average, almost 65% of the total bond issuance volume is held by MBMFs, and on average just two portfolios hold the bond.[29] The average EWFCH measure is 1.53% with a standard deviation of 2.63%. **Panel B of table 12** reports the summary statistics for the sample of muni bonds not held by any MBMFs at issuance. There are 958,991 bonds in the entire sample period. These bonds have an average bond size of $1.18 million, issue size of $23.61 million, and maturity of 9.321 years. 18% of the bonds are insured, 65% are general obligation, 46% are callable. Finally, 66.5% of the bonds are sold through competitive bidding. The average coupon rate is 3.266%, the raw yield (tax-adjusted yield spread) of the average bond is 2.266% (1.724%). Overall, bonds held by MBMFs at issuance have $\sim$ 50 bps higher offering yields than bonds not held by MBMFs at issuance, and tend to have a much larger bond size and total issuance volume. They are also much more likely to be revenue bonds with a higher coupon rate, and more likely to be offered through a negotiated method of sale than through competitive bidding.

3.4. Other Variables

For information on county demographic variables, I gather per capita income from the Bureau of Economic Analysis (BEA), county-level population from Surveillance, Epidemiology, and End Results (SEER) Program.[30] Unemployment rate, local wages, employment and establishments across industry sectors are obtained from the Bureau of Labor Statistics (BLS). My estimates of county level racial diversity index are calculated based on the data from the Census Bureau's

[29] The number of distinct portfolios holding the bond is heavily left-skewed, as most of the bonds are held by just one mutual fund portfolio.

[30] Although the BEA has population data, for a sample of cities in Virginia the BEA data provides populations for certain combination of cities. The SEER data in comparison gives the estimate for individual cities. Hence I obtain the final population numbers from SEER data. I use the per capita income of the city combinations as the per capita income of the individual cities.

Annual County Resident Population Estimates by Age, Sex, Race, and Hispanic Origin. I use data from the Federal Housing Finance Agency (FHFA) to measure housing price at the county level. FHFA has created single-family housing price indices by county since 1975. The indices are built by using repeat-sales and refinancings for houses whose mortgages have been purchased or securitized by Fannie Mae or Freddie Mac. I follow Chordia et al. (2022) to gather data on local government finances.

4. Empirical Methods and Results

4.1. Banks' Exposure to Interest Rate Risk and Offering Yields of Newly Issued Bank-Qualified Bonds

To test the main hypothesis, I estimate my core model, which links the local income gap (interest rate risk exposure of banks with local bank-branches) to offering yields of *bank-qualified bonds*. In particular, I report the results from the following regressions:

$$Y_{i,c,t} = \beta \cdot LIG_{c,t-4} + \text{Bond Controls} + \text{County Controls} + \gamma_c + \delta_{s,\tau} + \epsilon_{i,c,t}, \tag{1}$$

where i indexes the bank-qualified bond, c indexes the county in which the bond issuer is located, and t indexes year-month. $Y_{i,c,t}$ is either the *offering yield* or *tax-adjusted yield spread*. $LIG_{c,t-4}$ is the four-quarters lagged local income gap variable defined at the county-quarter level. *Bond controls* comprise time to maturity (TTM), inverse-TTM, the natural logarithm of the bond size, and dummies for general obligation bonds, insured bonds, callable, and whether the bond was sold using competitive underwriting method. I also supplement the above bond controls with categorical variables for debt types and fixed effects for credit ratings. *County controls* are a set of county variables to control for local economic conditions, including the one-year lagged variables of change in population, the change in employment, the natural logarithms of population and income per capita. γ_c are county fixed effects that remove time-invariant county characteristics, and control for endogenous bank-county matching. $\delta_{s,\tau}$ are state-by-year fixed effects, as a non-parametric control for any secular time trends. Standard errors are double-clustered at the county and year-month level.

The regression coefficient β captures the differential IR risk premium owing to banks' IR risk exposure. Note that since the benchmark treasury yield by itself incorporates the economy wide interest-rate risk premium (Haddad and Sraer (2020)), therefore the large sensitivity of the bond offering yield to the benchmark yield implies that municipal bonds already incorporate the

IR risk premium present in treasury bonds. **Table 3** presents the results from estimating equation (1). In columns (1) and (2), I estimate the regression equation using the offering yield as the dependent variable. For the regressions using offering yield, I include the benchmark yield as a control. In the tightest specification implemented in Column (2) with the issuer and underwriter fixed effects, I find that a one standard deviation decrease in the local income gap (LIG) leads to an increase of 1.89 basis points (bps) in the offering yield. The effect is statistically significant at the 1% level. Column (3) and (4) present results with the tax-adjusted yield spread as the dependent variable. In column (4), we see that the after-tax yield spread increases by 4.03 bps. To put this in perspective, the average offering yield spread between the highest rated (AAA) bonds and those just below investment grade (Ba1) bonds equals 86 bps, in my sample of bank-qualified bonds. This implies that the average increase in the offering yield of 1.89 bps represents about 2.2% of the default spread. In columns (5) and (6), I use the equal-weighted local income gap as my main explanatory variable. Instead of using deposit-weights as with my original measure, I construct the equal-weighted LIG by taking the average of the income gap of all banks which operate branches within that county. I obtain similar results when using this measure of the income gap.

The hypothesis in the paper is that as banks exposure to interest rate risk rises, they demand a higher compensation for bearing IR risk. The key identifying assumption is that banks' aggregate interest rate risk exposure is uncorrelated with omitted factors that drive offering yields of bank-qualified bonds. One concern might be that there is endogenous matching between banks and counties; in the sense that, bank characteristics that correlate with income gap[31], might explain the choice of their business operations or market entry in specific counties. To alleviate this concern, I use county fixed effects, which non-parametrically control for any time-invariant source of endogenous matching between banks and counties. Second, **panel C of table 1** shows that the within county and between county standard deviation in the LIG measure is rather similar, suggesting that local income gap exhibits strong within county time-series variation. This weakens the case for time-invariant omitted variables driving the results, as it is unlikely banks with either persistently high or low income gap select into different counties based on some unobservable characteristics. [32] Third, any effects emanating from macroeconomic variables (inflation, GDP growth) is controlled for by including the benchmark treasury yield[33] separately in the regression or by taking the yield

[31] Gomez et al. (2021) find that income gap is positively correlated with bank size. Similarly, banks with a large income gap tend to operate in local deposit markets that are more concentrated, and that see a larger share of adjustable rates mortgages (ARM) in mortgage issuance.

[32] Also, since the analysis is done at the county level and not at the bond level, it is unlikely that reaching for yield behavior (banks with lower income gap invest in higher yielding bonds) could drive the effects.

[33] Defined as the yield to maturity on a synthetic treasury bond with the same payoff structure as our municipal bond

spread between the offering yield and the benchmark yield. Fourth, one might be worried that time-varying local economic conditions can affect both the local income gap and offering yields of local muni bonds. Note however that the LIG measure is constructed from banks' aggregate IR risk exposures. Since these banks operate across state lines and asset classes, it is unlikely that local economic conditions prevailing in a particular county can affect banks' income gap.

I implement more rigorous tests to show that the relation between the local income gap and offering yields is indeed causal in nature. There might be endogenous matching of banks and local governments - similar findings could spuriously obtain if omitted factors (e.g., unobserved bank quality) may explain why banks take on more interest rate risk while simultaneously explaining credit risk of local governments. Papers in the banking literature address this issue by controlling for credit demand and changes in borrower fundamentals through borrower-time fixed effects (Khwaja and Mian (2008)). Using borrower $\times$ time fixed effects takes out any time-varying demand side shocks (like loan demand, credit quality, etc.) and hence isolates the supply side variation emanating from a specific lender. For bonds, where unlike firm loans, the marginal lender is not distinctly identified and there is no within bond supply-side variation that can be isolated.[34] I address this issue by implementing the following two tests. First, the quasi-ownership segmentation of the muni bond market into bank and non-bank qualified bonds implies that banks invest mostly in the bank-qualified segment of the market.[35] **Table IA.1** shows the results of the regression of tax-adjusted offering yield spreads of non bank-qualified (NBQ) bonds on the local income gap measure. In **column (1)**, I utilize the entire pooled sample of newly issued BQ & NBQ bonds, and find that the impact of LIG on NBQ bonds is positive and statistically significant at the 5% level. As expected, the impact on BQ bonds is strongly negative and significant at the 1% level. When I limit my sample to just the NBQ bonds, the effect on the tax-adjusted yield spreads is positive and statistically insignificant as shown in **column 2**. The weak correlation of the local income gap with offering yield spreads of NBQ bonds suggests that spurious correlation arising from omitted variables.

Since the distinction between BQ and NBQ bonds is based merely on the $10 million issuance size threshold, there are multiple issuers in my sample who have issued both BQ and NBQ bonds in different years. It is possible that banks are still the marginal price setters of interest rate risk for non-bank qualified bonds issued by these common issuers. To test this hypothesis, I divide my sample into two subsets based on the following criteria: if the issuer of the NBQBs has also issued a

[34]With the Covid-19 crisis as their setting. Li et al. (2021) use issuer $\times$ time FEs to argue for a mutual bond fund driven credit supply shock to the transaction volume and prices of their muni bond holdings.

[35]Dagostino (2018) shows that the holdings of bank qualified bonds make up between 4% and 5% of banks' assets compared to non-qualified bonds, which instead correspond to just over 0.5% of banks' assets on average.

bank-qualified bond in the past, I classify them into the subset with CI (common issuer) = 1. If the issuer has never issued a bank-qualified bond in the past, I classify those NBQBs into the subset with CI=0. Column (3) and (4) present the regression results in the two sub-samples. Next, I test the hypothesis that underwriters who underwrite both BQ and non-BQ bonds within a county might price in banks' IR risk exposure for NBQ bonds. I divide my sample into two subsets based on the following criteria: if the lead underwriter of the NBQB has served as the lead underwriter for a bank-qualified bond in the past, I classify those NBQBs into the subset with CU (common underwriter) = 1. If the lead underwriter has never underwritten a bank-qualified bond in the past, I classify those NBQBs as CU=0. Columns (5) and (6) present the regression results in the two sub-samples. The above results clearly indicate no statistically significant correlation between local income gap and NBQ tax-adjusted yield spreads. What more, the coefficient is positive, suggesting that any common omitted factors that might correlate with both offering yields and the local income gap, must correlate in opposite ways with offering yields of bank-qualified vs non-qualified bonds. It is hard to think of a plausible economic mechanism for this. This sharp discontinuity in bank ownership of local bonds and market segmentation thus provides a clean test for hypothesis that offering yield spreads of muni bonds exhibit a strong relationship with the underlying balance sheet risk exposure of their marginal investors, who in this case, are commercial banks.

In **table IA.2**, I augment equation (1) by including additional control variables. In column (1), I include the logarithm of the average county bank size and the average equity ratio of banks as additional explanatory variables. The coefficient on LIG becomes marginally smaller as bank size is positively correlated with the local income gap. Including average bank size and equity ratios as controls also strengthens the identification concern that omitted bank variables that could correlate with both local income gap and offering yields are driving the observed effects. Next, I examine the effect of the BHC repricing/maturity gap developed in English et al. (2018) on yields spreads of BQ bonds. The repricing/maturity gap is the mismatch between the maturity or repricing time of bank assets and that of their liabilities. The repricing gap captures the interest rate sensitivity of net worth (or equity value) and can be interpreted in terms of total equity value change upon an increase in the federal funds rate. In a frictionless environment, only the duration gap should be a relevant measure of interest rate risk. Indeed, net worth should then be the relevant state variable for a bank's capital structure decision, regardless of the timing of future cash flows. Yet, the exact timing of cash flows can matter in the presence of financial frictions, which is why the income gap, as the the cash-flow measure, is a more popular measure of IR risk. **Column 2 & 3 of table IA.2** presents the results with the repricing gap measure. The repricing gap is positively associated with offering yield spreads. In column (3), when the income gap measure is also included, the results show that a one standard deviation increase in the local repricing gap measure leads to a 1.8 bps increase in the offering yield spread. The opposite sign of the coefficient (to that on the local income gap) is explained by the fact that the income gap and the maturity gap are negatively correlated at the bank level. The effect of the local income gap on offering yield spreads barely change with the inclusion of the local repricing gap. **In column (4)**, I use the LIG with deposits as my main explanatory variable. The income gap with deposits is calculated by including all non-interest bearing deposits as liabilities that reprice in the short-term. Inevitably, this new income gap measure is significantly more negative. A one standard deviation decrease in the LIG with deposits is associated with a 2.42 bps increase in the tax-adjusted yield spread. Finally **in column (5)**, I augment the baseline regression specification by including macroeconomic variables that have been shown to forecast treasury bond risk premia: the inflation rate, the yearly growth in industrial production between, and the output gap.[36] A higher output gap and industrial production growth are associated with lower offering yield spreads. Yet, the coefficient on LIG only changes marginally upon the inclusion of these additional controls.

I implement a third set of tests to rule out the effect of time-varying omitted variables. Omitted borrower level credit risk factors could be negatively correlated with the interest rate risk exposure of banks, if banks with a lower income gap have relatively more business operations in high credit risk counties. To mitigate this alternative explanation, I check for the differential effect of local income gap on offering yields by credit ratings. **Table IA.3** reports the findings. In column (1), I interact the local income gap with the bond's credit rating. The coefficient on the interaction term is positive. However, upon the introduction of *credit rating* $\times$ *year* fixed effects in column (2), the coefficient on the interaction term becomes both economically & statistically insignificant. Next, I divide my sample into two sub-samples - a) non-investment grade bonds, which correspond to bonds with S&P ratings of BBB- & below and unrated bonds; b) investment-grade bonds, bonds with S&P ratings equal to or greater than BB+. The results show very little difference in the coefficients obtained in both sub-samples, thus weakening the case for a credit-risk driven channel. I also test if the local income gap depends on county demographics and economic conditions. **Column (1) of Panel A, table IA.4** presents the results of county-quarter level contemporaneous regressions of the local income gap on several county level variables, with the inclusion of county and state-year fixed effects. A high unemployment rate is associated with a lower value of the local income gap, whereas a high diversity index and high income inequality correlate positively with the local income gap. **Column (2)** shows that the coefficient on LIG remains unchanged when I include these additional county controls. In **Panel B**, I test whether

[36] All the macroeconomic control variables have been lagged by four quarters.

LIG predicts one-year ahead local government finances of municipal issuers. The results show that LIG has no meaningful association with either the ratio of total revenue or total debt to total expenditure, thus weakening any case for a default risk channel to be driving the results.

In **table IA.5**, I test the effect of the local income gap on tax-adjusted yield spreads across maturities. I augment my baseline specification with an interaction term between the local income gap and bond maturity. Column (1) shows that the coefficient on the interaction term is significantly positive, suggesting that the effect of LIG weakens with bond maturity. In columns (2) - (5), I divide my sample into five equal maturity quintiles. The results show that the impact of LIG on offering yields is maximum in the second maturity quintile (average maturity ~ 5 years), and the effect starts to diminish as we move up the maturity quintile. One possible explanation (although I have no specific test for this claim) for the maturity result may be that even among bank-qualified bonds, banks mostly invest in the shorter maturities, and the longest maturities have a larger share of retail investors (Bergstresser and Orr (2014)). In **table IA.6**, I examine the impact of LIG on offering yields by restricting my set of banks based on certain filters, and construct my LIG measure by only including these subset of banks. In columns (1) and (2), I construct the local income gap by only including banks that report holding any municipal securities in their Call Report filings. The effects are almost identical to the results obtained in Table 3. This is hardly surprising as most large depository institutions hold some amount of municipal bonds on their balance sheet. Next, in columns (3) and (4), I construct the LIG measure by limiting the sample to the top 20 largest banks by asset size every quarter. In columns (5) and (6), I construct the local income gap by limiting the sample of banks to banks with assets >10 billion USD (adjusted to 2010 dollars). The coefficients in columns (3) - (6) are larger than those in columns (1) and (2), suggesting that the local income gap of larger banks has a stronger correlation with offering yields. The results may not be that surprising, given that conditional on their deposit market share in the county, larger banks are more likely to be the marginal price setters of interest rate risk for bank-qualified bonds.

A potential concern with the income gap measure could be that it doesn't take into account banks' positions in the IR derivatives market.[37] If banks hedge their IR risk exposure through derivatives, the local income gap may overestimate banks' exposure to IR risk. Vuillemey (2019) finds that more than 90% of U.S. banks do not participate in the IR derivative market, and more than 50% of hedging banks use derivatives to take additional exposure to interest rate increases.

[37] The IR derivatives market is large in notional terms, with an outstanding gross exposure of USD 434 trillion as of end- 2015 (BIS, 2016). 97% of these contracts are used by financial intermediaries. Interest rate derivatives come in the form of several contract types, primarily swaps (which account for more than 70% of gross exposures), but also futures, forwards or options.

[38] Starting in 1995, banks report the notional amounts of the interest derivatives they contract, on Schedule RC-L (derivatives and off-balance sheet items) of the call report filings with FFIEC. Starting from 1997Q2, banks also report the notional amount of interest rate swaps where the bank has agreed to pay a fixed rate. I use the Call Reports data to construct different measures of the local income gap based on the banks' contracting of interest rate derivatives. First, I define *LIG net hedging* calculated by limiting the sample to banks with a zero or negative net hedging ratio. Net hedging is defined as the difference between the banks' notional holdings of fixed-rate swaps and floating-rate swaps. Banks with a negative net hedging therefore pay the floating rate and thus have additional exposure to interest rate risk through their contracts in the interest rate derivatives market. As with all the other measures, this classification of 'unhedged' banks is done every quarter. I define a second variable, *LIG gross hedging*, by limiting the sample of banks to banks with a zero or negative gross hedging volume. Clearly only the small banks with no participation in the IR derivatives market remain in the sample. Lastly, I define *LIG fixed rate swaps* measure by limiting the sample of banks to banks with a zero or negative holdings of fixed-rate swaps. The idea with all the construction of the above measures is to weed out any potential hedging by banks in the IR derivatives market. **Table 4** reports the results. Columns (1) & (2) show the effects of LIG net hedging on the offering yield and tax-adjusted yield spreads. The coefficients are smaller than the coefficients on LIG obtained with the baseline specification. The coefficients on LIG with gross hedging and LIG with fixed rate swaps have roughly one-third the economic magnitude compared to the coefficients obtained in **table 3**. The takeaway from this table is that it is unlikely that banks use their contracts in the IR derivatives market to hedge IR risk, since limiting the sample to unhedged banks actually reduces the sensitivity of the offering yield spreads with the local income gap. Second, the results resonate with the findings in **table IA.2**; large banks (which are highly active in the IR derivatives market) are pivotal to pricing interest rate risk for bank-qualified bonds.

4.2. Inter-temporal Heterogeneity in IR Risk Pricing

Monetary policy affects banks' profitability through different channels and it is not straightforward to determine what the overall effect of a rate change would be. The income gap as a purely cash-flow measure of IR risk, captures the sensitivity of the bank's one-year ahead net interest margin (NIM) to changes in the short-term interest rate. For banks with a higher income gap, NIMs benefit from a steeper yield curve, and conversely, are reduced when the yield curve flattens. However,

[38] Begenau et al. (2015) find that large U.S. banks tend to increase their interest rate risk exposure with derivatives, although Hoffmann et al. (2019) find that euro-area banks use derivatives to reduce some of the duration mismatch risk (although the exposures of Euro-area banks is more heterogeneous). U.S. banks carry much more maturity risk, probably owing to the predominance of 30-year fixed-rate mortgages in the U.S. housing market.

changes in interest rates will also affect bank profits through capital gains or losses on their outstanding fixed-income portfolio positions, discount rates on future profits, as well as through an effect on lending profits, stemming from a change in general macroeconomic conditions. Upon an interest rate hike, banks on average, see a decline in their equity values. Flannery and James (1984) show that banks with more long-term assets relative to short-term liabilities (lower income gap), experience more negative stock returns.[39] In contrast, during periods of monetary policy easing, even though banks with a higher income gap experience lower NIMs, this doesn't imply that their overall profitability is impacted negatively. Altavilla et al. (2018) show that accommodative monetary conditions asymmetrically affect the main components of bank profitability, with a positive impact on loan loss provisions and non-interest income offsetting the negative one on net interest income, thus improving bank stock prices.[40] The takeaway is straightforward - during monetary policy tightening, banks with a lower income gap have to pay more in deposit rates while at the same time earning less from their assets (which reprice later than their liabilities). During periods of monetary policy tightening though, banks with a positive income gap suffer lower NIMs in the short-run, which is counterbalanced by an increase in their equity value. Thus, it can be argued that interest rate risk from the bank's perspective is primarily concerned with the adverse balance sheet effects owing to a rise in interest rates. This asymmetry in IR risk pricing should therefore be correlated with the level of the federal funds rate.[41]

To test this hypothesis, I introduce an additional variable - interaction of the local income gap measure with the federal funds rate, while keeping the same baseline regression specification as in equation (1). **Column 1 of table 5** presents the results. The coefficient on the interaction term is negative and significant at the 10% level. A higher level of the fed funds rate increases the sensitivity of the tax-adjusted offering yield spread to income gap of price-setting banks. The results suggest that the pricing of IR risk varies with the level of the short-term interest rate, and thus should exhibit strong inter-temporal heterogeneity. To better understand the dynamic pricing

[39]Indeed, utilizing a large sample period going back to 1870 & spanning 17 countries, Zimmermann (2019) shows that upon interest rate hikes, even though loan and retail deposit spreads go up, bank profitability falls. The disconnect between spreads and profits is driven by a sharp increase in loan losses and a contraction in credit growth.

[40]A protracted period of low monetary rates has a negative effect on profits that, however, only materializes after a long time period. Brunnermeier and Koby (2018) show that the "reversal interest rate", the rate at which accommodative monetary policy reverses its intended effect and becomes contractionary for lending - occurs when banks' asset revaluation from duration mismatch is more than offset by decreases in net interest income on new business. Similarly, Ampudia and Van den Heuvel (2022) show that negative interest rates in the euro area had a negative impact on bank equity values, with the effect more pronounced for banks with a more deposit-intensive funding mix.

[41]A different way to look at this problem is to understand that NIM is not a perfect measure of the actual IR risk exposure of banks, as forcefully argued in Begenau and Stafford (2022). The authors posit that, to the extent that bank managers view interest income sensitivity to interest rate shocks as opposed to market value sensitivity to interest rate shocks and make decisions on this basis there may be some inconsistent pricing of bank and market pricing of related products.

of IR risk, I separate my sample into five different time periods based on the path of policy rates at the start of every period. The first sub-period stretches from 1998 (the start of my sample period) to 2000. The effective fed funds rate hovered above 5% in this sub-period. The Federal Reserve raised interest rates six times between June 1999 and May 2000 in an effort to cool the economy, with the internet boom in full swing, and against the backdrop of Alan Greenspan's famous "irrational exuberance" speech. **Column (2)** shows the effect of LIG on offering yield spreads in this sub-period. A one std. deviation increase in LIG raises the tax-adjusted yield spread by ∼6 bps, compared to the 4 bps in the full sample. The next sub-period studies IR risk pricing during the period from 2001 (starting with the tech stock crash) to 2004. In the wake of the dot-com crash and the subsequent 2001–2002 recession the Federal Reserve dramatically lowered interest rates to historically low levels, from about 6.5% to just 1%. The results in **column (3)** show that effect IR risk exposure of banks was not priced into offering yield spreads in this period. The coefficient on LIG is economically small and statistically insignificant at the 10% level. The next sub-sample focuses on the period between 2005 and 2008.[42] The Fed raised interest rates 17 times (25 bps very quarter), increasing them from 1% to 5.25%, in order to cool off the economy and the growing real estate bubble. I find that local income gap has a strong effect on offering yield spreads in the 2005-2008 period. **Column (4)** shows that a one std. deviation decrease (8.7%) in the local income gap would have raised tax-adjusted offering yield spreads by 13 bps. Thus the pricing of IR risk seems to be concentrated in this short period of interest rate hikes. I divide the remaining sample period from 2009-2020Q1 into sub-periods; from 2009 to 2017 (zero lower bound (ZLB) sub period), and from 2018 to 2020Q1. In response to the great financial crisis, the Fed, slashed interest rates to near-zero, and left rates at near-zero until 2015. While interest rates remained at the ZLB, the Fed undertook large-scale asset purchases (LSAPs), effectively buying trillions of dollars of long-duration mortgage backed securities and government bonds.[43] While the Fed raised rates by only 25 bps in 2015 and 2016, between 2017 and 2018, 7 more rate hikes brought rates up to 2.5%. Given that my income gap measure is lagged by four quarters, I include bonds issued in 2017 in the ZLB sub period. **Column 5 & 6** present the results. We see that the coefficient on LIG is statistically insignificant at the 10% level in both the sub-periods. Yet, the magnitude of the coefficient during the 2018-2019 period is almost five times larger than that in the

[42] My results change only marginally if I exclude 2008 from the analysis. Restricting the sample period to 2005-2007, the coefficient on LIG is -1.243 which is statistically significant at the 1% level. I include the bonds issued in the recession year 2008, as the decision to bring these bonds to the market were taken before the onset of the financial crisis. Second, since my income gap measure is lagged by four quarters, it is unlikely that the crisis years had any effect on the income gap measure.

[43] It can be argued that LSAPs removed long-duration assets from the portfolio of bond market investors (banks in our case). Given a large shock to the quantity of IR risk, this should then imply that the IR risk premium charged by bond investors should fall too.

2009-17 sub-period. Although the fewer number of observations in the 2018-19 period reduces the power of the test and weakens the interpretation of the results, but the magnitudes obtained seem consistent with the hypothesis that banks actively manager their IR risk exposure during times of rising interest rates.

Close to half of all municipal bond offerings in my sample have a call feature associated with them. Borrowers may choose to call their bond if market interest rates move lower, which will allow them to refinance at a lower rate. A positive value of the income gap leaves the bank vulnerable to decreases in the interest rates and reduces its one-year ahead net interest margins, thus exposing them to reinvestment risk. The call option is most likely to be exercised when interest rates are decreasing, and when the marginal utility of wealth for banks with a large income gap is higher (since interest rate cuts erode the NIMs of banks with large positive income gaps). The implication of this is that a more positive value of income gap should be associated with higher offering yield spreads for callable bonds. To test this hypothesis, I implement the following regression specification,

$$Y_{i,c,t} = \beta_1 \cdot LIG_{c,\,t-4} + \beta_2 \cdot LIG_{c,\,t-4} \times \text{Callable} + \text{Bond \& County Controls} + \gamma_c + \delta_{s,\tau} + \epsilon_{i,c,t} \tag{2}$$

where i indexes the bond, c indexes the county in which the bond issuer is located, and t indexes year-month. $Y_{i,c,t}$ is either *offering yield* or *tax-adjusted yield spread*. $LIG_{c,t-4}$ is the four-quarters lagged local income gap variable defined at the county-quarter level. *Bond and County Controls* include all the variables as specified in equation (1). γ_c are county fixed effects and $\delta_{s,\tau}$ are state-by-year fixed effects. Standard errors are double-clustered at the county and year-month level. **Column 1 of table 6** presents the regression results. Indeed, in full sample regressions, the coefficient on the interaction term between LIG and callable is strongly positive and significant at the 1% level. As with the results in table 5, this effect varies considerably in the time-series. Whereas local income gap has no differential impact on offering yield spreads of callable bonds in the 1998-2000 subperiod, the same is not true in the 2001-04 period, when the the Fed lowered interest rates by a total of 5.25% with steady rate cuts throughout 2001. Although he interaction term in the 2005-08 period is positive and significant, yet the overall effect of LIG on callable bond yield spreads is still highly negative. The coefficient on the interaction term is the largest in the 2009-17 period, when interest rates remained close to the zero lower bound. Overall, the evidence in table 6 is consistent with the hypothesis that banks which have a positive income gap would want to pay less for bonds with embedded call options.

4.3. The Role of Banks' (Deposit) Market Power

The ideal interest risk measure for a bank should account for the cumulative interest rate risk in all its assets and liability positions. The average bank in the U.S. finances over 70% of its assets with deposits. It is difficult to assign a maturity profile to demand deposits in order to assess their interest rate sensitivity. What more, the findings in Drechsler et al. (2017) show that there is substantial variation in banks' deposit market power across counties, and thus the IR sensitivity of deposit rates is much weaker in counties where banks have high deposit market power. Indeed, Drechsler et al. (2021) argue that the deposit franchise can be viewed as an interest rate swap in which the bank pays the fixed rate (operating costs of running the deposit franchise) and receives the floating rate in form of the deposit spread (the spread between the fed-funds rate and the deposit rate). An important insight from Drechsler et al. (2021)'s model is that a fundamental part of banks' interest rate exposure—the exposure of the deposit franchise — is not captured in book assets or book liabilities. The deposit market power channel generates a testable hypothesis - the sensitivity of offering yields to local income gap should be lower in counties exposed to banks with more deposit market power.[44]

To test the deposit market power hypothesis, I run the following regression

$$\begin{aligned} Y_{i,c,t} = \beta_1 \cdot LIG_{c,\,t-4} + \beta_2 \cdot LIG_{c,\,t-4} \times \text{Avg. Bank HHI}_{c,\,t-4} + \beta_3 \cdot \text{Avg. Bank HHI}_{c,\,t-4} \\ + \text{Bond \& County Controls} + \gamma_c + \delta_{s,\tau} + \epsilon_{i,c,t} \quad (3) \end{aligned}$$

where i indexes the bond, c indexes the county in which the bond issuer is located, and t indexes year-month. $Y_{i,c,t}$ is either *offering yield* or *tax-adjusted yield spread*. $LIG_{c,t-4}$ is the four-quarters lagged local income gap variable defined at the county-quarter level. *Bond and County Controls* include all the variables as specified in equation (1). In addition, the average Bank HHI is also included as a county level control. γ_c are county fixed effects and $\delta_{s,\tau}$ are state-by-year fixed effects. Standard errors are double-clustered at the county and year-month level. The deposit market power hypothesis predicts that the coefficient β_2 should be positive. **Column 1 & 2 of table 7** report the regression results. In column (2), we see that the coefficient on the interaction between LIG and the average bank HHI in the county is positive and significant at the 10% level, consistent with our hypothesis. A one-standard deviation increase in the average bank HHI reduces the effect of a change in tax-adjusted offering yield spreads by 2.329 * 0.138 / 0.843 = 38.1%.[45]

[44] Li et al. (2019) show in a sample of floating small business loans that the deposit market power of banks helps them extend longer maturity loans, and also charge lower maturity premiums.

[45] The standard deviation of average bank HHI is 0.138, while the coefficient on LIG term is -0.843, and the

The coefficient β_3 though is positive (though statistically insignificant), implying that *ceteris paribus*, banks with higher deposit market power charge higher offering spreads for investing in local bank-qualified bonds. The result may seem at odds with Drechsler et al. (2021) and Li et al. (2019) findings; since deposit market power enables interest-rate insensitivity of bank's net income and funding stability over the business cycle, this should lead to lower interest spreads for borrowers borrowing from these banks. However, a key point to note here is that although banks operating branches in high HHI counties have low interest rate sensitivity of deposits, yet they might also have higher market power as lenders of capital. Since the average bank HHI is correlated with county HHI, and if county level HHI operates primarily through a market power channel,[46] this would explain the positive sign on β_3. I implement an additional test to separate the effects of banks' lending market power from their deposit market power. The idea is that conditional on banks' deposit market HHI and their local income gap, banks should be able to exert more pricing power in counties where there are fewer banks (high county HHI). I test this hypothesis through the following regression -

$$Y_{i,c,t} = \beta_1 \cdot LIG_{c,t-4} + \beta_2 \cdot LIG_{c,t-4} \times \text{Avg. Bank HHI}_{c,t-4} + \beta_3 \cdot \text{Avg. Bank HHI}_{c,t-4} +$$
$$\beta_4 \cdot LIG_{c,t-4} \times \text{Avg. Bank HHI}_{c,t-4} \times \text{County HHI}_{c,t-4} + \beta_5 \cdot LIG_{c,t-4} \times \text{County HHI}_{c,t-4} +$$
$$\beta_6 \cdot \text{Avg. Bank HHI}_{c,t-4} \times \text{County HHI}_{c,t-4} + \beta_7 \cdot \text{County HHI}_{c,t-4} + \text{Bond \& County Controls} + \gamma_c + \delta_{s,\tau} + \epsilon_{i,c,t} \tag{4}$$

The variables are the same as defined in equation (2). The county HHI variable captures the market power of the bank as capital lenders. I include its interaction terms with LIG and the average bank HHI (which captures the aggregate funding conditions of the average bank in the county), and the finally the main variable of interest - the triple interaction term. **Column 3 of Table 7** presents the results. We see that the coefficient β_4 is negative and statistically significant. This implies that the sensitivity of offering yield spreads to the local income gap is higher in counties where banks have higher market power, conditional on the average aggregate deposit market power of banks.

4.4. Additional Findings

4.4.1. The Role of the Underwriter

Butler (2008) finds that the underwriting market for municipal bonds is highly local (defined as

coefficient on the interaction term is 2.329.

[46] Scharfstein and Sunderam (2016) show that high concentration in mortgage lending reduces the sensitivity of mortgage rates and refinancing activity to mortgage-backed security (MBS) yields.

within the same state). 80% of municipal bonds are underwritten by investment banks with an office in the same state. Local underwriters may have better knowledge of local investors who might be interested in buying the bonds, including relationships with banks, local money managers and institutional investors who could assist in the placement of the securities. If markets are to be cleared locally, it is thus essential that local underwriters are better able to understand local demand, and thus have a comparative advantage in pricing "local risk" factors. Cestau (2019) shows that the municipal bond underwriting industry is fragmented by state and bid type. His findings show that industry leaders in negotiated sales (akin to a book-building mechanism) tend to be local, headquartered in the same state where they are leaders. Industry leaders in the competitive sales (akin to a sealed bid auction mechanism) tend to be national banks. This suggests that the sensitivity of bank-qualified bonds to the IR risk preferences of local investors, in this case, banks, should be higher for bonds offered through a negotiated offering. I test this hypothesis through the following regression specification -

$$Y_{i,c,t} = \beta_1 \cdot LIG_{c,\,t-4} + \beta_2 \cdot LIG_{c,\,t-4} \times \text{Negotiated} + \text{Bond \& County Controls} + \gamma_c + \delta_{s,\tau} + \epsilon_{i,c,t} \quad (5)$$

The dependent variable is the tax-adjusted offering yield spread. The sample used for this regression is different from the earlier samples, in that I limit my sample to bonds issued only through a negotiated or a competitive method of sale, and thus exclude observations where the method of sale is either missing or which are placed through private placements. The variable *negotiated* takes the value one if the bond was offered through a negotiated offering. Since negotiated bonds are more likely to price in banks' risk preferences, we expect the coefficient β_2 to be negative.

Table 8 reports the results. In column (1), we see that β_2 is both statistically and economically significant. In fact, the coefficient β_2 is almost twice as large as β_1. It may be possible that the incidence of a negotiated offering may be associated with more callable bonds or bonds with longer maturities. Hence, in column (2), I repeat the analysis by including the interaction of the local income gap with a dummy for bond callability and the maturity of the bond. The results do not seem to change post the inclusion of these controls. Yet, although the risk exposures of relatively smaller regional banks may be better priced by negotiated underwriting method owing to "soft" local information (Liberti and Petersen (2019)), it shouldn't be the case for pricing in interest rate risk exposure of large national banks. In columns (3) and (4), I replace the local income gap with the local income gap of only the largest 20 banks in the US, and banks with assets size greater than size > 10 Billion USD (adjusted to 2010 dollars) respectively. I find that the coefficient on the interaction term significantly reduces in both the columns, and for the local income gap of the top

20 banks, it almost disappears. The above results show that the pricing of the local income gap exhibits strong heterogeneity - both by the type of underwriting method, and by the underlying banks whose IR risk exposure the LIG is constructed from.

4.4.2. Ruling out Local Households as Marginal Investors in BQ Bonds

The intermediary asset pricing channel stresses the role of intermediaries as the marginal investors setting asset prices. Such an approach essentially substitutes the household Euler equation with its intermediary counterpart. Thus any study arguing for a intermediary based channel must take care to control for the households' risk exposures that may instead be pricing the assets. I argue that this is less so of a concern in my setting with the municipal bond market, since, as the cross-state variation in tax privilege incentivizes retail investors to hold within state issued muni bonds, my inclusion of state-year fixed effects takes out any state-level time-varying demand factors. Second, the federal tax exemption for banks for bank-qualified bonds doesn't vary across states. Since these banks operate across state lines and asset classes, it is unlikely that omitted variables pertaining to local households' risk exposures correlate with the interest rate risk exposure of banks. Nonetheless, it could be that economy-wide household interest rate risk exposure correlates with the IR risk exposure of banks. Constructing households' interest rate risk exposure is a challenging task.[47] Instead, I take an alternative approach. I argue since investor profile for municipal bonds is increasingly concentrated amongst high net worth households in the top 1% of the wealth distribution in the U.S. (Bergstresser and Cohen (2016)), this implies that other than banks, local households in high income areas are more likely to be the marginal investors for locally issued bank-qualified bonds. Thus I separate my sample of bank-qualified bonds into terciles based on per capita income with the assumption that households with higher per capita income are more likely to hold local bank qualified bonds. The null hypothesis is that if the local income gap was a mere proxy for local households' IR risk exposure, then the effects should be the strongest in the highest per capita income tercile. **Table 9** presents the results. In column (1), we see that the coefficient on the interaction between LIG and tercile 3 (highest per capita income) dummy is negative, but insignificant. The coefficient on the interaction between LIG and per capita income tercile 2 is economically insignificant, suggesting that there is no discernible difference in the pricing of LIG into offering yield spreads across per capita income terciles. Splitting into sub-samples and running separate regressions, I observe a similar pattern in columns (2) & (3). The

[47] A proxy for the IR risk exposure of local households could be the share of adjustable rate mortgages (ARMs) held by households at the county level. Such granular data is only available at the state level & is missing for a good number of state-years.

takeaway from **table 9** is that there is little to suggest that household IR risk exposure might be the omitted variable driving the main effect. Finally in **column 4**, I test if banks in neighbouring counties are likely to be the marginal investors. We see that the coefficient on the local income gap of neighbouring counties banks is significantly smaller as compared to the coefficient on banks' income gap. The results provide additional evidence for banks as the marginal price setters of interest rate risk.

4.4.3. Effects on the Extensive Margin

In this section, I study whether the interest rate risk exposure of banks has an effect on non-price terms. Indeed, aggregate market returns can affect security issuance decisions of borrowers in public markets. Ma (2019) finds that non-financial firms engage in cross-market arbitrage, issuing debt and repurchasing equity when either debt market conditions are favorable or when the expected return on equity is high. To study the effect of banks' IR risk exposure on non-price terms, I report my results in **table IA.7** using the following specification -

$$Y_{i,c,t} = \beta \cdot LIG_{c,t-4} + \text{Bond Controls} + \text{County Controls} + \gamma_c + \delta_{s,\tau} + \epsilon_{i,c,t}, \tag{6}$$

where i indexes the bond, c indexes the county in which the bond issuer is located, and t indexes year-month. $Y_{i,c,t}$ is either the logarithm of the bond offering amount, or the logarithm of the maturity, or a dummy variable that takes the value of 1 if the bond is offered through a negotiated sale method and zero otherwise, or a dummy variable that equals 1 if the bond has a call option attached to it. $LIG_{c,t-4}$ is the four-quarters lagged local income gap variable defined at the county-quarter level. *Bond Controls* vary depending on the dependent variable in the regression. I also include credit rating interacted with year fixed effects, debt type fixed effects, issuer fixed effects, county fixed effects and state-year fixed effects. *County Controls* are a set of county variables to control for local economic conditions, including the one-year lagged variables of change in population, the change in employment, the natural logarithms of population and income per capita. Standard errors are double-clustered at the county and year level.

The results in **columns 1 to 4** show that the local income gap has no statistically or economically significant effect on the size, maturity or callability of the bond, but has a strong positive effect on the type of bond offering. A one standard deviation increase in the local income gap is associated with a 6.4% increase[48] in the number of negotiated bond offerings. Sub-period

[48] The standard deviation of LIG is 0.12, while the coefficient on LIG term is 0.141, and the average number of negotiated bond offerings equal 0.26 for bank-qualified bonds. Thus, a one-standard deviation increase in LIG increases the number of negotiated bond offerings by by 0.141 * 0.12 / 0.26 = 6.4%.

analysis reveals that this effect is concentrated in the 2005-08 period. Interpreted in the light of the findings in **table 8**, the findings suggest that since negotiated bond offerings have a greater sensitivity to LIG, local issuers are more willing to place their issues through negotiated sales when banks have a higher income gap. Since a higher income gap translates into even lower offering yield spreads in negotiated offerings, it makes sense that issuers prefer the negotiated method of bond sales.

5. Do Banks Price their Liquidity Risk Exposure?

Following the results on interest rate risk pricing, an imperative question to ask is if banks price their liquidity risk exposure. Since banks have maturity-mismatched balance sheets, with long-duration nominal assets and short-duration nominal liabilities, this exposes them to both duration mismatch (interest rate risk) and funding risk (illiquidity). The widely accepted notion is that banks have a natural advantage in providing liquidity to businesses through credit lines and other commitments established during normal times (Gatev and Strahan (2006); Gatev and Strahan (2009)). Yet, Acharya et al. (2013) find that bank lines of credit are costlier for firms with greater market risk and such firms opt for cash in spite of the incurred liquidity premium. Li et al. (2019) show that banks raising deposits in more concentrated markets have more funding stability, which enhances banks' ability to extend longer-maturity loans and charge lower maturity premiums. Thus, it is not clear ex-ante if banks with higher liquidity risk exposures charge a higher premium for investing in illiquid municipal bonds.

To test whether banks price the liquidity risk exposure they carry on their balance sheets, I construct various measures of bank level liquidity risk exposure following Gatev and Strahan (2006) and Acharya and Mora (2015). These include the share of illiquid assets, the undrawn commitments ratio, the wholesale funding ratio and the core deposits ratio.[49] Core deposits form a stable source of funds for lending banks. Wholesale funding measures net wholesale borrowing including gross federal funds purchased less gross federal funds sold and repos less reverse repos. Uninsured short-term wholesale liabilities, such as repos and commercial paper, are an important source of funding for many banks. These funding markets dried up suddenly in 2008, causing negative shocks to bank liquidity (Gorton and Metrick (2012)). The undrawn portion of banks' lines of credit to consumers and firms are referred to as unused commitments. Such drawdowns are a source of liquidity risk to banks especially during market liquidity crunches. Similar to the local income gap measure, I construct my county level weighted bank liquidity risk ratios by weighting

[49] All the variable definitions are provided in **appendix table A.1**

each bank measure by its share of the county's deposits. The results of the regression of offering yield spreads on these county level liquidity risk ratios is presented in **table 10**. We see that the coefficient on none of the liquidity risk measures seems to be either statistically or economically significant. The F-test for overall significance of all the liquidity ratios yields a value of 0.23, and a corresponding p-value of 0.9242. Thus we cannot reject the null hypothesis that all the coefficients are simultaneously zero. In addition, when the local income gap is included in the regression, the coefficient on the local income gap changes marginally. Thus, the results suggest that the null hypothesis that banks' liquidity risk exposure has no relationship with muni bond offering yield spreads, cannot be rejected.

These results may not be too surprising if interpreted in light of the arguments proposed in Kashyap et al. (2002) or Gatev and Strahan (2006). Kashyap et al. (2002) propose a risk management motive to explain why commercial banks combine demand deposits with loan commitments or lines of credit. As long as the demand for liquidity from depositors and borrowers is not too highly correlated, the bank will pool these two classes of customers together to conserve on its need to hold costly liquid assets—the buffer against unexpected deposit withdrawals and loan takedowns. Gatev and Strahan (2006) extend this argument further to highlight the unique ability of banks to hedge against systematic liquidity shocks, since only banks have funding inflows that co-vary negatively with market liquidity, and hence banks can insure firms against systematic declines in liquidity at lower costs than other institutions. My results show that the null hypothesis of the liquidity risk exposure of banks being a non-priced risk factor for offering yield spreads of bank-qualified bonds cannot be rejected.

6. Municipal Bond Mutual Funds and the Price of Liquidity Risk

The results in the above section point to the fact that although banks manage their interest rate risk exposure by adjusting their risk premiums for investing in fixed-rate long term muni bonds, the same cannot be said for their liquidity risk exposure. Other than banks, mutual funds provide liquidity services to their investors by allowing them to redeem any number of shares at the fund's end-of-day net asset value (NAV). Through their investments in illiquid assets, open-end mutual funds engage in substantial liquidity transformation. Recent studies have mostly focused on how the growth of fixed-income funds (spurred by the low interest-rate environment), have contributed to draining liquidity from bond markets during times of market stress (He et al. (2022b); O'Hara and Zhou (2021);Li et al. (2021)). Liquidity provision is time-varying, and although evidence suggests that liquidity has overall improved in the post-crisis era, yet it remains scarce precisely when it is needed the most. Pivotal to liquidity provision during times of market turmoil is the deployment of patient capital[50], as well as the intrinsic characteristics of the asset.[51]. Since bond funds lack the patient capital and a stable source of funding that accompanies banks or even insurance firms, they should look to invest in securities with intrinsic characteristics that help them maintain liquidity even during times of market stress. Thus, I hypothesize that more illiquid bonds or securities which become harder to sell exactly around the times when bond funds demand liquidity, have to pay a higher premium if their marginal investor is a bond mutual fund.

Municipal bond funds are attractive because they pay a stream of income free from federal tax and, in most cases, state taxes for residents of the issuing state. Open-end mutual funds have quickly grown to be the largest institutional investors in the muni bond market, holding about 20 percent of outstanding munis. To test whether muni bonds owned at issuance by muni bond funds pay a higher liquidity premium, I use the average cash-holdings of the bond funds as my main explanatory variable. The use of the cash holdings measure is motivated by the revealed preference argument of Chernenko and Sunderam (2016), who suggest that funds' cash management practices can be used to measure their liquidity transformation. The fund cash-holding measure triumphs as a better measure of "perceived" illiquidity, as illustrated in Chernenko and Sunderam (2020). The authors show that especially in the municipal bond market, where most bonds do not trade at all in a month, transaction volume is not a strong proxy for perceived liquidity in the municipal bond market.[52]

6.1. Main Results

To construct the bond level cash-holding measure, I either equal-weight or value-weight the cash holdings of all municipal bond mutual fund portfolios that report holding the bond in *same quarter* as the bond's offering date. I use the credit rating of the bond at issuance as a measure of perceived liquidity (Li et al. (2021)). I interact the bond level cash-holding measure with the credit rating of the bond, the hypothesis being that, *ceteris paribus*, muni bond funds with higher liquidity management needs or a stronger "distaste" for illiquidity, would pay lower prices for lower rated or more illiquid bonds. Using the sample of all municipal bond offerings from 2010Q2 - 2020Q1, I

[50]Post-crisis regulations are often blamed for decreased willingness of dealers to provide liquidity which could stabilize prices during frenzied selling episodes. In fact, Li et al. (2021) find that amidst the surge in demand liquidity at the height of the Covid-19 crisis, dealers actually shifted from buying to selling, especially in bonds with mutual fund ownership.

[51]For example, the liquidity premium for "money-like safe assets" rises during crisis periods (Nagel (2016))

[52]They find that although municipal bonds that do not trade are perceived to be even less liquid than the average municipal bond, the magnitudes are moderate.

implement the following regression -

$$Y_{i,j,t} = \alpha_j + \beta \cdot \text{Fund Cash flow Holdings}_{c,t-1} + \beta_2 \cdot \text{Fund Cash flow Holdings}_{c,t-1} \cdot \text{Bond Credit Rating} + \text{Bond Controls} + \gamma_{c,\tau} + \delta_{\text{credit rating},t} + \epsilon_{i,t}, \quad (7)$$

where i indexes the bond, j indexes the issuer and t indexes year-month of issuance. $Y_{i,c,t}$ is the *tax-adjusted yield spread*. *Fund Cash flow Holdings*$_{c,t-1}$ is the one-quarter lagged equal-weighted fund cash holdings (EWFCH) or the value-weighted fund cash holdings measure (VWFCH), and is defined at the bond level. α_j denotes issuer fixed effects to control for unobservable issuer characteristics. *Bond Controls* comprise time to maturity (TTM), inverse-TTM, the natural logarithm of the bond size, and dummies for general obligation bonds, insured bonds, callable, and whether the bond was sold using competitive underwriting method. I use a variable dummy which takes the value of 1 if the bond is held by any mutual fund at issuance. $\gamma_{c,\tau}$ are county $\times$ year fixed effects that remove time-varying changes in county fundamentals. $\delta_{\text{credit rating},t}$ are ratings $\times$ year-month fixed effects, used as a non-parametric control for time-varying effects of credit ratings. Standard errors are double-clustered at the issuer and year-month level. In some specifications, I also include maturity $\times$ year-month fixed effects.

The results in **table 13** show that the effect of fund level cash-holdings on offering yield spreads is positive and significant, whereas the coefficient on the interaction term β_2 is negative. In the most complete specification in column (3), the results suggest that a 1% increase in EWFCH leads to a 2.6 basis points increase in the tax-adjusted offering yield spread. Yet, the negative coefficient on the dummy for mutual fund ownership suggests that mutual fund ownership reduced offering yield spreads on average. For bonds in the highest credit rating category, the cumulative effect is a decrease in tax-adjusted offering yield spreads, suggesting that bond funds actually pay a premium for these securities. Next, I focus on examining how variation in other fund characteristics related to liquidity, potentially affect the pricing of liquidity risk. First, given the retail dominated muni market and limited number of institutional buyer in a bond, it is hard for dealers to locate potential buyers for mutual funds' bulk sales. Thus inventory risk is a major concern for dealers looking to take in sell orders from bond funds (Li et al. (2021)). Bond funds are less likely to be concerned about bond liquidity, if there are other mutual fund investors who can absorb the selling from mutual funds. As long as bond funds do not face correlated liquidity shocks, having a wider investor base would be perceived as liquidity enhancing. Indeed, Li and Yu (2022) find that corporate bonds with more investors (less concentration) have better liquidity and lower yield. Similarly the presence of a rear-end load fee, fee that is charged when an investor redeems the mutual fund shares, deters potential outflows and is indicative of better liquidity management, and hence should be associated with lower offering yields. I test the above hypotheses using the following regression specification -

$$Y_{i,j,t} = \alpha_j + \beta \cdot \text{Fund Cash flow Holdings}_{c,t-1} + \beta_2 \cdot \text{Fund Cash flow Holdings}_{c,t-1} \cdot \text{Bond Credit Rating} + \beta_3 \cdot \text{Fund Characteristic}_{c,t-1} + \text{Bond Controls} + \gamma_{c,\tau} + \delta_{\text{credit rating},t} + \epsilon_{i,t}, \quad (8)$$

where variables follow the same definition as in equation (7). Fund Characteristic is either distinct number of fund-portfolios at issuance or the rear-end load fee or the average portfolio turnover ratio. The results of the regression are presented in **table 14**. We see that a higher number of bond mutual funds holding the bond at issuance is associated with reduced offering yield spreads. In terms of magnitudes, an increase in one additional fund-portfolio leads to a 4 bps decrease in the tax-adjusted offering yield spread. The coefficient on the rear-load is negative and statistically & economically significant, suggesting that funds view rear load fees as a safety net against potential liquidity withdrawals. Finally, I find that bonds placed with funds with higher average portfolio turnover ratio have lower offering yield spreads.

6.2. Instrumental Variable Approach

The positive relationship between EWFCH and bond offering yield spreads suffers from a potential reverse causality problem. Bond mutual funds could be "reaching for yield", if they select into holding higher yield bonds, especially bonds with higher credit risk.[53] As a result of holding riskier bonds, these funds may then have to keep additional cash buffers to mitigate against the possibility of future defaults and investor redemptions.[54] This reaching for yield channel would then imply that bond mutual funds are not the marginal price setters though for these bonds that they hold, rather some other omitted variable (e.g. credit risk) explains the higher offering yield spreads. An ingenious way to address confounding factors when studying the effect of a supply side shock is the use of borrower $\times$ time fixed effects,[55] which takes out any time-varying demand side shocks (like loan demand, credit quality, etc.) and hence isolates the supply side variation from specific lenders. Yet, for bonds (where unlike syndicated loans), the marginal lender for a bond is not distinctly

[53] A lot of munis are unrated, thus credit rating fixed effects may not fully control for default risk.

[54] Evidence of reaching for yield behavior has been documented among insurance firms, who are more likely to choose high yielding securities conditional on minimizing their risk adjusted capital ratios (Becker and Ivashina (2015)), and also among corporate bond mutual funds (Choi and Kronlund (2018)).

[55] This approach is used extensively in the banking literature, after being introduced in a seminal paper by Khwaja and Mian (2008).

identified and hence there is no within bond supply-side variation that can be isolated.[56] Another approach is to use an exogenous shock to a set of lenders and study the effect on security pricing and volume.[57] The last approach relies on a shift-share type instrument, which is motivated by the idea that an investment mandate of institutional investors is pre-determined and should be exogenous to contemporaneous shocks to firms' fundamentals. I implement this approach to construct an instrumental variable for portfolio-fund cash holdings at the bond level.

Following the approach of Koijen and Yogo (2019), for each portfolio at each reporting date-end, I construct a hypothetical portfolio that equally divides the portfolio's total net assets over its investment universe. The investment universe of the portfolio is defined as the set of all issuers whose bonds have been held by the fund at least once within the last three years. Koijen and Yogo (2019) motivate this measurement of the investment universe with the argument that institutional investors typically limit their portfolio holdings to a relatively small set of investments and that the set of investments that they have held rarely changes over time. The reasoning is that the investment universe of bond mutual funds is largely predetermined and the hypothetical holdings allocate a fund's total net assets equally, regardless of individual local government's credit or liquidity risk. I refer to the equal-weighted holdings based on a fund's investment universe as its hypothetical holdings. To construct the IV for EWFCH for a bond, I equal-weight the cash-holdings of all portfolios that "hypothetically may have held the bond at issuance", and use this IV for EWFCH in two-stage least square (2SLS) regressions. The use of the hypothetical cash-holdings measure alleviates the concern of any omitted variables that may drive contemporaneous changes in both the cash-holdings of the bond mutual funds and the offering yield spreads. Thus, one may expect this hypothetical cash-holding measure to be a exogenous supply side shock from investors that determine the marginal prices of these bonds.

Table 15 presents the results of the first and second IV regression. In the first-stage, I regress the bond level one quarter lagged cash-holdings measure (EWFCH) on the instrument - the one quarter lagged hypothetical cash-holdings measure. **Column 1** shows that there the instrument strongly correlates with the main explanatory variable. The first-stage Kleibergen-Paap F-statistics of over 90 in both instances strongly indicate that the instrument is highly relevant in explaining the actual EWFCH. **Columns 2** and **3** show the effect of the instrumented cash-holdings measure on the tax-adjusted offering yield spreads. We see that the effect of fund level cash-holdings on offering

[56] Li et al. (2021) instead use issuer $\times$ time fixed effects to isolate supply side variations across bonds belonging to the same issuer.

[57] Adelino et al. (2021) use an identification strategy based on Morningstar star rating introductions to isolate the supply-side effects that are orthogonal to both fund and bond issuer fundamentals. Siriwardane (2019)) studies how seller capital shocks—measured as CDS portfolio margin payments, impact pricing in the CDS market.

yield spreads is is even larger than the coefficient obtained with the regression in (7). In terms of magnitudes, the estimates in **column 3** suggest that a 1% increase in EWFCH leads to a 2.6 basis points[58] increase in the tax-adjusted offering yield spread for an unrated bond. For bonds in the highest credit rating category (AAA rated bonds), the cumulative effect is a decrease of 6.2 bps[59] in the tax-adjusted offering yield spreads. In **table 16**, I use a similar IV approach to examine the effect of other fund characteristics that are potentially associated with funds' liquidity management on tax-adjusted offering yield spreads. To construct the IV for the particular fund characteristic for bond, I equal-weight the fund characteristics of all portfolios that hypothetically could hold the bond at issuance. The results show that a higher number of bond mutual funds holding the bond at issuance is associated with a reduction offering yield spreads. In terms of magnitudes, an increase in one additional fund-portfolio leads to a 3.5 bps decrease in the tax-adjusted offering yield spread. The coefficients on the rear-load dummy variable and the portfolio turnover ratio are both statistically & economically insignificant.

7. Conclusion

I show that the interest rate risk exposure of banks with local bank branches drives the price of interest rate risk for locally issued bank-qualified bonds. Consistent with the risk management motive of banks, banks with a lower income gap charge a higher premium for investing in fixed-rate municipal bonds. The pricing of interest rate risk exhibits strong intertemporal heterogeneity, with almost all the effect concentrated in the period from 2003 to 2008. Further empirical analysis shows that other aspects of banks' operations, like their market power in deposit markets, also matters for the pricing of interest rate risk. In further evidence of a bank interest rate risk exposure channel, I find that local income gap, the main explanatory variable, doesn't predict offering yield spreads of local non-bank qualified bonds, bonds which see much less investment from banks. Apart from maturity transformation, banks also engage in substantial liquidity transformation. Yet, although banks price their interest rate risk exposure, they do not seem to price their liquidity risk exposure. Other than banks, open-end mutual funds engage in substantial liquidity transformation through their investments in illiquid assets. I find that the liquidity management by municipal bond mutual funds, as measured by their cash-holdings, is positively associated with offering yields of bonds held at issuance by these bond funds.

[58]the coefficient on the instrumented EWFCH variable is 8.3 bps, whereas the coefficient on the mutual fund ownership dummy variable is 5.7 bps. 8.3 bps - 5.7 bps = 2.6 bps.

[59]the coefficient on the instrumented interaction term between EWFCH & credit rating is -0.4 bps. Multiplying with 22 (numerical rating for AAA rated bonds) and adding 2.6 bps (from the calculation for unrated bonds) gives us 8.8 - 2.6 = 6.2 bps

Overall my results highlight the key role that the industrial organization of intermediaries plays in determining the price of risk that borrowers pay to place their debt with these intermediaries. He and Krishnamurthy (2018) present a wish-list for future work in this area, by positing the following questions - "What are the most salient financial frictions driving intermediary asset pricing? How much does heterogeneity within the intermediary sector matter?" The findings in this paper add to the above research agenda. The results show that in segmented asset markets, the price of risk for otherwise similar securities may vary depending on the risk exposure of their marginal investors.

The cost of placing their debt has direct consequences on real investment decisions of issuers, and in the case of municipal bonds, an effect on the local public infrastructure and quality of life. I focus on the pricing of two important risk factors, interest rate risk and liquidity risk. In the current interest rate environment, banks' exposure to interest rate risk is an important concern. The rapid growth of bond mutual funds in bond markets has raised concerns about financial fragility. Bond mutual funds face large redemptions particularly during times of credit stress and market uncertainty. Bond funds that invest primarily in lower rated & more illiquid bonds, face the greatest risk of runs. An interesting research question might be to compare the regulatory cost of holding lower-rated bonds (for banks and insurance companies) versus the liquidity cost of holding such securities for muni bond funds.

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Table A.1

For the income gap measure which is my main explanatory variable, I use BHC (bank holding company) level information from FR Y-9C reports to construct the measure. However, the FR Y-9C data doesn't include several standalone and smaller banks. Therefore, I construct an equivalent measure based on the Call Report Data, which includes all commercial banking institutions in the US. For the sample of matched banks (banks which have both FR Y-9C and Call reports data), I verify that the income gap measures match closely.

I build my panel data set from the quarterly FFIEC Call Reports, which all regulated commercial banks file with their primary regulator. Because some banks are owned by a common holding company, I aggregate the bank-level data for banks with common ownership. Specifically, I sum Call Reports data at the highest holding company level for multibank holding companies. Note that in calculating all the ratios below, the numerator and denominator are separately summed for all the commercial banks within the holding company (for standalone banks, there is no summing involved). Next, I construct my county level weighted bank ratios by weighting each bank by its share of the county's deposits.

Variables	Description	Source
Income Gap (IG)	[Assets that reprice or mature within one year (bhck3197) – interest bearing deposits that reprice or mature within one year (bhck3296) – long term debt that reprices within one year (bhck3298) – long term debt that matures within one year (bhck3409) – variable rate preferred stock (bhck3408)] / total assets (bhck2170)	FR Y-9C Filings, Individual Bank Call Reports
Local Income Gap (LIG)	The local income gap is constructed by weighting each bank b's total income gap by its deposit share in each county c. $LIG_{ct} = \Sigma_{i \in b}$ Deposit Market $Share_{bct} * IG_{bt}$	FDIC SOD
LIG including Deposits	From the numerator of the IG measure, I subtract non-interest bearing deposits (RCON6631). The rest of the calculations are the same as above.	Call Reports
LIG Muni Holdings	The LIG measure is calculated by limiting the sample of banks to banks that report holding municipal bonds on their balance sheet (RCFD8496 + RCFD8498 > 0).	Call Reports
LIG Top 20	The LIG measure is calculated by limiting the sample to the top 20 largest banks by asset size, every quarter.	Call Reports
LIG Large Banks	The LIG measure is calculated by limiting the sample of banks to banks with assets >10 billion USD (adjusted to 2010 dollars).	Call Reports
LIG Net Hedging	This measure is calculated by limiting the sample to banks with a zero or negative net hedging ratio. Net hedging is the difference between fixed-rate swaps (RCFDA589; where the bank has agreed to pay a fixed rate) and the floating-rate swaps (RCFD3450 - RCFDA589; where the bank has agreed to pay the floating leg of the swap). Banks with a negative net hedging are therefore have additional exposure to interest rate risk through their portfolios in the interest rate derivatives market. As with all the other measures, this classification of 'unhedged' banks is done every quarter.	Call Reports
LIG Gross Hedging	The LIG measure is calculated by limiting the sample of banks to banks with a zero or negative gross hedging volume (RCFD8725+RCFD8729).	Call Reports

LIG Fixed Rate Swaps	The LIG measure is calculated by limiting the sample of banks to banks with a zero or negative holdings of fixed-rate swaps (RCFDA589)	Call Reports
County HHI	The bank-level Herfindahl-Hirschman Index of deposit concentration for the local markets at the county level. $\text{County HHI}_{ct} = \Sigma_b\, (\text{Deposit Market Share}_{bct})^2$	FDIC SOD
Avg. Bank HHI	$\text{Bank HHI}_{bt} = \Sigma_c\, (\text{Deposit Market Share}_{bct}) * \text{County HHI}_{ct}$ $\text{Avg. Bank HHI}_{bt} = \Sigma_b\, (\text{Deposit Market Share}_{bct}) * \text{Bank HHI}_{bt}$	FDIC SOD

Variables defined at the highest holding company level	Description
Repricing Gap	A^j_{ibt} is the dollar amount in asset category j reported by commercial bank i, belonging to BHC b , in quarter t . A^{IE}_{ibt} denotes bank i's total interest-earning assets. m^j_A is the average repricing/maturity period (in months) for asset category j , as defined in English et al. (2018) (midpoint of category j 's maturity or repricing range reported on the Call Report). Similarly, L^j_{ibt} is the dollar amount of liability item j, L_{ibt} are commercial bank i's total liabilities and m^j_L denotes the average repricing/maturity period (in months) for liability item j. I then define the maturity/repricing gap for bank b as: $$\text{Repricing Gap}_{bt} = \frac{\Sigma_{i\in b}\Sigma_j m^j_A A^j_{ibt}}{\Sigma_{i\in b} A^{IE}_{ibt}} - \frac{\Sigma_{i\in b}\Sigma_j m^j_L L^j_{ibt}}{\Sigma_{i\in b} L_{ibt}}$$
Equity Ratio	[Available for sale securities (bhck1773) + Held to Maturity Securities (bhck1754)] / total assets (bhck2170)
Liquid Assets Ratio	Liquid assets are cash, federal funds sold and reverse repos, and securities excluding MBS/ABS securities: Cash: RCFD0010; Federal funds sold: RCFD1350 (before 2002Q1) and RCONB987 + RCFDB989 (from 2002Q1); Securities excl.MBS/ABS before 2009Q2: RCFD1754+RCFD1773 -(RCFD8500 + RCFD8504 + RCFDC026 + RCFD8503 + RCFD8507 + RCFDC027); Securities excl. MBS/ABS from 2009Q2: RCFD1754 + RCFD1773 - (RCFDG300 + RCFDG304 + RCFDG308 + RCFDG312 + RCFDG316 + RCFDG320 + RCFDG324 + RCFDG328 + RCFDC026 + RCFDG336 +RCFDG340 + RCFDG344 + RCFDG303 + RCFDG307 + RCFDG311 + RCFDG315 + RCFDG319 + RCFDG323 + RCFDG327 + RCFDG331 + RCFDC027 + RCFDG339 + RCFDG343 + RCFDG347).
Illiquid Assets Ratio	Loans and leases net of unearned income and loss allowance (RCFD2122-RCFD3123) + MBS/ ABS Securities (as defined above)
Core Deposits Ratio	Transaction deposits (RCON2215) + Savings Deposits (RCON0352+RCON6810) + Small time deposits (RCON6648) / Total Domestic Deposits (RCON2200) + Foreign Deposits (RCFN2200)
Wholesale Funding to Assets Ratio	Wholesale funds are the sum of large time deposits, deposits booked in foreign offices, subordinated debt and debentures, gross federal funds purchased, repos, and other borrowed money: RCON2604 + RCFN2200 + RCFD3200 + RCFD2800 (RCONB993+RCFDB995 from 2002q1) + RCFD3190.
Unused Commitments Ratio	Unused commitments divided by the sum of unused commitments and loans. Unused commitments are - RCFD3814 + RCFD3816 + RCFD3817 + RCFD3818 + RCFD6550 + RCFD3411.

Variables (Defined at the Muni Bond level)	Description
Benchmark Yield	*Benchmark Yield* (r_t) is the risk free yield is based on the present value of coupon payments and the face value of the municipal bond using the US treasury yield curve based on maturity-matched zero-coupon yields as given by Gurkaynak et al. (2007).
Tax-adjusted Yield Spread	This is calculated as the difference between the municipal bond offering yield and the tax-adjusted risk-free rate (benchmark yield) on the coupon-equivalent U.S. Treasury bond. I follow Schwert (2017) in applying the tax adjustment. $$\text{Tax-adjusted Yield Spread}_{it}=\frac{Y_{it}}{\left(1-\tau_t^{fed}\right)*\left(1-\tau_{s,t}^{state}\right)}-\text{Benchmark Yield }(r_t)$$
Callable	Dummy = 1 if the issue is callable and is 0 otherwise
Negotiated Offering	Dummy = 1 if the issue is offered through a negotiated offering. In a negotiated sale, the issuer negotiates a price with one underwriter after an ad-hoc selection process, as in book building.
GO Bond Dummy	Dummy =1 if the bond is a general obligation bond, i.e. it backed solely by the credit and taxing power of the issuing jurisdiction rather than the revenue from a given project.
Previous Connection (Underwriter)	Dummy =1, if the lead underwriter for the current offering has participated in an offering from the same issuer in the last three years.
Credit Rating	The Mergent data contains the bond credit ratings history from S&P, Moody's, and Fitch. When rating information is available from multiple rating agencies, I use the lowest rating among the three. Since my variable of interest is the offering yield at issuance, I use the credit rating of the individual bond at issuance. If the bond rating is missing, I use the rating of the issuer instead to fill the missing values. I convert character ratings into numeric ratings with 21 corresponding to the highest credit quality and 1 the lowest.
Equal-weighted Fund Cash flow Holdings [EWFCH]	The EWFCH is constructed at the individual bond level by taking an equal–weighted average of the cash-flow holdings of the fund portfolios that hold the bond at issuance (P^i). The portfolio cash-holdings in turn are calculated by taking a value-weighted average (by total net assets) of all funds within the portfolio. Num_port is the number of distinct portfolios which hold the bond at the time of issuance. $$\text{EWFCH}_{it}=\frac{\sum_{p\in P^i}\text{ Cash holdings}_{pt}}{\text{Num\_port}}$$

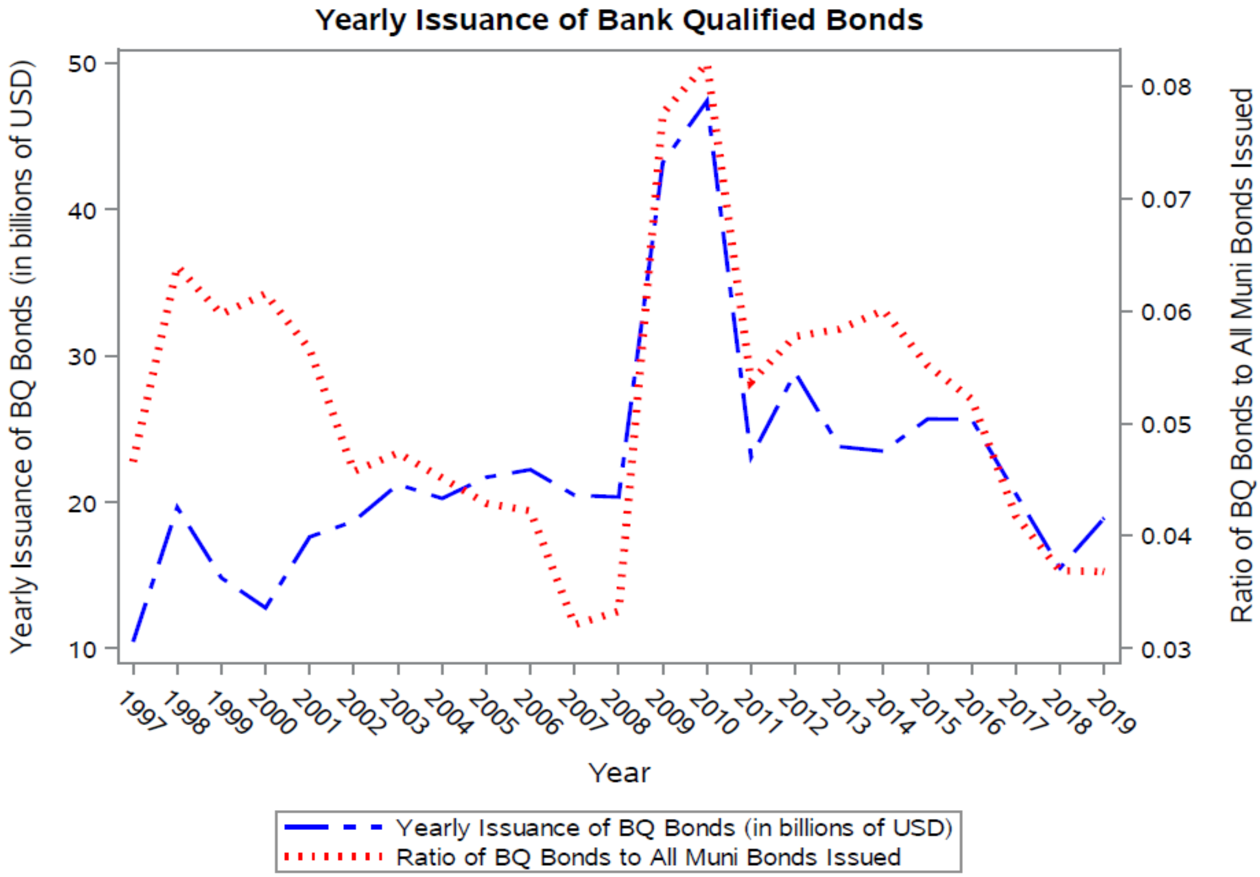

Figure 1: Issuance of Bank-qualified Bonds: The total issuance of tax-exempt bank-qualified bonds (in billions of USD) in each year is plotted against the left Y axis. The ratio of bank qualified bonds to total muni bond issuance (tax-exempt) in a given year is plotted against the right Y axis.

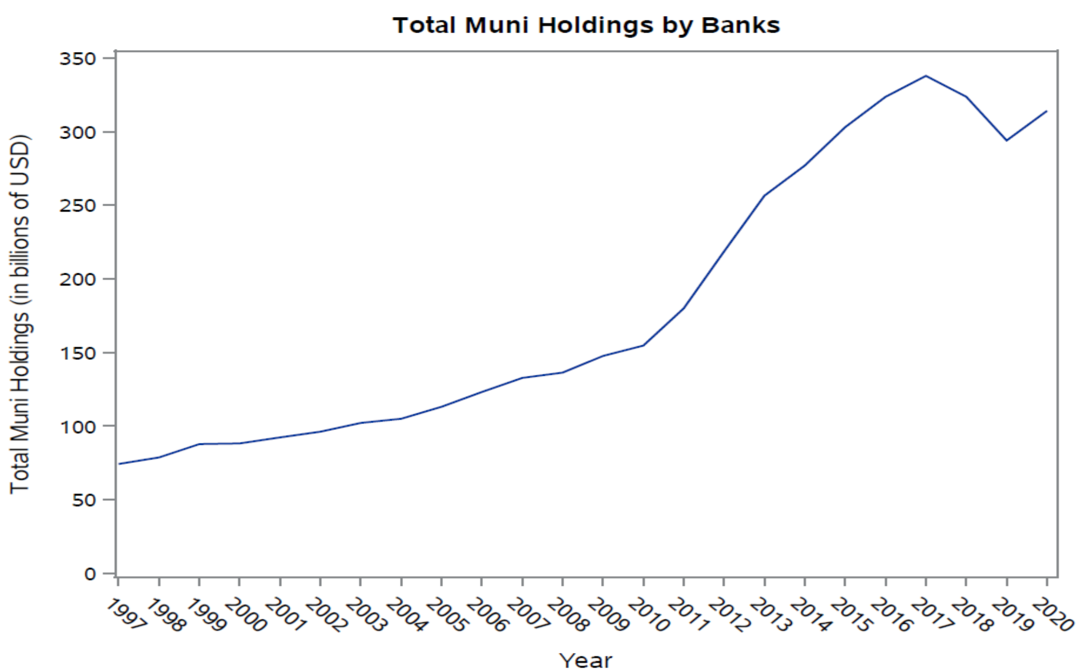

Figure 2: Bank Holdings of Municipal Bonds: This figure plots the total municipal bond holdings across all BHCs and Standalone banks in the US from 1997-2020.

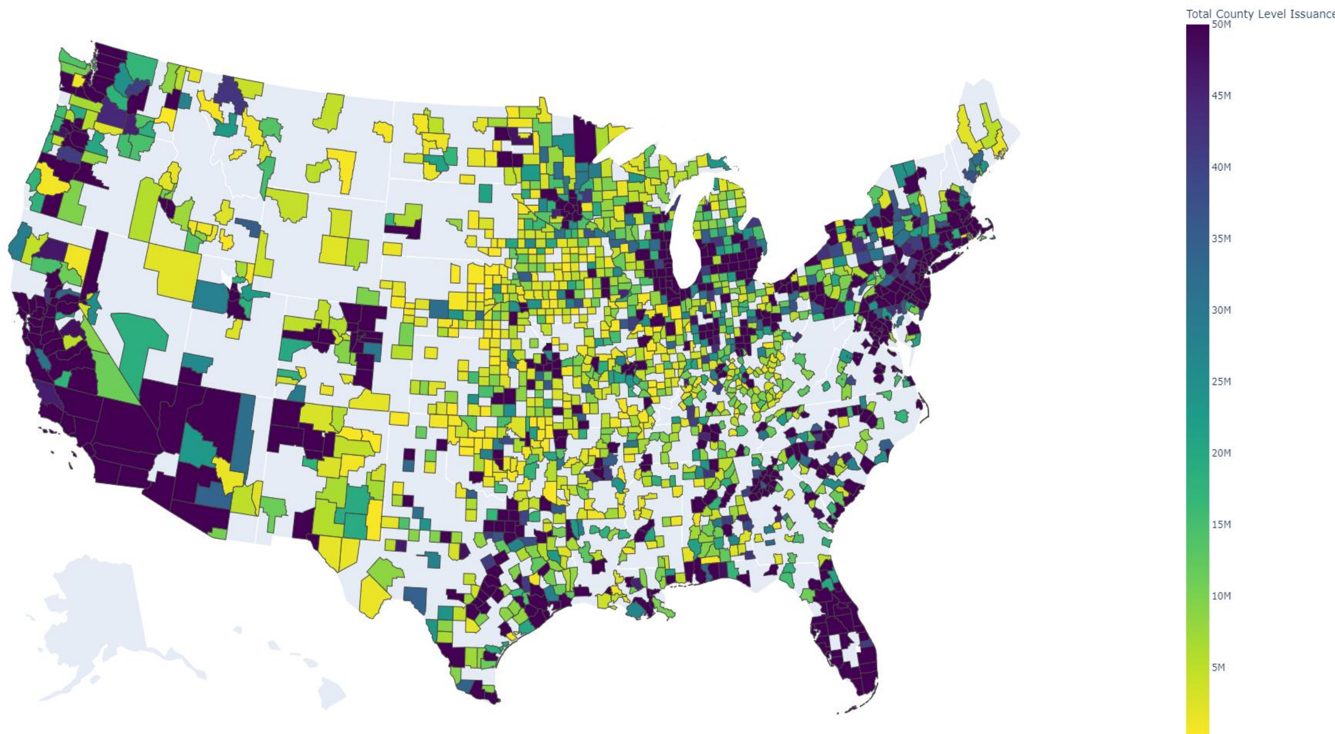

Figure 3.1: County Level Muni Bond Issuance - This map shows the total issuance of tax-exempt bank and non-bank qualified municipal bonds (in millions of USD) at the county level for the year 2005.

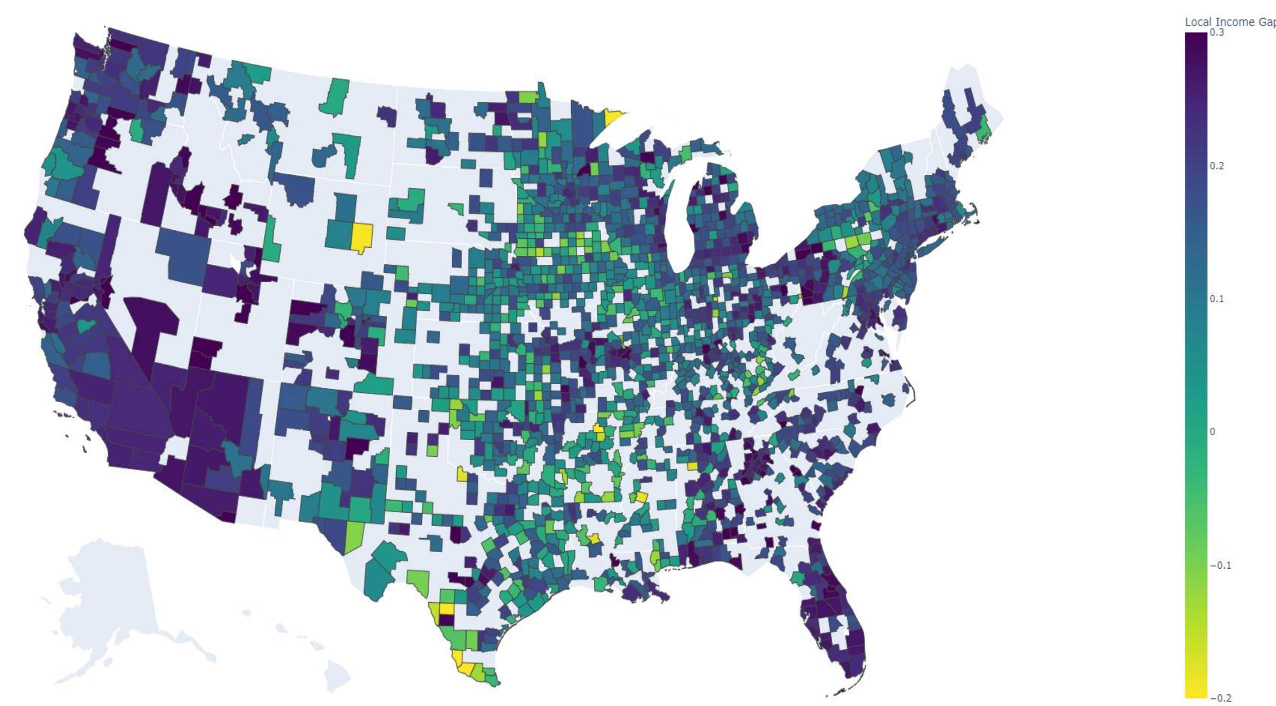

Figure 3.2: County Income Gap - This map shows the local income gap ratio at the county level for the year 2005.

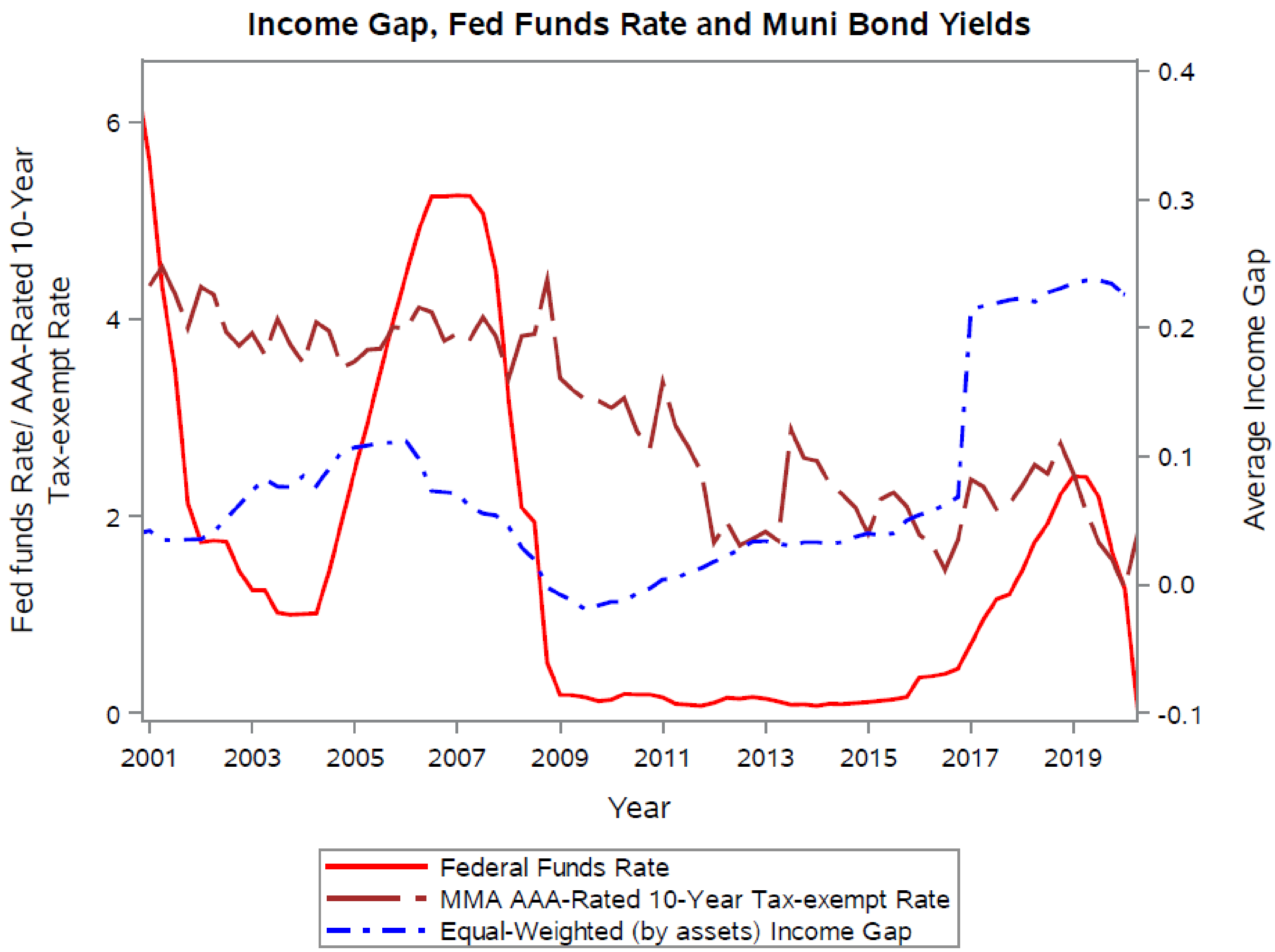

Figure 4: This figure plots the MMA AAA-rated 10-year tax exempt rate (against the left Y-axis), the equal-weighted (by assets) total income gap (includes all BHCs and standalone banks in the US) against the right Y-axis and the federal funds rate (against the left Y-axis). The MMA curve is reported daily on Bloomberg from 2001 onward and the asset-weighted income gap is constructed from FR Y-9 C & Call Reports. The federal funds rate is from the St. Louis Fed's FRED database. The sample is from 1997Q2-2020Q1.

TABLE 1: Summary Statistics – Bank Balance Sheet Variables

Panels A, B & C report summary statistics for the relevant explanatory variables at the county year quarter level, which are constructed from bank balance sheets, for all commercial banking institutions that file their Call Reports, from 1997Q2-2019Q1. All variable definitions are included in Tables A.1.

Panel A:

Variables	N	Mean	Std. Deviation	P25	Median	P75
Local Income Gap	294218	0.127	0.120	0.0439	0.129	0.216
Local Income Gap (including deposits)	294122	-0.0206	0.111	-0.0919	-0.0121	0.0594
Local Repricing Maturity	294188	4.228	1.510	3.099	4.111	5.263
Local Equity Ratio	294241	0.105	0.0183	0.0924	0.103	0.115
Local Liquid Assets Ratio	294220	0.219	0.0806	0.166	0.204	0.259
Local Illiquid Assets Ratio	294111	0.712	0.0816	0.675	0.727	0.766
Local Core Deposits Ratio	294283	0.837	0.0696	0.803	0.846	0.883
Local Wholesale Funding Ratio	294206	0.206	0.0689	0.154	0.198	0.254
Local Unused Commitments Ratio	293993	0.137	0.0559	0.0987	0.128	0.170
Average Bank HHI	294403	0.271	0.138	0.183	0.230	0.312
County HHI	294403	0.348	0.222	0.194	0.282	0.430

Panel B: Matrix of correlations (County level variables)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Local Income Gap	1.000									
(2) Local Repricing Maturity	0.128	1.000								
(3) Local Equity Ratio	0.116	0.180	1.000							
(4) Local Liquid Assets Ratio	-0.357	0.066	0.166	1.000						
(5) Local Illiquid Assets Ratio	0.198	0.137	-0.189	-0.572	1.000					
(6) Local Core Deposits Ratio	0.251	0.146	0.190	0.050	0.069	1.000				
(7) Local Wholesale Funding Ratio	-0.138	-0.059	-0.353	-0.204	0.219	-0.622	1.000			
(8) Local Unused Commitments Ratio	0.173	-0.117	-0.143	-0.215	0.220	-0.023	0.303	1.000		
(9) County HHI	-0.099	-0.037	0.029	0.147	-0.121	-0.053	-0.022	-0.101	1.000	
(10) Average Bank HHI	-0.269	-0.170	0.059	0.289	-0.284	-0.104	-0.083	-0.220	0.716	1.00

Panel C: Between and Within Std. Dev. Of the Local Income gap at the County year-quarter level

Variable		Mean	Std. Dev.	Min	Max	Observations
Local Income Gap	Overall	0.127	0.119	-0.396	0.576	N = 294218
	Between		0.082	-0.151	0.429	n = 3224
	Within		0.088	-0.449	0.588	T-bar = 91.259

Panels D, E & F report summary statistics for variables constructed from bank balance sheets, for all commercial banking institutions that file their Call Reports, from 1997Q2-2019Q1. All variable definitions are included in Table A.1.

Panel D:

Variables	N	Mean	Std. Deviation	P25	Media	P75
Income Gap	521224	0.0672	0.162	-0.0471	0.0571	0.175
Income Gap (including deposits)	521534	-0.0794	0.159	-0.190	-0.0872	0.0242
Repricing Maturity	521754	3.468	2.017	1.961	3.044	4.579
Equity Ratio	522505	0.108	0.0342	0.0863	0.101	0.121
Liquid assets ratio	527547	0.229	0.142	0.121	0.210	0.319
Illiquid Assets Ratio	521304	0.680	0.136	0.603	0.704	0.783
Core Deposits Ratio	522236	0.833	0.0946	0.782	0.850	0.902
Wholesale Funding to Assets ratio	522579	0.183	0.0957	0.111	0.170	0.241
Unused Commitments Ratio	526144	0.119	0.0693	0.0684	0.111	0.161

Panel E: Matrix of correlations (Bank level variables)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) Income Gap	1.000								
(2) IG with Deposits	0.886	1.000							
(3) Repricing Maturity	-0.305	-0.351	1.000						
(4) Equity Ratio	0.074	0.085	-0.005	1.000					
(5) Liquid Assets Ratio	-0.259	-0.255	0.280	0.187	1.000				
(6) Illiquid Assets Ratio	0.076	0.152	0.028	-0.285	-0.610	1.000			
(7) Core Deposits Ratio	0.185	0.092	0.085	0.020	0.195	-0.209	1.000		
(8) Wholesale Funding Ratio	-0.193	-0.064	0.009	-0.169	-0.198	0.335	-0.862	1.000	
(9) Unused Commitments Ratio	0.254	0.206	-0.045	-0.054	-0.090	0.144	0.011	0.071	1.000

Panel F: Between and Within Std. Dev. Of the Local Income gap at the Bank year-quarter level

Variable		Mean	Std. Dev.	Min	Max	Observations
Income Gap	Overall	0.067	0.162	-0.411	0.581	N = 521224
	Between		0.133	-0.362	0.548	n = 11195
	Within		0.118	-0.648	0.739	T-bar = 46.559

Table 2: Summary Statistics: Municipal Bonds

This table summarizes the municipal bond level primary market characteristics during 1998Q2-2020Q1 for my sample of municipal bonds. Panel A presents the summary statistics for the bank-qualified bonds and Panel B presents the summary statistics for the non-bank qualified bonds. The key variables are described in Table A1.

Panel A: Bank Qualified Bonds

Variables	N	Mean	Std. Dev.	P25	Median	P75
Offering Yield	1,099,694	3.096	1.327	2.050	3.200	4.080
Tax-adjusted Yield Spread	1,099,694	1.809	1.173	1.112	1.723	2.386
Benchmark Yield	1,099,694	3.337	1.622	2.075	3.221	4.667
Coupon	1,099,694	3.518	1.152	2.750	3.625	4.250
Maturity	1,099,694	8.971	5.690	4.350	8.019	12.611
General Obligation	1,099,694	0.715	0.451	0	1	1
Callable	1,099,694	0.460	0.498	0.000	0.000	1.000
Insured	1,099,694	0.376	0.484	0.000	0.000	1.000
Negotiated	1,099,694	0.261	0.439	0.000	0.000	1.000
Bond Size	1,099,694	357,054	483,925	115,000	230,000	430,000
Issuer Offering amount	1,099,694	4,669,381	3,487,762	2,000,000	3,950,000	6,750,000

Panel B: Non-Bank Qualified Bonds

Variables	N	Mean	Std. Dev.	P25	Median	P75
Offering Yield	1,154,939	3.300	1.385	2.320	3.450	4.280
Tax-adjusted Yield Spread	1,154,939	2.051	1.646	1.274	1.984	2.696
Benchmark Yield	1,154,939	3.489	1.692	2.273	3.422	4.750
Coupon	1,154,939	4.150	1.014	3.500	4.150	5.000
Maturity	1,154,939	10.618	6.542	5.361	9.789	14.889
General Obligation	1,154,939	0.518	0.500	0	1	1
Callable	1,154,939	0.488	0.500	0.000	0.000	1.000
Insured	1,154,939	0.404	0.491	0.000	0.000	1.000
Negotiated	1,154,939	0.377	0.485	0.000	0.000	1.000
Bond Size ($ in thousands)	1,154,939	3011.6	14448.5	415	995	2360
Issue Size ($ in millions)	1,154,939	54.989	140.823	10.960	21.215	48.000

Table 3: The effect of Local Banks' Income Gap on Municipal Bond Offering Yields

This table reports the baseline results for the sample using Equation (1). The dependent variable in columns (1) and (2) is the offering yield and from columns (3) – (6) is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in columns (1)-(4). I provide the description of key variables in Tables A.1, A.2 & A.3. Column (1) reports the results using credit-rating & debt-type effects, county fixed effects, state-year fixed effects, bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity and a set of time varying county-level controls – the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. In column (2), I introduce issuer fixed effects and underwriter fixed effects. I repeat the above steps with tax-adjusted yield spread as the dependent variable in columns (3), (4). In columns (5) and (6), I use the equal-weighted local income gap as my main explanatory variable. My sample period extends from 1998Q2 – 2020Q1. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels.

	BANK-QUALIFIED BONDS					
Dependent Variable	Offering Yield		Tax-adjusted Yield Spread			
	(1)	(2)	(3)	(4)	(5)	(6)
Local Income Gap (LIG)	-0.140***	-0.158***	-0.310***	-0.336***		
	(-3.23)	(-4.25)	(-3.86)	(-5.07)		
Local Income Gap (equal-weighted)					-0.279***	-0.281***
					(-2.80)	(-3.31)
Maturity	0.065***	0.061***	0.099***	0.093***	0.099***	0.093***
	(46.69)	(47.64)	(44.28)	(44.73)	(44.28)	(44.74)
Inverse Maturity	-0.410***	-0.412***	-0.483***	-0.523***	-0.483***	-0.523***
	(-12.89)	(-12.96)	(-10.68)	(-12.07)	(-10.68)	(-12.04)
Log (Bond Size)	-0.062***	-0.039***	-0.099***	-0.060***	-0.099***	0.046***
	(-19.54)	(-18.38)	(-18.85)	(-16.41)	(-18.86)	(10.57)
General Obligation	-0.166***	-0.009	-0.284***	-0.007	-0.284***	-0.016*
	(-19.56)	(-1.54)	(-20.06)	(-0.70)	(-20.09)	(-1.71)
Callable	0.070***	0.068***	0.077***	0.078***	0.077***	0.644***
	(10.26)	(11.59)	(8.00)	(10.43)	(8.00)	(40.38)
Insured	-0.119***	-0.098***	-0.202***	-0.162***	-0.202***	-0.158***
	(-10.25)	(-7.90)	(-10.92)	(-8.58)	(-10.91)	(-7.63)
Competitive	-0.114***	-0.059***	-0.194***	-0.097***	-0.194***	-0.067***
	(-10.87)	(-9.13)	(-11.41)	(-9.00)	(-11.35)	(-4.82)
Benchmark Yield	0.499***	0.514***				
	(35.27)	(36.61)				
Observations	1,096,292	1,082,884	1,096,292	1,082,884	1,096,292	1,082,884
R-squared	0.873	0.953	0.522	0.788	0.523	0.783
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Issuer FE		Yes		Yes		Yes
Underwriter FE		Yes		Yes		Yes

Table 4: Effect of Local Income Gap on Municipal Bond Offering Yields - Thresholds based on Banks' Use of Interest Rate Derivatives

This table reports the results using the same baseline specification as in Equation (1) but uses different definitions of the local income gap. The dependent variable is either the raw offering yield or the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. I provide the description of key variables in Tables A.1, A.2 & A.3. In columns (1) and (2), I construct the local income gap at the county quarter level by limiting the sample to banks with a zero or negative net hedging ratio. In columns (3) and (4), I construct the local income gap by limiting the sample of banks to banks with a zero or negative gross hedging volume. In columns (5) and (6), I construct the local income gap by limiting the sample of banks to banks with a zero or negative holdings of fixed-rate swaps. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Offering Yield	Tax-adjusted Yield Spread	Offering Yield	Tax-adjusted Yield Spread	Offering Yield	Tax-adjusted Yield Spread
	(1)	(2)	(3)	(4)	(5)	(6)
LIG Net Hedging	-0.113***	-0.257***				
	(-2.78)	(-3.43)				
LIG Gross Hedging			-0.050	-0.115*		
			(-1.43)	(-1.95)		
LIG Fixed Rate Swaps					-0.051*	-0.110**
					(-1.69)	(-2.07)
Observations	1,096,130	1,096,130	1,077,713	1,077,713	1,092,085	1,092,085
R-squared	0.873	0.522	0.871	0.519	0.872	0.521
Bond controls	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: Effect of Local Income Gap on Municipal Bond Offering Yields - Across Time Periods

This table reports the results using the same baseline specification as in Equation (1), and I estimate the effect across different time periods. The dependent variable is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. In column (1), I include the interaction between the local income gap and the federal funds rate as an additional explanatory variable in full sample regression. In columns (2) and (3), I restrict my sample to years between 1998 & 2000, and 2001 & 2004 respectively. In column (4), I restrict my sample to the years between 2005 and 2008. In columns (5) and (6), I restrict my sample to years between 2009 &2017, and 2018 & 2019 respectively. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread					
	Full sample	1998-2000	2001-2004	2005-2008	2009-2017	2018-2019
	(1)	(2)	(3)	(4)	(5)	(6)
Local Income Gap	-0.171***	-0.491*	-0.012	-1.425***	-0.153	-0.769
	(-2.94)	(-1.80)	(-0.09)	(-3.98)	(-1.10)	(-0.53)
LIG * Federal Funds Rate	-0.063*					
	(-1.94)					
Observations	1,096,292	96,182	226,905	193,364	500,017	79,724
R-squared	0.528	0.659	0.718	0.639	0.616	0.499
Bond control	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Intertemporal analysis with Callable Bonds

This table reports estimates from running the regression specified in equation (2). The dependent variable is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. The dummy variable callable equals 1 if the bond has a call option associated with it. In all regression specifications, I include the interaction term LIG * callable to capture the differential effect of local income gap on offering yields & yield spreads for callable bonds. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread					
	Full Sample	1998-2000	2001-2004	2005-2008	2009-2017	2018-2019
	(1)	(2)	(3)	(4)	(5)	(6)
LIG	-0.612***	-0.448	-0.116	-1.621***	-0.382***	-1.155
	(-6.76)	(-1.36)	(-0.84)	(-4.37)	(-2.75)	(-0.70)
LIG * Callable	0.810***	-0.086	0.229***	0.414***	0.513***	0.443*
	(8.62)	(-0.74)	(3.07)	(3.23)	(6.21)	(1.82)
Observations	1,096,292	96,182	226,905	193,364	500,017	79,724
R-squared	0.526	0.659	0.718	0.640	0.616	0.499
Bond control	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: The Role of Banks' (Deposit) Market Power

This table reports the baseline results for the sample using Equation (3) and (4). The dependent variable is either the raw Offering yield or the Tax-adjusted Offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. In columns (1) and (2), I include the average Bank HHI and its interaction with the local income gap as my explanatory variables of interest. In column (3), I include the triple interaction term between County HHI, the average Bank HHI and the local income gap as my explanatory variables of interest. I define average Bank HHI and County HHI in Table A.1. I also include two way interaction terms between the average local income gap, bank HHI, County HHI. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Offering Yield	Tax-adjusted Yield Spread	Tax-adjusted Yield Spread
	(1)	(2)	(3)
LIG	-0.417**	-0.843**	-0.975***
	(-2.04)	(-2.35)	(-2.67)
LIG * Avg. Bank HHI	1.212	2.329*	2.463
	(1.49)	(1.67)	(1.58)
Avg. Bank HHI	0.362	0.600	0.304
	(1.12)	(1.08)	(0.80)
LIG * Avg. Bank HHI * County HHI			-1.026**
			(-2.13)
Avg. Bank HHI * County HHI			0.343
			(1.07)
LIG * County HHI			0.640*
			(1.67)
County HHI			0.037
			(0.35)
Observations	1,096,292	1,096,292	1,096,292
R-squared	0.873	0.522	0.522
Bond control	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes
County Control	Yes	Yes	Yes
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes

Table 8: Does the Underwriting Method Matter?

This table reports the baseline results for the sample using Equation (5). The dependent variable is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. In column (1), I include dummy negotiated and its interaction with the local income gap as my explanatory variables of interest. The dummy negotiated takes a value of 1 if the bond was offered through a negotiated offering and a value of 0 if the bond was offered through a competitive sale method. I do not include other types of underwriting process (e.g., private placements) in this specification. I include the interaction of LIG with callable dummy and maturity variable as additional controls in column (2). In columns (3) and (4), I construct the local income gap by limiting the sample to only the top 20 banks by asset size and banks with asset size > 10 billion USD (adjusted to 2010 dollars) respectively and interact the respective LIG variables with the negotiated dummy. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread		
	(1)	(2)	(3)
Local Income Gap	-0.248***		
	(-2.82)		
LIG * Negotiated	-0.459***		
	(-4.96)		
LIG Top 20		-0.737***	
		(-4.62)	
LIG Top 20 * Negotiated		0.016	
		(0.12)	
LIG Large Banks			-0.523***
			(-4.11)
LIG Large Banks * Negotiated			-0.197*
			(-1.79)
Observations	828,730	882,353	741,935
R-squared	0.670	0.669	0.667
Bond control	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes
County Control	Yes	Yes	Yes
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes

Table 9: Ruling out Households as the Marginal Price-setters of IR Risk

This table reports the results using the same baseline specification as in Equation (1). The dependent variable is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable. I divide the sample into three terciles based on the per capita income of the county, and for the regression specification in column (1), I create dummy variables for tercile 2 and 3 interact them with the local income gap variable. In columns (2) and (3), I run the use the baseline specification but limit my sample to terciles 1 and 3 respectively. Column (4) reports the regression results using the average income gap across all the neighboring counties. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread			
	Full sample	Per Capita Income Tercile = 1	Per Capita Income Tercile = 3	Neighboring Counties LIG
	(1)	(2)	(3)	(4)
Local Income Gap	-0.275***	-0.307***	-0.329*	-0.036**
	(-4.04)	(-3.00)	(-1.78)	(-1.98)
LIG * Tercile 2 Dummy	-0.018			
	(-0.32)			
LIG * Tercile 3 Dummy	-0.087			
	(-1.19)			
Observations	1,096,292	353,084	372,021	1,076,395
R-squared	0.522	0.692	0.579	0.520
Bond control	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes

Table 10: Do Banks Price their Liquidity Risk Exposure?

This table reports the results with various bank illiquidity ratios (defined in Table A.2) as the explanatory variables of interest. The dependent variable is the tax adjusted offering yield spread. All the bank explanatory variables are lagged by 4 quarters in all the specifications. I define the construction of all the bank variables and their aggregation to the county level in Table A.2. My sample period extends from 1998Q2 – 2020Q1. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread					
	(1)	(2)	(3)	(4)	(5)	(6)
Local Income Gap						-0.310***
						(-3.85)
Local Illiquid Assets Ratio	-0.107				-0.110	-0.108
	(-0.91)				(-0.88)	(-0.86)
Local Core Deposits Ratio		-0.077			-0.030	0.024
		(-0.34)			(-0.15)	(0.12)
Local Wholesale Funding Ratio			0.085		0.088	0.063
			(0.37)		(0.39)	(0.28)
Local Unused Commitments Ratio				-0.059	-0.067	-0.073
				(-0.36)	(-0.40)	(-0.43)
Observations	1,096,292	1,096,292	1,096,292	1,096,292	1,096,292	1,096,292
R-squared	0.521	0.522	0.521	0.521	0.522	0.523
Bond control	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 11: Summary Statistics for Municipal Bond Mutual Funds

PANEL A: Municipal Bond Mutual Funds (MBMFs)

This table summarizes the municipal bond fund holdings during 2010Q1-2020Q1 for my sample of municipal bond funds. It provides information about the size of overall assets, cash holding, and municipal bond holding for municipal mutual funds. The data are obtained from the database of the Center for Research in Security Prices (CRSP). The first column reports the number of fund portfolios studied in each year. The second and third column provide the average size of the portfolio (in USD millions), and the total value of aggregate bond fund holdings of muni bonds in each year (in USD billions respectively). The fourth & fifth columns list the distinct number of municipal bonds and issuers, held on average by each fund portfolio. Column six shows muni bonds as a percentage of the total portfolio holdings. Finally columns seven & eight report the average government bond and cash holding of the portfolios.

Year	# of Portfolios	Mean TNA ($ Million)	Total Holdings by all Muni Bond Funds ($ Billion)	# of Distinct Muni bonds	# of Distinct Muni Issuers	Amount of Portfolio invested in Muni bonds (%)	Amount of Portfolio invested in Govt. bonds (%)	Amount of Portfolio invested in Cash (%)
2010	686	742.46	508.58	227	135	97.93	0.01	0.85
2011	649	809.96	521.61	228	133	97.17	0.03	-1.01
2012	608	984.56	594.68	269	149	97.24	0.04	1.85
2013	621	834.26	517.24	255	140	97.22	0.13	1.58
2014	628	941.76	583.89	267	143	97.15	0.13	2.17
2015	634	963.72	610.03	287	148	96.74	0.07	1.97
2016	636	1031.25	654.84	330	160	97.53	0.29	1.40
2017	637	1123.08	706.42	364	166	97.35	0.38	1.43
2018	632	1142.59	716.41	378	169	97.15	0.25	1.16
2019	630	1381.02	860.37	434	188	97.50	0.27	0.99
2020	620	1289.35	798.11	429	184	96.83	0.48	1.21

PANEL B: Matrix of Correlations (Portfolio level variables)

Variables	(1)	(2)	(3)	(4)	(5)
(1) Portfolio % Cash Holdings	1.000				
(2) Portfolio Income Yield	-0.019	-0.019			
(3) Portfolio Avg. Maturity	-0.037	-0.037	-0.037		
(4) Portfolio Turnover Ratio	0.098	0.098	0.098	0.098	
(5) Portfolio Rear Load Indicator	-0.066	-0.066	-0.066	-0.066	-0.066

PANEL C: Between and Within Std. Dev. of the Portfolio % of Cash Holdings at the year-quarter level

Variable		Mean	Std. Dev.	Min	Max	Observations
Portfolio % Cash Holdings	Overall	1.378	2.284	-5.390	14.910	N = 53393
	Between		1.483	-3.480	14.320	n = 1432
	Within		1.939	-9.362	15.494	T-bar = 37.286

Table 12: Summary Statistics for Municipal Bonds

PANEL A: Municipal Bonds (Held by MBMFs at Issuance)

This table summarizes the municipal bond level primary market characteristics during 2010Q2-2020Q1 for my sample of municipal bonds. Panel A presents the summary statistics for the bonds held by MBMFs at issuance, and Panel B presents the summary statistics for bonds without any MBMF holdings. The variable equal-weighted fund cash holdings is defined in appendix table A3. I follow a similar procedure in constructing the average income yield, average fund-weighted maturity, average rear-load indicator and the average portfolio-turnover ratio, by taking an equal-weighted average across all portfolios that hold the bond at issuance. The only difference with the cash holdings measure is that for the portfolio characteristics - income yield, weighted maturity, and rear load and turnover ratio; instead of taking a value-weighted average of these characteristics of funds belonging to one portfolio, I just use these measures from the fund with the maximum total net assets amongst all funds within the portfolio.

Variables	N	Mean	Std. Dev.	P25	Median	P75
Offering Yield	77609	2.796	1.209	1.970	2.700	3.440
Tax-adjusted Yield Spread	77609	2.509	1.633	1.418	2.339	3.309
Benchmark Yield	77609	2.301	0.798	1.863	2.352	2.808
Coupon	77609	4.555	0.877	4	5	5
Maturity	77609	12.91	7.774	6.789	11.99	17.79
General Obligation	77609	0.324	0.468	0	0	1
Callable	77609	0.590	0.492	0	1	1
Insured	77609	0.132	0.339	0	0	0
Competitive	77609	0.309	0.462	0	0	1
Bond Size ($ in thousands)	77609	10858	28150	1705	4000	10000
Issue Size ($ in millions)	77609	123.3	199.4	23.59	55.10	134.3
Percentage held by bond funds (at issuance)	77609	0.650	0.344	0.359	0.615	0.962
Distinct # of Portfolios at issuance	77609	2.047	2.608	1	1	2
Fund Cash Holdings (Equal weighted)	77609	1.534	2.633	0	0.750	2.110
Average Rear Load Indicator	77609	0.196	0.345	0	0	0.333
Average Portfolio-Turnover Ratio	77609	0.341	0.342	0.150	0.237	0.390

PANEL B: Municipal Bonds (Not Held by MBMFs at Issuance)

Variables	N	Mean	Std. Dev.	P25	Median	P75
Offering Yield	958991	2.266	1.085	1.490	2.200	3
Tax-adjusted Yield Spread	958991	1.724	1.346	0.867	1.581	2.448
Benchmark Yield	958991	2.109	0.915	1.518	2.186	2.732
Coupon	958991	3.266	1.125	2.250	3	4
Maturity	958991	9.321	5.962	4.536	8.264	13.10
General Obligation	958991	0.650	0.477	0	1	1
Callable	958991	0.462	0.499	0	0	1
Insured	958991	0.180	0.384	0	0	0
Competitive	958991	0.665	0.472	0	1	1
Bond Size ($ in thousands)	958991	1180	3332	200	445	1070
Issue Size ($ in millions)	958991	23.61	63.39	3.660	8.050	19.56

Table 13: Municipal Bond Mutual Funds' Cash Holdings and Offering Yield Spreads

This table reports the baseline results for the sample using Equation (7). The dependent variable in all the columns is the Tax-adjusted Offering yield spread. I use the one quarter lagged equal-weighted fund cash holdings (EWFCH) and the value-weighted fund cash holdings as my main explanatory variables in columns (1), (2) & (4), and columns (3) and (5) respectively. I define the EWFCH measure in table A.3. The value-weighted counterpart is constructed by substituting uniform weights with the size of portfolio holdings of the bond at issuance. I also include the interaction of EWFCH (VWFCH) with the bond credit rating in all regression specifications. In all the columns from (1) to (5), I use bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. I also use time-invariant fixed effects that include the debt-type and issuer fixed effects. Next, I introduce a set of time varying fixed effects – the credit-rating of the bond interacted with the year & month of bond issuance fixed effects, county * year fixed effects, and finally the maturity of the bond interacted with the year & month of bond issuance fixed effects in columns (4) & (5). Additionally, in columns (2)-(5), I use a variable dummy which takes the value of 1 if the bond is held by any mutual fund at issuance. My sample period extends from 2010Q2 – 2020Q1. T-statistics are reported in parentheses and standard errors are double clustered at the issuer and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread				
	(1)	(2)	(3)	(4)	(5)
Fund Cash Holdings (Equal weighted)	0.023***	0.026***		0.018***	
	(4.25)	(4.99)		(4.61)	
Fund Cash Holdings (Equal weighted) × Credit Rating	-0.001***	-0.001***		-0.001***	
	(-3.45)	(-3.45)		(-4.01)	
Fund Cash Holdings (Value weighted)			0.019***		0.014***
			(3.96)		(3.70)
Fund Cash Holdings (Value weighted) × Credit Rating			-0.001***		-0.001***
			(-3.07)		(-3.09)
Dummy for Mutual Fund Ownership		-0.025***	-0.022***	-0.003	-0.002
		(-3.43)	(-2.85)	(-0.59)	(-0.40)
Observations	1,034,061	1,034,061	1,034,061	841,840	841,840
R-squared	0.781	0.781	0.781	0.947	0.947
Bond controls	Yes	Yes	Yes	Yes	Yes
Credit Rating × Year-Month FE	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes	Yes
Maturity × Year-Month FE				Yes	Yes

Table 14: Effects of Other Fund Characteristics on Offering Yield Spreads

This table reports the baseline results for the sample using Equation (8). The dependent variable in all the columns is the Tax-adjusted Offering yield spread. I use the one quarter lagged equal-weighted fund cash holdings (EWFCH), its interactions with bond credit rating, and other fund-portfolio characteristics as my main explanatory variables. In column (1), I include the distinct number of portfolios which holding the bond at issuance. In column (2), I use the average of the rear load indicator (=1, if the fund has fees associated with shares redemption) across all portfolios holding the bond. In column (3), I use the average of the turnover ratio (defined as the minimum of aggregated sales or aggregated purchases of securities), divided by the average 12-month Total Net Assets of the fund) across all portfolios holding the bond. In all the columns from (1) to (3), I use bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. I also use time-invariant fixed effects that include the debt-type and issuer fixed effects. Next, I introduce a set of time varying fixed effects – the credit-rating of the bond interacted with the year & month of bond issuance fixed effects, county * year fixed effects. My sample period extends from 2010Q2 – 2020Q1. T-statistics are reported in parentheses and standard errors are double clustered at the issuer and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread		
	(1)	(2)	(3)
Equal weighted Fund Cash Holdings (EWFCH)	0.022***	0.025***	0.025***
	(3.85)	(4.79)	(4.76)
Credit Rating × EWFCH	-0.001***	-0.001***	-0.001***
	(-3.63)	(-3.32)	(-3.38)
Distinct # of Fund-Portfolios at issuance	-0.040***		
	(-10.74)		
Average Rear Load Indicator		-0.075***	
		(-6.05)	
Average Portfolio Turnover Ratio			-0.055***
			(-4.28)
Observations	1,056,680	1,056,680	1,056,680
R-squared	0.766	0.766	0.766
Bond controls	Yes	Yes	Yes
Credit Rating × Year-Month FE	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes
County Control	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes

Table 15: Instrumental Variable Approach

In this table, I report the first stage and second-stage results of two-stage least squares year-month level panel regression of tax-adjusted offering yield spreads (in percentage points) on the equal-weighted fund cash holdings variable (EWFCH). To construct the instrumental variable for a given bond, I implement an IV approach as motivated in Koijen and Yogo (2019). For each portfolio at each quarter-end, I construct a hypothetical portfolio that equally divides the fund's total net assets over its investment universe, which is measured as a set of all issuers whose bonds have been held by the fund at least once within the last three years. I refer to the equal-weighted holdings based on a fund's investment universe as its hypothetical holdings. To construct the IV for EWFCH for bond *i*, I equal-weight the one quarter-lagged cash holdings of all portfolios (that hypothetically could hold the bond at issuance). I report the first-stage results in column (1). I further report the Kleibergen-Paap F-statistic for the weak instrument test. In all the columns from (1) to (4), I use bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. I also use time-invariant fixed effects that include the debt-type and issuer fixed effects. Next, I introduce a set of time varying fixed effects – the credit-rating of the bond interacted with the year & month of bond issuance fixed effects and county * year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at the issuer and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. I limit my sample period to start from 2012Q1, and it extends till 2020Q1.

Dependent Variable	EWFCH	Tax-adjusted Yield Spread		
	Full Sample			Insured bonds
	(1)	(2)	(3)	(4)
Hypothetical Fund Cash Holdings (Instrument)	0.352***			
	(24.77)			
Equal weighted Fund $\widehat{\text{Cash}}$ Holdings (EWFCH)		0.0232***	0.083***	0.026**
		(-3.01)	(4.85)	(2.27)
EWFCH × $\widehat{\text{Credit}}$ Rating			-0.004***	
			(-4.25)	
Dummy for Mutual Fund Ownership	1.047***	-0.053***	-0.057***	-0.065***
	(39.6)	(-3.74)	(-4.02)	(-2.77)
Observations	863,551	863,551	863,551	152,022
Kleibergen-Paap F-statistic		613.75	298.11	71.15
Bond controls	Yes	Yes	Yes	Yes
Credit Rating × Year-Month FE	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes

Table 16: Instrumental Variable approach (Cross-Section of Fund Characteristics)

In this table, I report the second-stage results of two-stage least squares year-month level panel regression of tax-adjusted offering yield spreads (in percentage points) on other fund characteristics (as described in Table 14). To construct the instrumental variables for a given bond for each of the characteristics and their interactions, I implement the exact same IV approach as described in Table 15. For each portfolio at each quarter-end, I construct a hypothetical portfolio that equally divides the fund's total net assets over its investment universe, which is measured as a set of all issuers whose bonds have been held by the fund at least once within the last three years. I refer to the equal-weighted holdings based on a fund's investment universe as its hypothetical holdings. To construct the IV for fund characteristic j for bond *i*, I equal-weight the characteristics j of all portfolios (that hypothetically could hold the bond at issuance). I report the Kleibergen-Paap F-statistic for the weak instrument test. In all the columns from (1) to (3), I use bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. I also use time-invariant fixed effects that include the debt-type and issuer fixed effects. Next, I introduce a set of time varying fixed effects – the credit-rating of the bond interacted with the year & month of bond issuance fixed effects and county * year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at the issuer and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. I limit my sample period to start from 2012Q1, and it extends till 2020Q1.

Dependent Variable	Tax-adjusted Yield Spread		
	(1)	(2)	(3)
$\widehat{\text{EWFCH}}$	0.088***	0.083***	0.088***
	(5.10)	(4.78)	(4.38)
$\widehat{\text{EWFCH} \times \text{Credit Rating}}$	-0.004***	-0.004***	-0.003***
	(-4.60)	(-4.22)	(-4.03)
$\widehat{\text{Distinct \# of Fund Portfolios at issuance}}$	-0.035***		
	(-4.41)		
$\widehat{\text{Average Rear Load Indicator}}$		-0.006	
		(-0.08)	
$\widehat{\text{Average Portfolio Turnover Ratio}}$			-0.039
			(-0.83)
Observations	863,551	863,551	863,551
Kleibergen-Paap F-statistic	196.5	56.65	21.21
Bond controls	Yes	Yes	Yes
Credit Rating × Year-Month FE	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes
County Control	Yes	Yes	Yes
County × Year FE	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes

INTERNET APPENDIX - FOR ONLINE PUBLICATION ONLY

Table IA.1: Effect of Local Income Gap on Non-Bank Qualified Bonds

This table reports the results using the same baseline specification as in Equation (1), but the sample here includes 'non-bank qualified bonds' (NBQBs). The dependent variable all the columns is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. In column (1), I include the full sample of bank-qualified and NBQBs from 1998Q2-20202Q1, whereas in column (2), I limit my sample to just the NBQBs. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects and state-year fixed effects. In columns (3) and (4), I divide my sample into two subsets based on the following criteria: if the issuer of the NBQBs has also issued a bank-qualified bond in the past, I classify them into the subset with CI (common issuer) =1. If the issuer has never issued a bank-qualified bond in the past, I classify those NBQBs into the subset with CI=0. In columns (5) and (6), I divide my sample into two subsets based on the following criteria: if the lead underwriter of the NBQB has served as the lead underwriter for a bank-qualified bond in the past, I classify those NBQBs into the subset with CU (common underwriter) =1. If the lead underwriter has never underwritten a bank-qualified bond in the past, I classify those NBQBs as CU=0. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread					
	Full Sample	Non-bank Qualified Bonds				
			[CI = 0]	[CI = 1]	[CU = 0]	[CU = 1]
	(1)	(2)	(3)	(4)	(5)	(6)
Local Income Gap	0.223**	0.129	0.265	0.064	0.141	0.206
	(2.00)	(0.83)	(1.37)	(0.41)	(0.88)	(0.91)
Local Income Gap × Bank-Qualified	-0.603***					
	(-7.07)					
Observations	2,251,286	1,154,939	772,275	382,414	673,809	480,890
R-squared	0.414	0.373	0.400	0.327	0.635	0.264
Bond control	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.2: Robustness Checks

This table reports the results using the same baseline specification as in Equation (1). The dependent variable in all the columns is the tax-adjusted offering yield spread. In column (1), I include the logarithm of the average county bank size and the average equity ratio of local banks as additional explanatory variables. In column (2) and (3), I include the repricing or duration gap as defined in table A.2. In column (4), I use the local income gap with deposits as my main explanatory variable. In column (5), I include a set of macroeconomic controls - Inflation is the one-year growth rate in the CPI, taken from the FRED database. Output gap corresponds to the difference between real seasonally adjusted GDP (GDPC1 from the FRED database) and real potential GDP (GDPPOT from FRED), normalized by real seasonally adjusted GDP. IP growth is the one-year growth rate in industrial production (INDPRO in FRED). In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects and state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread				
	(1)	(2)	(3)	(4)	(5)
Local Income Gap (LIG)	-0.282***		-0.319***		-0.296***
	(-3.64)		(-3.82)		(-3.39)
Log (Average Bank Size)	-0.031				
	(-1.12)				
Local Banks' Average Equity Ratio	0.954				
	(1.17)				
Local Repricing Gap/ Duration Gap		0.017***	0.012**		
		(2.9)	(1.97)		
LIG with Deposits				-0.220***	
				(-3.51)	
Inflation					0.033
					(0.74)
Output Gap					-0.088**
					(-2.31)
IP Growth					-0.023**
					(-2.56)
Observations	1,096,292	1,096,292	1,096,292	1,096,292	1,096,292
R-squared	0.661	0.525	0.525	0.525	0.525
Bond controls	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes

Table IA.3: Do the effects vary by Credit Ratings?

This table reports the results using the same baseline specification as in Equation (1). The dependent variable is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. In column (1), I include the interaction term between local income gap and the credit rating variable, which takes values starting from 22 (=AAA) to 1 (=D), and is zero for unrated bonds. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. In column (2), I also include time-varying rating fixed effects. In columns (3), I restrict my sample to the set of non-investment grade bonds – which correspond to bonds with S&P ratings of BBB- & below, and unrated bonds. In column (4), I restrict my sample to bonds with S&P ratings equal to or greater than BB+. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread			
	Full Sample		Non-Investment Grade	Investment Grade
	(1)	(2)	(3)	(4)
Local Income Gap	-0.470***	-0.344***	-0.365***	-0.417***
	(-6.16)	(-4.18)	(-3.10)	(-4.80)
LIG × Credit Rating	0.014***	0.000		
	(3.16)	(0.01)		
Observations	1,095,496	1,095,494	575,315	519,460
R-squared	0.659	0.662	0.650	0.767
Bond controls	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	No	No	No
Credit Rating × Year FE	No	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes

Table IA.4: Local Income Gap and Local Economic Conditions

This table reports the correlations between local income gap and contemporaneous and one-period ahead county level economic outcome variables. Column (1) of Panel A reports the results of the regression of local income gap on contemporaneous county level variables. The observations are at the county year level, and my sample period extends from 1998-2019. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, the log of per capita county level income, the unemployment rate, the GINI/ Income Inequality index, the diversity index, and the percentage of population without a high-school diploma. Standard errors are clustered at the county level. In Column (2), I use the same baseline specification as in Equation (1) along with including the additional set of county level controls I included in column (1). Panel B reports the effect of local income gap on local governments' one–period ahead issuer finances. The observations are at the local government-year level, and my sample period extends from 1998-2019. The set of time varying county-level controls include the log of the county population, the log of per capita county level income, the unemployment rate, the GINI/ Income Inequality index, the diversity index, and the percentage of population without a high-school diploma. Additionally, in all my specifications, I include local government fixed effects and state-year fixed effects. Column (1) reports the results of the regression of the one period ahead total revenue to expenditure ratio on the local income gap. In Columns (2), (3) and (4), I use the ratio of the total debt outstanding to the total expenditure, the ratio of the total long-term debt outstanding to total expenditure and the interest on general debt to total expenditure respectively, as my main dependent variables. T-statistics are reported in parentheses and *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

PANEL A

Dependent Variable	Local Income Gap	Tax-adjusted Yield Spread
	(1)	(2)
Local Income Gap		-0.358***
		(-5.11)
Log (Population)	0.001	-0.186
	(0.12)	(-1.58)
Log (Per Capita Income)	0.001	-0.360***
	(0.11)	(-2.61)
Unemployment Rate	-0.002***	-0.033**
	(-3.45)	(-2.24)
GINI Index (Income Inequality)	0.081*	-0.578
	(1.73)	(-1.39)
Diversity Index	0.097**	-0.784*
	(2.39)	(-1.91)
% without High School Diploma	0.000	0.003
	(0.64)	(0.70)
Observations	71,252	1,087,808
R-squared	0.774	0.656
County FE	Yes	Yes
State × Year FE	Yes	Yes
Bond controls		Yes
Credit Rating × Year FE		Yes
Issuer FE		Yes
Debt Type FE		Yes

PANEL B

	(1)	(2)	(3)	(4)
Dependent Variable	Total Revenue/ Total Expenditure	Total Debt Outstanding/ Total Expenditure	Long-term Debt Outstanding/ Total Expenditure	Interest on Debt/ Total Expenditure
Local Income Gap	-0.012	0.016	0.013	0.000
	(-1.12)	(0.61)	(0.49)	(0.29)
Observations	1,725,429	1,742,997	1,743,003	1,743,045
R-squared	0.421	0.702	0.702	0.680
County Controls	Yes	Yes	Yes	Yes
Entity FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes

Table IA.5: Effect across Bond Maturities

This table reports the results using the same baseline specification as in Equation (1). The dependent variable in all columns is the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable. Column (1) interacts the local income gap with bond maturity (in years). Next, I segregate the sample into five quintiles based on the bond maturity. Columns (2) - (6) report the results for each maturity quintile. In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Tax-adjusted Yield Spread					
		Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
	(1)	(2)	(3)	(4)	(5)	(6)
Local Income Gap	-0.930***	-0.336***	-0.391***	-0.323***	-0.209**	-0.243**
(LIG)	(-8.51)	(-3.62)	(-4.63)	(-3.36)	(-2.36)	(-2.25)
LIG × Maturity	0.070***					
	(8.40)					
Maturity	0.089***	0.170***	0.106***	0.025*	0.104***	0.070***
	(32.14)	-10.09	(6.10)	(1.80)	(7.84)	(14.27)
Observations	1,096,292	220,400	219,929	219,185	218,241	218,329
R-squared	0.523	0.359	0.328	0.358	0.389	0.586
Bond control	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.6: Effects Using Different Bank level Thresholds

This table reports the results using the same baseline specification as in Equation (1), but uses different definitions of the local income gap. The dependent variable is either the raw offering yield or the tax-adjusted offering yield spread. I use the 4 quarters lagged local income gap as my main explanatory variable in all the columns. I provide the description of key variables in Tables A.1, A.2 & A.3. In columns (1) and (2), I construct the local income gap by only including banks that report holding any municipal securities in their Call Report filings. In columns (3) and (4), I construct the Local income gap by limiting the sample to the top 20 largest banks by asset size every quarter. In columns (5) and (6), I construct the local income gap by limiting the sample of banks to banks with assets >10 billion USD (adjusted to 2010 dollars). In each specification, I include bond level controls consisting of log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. The set of time varying county-level controls include the level (log) and percentage change in population of the county, the percentage change in employment, and the log of per capita county level income. Finally, I include credit rating fixed effects, debt type fixed effects, county fixed effects ad state-year fixed effects. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	Offering	Tax-adjusted	Offering	Tax-adjusted	Offering	Tax-adjusted
	(1)	(2)	(3)	(4)	(5)	(6)
LIG Muni Holdings	-0.142***	-0.309***				
	(-3.40)	(-4.04)				
LIG Top 20			-0.168***	-0.386***		
			(-2.60)	(-3.52)		
LIG Large Banks					-0.153**	-0.357***
					(-2.55)	(-3.49)
Observations	1,095,996	1,095,996	892,117	892,117	976,146	976,146
R-squared	0.873	0.522	0.859	0.500	0.865	0.510
Bond control	Yes	Yes	Yes	Yes	Yes	Yes
Credit Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.7: Effect of Local Income Gap on Non-Price Terms

This table reports the results for the sample using equation (6). The observations are at the bond year-month level. I use the 4 quarters lagged local income gap as my main explanatory variable. In columns (1), (2), (3) and (4), my dependent variables are the logarithm of the bond offering amount, the logarithm of the maturity, a dummy variable that takes the value of 1 if the bond is offered through a negotiated sale method and zero otherwise, and finally a dummy variable that equals 1 if the bond has a call option attached to it. In each specification, I include a set of time varying county-level controls that include the level (log) and percentage change in population of the county, the percentage change in employment, the county level unemployment rate, and the log of per capita county level income. I also include credit rating interacted with year fixed effects, debt type fixed effects, issuer fixed effects, county fixed effects and state-year fixed effects. In column (1) in panels A, B & C, I include bond level controls consisting of dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds and the remaining years to maturity. In column (2) in all the panels, I include bond level controls consisting of log (amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds. In column (3), I include bond level controls consisting of log (amount issued in $); dummies for callable bonds, bond insurance, general obligation bond; remaining years to maturity. In column (4), I include bond level controls consisting of log (amount issued in $); dummies for bond insurance, general obligation bond and the remaining years to maturity. T-statistics are reported in parentheses and standard errors are double clustered at county and year-month level. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: FULL SAMPLE				
Dependent Variable	Log (Bond Size)	Log (Maturity)	Negotiated	Callable
	(1)	(2)	(3)	(4)
Local Income Gap	0.011	0.010	0.141***	0.006
	(0.18)	(0.46)	(3.30)	(0.53)
Observations	1,098,862	1,098,862	1,098,862	1,098,862
R-squared	0.544	0.597	0.683	0.645
Panel B: 2004-2008				
Local Income Gap	0.088	0.198**	0.139	-0.043
	(0.37)	(2.45)	(1.19)	(-1.11)
Observations	247,998	249,186	249,186	249,186
R-squared	0.656	0.629	0.891	0.664
Panel C: 2009-2017				
Local Income Gap	0.047	-0.009	0.036	0.013
	(0.34)	(-0.27)	(0.58)	(0.67)
Observations	499,554	499,752	499,752	499,752
R-squared	0.552	0.606	0.789	0.659
Bond control	Yes	Yes	Yes	Yes
Credit Rating × Year FE	Yes	Yes	Yes	Yes
Debt Type FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes
County Control	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes

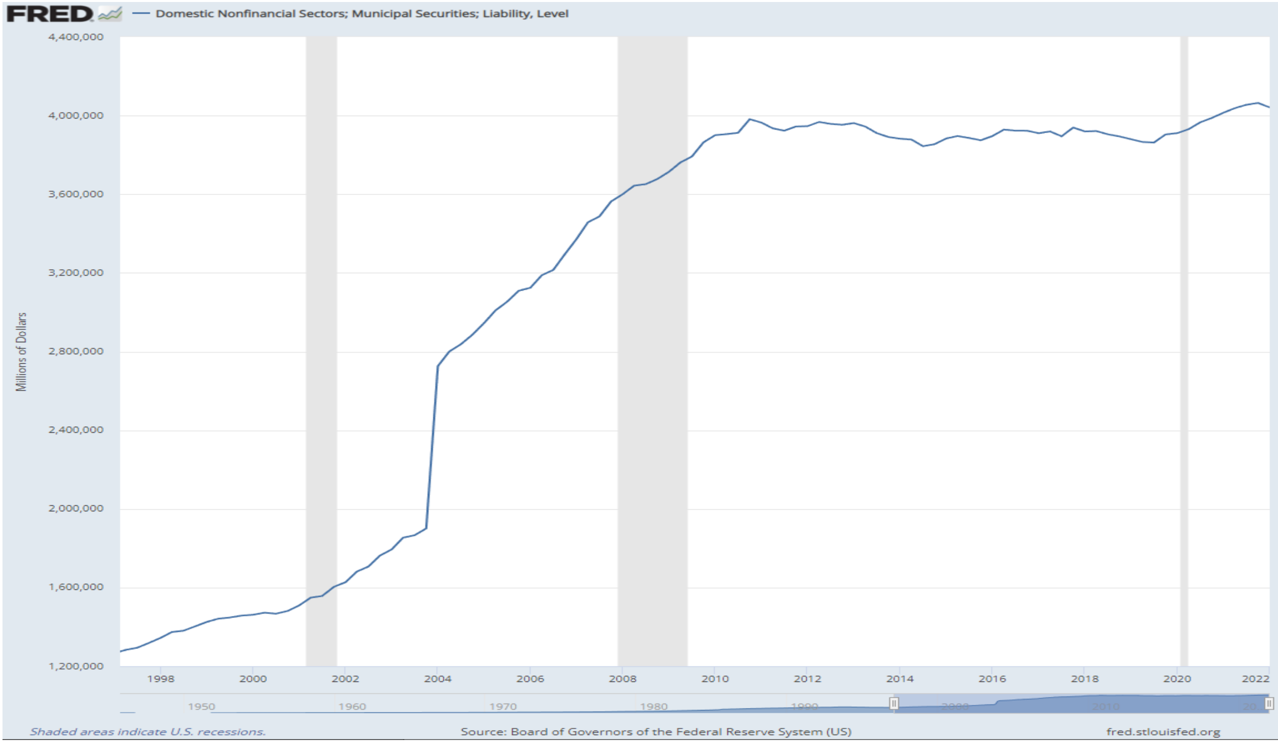

Figure IA.1: This figure plots the total holdings of municipal securities (in millions of USD) by all domestic nonfinancial sectors. The data is from the St. Louis Fed's FRED database based on releases Z.1 financial accounts of the US.

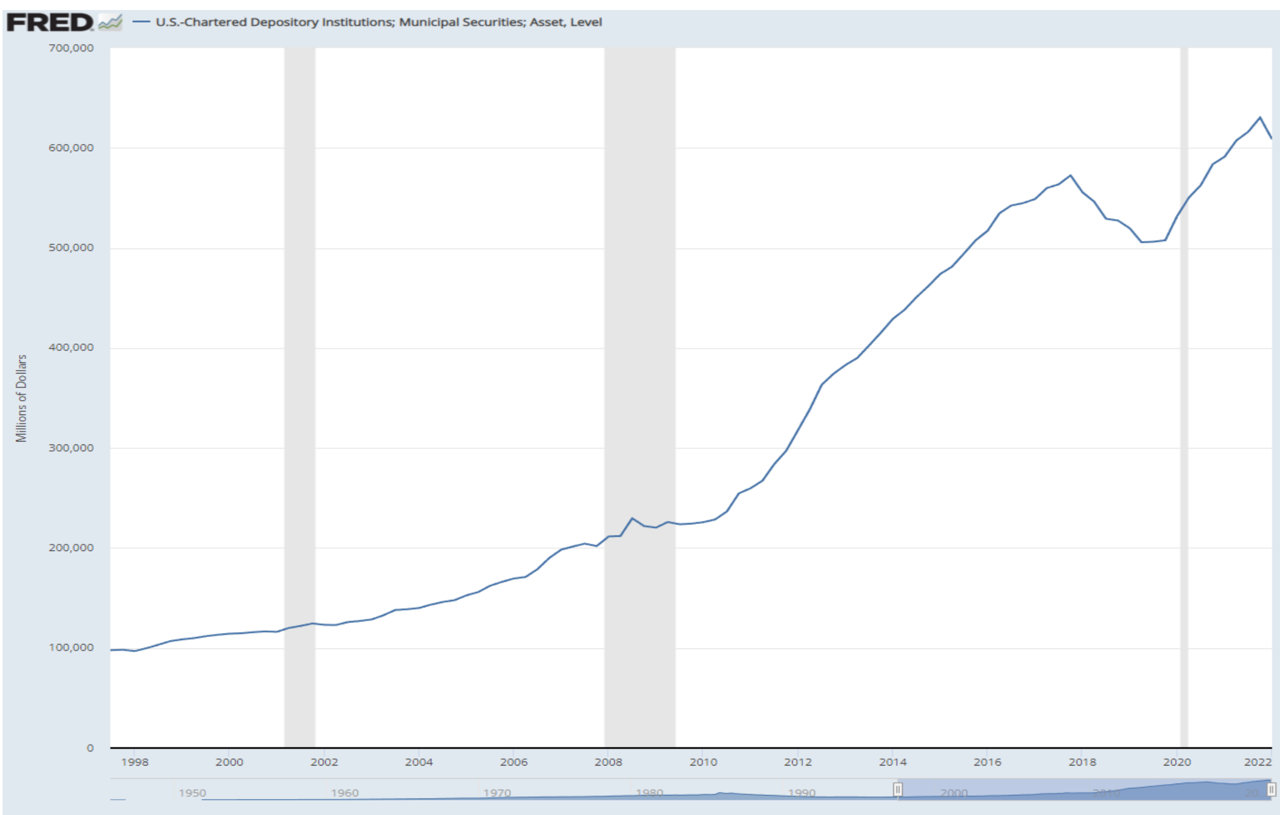

Figure IA.2: This figure plots the total holdings of municipal securities (in millions of USD) by all US chartered depository institutions. The data is from the St. Louis Fed's FRED database.

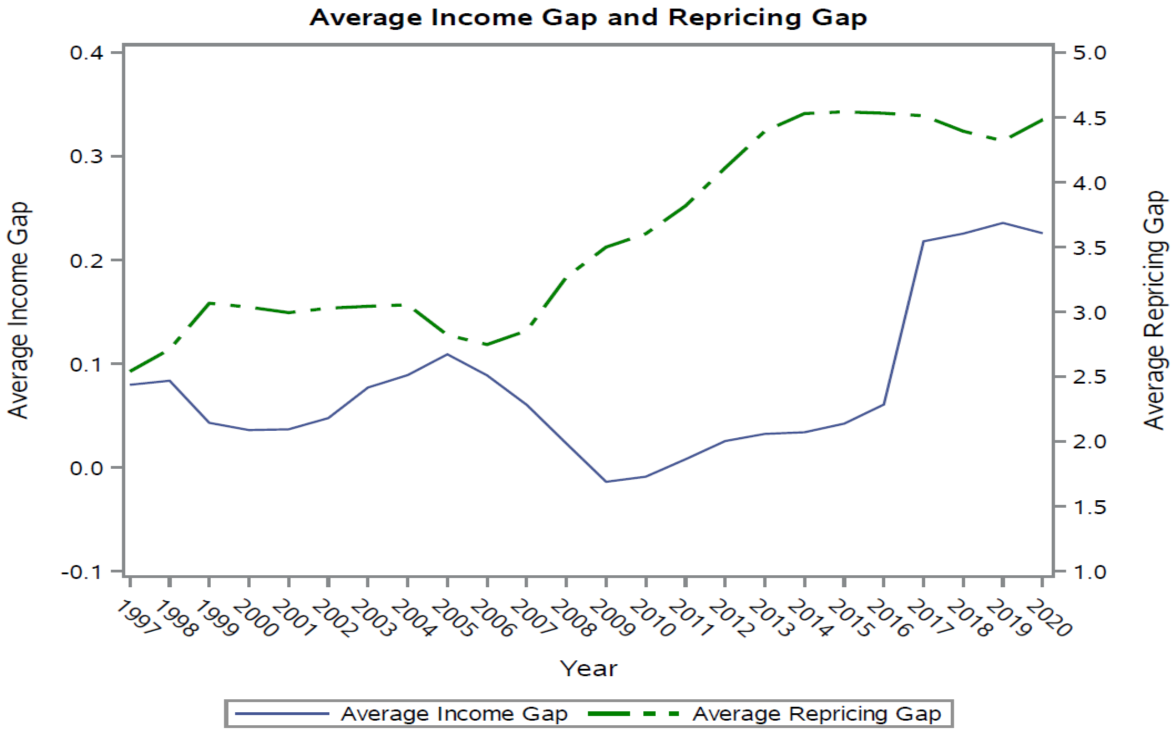

Figure IA.3: This figure plots the average equal-weighted income gap averaged across all commercial banking institutions in the US which file Call reports data, against left Y-axis. The average equal weighted repricing gap is plotted against the right Y-axis. The ratios are plotted every year from 1997 – 2020 by averaging across the four quarters within a year.

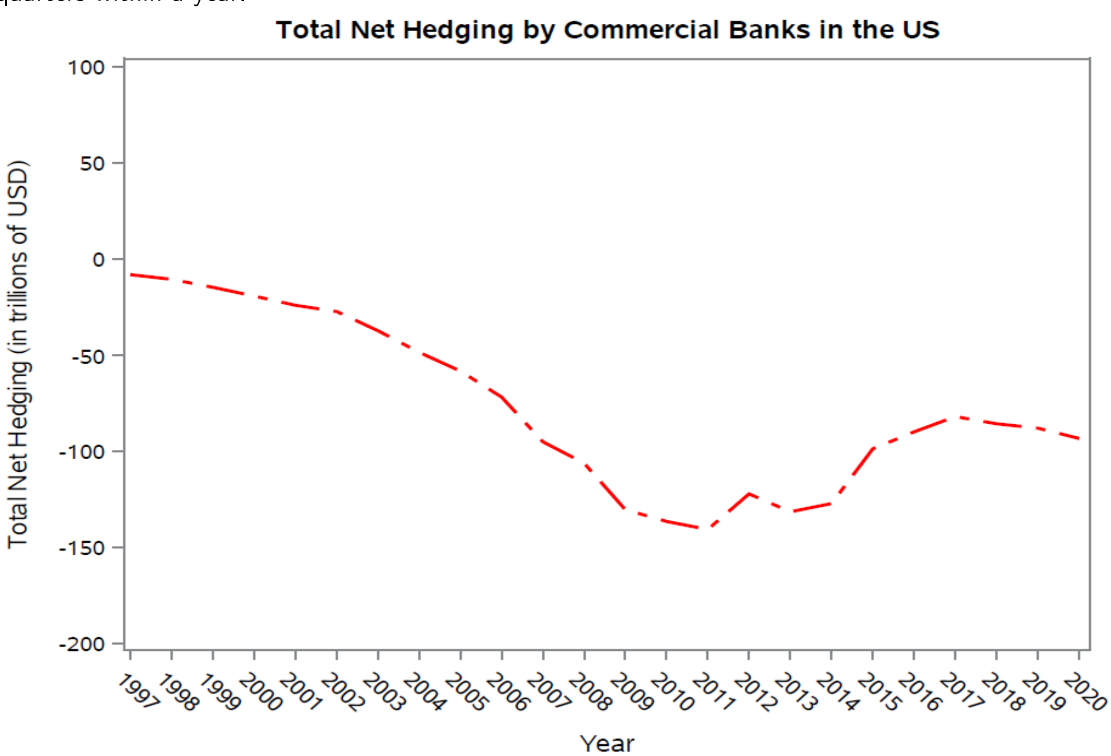

Figure IA.4: This figure plots the total net hedging, defined as the difference between the total bank holdings of all fixed-rate swaps and total holdings of all floating-rate swaps. The ratios are plotted every year from 1997 – 2020 by averaging across the four quarters within a year.

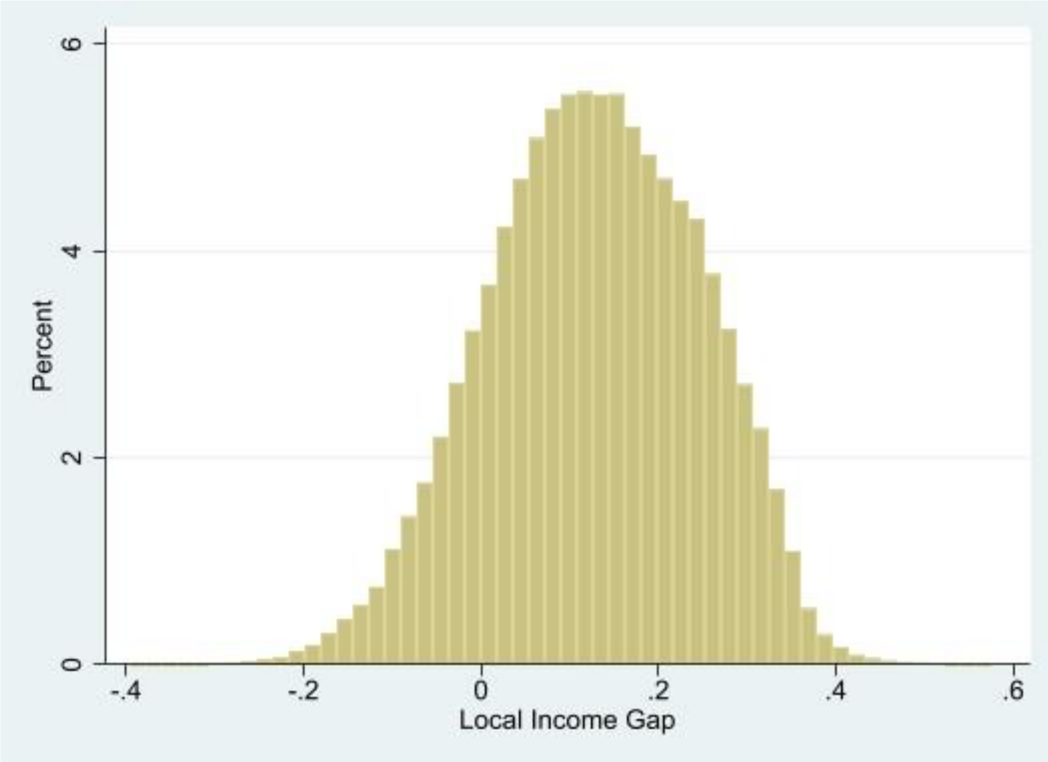

Figure IA.5: Local Income Gap - This table plots the frequency distribution (percentage of observations in each local income gap bin) for all county-quarter level observations. The local income gap is defined in Table A.1.

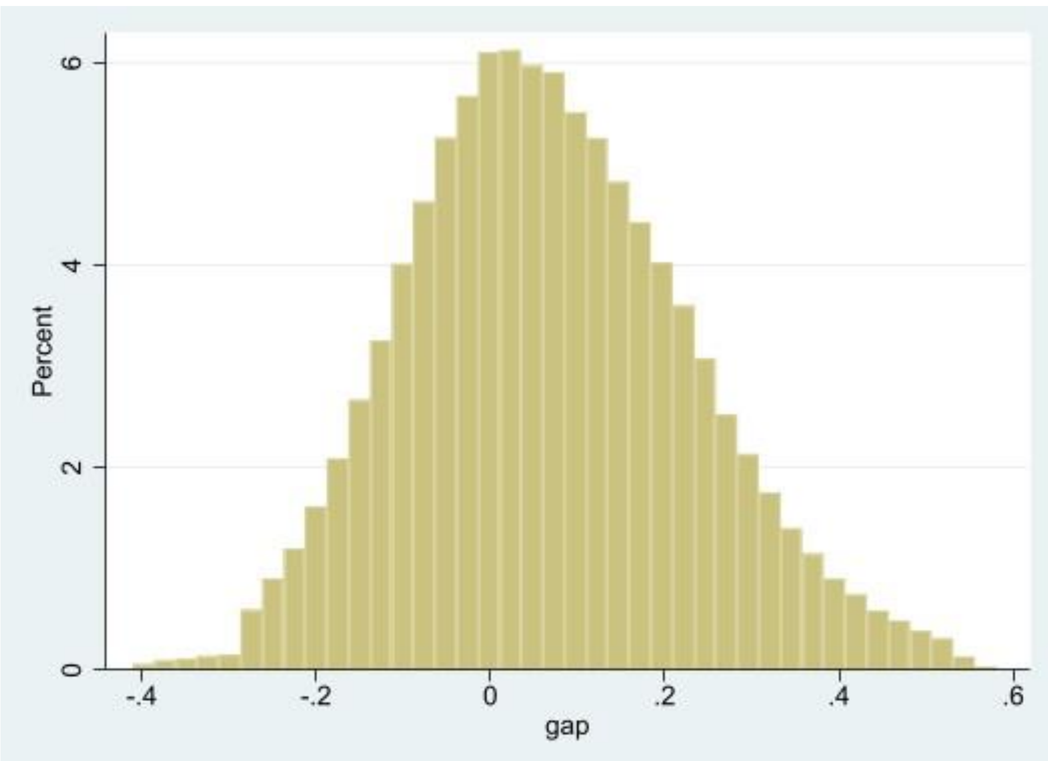

Figure IA.6: Bank Income Gap - This table plots the frequency distribution (percentage of observations in each bank income gap bin) for all bank-quarter level observations.

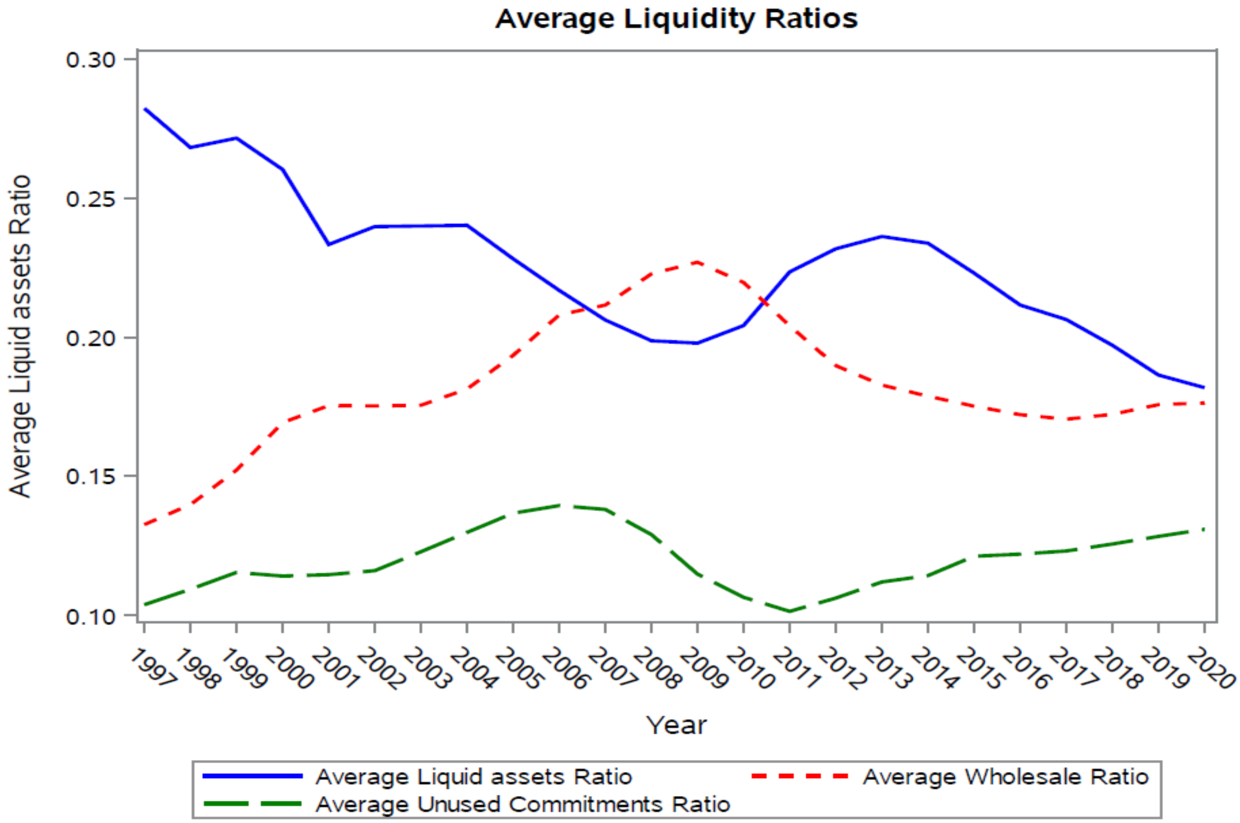

Figure IA.7: This figure plots the average equal-weighted liquid assets ratio, wholesale funding ratio and the unused commitments ratio averaged across all commercial banking institutions in the US which file Call reports data. The variable definitions are available in Table A.3. The ratios are plotted every year from 1997 – 2020 by averaging across the four quarters within a year.

Figure IA.8: This figure plots the total financial assets (in millions of USD) across all municipal bond funds in the US. The data is from the St. Louis Fed's FRED database based on releases Z.1 financial accounts of the United States.

Heterogeneity in Enforcement Stringency and Environmental Pollution in the U.S. *

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Abstract

Legislation guiding environmental policy in the US is set largely at the federal level, whereas, the primary monitoring and enforcement responsibility is decentralized to state & local authorities. Consistent with the idea that households' marginal willingness to pay for environmental quality increases with their wealth, I show that enforcement actions under the Clean Air Act correlate positively with median housing wealth at the county level. I establish causality using an instrumental variable approach. The effect is stronger in counties with higher social capital, and in states with democrat governors. Exogenous increases in local house prices lead to a decrease in toxic releases from local polluting plants and an increase in local plants' pollution abatement initiatives. My results highlight that under federalism, environmental enforcement can become fragmented when households differ in their marginal willingness to pay for environmental quality.

Keywords— Environmental Regulation, Toxic Releases, House Prices, Resident Preferences

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1 Introduction

Shapiro and Walker (2018) show that changes in environmental regulations, not productivity or trade, account for the majority of reduction in pollution per unit of output. This "technique effect" has resulted in major declines in air pollution levels in the US over the last three decades. Legislation guiding environmental policy in the United States is set largely at the federal level. In contrast, primary monitoring and enforcement responsibility is typically decentralized to states' departments of environmental protection and to local authorities. Under federalism, environmental authority has oscillated between periods of relatively greater centralized and decentralized control. This makes it critical to understand the factors that influence regulation at the state and the local level. In the first part of the paper, I show that local house price movements matter for the enforcement stringency of environmental regulation under the Clean Air Act (CAA). Polluting facilities face relatively high marginal compliance costs in counties with rising median housing wealth. This is consistent with the regulator considering violations by these facilities to pose larger potential harm to the county residents or their potential political repercussions. Later, I show that local polluting plants adjust their pollution profile, by investing in more pollution abatement initiatives and reducing their toxic chemical releases, in response to changes in local enforcement stringency.

Standard preferences imply that environmental quality is a normal good with an income elasticity well above one. That is, people want more of it as their real incomes or wealth increase. As a result people with higher wealth tend to place a higher value on a clean environment, and wealthy nations tend to have more rigorous environmental laws than poorer nations.[1] While the literature has examined cross-country differences in environmental regulation and quality, there are few studies that examine within country differences. In this paper, I attempt to fill this gap in the literature. By exploiting exogenous changes in county level house prices, I show that the average housing wealth per resident affects the stringency of enforcement actions under the CAA.[2]

Why should regulators care about resident preferences' for environmental quality? Regulators have an incentive to respond to local residents' preferences because of their career concerns,

[1] Greenstone and Jack (2015) attempt to understand the determinants of households' marginal willingness to pay (MWTP) for environmental quality, across countries. They propose four explanations for why environmental quality is poor in developing countries (1) due to low income levels, individuals value increases in income more than marginal improvements in environmental quality; (2) the marginal costs of environmental quality improvements are high; (3) political economy factors undermine efficient policy making; and (4) market failures such as weak property rights and missing capital markets distort MWTP for environmental quality.

[2] The CAA is one of the most complex laws on the books with over 9,500 pages of regulations that have evolved over sixty years (Belden (2001)). However, the basic framework is straightforward, as the EPA sets national standards for air quality and states, in partnership with federal and, at times, their local governments implement.

as their board appointments are decided by state and local politicians, who should in principle reflect the median voter's preferences in policy making. The intuition comes from Tiebout (1956), who posited that local expenditures reflect household sorting and preferences for public goods, and that residents "vote with their feet" to find the community that provides their optimal bundle of taxes and public goods. Banzhaf and Walsh (2008) provide strong empirical support for the notion that households vote with their feet for environmental quality.[3] This channel is valid under the assumption that voters vote for local or statewide elections based on their climate policy preferences. Although climate policy is more a national issue, there is growing evidence of nationalization in U.S. Senate and gubernatorial elections (Sievert and McKee (2019)). Also, since states can have their own climate agenda and much of the regulation enforcement is done at the state level, it is plausible to assume that voters take their environmental policy preferences into account while voting. Additionally, when regulators are resource constrained, they decide where to best spend their limited resources. A marginal dollar spent in abating pollution in high-preference area 'buys' the incumbent government more votes than the same dollar spent in a low-preference area.

We focus our analysis on the regulation and enforcement of the Clean Air Act (42 U.S.C. 7401 et seq.). The CAA establishes a number of programs designed to carry out the goals of the Act. Some of these programs are directly implemented by EPA through its regional offices but most are carried out by states, local agencies and approved tribes. Primary among those, is the U.S. National Ambient Air Quality Standards (NAAQS), which sets limits on atmospheric concentration of six pollutants that cause smog, acid rain, and other health hazards. [4]

To begin with, we consider enforcement actions under the CAA from the year 1991 to 2019, for all facilities that emit toxic air pollutants. We match these facilities to the NETS (National Time Series Establishment) database through the NETS-TRI crosswalk files. The NETS database allows us to observe the history of the facility, from the time it was established to the last recorded date it appears in the database. For each facility, in each year, we count the number of formal & informal enforcement actions, and associated penalty actions, and regress these on contemporaneous county level variables. To capture heterogeneous household wealth across counties, our main independent

[3]The local community too, can impose discipline on local and state regulators through voting on ballot measures, citizen suits, protests.

[4]Each year in July, the EPA sets the determines the set of counties that are in "nonattainment" of a particular standard. State governments must develop a pollutant-specific "State Implementation Plan" describing how these nonattainment counties will be brought into compliance. The 1977 amendments introduced "New Source Review," a policy designed to regulate major new or modified sources of pollution in attainment counties, whereas facilities in those counties had not previously faced much regulatory scrutiny. Similarly, the 1990 amendments began regulating "toxic" air pollutants, identifying 189 hazardous air pollutants and requiring that the EPA establish emission standards that provide for "an ample margin of safety to protect public health" by minimizing the amount of toxic pollution released into the air to the extent that technology allows.

variable of interest, the *median housing wealth*, is defined as the median home value in the county $\times$ home-ownership rate for each county-year observation. The results suggest that conditional on violation, enforcement outcomes at the facility level depend on the median housing wealth per resident in the county, county level income and unemployment rate, and the size of the workforce employed in the facility. This suggests that factors unrelated to the violation attributes (the crime) influence enforcement and penalty outcomes (the punishment). Becker (1968) provides a theoretical justification for disparities in punishment, by considering the optimal amount of enforcement when it is costly to impose sanctions. Under this framework, disparities in punishment may have a few, non-exclusive sources: heterogeneity in the private violation gains, the social harms of violations, and the costs of punishment. The results suggest that the cost of pollution is more in a county with higher housing wealth than in a county with lower housing net worth.

The analysis can be potentially plagued by endogeneity issues. Reverse causality can be a concern, if tighter enforcement standards in the county which result in better air quality in the future, lead to higher house prices in the present. Indeed, Chay and Greenstone (2005) show that county-level house prices increase when counties are designated as non-attainment counties with respect to concentrations of particulate matter. Omitted variables present a concern as well, if any endogenous local factors (demand shocks) that predict house price changes, could also be correlated in the same direction with CAA violations and enforcement actions. We follow Chaney et al. (2012) and use the interaction of the long-term mortgage interest rates with local housing supply elasticity to give us a plausibly exogenous source of variation in house prices. Lutz and Sand (2022) combine high-resolution satellite imagery with modern machine learning techniques to construct the geographic determinants of U.S. housing supply. Their Land Unavailability (LU) measure is a more accurate house price predictor than the popular proxy of Saiz (2010), and unlike the Saiz measure, which is available at the MSA level, the LU measure is available at the county level. LU is also uncorrelated with housing demand proxies, supporting its use as an instrument for house prices. We also use a second instrument, the sensitivity of local house prices to housing cycles at the census region level from Guren et al. (2021). Their identification strategy exploits systematic differences in county-level exposure to regional house price cycles as an instrument for house prices.

There are three potential concerns with using the LU or housing supply elasticity measure as an instrument in this setting. First, a concern could be that counties with more 'undevelopable' land are likely to be subject to stricter environmental regulations, owing to more natural features. The second concern could be that residents' incentive to demand more stringent regulation varies across high vs low supply elasticity counties. In areas with more housing supply elasticity, it is possible that homes are built further away from the facility, hence the impact of pollution on house prices is muted. Any regulation induced reduction in pollution thus matters only for houses in denser areas (areas with low housing supply elasticity). This implies that only in low-supply elasticity areas, residents have an incentive to actively demand stronger regulation. Both the above channels could potentially violate the exclusion criterion if our instrument just relied on cross-sectional variation. It is hard to imagine a reason why enforcement outcomes for low vs high supply elasticity counties should vary differentially depending on the level of interest rates in the economy. Third, any confounding effect that comes from a human capital channel must be controlled for; better educated and skilled population would demand more stricter enforcement, while at the same time driving house prices upwards. Davidoff (2015) shows that human capital and agglomeration economies are more concentrated in places with better amenities, which also happen to be MSAs with low supply elasticity. Mian et al. (2013) show that the Saiz (2010) elasticity measure is uncorrelated with the change in wage growth, employment share in construction and construction employment growth. Inelastic cities differ from others in having higher income per capita and higher net worth per capita. However, these differences are constant; there is no evidence of a stronger permanent income shock in more inelastic cities during the credit boom years. So while housing net worth varied tremendously during the boom and bust years, per capita income didn't vary as much. Similarly, Lutz and Sand (2022) show that at the zip code level, LU is negatively correlated with the amenities index, foreign share, and housing density, while being uncorrelated with college share. These results are not surprising as increased Land Unavailability makes the construction of housing and amenities more expensive. At the three-digit zip code level, LU is slightly negatively correlated with amenities but uncorrelated with the other housing demand factors.

In the first set of tests, we run regressions of average enforcement outcomes per facility within a given county and industry-year[5] on contemporaneous county level variables. This specification allows us to use $state \times industry \times year$ fixed effects which control for any change in state or nationwide regulations, budgetary changes at the state level, and industry level demand shocks. We also include county fixed effects to account for any time-invariant differences in enforcement stringency across counties, thus focusing on within county variation in enforcement outcomes. The results show that total enforcement outcomes per establishment in the county increase by 8 percentage points for a one std. deviation increase in the median housing wealth of the county. This increase is mostly driven by an rise in informal enforcement actions (oral notifications and

[5]The industry sector is identified by the first two digits of North American Industry Classification System (NAICS) codes.

warning letters) carried out primarily by state and local agencies. Sub-period analysis reveals that the effect of housing wealth on enforcement actions was strongest during the 2000-2011 period, which witnessed large swings in house prices across regions in the US.

We obtain similar results when the analysis is conducted at the facility or plant level, with the inclusion of facility and $state \times year$ fixed effects. Interpreted in terms of standard deviation of their respective dependent variables, a one std. deviation increase in contemporaneous median housing wealth in the county leads to a 10% increase in total enforcement outcomes and a 11% increase in penalty amounts at the plant level. Larger facilities which employ more workers face relatively lower cost of compliance, suggesting that regulators perceive a higher marginal benefit of employment at these facilities. In the cross-section of counties, we find that enforcement outcomes are more sensitive to housing wealth in counties with higher social capital. Intuitively, regulators are more likely to respond to changes in residents' preferences in communities that are more close-knit and share a higher degree of trust. Borrowing the social capital index from Rupasingha et al. (2006), we find evidence consistent with the above hypothesis. We next test if, *ceteris paribus*, the sensitivity of enforcement outcomes to county level house prices, vary depending on the party of the governor. Beland and Boucher (2015) find that pollution is lower under Democratic governors. Examining close U.S. congressional elections, Bisetti et al. (2022) show that plants increase pollution and invest less in abatement following close Republican wins.[6] Consistent with these studies, we also find that enforcement and penalty amounts respond more strongly to changes in local housing wealth in states with democrat governors.

How do local polluting plants respond to these changes in regulators' monitoring and enforcement stringency? For firms with a higher likelihood of exceeding permitted emissions levels, increases in the probability of enforcement and penalties should lead to increases in the marginal cost of maintaining high pollution levels. Polluting firms would thus want to either invest in pollution reduction activities or reduce the overall quantity of chemicals used in the production process or reduce the amount of pollution per unit of output - "the technique effect". To study the effect of median housing wealth on one=period ahead pollution levels, we employ the EPA's Toxic Releases Inventory (TRI) data. Although, we focused on enforcement outcomes under the Clean Air Act, while studying the effect on pollution outcomes, we focus on all regulated pollutants in general and implement the analysis at the facility-chemical-year level. Following Akey and Appel (2019), we

[6] Innes and Mitra (2015) show that inspections rise following Democratic victories in close Congressional elections. Di Giuli and Kostovetsky (2014) find that firms score higher on corporate social responsibility (CSR) when they have Democratic rather than Republican founders, CEOs, and directors, and when they are headquartered in Democratic rather than Republican-leaning states.

scale the total amount of toxic chemical releases by the production ratio - defined as the quantity of output in a given year, scaled by the output of the previous year for each chemical that is reported. Holding production constant, we see that total toxic releases reduce by about 4% relative to their standard deviation, for a one std. deviation increase in contemporaneous median housing wealth in the county. Decomposing the source of reductions, we find that both reductions in both active on-site and off-site pollutant releases. Studying the medium of releases, we find that the toxic air releases decrease by about 3% and the improvements arise primarily from reductions in fugitive-air emissions. Water and underground pollutant releases reduce by about 6% and 4.3% relative to their standard deviations. Finally, consistent with the intertemporal heterogeneity in sensitivity of enforcement outcomes to housing wealth changes, we find a similar pattern in toxic chemical reductions as well - most of the effect is concentrated in the 2000-11 period, which witnessed large variations in house prices across the US.

Next, we focus on the effects of changes in local housing wealth on one year ahead emissions of criteria air pollutants (CAP) and greenhouse gas emissions at the plant-year level between 2008 and 2019. The NEI (National Emissions Inventory) dataset includes emissions of carbon monoxide, ammonia, nitrogen oxides, particle pollution, sulfur dioxide and other volatile organic compounds, and is available from 2008 onward. The GHGRP (Greenhouse Gas Reporting Program) data-set reports carbon dioxide emissions at the plant level, starting from 2010. Our estimates suggest that total emissions of CAP goes down by about 10% upon a one std. deviation increase in median housing wealth in the county. This decrease is driven predominantly by reductions in the amount of carbon-monoxide and nitrogen oxide, two of the most lethal pollutants. The reduction in both toxic chemical releases and CAP is higher in counties with a higher degree of social capital, consistent with the pattern observed for enforcement outcomes. Finally, we show that plants achieve these reductions by focusing on abatement initiatives, rather than changing their production activities. We find evidence that plants increase their number of abatement activities adopted at the chemical-level. Consistent with EPA's waste management hierarchy, plants implement more source reduction activities by reducing the total amount of toxic wastes released. Second, plants significantly increase their percentage of generated waste getting recycled.

Although this paper argues that local housing wealth affects local polluting plants' pollution profile through the enforcement channel, it is also plausible that changes in house prices may have a similar effect through by affecting the collateral value of firms' real estate. Since the majority of polluting plants studied in this paper belong to tradable industries, it is unlikely that local house prices affect plant level employment or production decisions through a local demand channel. Yet, if these plants own local real estate, a rise in house prices can relax financing constraints, and thus increase firm's investments in pollution abatement activities. Indeed, Xu and Kim (2022) document evidence that financial constraints increase firms' toxic emissions given that firms actively trade off abatement costs against potential legal liabilities. Yet, Mian and Sufi (2014) and Giroud and Mueller (2019) find that house price changes had limited impact on local employment of firms in tradable industries, though their sample period is limited to the housing bust phase from 2006-09. Whereas the hypothesis in Giroud and Mueller (2019) holds only for financially constrained firms, the regulation based channel (our hypothesis) holds both for financially constrained and unconstrained firms. Financially unconstrained firms would find it cheaper to just pay up in response to increased enforcement stringency. But constrained firms might find it cheaper to cut production or engage in within firm pollution shifting (Bartram et al. (2022)). Since we argue for an enforcement based channel, this means that financially unconstrained firms in tradable industries should also be affected from changes in local house prices.

Our paper adds to the literature on environmental regulation and heterogeneity in enforcement outcomes. Focusing on political incentives, Bisetti et al. (2022) use the outcomes of close U.S. Congressional elections to show that local politician's ideology matters for enforcement stringency and thereby firms' industrial pollution decisions. Heitz et al. (2021) show that campaign contributions can indirectly benefit firms by way of reduced environmental regulatory enforcement and penalties. Gulen and Myers (2017) show that the EPA does not uniformly enforce the Clean Water Act in battleground states. Related to the "local community preferences" channel examined in this paper, Dion et al. (1998) show that local labor market conditions have an impact on the monitoring strategy adopted by the regulator. Examining hazardous waste cleanup decisions, Viscusi and Hamilton (1999) show that these decisions are driven by efficiency concerns, biases in risk perceptions, and political factors. Conducting a nationwide field experiment in China, Buntaine et al. (2022) find that public appeals to the regulator through social media substantially reduce violations and pollution emissions. Focusing on the regulators' pay incentives and using individual compensation data on attorneys at the EPA, Kalmenovitz and Chen (2021) find that high-inequality EPA offices pursue more enforcement actions with higher monetary penalties, especially against severe misconduct.

The findings in the paper also contribute to the literature on how changes in local economic conditions lead to heterogeneity in public or private goods provision. Marchand and Weber (2020) show that the Texas boom in shale oil and gas drilling, with its large and localized effects on wages and the tax base, reduced test scores and student attendance, despite tripling the local tax base and creating a revenue windfall. Davis and Ferreira (2017) show that the "housing disease" - fiscal externality from housing markets due to unexpected booms, lead to large increase in expenditures per student in U.S. public schools in the 1990s and 2000s. Stroebel and Vavra (2019) show that markups on retail products increase in areas with local house price booms, because of reduced price sensitivity of homeowners owing to greater housing wealth. In this paper, we show that changes in local housing wealth lead to variation in local demand for environmental quality through enforcement stringency and thereby local plants' emission profiles.

Finally, a growing body of work in the climate finance literature has uncovered important determinants of firms' environmental policies. Primary among those are parent firm liability (Akey and Appel (2021)), bankruptcy claim dischargeability (Ohlrogge (2020)), capital-lender liability (Bellon (2021)) and lender's own environmental profile (Houston and Shan (2021)), investor activism (Naaraayanan et al. (2019); Akey and Appel (2019)), financial constraints (Bartram et al. (2022); Xu and Kim (2022)), the listing status of firms (Shive and Forster (2020)), and supplier networks (Schiller (2018)). What is absent from the literature is the role of local communities - who are important stakeholders in the polluting firm. We show that changing environmental preferences of local communities can play a major role in determining firms' environmental policies, by affecting the enforcement stringency of environmental regulation.

2 Enforcement of Environmental Regulation in the US

Most of the major U.S. pollution control programs have been designed under a model of regulatory federalism, in which responsibility for providing environmental protection is to be shared by multiple levels of government. While the federal government (i.e., the EPA) is generally responsible for setting national standards, the details of implementation and enforcement are left largely to state environmental agencies. EPA establishes national regulatory standards and the procedures by which these standards are to be enforced. States are then invited or required to develop regulatory programs that are consistent—that is, at least as stringent—with federal standards as a condition for being authorized to enforce these standards within their borders.[7] There is significant variation in enforcement performance across the states (and within states over time), due in large measure to the discretion states are afforded to determine with how much vigor to enforce federal environmental statutes (Sigman (2003); Konisky (2007)).

[7] If a state fails to obtain or chooses not to seek authorization, the EPA carries out the programs itself through one of its 10 regional offices.

Enforcing environmental laws is a central part of EPA's strategic plan to protect human health and the environment. EPA works to ensure compliance with environmental requirements. The EPA and its regulatory partners perform compliance monitoring activities for forty-four programs authorized by seven statutes. These statutes include the Clean Air Act (CAA), Clean Water Act (CWA), Safe Drinking Water Act (SDWA), Federal Insecticide, Fungicide, and Rodenticide Act (FIFRA), Resource Conservation and Recovery Act (RCRA), Toxic Substances and Control Act (TSCA), and Comprehensive Environmental Response, Compensation and Liability Act (CERCLA). These activities include - i) conducting inspections and investigations, ii) overseeing imports and exports of environmental substances, and iii) providing training to federal, state, and tribal personnel. EPA and authorized states make decisions about compliance monitoring based on either implementing an EPA or state plan, or because of tips or complaints, or as a follow-up to previous monitoring activities. Enforcement actions usually fall into three categories - a) civil administrative actions, b) civil judicial actions, c) criminal actions. Civil Administrative Actions are non-judicial enforcement actions taken by EPA or a state under its own authority. These actions do not involve a judicial court process. An administrative action by EPA or a state agency may be in the form of: i) a notice of violation or a Superfund notice letter, or ii) an order (either with or without penalties) directing an individual, a business, or other entity to take action to come into compliance, or to clean up a site. Civil Judicial Actions are formal lawsuits that are filed in court, against persons or entities that have failed to: comply with statutory or regulatory requirements, comply with an administrative order, pay EPA the costs for cleaning up a Superfund site or commit to doing the cleanup work.[8] Finally, criminal actions can occur when EPA or a state enforce against a company or person through a criminal action.[9]

Civil enforcement results fall into four categories: i) Settlements, which in administrative actions are often in the form of consent agreements or administrative orders on consent, and in judicial actions, are in the form of consent decrees signed by all parties to the action and filed in the appropriate court; ii) Civil penalties, which are monetary assessments paid due to a violation or noncompliance; iii) Injunctive relief, which requires a regulated entity to perform, or refrain from performing, some designated action; iv) Supplemental Environmental Projects (SEPs), which are environmental improvement projects that a violator voluntarily agrees to perform. Criminal penalties are federal, state or local fines imposed by a judge at the sentencing.

[8] These cases are filed by the U.S. Department of Justice on behalf of EPA. In civil cases they are typically filed by the State's Attorneys General on behalf of the states.

[9] Criminal actions are usually reserved for the most serious violations, those that are willful, or knowingly committed. A court conviction can result in fines or imprisonment.

In this paper, I focus on the enforcement of the Clean Air Act. The CAA is the primary federal law governing air pollution. EPA monitors compliance of regulated operations (facilities, activities, and entities) pursuant to CAA in several major program areas. Primary among them are state & federal implementation plans (SIPs) for national primary and secondary ambient air quality standards, national emission standards for hazardous air pollutants, acid rain inspection and trading program, new source review, standards of performance for new stationary sources and chlorofluorocarbons tracking. The Integrated Compliance Information System for Air (ICIS-AIR) contains emissions, compliance, and enforcement data on stationary sources of air pollution. Regulated sources cover a wide spectrum; from large industrial facilities to relatively small operations such as dry cleaners (automobiles and other mobile air pollution sources are tracked by a different EPA system).

3 DATA

3.1 Enforcement and Pollution Data

The Enforcement and Compliance History Online (ECHO) database focuses on inspection, violation, and enforcement data for the Clean Air Act (CAA), Clean Water Act (CWA) and Resource Conservation and Recovery Act (RCRA) and also includes Safe Drinking Water Act (SDWA) and Toxics Release Inventory (TRI) data. We use the ECHO to obtain the civil enforcement and compliance information at the facility level. Facility-level data treats the entire plant as a unit instead of looking at individual emitters, processes, or stacks.[10] So far, we only take the enforcement and compliance cases with Clean Air Act program. ECHO contains comprehensive enforcement information such as filing date, activity type, and monetary penalties.

EPA uses compliance monitoring activities (e.g., interviewing facility representatives, collecting samples) to ensure that the regulated community obeys environmental laws and regulations. Monitoring activities include on-site and off-site full/partial compliance evaluations (FCE and PCE). [11] Civil enforcement activity includes three categories: judicial (JDC), administrative formal (AFR) and administrative informal (AIF). Following the definition with ICIS-AIR, we combine JDC and AFR cases at each facility as formal enforcement and check whether they are lead by EPA

[10] Another usable database is the ICIS-AIR. Different from ECHO, ICIS-AIR focuses on unique air sources (a facility may have more than one emission points) and maintains data at several levels of details. ECHO summarizes the details of facilities and incorporates information from ICIS-AIR.

[11] We use records in ICIS-AIR FCE&PCE database to count facility-year level investigations/evaluations numbers (*Fed_Invg_Num, StateLocal_Invg_Num, Total_Invg_Num*).

or state/local institutions and departments (*Fed_FormalEnf_Num, StateLocal_FormalEnf_Num, Total_FormalEnf_Num*). Depending on results of investigations, facilities may receive monetary penalties, or non-penalty compliance orders such as clean up a site. We separately record monetary penalties (*Fed_Penalty_Num/Amt, StateLocal_Penalty_Num/Amt, Total_Penalty_Num/Amt*). Besides formal enforcement, facilities may also receive non-penalty AIF compliance orders in the form of oral notifications and warning letters. We summarize these AIF activities and define it as informal enforcement (*Fed_InformalEnf_Num, StateLocal_InformalEnf_Num, Total_InformalEnf_Num*). Finally, we combine both formal and informal enforcement as total enforcement (*Fed_Enf_Num, StateLocal_Enf_Num, Total_Enf_Num*).

Figure 1.1 plots total civil enforcement actions taken each year by the EPA and state & local agencies. The graph shows an overall declining trend in total civil enforcement, with a peak around 1998 of about 250 enforcement actions. By 2020, total civil enforcement actions under the Clean Air Act fell to around 125. Figure 1.2 displays the total number of facility investigations conducted each year under the CAA by the EPA and state& local agencies. Investigations include on-site and off-site compliance evaluations. The trend in investigations follows a similar pattern to civil enforcement, peaking in 1998 at around 900 investigations. Both EPA and state/local agencies contribute to the total investigations. By 2020, the number of investigations under the Clean Air Act declined to around 400.

Chemical releases are recorded in several systems under EPA. In 1986, Congress passed the Emergency Planning and Community Right-to-Know Act (EPCRA) to support and promote emergency planning and to provide the public with information about releases of toxic chemicals in their community. Section 313 of EPCRA established the Toxic Release Inventory (TRI) program. We use TRI, National Emissions Inventory (NEI), and Greenhouse Gas Reporting Program (GHGRP) to get a comprehensive view of chemical releases from facilities. TRI tracks the management of certain toxic chemicals that may pose a threat to human health and the environment[12]. Every year, TRI reports release amount of more than 300 chemicals regulated under CAA. We sum up the air emission amount of all of these chemicals as toxic releases (*TRI_Total_Releases*). Specially, there are more than 160 of these chemicals categorized as Hazardous Air Pollutants (HAP) by EPA. The institution emphasizes the regulation of HAP emissions under CAA.

Starting from 2011, GHGRP covers greenhouse gas emissions from different aspects of the

[12]Facilities that report to TRI are typically larger facilities involved in manufacturing, metal mining, electric power generation, chemical manufacturing and hazardous waster treatment. It's worth noticing here that, not all industry sectors are covered by TRI, and not all facilities in covered sectors are required to report to TRI. For further reference: Toxic Releases Inventory

oil and the gas industry through several of its sub-parts. It reports emissions of carbon dioxide (CO_2), methane (CH_4), nitrous oxide (N_2O) and fluorinated greenhouse gasses, in millions of metric tons. We take the summation of all these greenhouse gas CO_2-equivalent emissions as the final greenhouse gas releases (CO_2). NEI dataset includes emissions of carbon monoxide (CO), nitrogen oxides (NO_x), particle pollution (PM2.5 and PM10), and sulfur dioxide (SO_2). According to CAA, these chemicals are 'criteria air pollutants (CAP)' which can be harmful to public health and the environment. We use EPA air stationary source combined releases to get reliable release amount for each criteria pollutant record by NEI at the facility level[13] as well as the summation of all the releases (*CAP_Releases*). NEI data is collected every three years starting from 2008. We use state estimates of CAP release trend to fill the missing values. All the toxic chemicals and criteria air pollutants releases are in pounds unit and all the greenhouse gases are measured in metric tons carbon dioxide equivalent per year ($MTCO_2$/year).

Finally, we merge the above facility-level data with National Establishment Time Series (NETS) dataset, which consist of important plant-level information such as sales, employment, and historic geographic locations since 1990. A small portion of establishments in NETS correspond to multiple facilities in EPA system[14]. These are the cases where one big establishment has several facilities at the same location, but they report individually to EPA. We aggregate the enforcement numbers and chemical releases across affiliated facilities for these establishments. After retrieving the clean historic geographic locations for establishments/facilities, we again aggregate all enforcement and chemical release variables at county-industry/county level. The industry sector is identified by the first two digits of North American Industry Classification System (NAICS) codes.

All facility, county-industry, and county level observations are kept. We fill the missing values of enforcement variables (e.g., *Total_Enf_Num*) as zeros, which is based on the belief that establishments/counties didn't undergo any enforcement and compliance order if no records are found in the EPA system. However, we do not fill the chemical releases as zeros but treat it as missing. A fraction of facilities report their pollutant release as zero even if they don't produce any chemicals. We treat those releases as zeros, but we take any other missing values as unavailable[15].

[13]There are another two CAP: Ozone and Lead. Ozone releases are missing from the combined release database. Lead was excluded from NEI because lead compounds emissions sharply declined after it was eliminated from gasoline and have remained low.

[14]By definition in NETS, an 'establishment' is a unique primary market (SIC8) at a unique location. This definition is slightly different from the definition of 'facility' in all EPA systems. Even though over 99% of establishment correspond to a unique facility in EPA Facility Registry Service (FRS). Our analysis uses NETS' establishment identifier, but we don't differentiate between calling the objects 'establishment' or 'facility'.

[15]As mentioned before, not all facilities are required to report to every EPA systems

Chemical releases are monotonically increasing with total production output. We scale all the chemical releases with the following the method used in Akey and Appel (2019). Firstly, TRI records the production growth ratio at facility-chemical level, which indicates the level of increase or decrease from the previous year of the production process or other activity in which the toxic chemical is used. For example, a production ratio or activity index of 1.5 would indicate that production associated with the use of the toxic chemical has increased by about 50 percent. Following Akey and Appel (2019), we identify first year production as one and then multiply cumulatively each year by the reported production ratio for each plant-chemical set of observation (*Proxied_Production*). We replace the missing growth ratios with 1. Finally, to assess plants' engagement in post-production waste management activities, following Li et al. (2021), we trace the percentage of total generated toxic waste (Total Waste) reduced through recycling, energy recovery, treatment, and released to the environment.

3.2 County Variables

Median Housing Wealth - To capture heterogeneous household wealth across counties, our main independent variable of interest, the *median housing wealth*, is defined as the median home value in the county × home-ownership rate for each county-year observation. Zillow provides monthly estimates of median values for single family homes at the county level. We take the average monthly housing price estimates for the year 2019 (using 2019 as the base year) and use annual change of house price index (HPI) from Federal Housing Finance Agency (FHFA) to proxy median home prices for each county starting from 1990. County-level home-ownership rate, or owner occupation rate, is obtained from American Census Survey (ACS). Due to data limitation, we only exploit the five-year estimates in year 2010.

Instrumental Variables - Controlling for the potential endogeneity of local real estate prices in enforcement and pollution regressions is an important step in our analysis. Following Chaney et al. (2012), we instrument local real estate prices using the interaction of long-term interest rates and local housing supply elasticity. A popular instrument in the housing literature is the topological Land Unavailability proxy of Saiz (2010), a key component in the determination of housing market elasticity. We use the improved measure of land Unavailability at 2010 census county level from Lutz and Sand (2022), who extend the computational method to various units of geographic disaggregation. These elasticities capture the amount of developable land in each county and are estimated by processing satellite-generated data on elevation and presence of water bodies. As a measure of long-term interest rates, we use the "contract rate on 30-year, fixed rate conventional home mortgage commitments" from the Federal Reserve website.

We also use a second instrument, the sensitivity of local house prices to housing cycles at the census region level from Guren et al. (2021). The authors use the instrument to identify variation in house prices that is exogenous to local demand shocks. These sensitivities are estimated by running a panel regression of annual changes in Core-Based Statistical Areas (CBSA) house prices on changes in house prices at the census region level (West, Midwest, Northeast, and South) from 1975 to 2017. The authors show that regional housing cycles explain about 40% of the variation in local house prices in their panel estimation even after controlling for local economic conditions. We match these sensitivity estimates for 380 CBSAs to 1,149 counties in our sample. To use these sensitivity estimates as an IV in our setting, we create a census-region-level median house price for each county by weighting house prices across counties in the same census region by their 2001 population and leaving out the county in question. We then create an interaction term between the county's sensitivity and the census region house prices.

$$\text{Median Housing Wealth}_{c,t} = \text{Homeownership rate}_c \times \text{CBSA sensitivity}_c \times \text{House Prices}_{Census-region,t}$$

Salient Manmade Spills - Public attention towards environmental quality is likely to increase post environmental disasters. Further, this attention effect is likely to be stronger if the disasters occur in areas that are more socially connected with the county. We obtain the environmental man-made spill data from the U.S. Coast Guard's National Response Center (NRC). This database records incidents involving the releases of different substances and maritime security, reported by either the responsible party or a third party, including individuals calling the NRC hot-line. We only consider the salient events (Chu et al. (2021)), which are defined as the incidents involving more than 207 people evacuations (the 90th percentile). To capture the exposure of residents in a county to man-made disasters across locations in the US, we then exploit Bailey et al. (2018)'s Facebook connectedness measure. The authors calculate the Social Connectedness Index (SCI) for pairs of counties as the number of Facebook friendship links between individuals located in those two counties. We combine these definitions together to get the effective county level exposure to man-made spills through the air medium. For each county i, county j's weight is defined as $SCI_{i,j}/\sum_j SCI_{i,j}$. Our variable of interest is the weighted counts of all man-made air chemical spills in other counties:

$$Salient\ Manmade\ Spills = \sum_j \frac{SCI_{i,j}}{\sum_j SCI_{i,j}} \times Number\ of\ Air\ Chemical\ Spills\ in\ county\ j$$

Demographic Variables - For information on county demographic variables, we gather per capita income from the Bureau of Economic Analysis (BEA), county-level population from Surveillance, Epidemiology, and End Results (SEER) Program. Unemployment rate is obtained from the Bureau of Labor Statistics (BLS) database. Our estimates of county level racial diversity index are calculated based on the data from the Census Bureau's Annual County Resident Population Estimates by Age, Sex, Race, and Hispanic Origin. We also use the level of social capital in a county as one of our explanatory variables. We use the social capital measure of Rupasingha et al. (2006), who provide county-level estimated stock of social capital for the years 1990, 1997, 2005, 2009, and 2014. We take their social capital index created by using principal component analysis using four factors: number of establishments in variety of organizations (e.g., political organizations and labor organizations), voter turnout, census response rate and number domestic non-profit organizations. The four factors are standardized to have a mean of zero and a standard deviation of one.

Non-attainment Area and EPA region - In US environmental law, a non-attainment area is considered to have air quality worse than National Ambient Air Quality Standards for the criteria air pollutants. One area may be a non-attainment area for one pollutant and an attainment area for others. These areas must have plans to meet the requirements in the recent future. We put indicators for county-year if it's found in non-attainment areas. Meanwhile, EPA has ten regional offices, each of which is responsible for the execution of EPA programs within several states and territories. We identify affiliated EPA region for each observation.

3.3 Graphical Analysis and Summary Statistics

Figure 2 illustrates the inverse relationship between the real S&P/Case-Shiller national house price index and the total civil enforcement activity under the Clean Air Act in the United States from 1990 to 2020. As depicted, enforcement activity declined as housing prices increased leading up to the housing market crash in 2008. **Figure 3** highlights the distribution of industries for all polluting plants included in the sample from the Toxic Releases Inventory (TRI) database. The most prevalent industries represented are chemicals, primary metals, and petroleum-related sectors. **Figure 4** provides a visual representation of the geographic disparities in county-level total toxic emissions in pounds for the year 2007 across the United States.

Figures 5 through 7 demonstrate that counties in the top quintile of median housing wealth had significantly higher levels of enforcement activity under the CAA (Figure 5) and substantially lower emissions, including toxic emissions (Figure 6), criteria air pollutants (Figure 7.1), and greenhouse gases (Figure 7.2), compared to counties in the bottom quintile of median housing wealth. Moreover, the disparity between high and low wealth counties increased substantially over the four time periods analyzed from 1990 to 2019.

These figures provide compelling graphical evidence that counties with higher median housing wealth tend to experience both more vigorous enforcement of clean air regulations and lower pollution emissions from industrial plants relative to counties with lower median housing wealth. Furthermore, the gap between high and low wealth counties appears to have widened markedly from 1990 to 2019 based on the data presented in these images. We delve into a formal causal analysis of the empirical relationship between median housing wealth, and enforcement, and emissions of local polluting plants in the next section.

Table 1 reports summary statistics for our main variables on enforcement outcomes and plant level toxic emissions. All the variables are defined at the plant–year level. The average facility in our sample releases 52,767 pounds of toxic chemicals each year, experiences 0.3 inspections per year, is subject to 0.06 enforcement actions per year, including both formal and informal actions, and employs 180 workers on average.

4 Empirical Analysis

4.1 The Effect of County-Level Median Housing Wealth on Local Enforcement Outcomes

To test the main hypothesis, I estimate my first model, which links the median housing wealth at the county level to the enforcement actions at the county-industry-year level. In particular, I report the results from the following regressions:

$$Y_{j,c,t} = \beta_1 \cdot \textit{Median Housing Wealth}_{c,t} + \beta_2 \cdot \textit{Salient Manmade Spills}_{c,t} + \text{County Controls}+ \gamma_c + \delta_{s,j,\tau} + \epsilon_{j,c,t} \quad (1)$$

where j indexes the NAICS two-digit industry classification, c indexes the county in which the polluting plant is located, and t indexes the year. $Y_{j,c,t}$ denotes the natural logarithm of one plus the enforcement outcomes at the county-industry-year level (aggregated across all plants within each industry in a county that emit toxic releases), divided by the total number of toxic-waste emitting plants within each industry. $\textit{Median Housing Wealth}_{c,t}$ is defined as the median home value in the county $\times$ home-ownership rate for each county-year. $\textit{Salient manmade spills}_{c,t}$, as defined above, captures time-variation in public attitude towards environmental quality, triggered by salient man-made spills in socially connected counties. *County controls* include the natural logarithm of one plus the average toxic air release per plant within each industry in the county, which linearly controls for the average amount of toxic air releases at the county-industry level. They also include the include the logarithm of the per capita income and population level of the county, the unemployment rate, diversity index and a dummy variable that equals one if the county is designated as a nonattainment county. All the dependent variables along with the median housing wealth and salient manmade spills are standardized each year so as to have zero mean and unit standard deviation. γ_c are county fixed effects that remove time-invariant county characteristics, and control for county specific geographic features or other time invariant enforcement stringency predictors. $\delta_{s,j,\tau}$ are state-by-industry-year fixed effects, as a non-parametric control for any secular time trends which control for any change in state or nationwide regulations, budgetary changes at the state level, and industry level demand shocks.. Standard errors are clustered at the county level and are robust to heteroscedasticity.

The regression coefficient β_1 captures the effect of a unit std. deviation increase in the median housing wealth of a county on the percentage change in enforcement outcomes. **Table 2** presents the results of the regression. Column (1) shows that total enforcement increases by 3.4% upon a one std. dev. increase in the median county level housing wealth, with almost all the increase coming from informal enforcement actions (column (3)). Similarly, column (4) shows that penalty numbers increase by 6.6% and column (5) shows that penalty amounts increase by 6.6%. The coefficient on salient manmade spills is positive and economically significant for almost all enforcement outcomes, yet is only statistically significant for the penalty amount. Not surprisingly, higher amount of toxic releases are associated with higher enforcement actions, as is the case, if the county is designated as a non-attainment county.

The above analysis is obviously confounded by endogeneity concerns. Reverse causality can be a concern, if tighter enforcement standards in the county which result in better air quality in the future, lead to higher house prices in the present. Omitted variables present a concern as well, if any endogenous local factors (demand shocks) that predict house price changes, could also be correlated in the same direction with CAA violations and enforcement actions. Following Chaney et al. (2012), we use the interaction of the long-term mortgage interest rates with local housing supply elasticity to give us a plausibly exogenous source of variation in house prices. Lutz and Sand (2022) combine high-resolution satellite imagery with modern machine learning techniques to construct the geographic determinants of U.S. housing supply. Their Land Unavailability (LU) measure is a more accurate house price predictor than the popular proxy of Saiz (2010), and unlike the Saiz measure, which is available at the MSA level, the LU measure is available at the county level. LU is also uncorrelated with housing demand proxies, supporting its use as an instrument for house prices.

Table 3 presents the regression results using the same specification as in (1), with instrumented house prices. We instrument the median housing wealth with the interaction between the land unavailability (LU) measure and the national mortgage interest rate (NIMR). Column 1 shows the results of the first-stage regression. The Kleibergen-Paap Wald F statistic of 44.60 suggests that the instrument is strong, as does the statistically significant negative coefficient on the housing wealth instrument. In columns (2) - (6), we run two-stages least squares (2SLS) regressions of enforcement outcomes on the median housing wealth variable. We observe a significant increase in the size of the effect on all enforcement outcomes. Total enforcement goes up by 8.1%, penalty numbers increase by 19%, and penalty amounts by 12.6% for a one std. deviation increase in the median housing wealth, with most of the effect coming from informal enforcement. This suggests that potential confounding variables affect enforcement outcomes and the median housing wealth in a county in opposite directions.

Table IA.1 presents the results from an sub-period analysis of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act on contemporaneous county level variables across three separate time periods between 1991 and 2019, with the same specification as above. We divide the sample into three sub-periods. The first sub-period stretches from 1990 - 1999. Real home prices peaked in 1989, the recession hit in 1990, home prices fell 7% from the peak until the end of 1990, the recession ended in the spring of 1991 but real U.S. home prices continued to fade for years until they bottomed out in 1997, down 14% from the 1989 peak eight years earlier.[16] In the 1991-1999 period (Panel A), the data suggests a positive, but marginally significant relationship between median home value and total and informal enforcement, with coefficients of 0.093 and 0.091 respectively. Formal enforcement shows no significant relationship with median home value during this period. Furthermore, salient manmade spills show a significant positive relationship with total and formal enforcement, but only a marginally significant relationship with informal enforcement.

As the economy recovered from the early decade recession and interest rates stayed low, the national housing market was quick to rebound. In the wake of the dot-com crash and the subsequent

[16] In 1992, the Federal Housing Enterprises Financial Safety and Soundness Act required mortgage-purchasing government-sponsored enterprises (GSEs) Fannie Mae and Freddie Mac to increase their lending support for affordable housing by buying mortgages made to underserved borrowers. By 1995, Fannie Mae was receiving affordable housing credits for purchasing subprime securities—commodities backed by mortgages made to higher-risk borrowers with lower credit scores. In 1999, Fannie Mae further eased credit requirements for mortgages.

2001–2002 recession the Federal Reserve dramatically lowered interest rates to historically low levels, from about 6.5% to just 1%. House prices rose sharply during the turn of the century. Housing prices peaked in early 2006, started to decline in 2006 and 2007, and reached new lows in 2011. Thus, our second sub-period stretches from 2000 to 2011. Finally, our third sub-period extends from 2012 to the end of our sample period in 2019. Sub-period analysis reveals that the effect of housing wealth on enforcement actions was strongest during the 2000-2011 period, which witnessed large swings in house prices across regions in the US. Median home value shows a significant positive association with all enforcement outcomes, with the strongest relationship seen with informal enforcement (coefficient of 0.256). Salient manmade spills show a generally positive but not significant relationship with all enforcement outcomes. The effects in the final sub-period from 2012 to 2019 show rather weak effects of median county level housing wealth on enforcement outcomes. It is significantly positive only for informal enforcement (coefficient of 0.109). Interestingly, during this period, salient manmade spills show a strong and significant positive relationship with total and formal enforcement, and the penalty amount. These findings suggest an evolving relationship between median home value, salient manmade spills, and enforcement outcomes over time, with the strength and direction of the relationship varying across different enforcement measures and time periods. This underscores the complex dynamics between economic factors, environmental incidents, and regulatory enforcement in the context of air pollution control.

Although we have so far focused our analysis at the county-industry-year level, in this section, we implement the analysis at the plant-year level. Specifically, we implement the following regression:

$$Y_{i,c,t} = \beta_1 \cdot \textit{Median Housing Wealth}_{c,t} + \text{Plant-Year Controls} + \text{County Controls} + \gamma_i + \delta_{s,\tau} + \epsilon_{i,c,t} \tag{1}$$

In contrast to the specification in (1), the dependent variable denotes the natural logarithm of one plus the enforcement outcomes at the plant-year level. The natural logarithm of one plus the average toxic waste emitted by the plant, and the natural logarithm of the plants' workforce size are included as additional *plant level* controls. Other control variables include the logarithm of the per capita income and population level of the county, the unemployment rate, diversity index and a dummy variable that equals one if the county is designated as a nonattainment county. All regressions include State $\times$ Year fixed effects and plant level fixed effects. Standard errors are clustered at the plant level and are robust to heteroscedasticity.

We obtain similar results when the analysis is conducted at the facility or plant level. Interpreted in terms of standard deviation of their respective dependent variables, column (2) of **table 4** shows that a one std. deviation increase in contemporaneous median housing wealth in the county leads to a 10% increase in total enforcement outcomes and a 11% increase in penalty amounts at the plant level. Larger facilities which employ more workers face relatively lower cost of compliance, suggesting that regulators perceive a higher marginal benefit of employment at these facilities. The increase in total enforcement is driven by increases in both formal and informal enforcement as indicated in columns (3) and (4). Similarly, both the number of penalties and the penalty amount increase by more than 10% upon a one std. deviation increase in the instrumented median housing wealth measure.

We also use a second instrument, the sensitivity of local house prices to housing cycles at the census region level from Guren et al. (2021). Their identification strategy exploits systematic differences in county-level exposure to regional house price cycles as an instrument for house prices. They construct their instrument by first estimating the systematic historical sensitivity of local house prices to regional housing cycles and then interacting these historical sensitivity estimates — which they interpret as proxies of housing supply elasticities—with today's shock to regional house prices. The basic shift-share structure of the sensitivity instrument is the same as that of the Saiz instrument (and similar to the well-known Bartik instrument) but with a different proxy for the housing supply elasticity. [17] **Table IA.2** presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the CAA on contemporaneous county level variables between 1991 and 2019 across counties in the US. We replace the median housing wealth with the county level homeownership rate interacted with the local house price sensitivity to regional house price cycles from Guren et al. (2021), and the median housing wealth at the census-region level. All regressions include State $\times$ Year fixed effects and plant fixed effects. The results are qualitatively similar to the results obtained in **table 4**, although the coefficient on the independent variable of interest, is smaller.

Enforcement outcomes can be broken down into enforcement outcomes stemming from either federal or state & local investigations. **Table IA.3** presents the estimation results of the 2SLS regressions of enforcement outcomes under the Clean Air Act at the federal and state & local level, on contemporaneous county level variables, between 1991 and 2019 across counties in the US. In panel A, the dependent variable is the log of one plus the federal level enforcement outcomes at the plant-year level. In panel B, the dependent variable is the log of one plus the state & local level enforcement

[17] This approach infers the housing wealth elasticity from the differential response of economic activity in cities like Providence relative to cities like Rochester when the Northeast region experiences a housing boom or bust.

outcomes at the plant-year level. We see that although an increase in the median county level housing wealth increases the number of penalties and the penalty amount at both the federal and state & local level, the effect on informal federal enforcement actions works in the opposite direction. In contrast, formal federal level enforcement goes up. Given that most of the enforcement is carried out by state and local agencies, the results strongly indicate that increases in local housing wealth have a direct impact on the incentives or resources of local enforcement agencies.

4.1.1 Do Social and Political Factors Impact the Relationship between Local Housing Wealth and Enforcement?

We next focus on the variation in the sensitivity of enforcement to median county level housing wealth cross-section of counties. If the increase in regulatory actions by state & local agencies is brought about through a local "demand for better environmental quality" channel, it is likely that this effect should be stronger in communities that are more close-knit and share a higher degree of trust. Similarly, regulators are more likely to be more responsive to changes in local demand for better environmental quality in states with greater political support for environmental policies.

Rupasingha et al. (2006) provide county-level estimated stock of social capital for the years 1990, 1997, 2005, 2009, and 2014. Their social capital index is created by using principal component analysis using four factors: number of establishments in variety of organizations (e.g., political organizations and labor organizations), voter turnout, census response rate and number domestic non-profit organizations. The four factors are standardized to have a mean of zero and a standard deviation of one. To proxy for the political environment in a state, we use the part of the state governor. A number of academic studies have found that environmental enforcement is higher under democrat governors. To test both these hypotheses, we implement the following regression:

$$\begin{aligned} Y_{i,c,t} = \beta_1 \cdot \textit{Median Housing Wealth}_{c,t} + \beta_2 \cdot \textit{Median Housing Wealth}_{c,t} \times \Gamma_t + \\ \text{Plant-Year Controls} + \text{County Controls} + \gamma_i + \delta_{s,\tau} + \epsilon_{i,c,t} \end{aligned} \tag{2}$$

where the dependent variable and the control variables are the same as defined in (1). The variable Γ_t is either the social capital index of the county in year t, or is a dummy variable that equals 1 if the county's state has a democrat governor in year t. **Table 5** presents the regression results. Column (1) suggests that the total enforcement at the facility goes up by an additional 3.3% upon a one std. deviation increase in the county level social capital index. Similarly, column (4) suggests that *ceteris paribus*, the total enforcement at the facility goes up by an additional 4.9% if the county belongs to a state with a democrat governor. The number of penalties (penalty amount) increase by 4.3% (3.7%) upon a one std. deviation increase in the county level social capital index. Similarly, the number of penalties (penalty amount) increase by 5.8% (5.2%) if the county belongs to a state with a democrat governor. Overall, the cross-sectional tests show a strong heterogeneity in the sensitivity of enforcement stringency to both the county level social capital, and the party of the governor.

4.2 Real Effects on Local Polluting Plants' Environmental Profile

What is the response of local polluting industries to the alterations in the intensity of scrutiny and enforcement implemented by regulators? For corporations where the possibility of exceeding sanctioned emission limits is substantial, a rise in the likelihood of enforcement and punitive measures should correspondingly amplify the marginal cost associated with sustaining high levels of pollution. Consequently, firms responsible for considerable pollution might find it strategically prudent to either channel investments towards initiatives aimed at minimizing pollution, curtail the aggregate quantity of chemicals employed during the production process, or decrease the ratio of pollution per unit of output — a phenomenon referred to as the 'technique effect'.

In order to investigate the influence of median housing wealth on subsequent pollution levels, we utilize data from the EPA's Toxic Releases Inventory (TRI). While our attention was primarily directed towards enforcement outcomes under the Clean Air Act, our examination of pollution outcomes encompasses all regulated pollutants in a broader context. The analysis is executed at a granular level, focusing on each facility, specific chemical, and year. In alignment with the methodology described by Akey and Appel (2019), we adjust the total volume of toxic chemical releases by the production ratio. This ratio is established by taking the quantity of output for a particular year and normalizing it by the output volume of the preceding year for each reported chemical. We test our empirical predictions using the following regression specification:

$$\begin{aligned} Y_{k,i,c,t} = \beta_1 \cdot \textit{Median Housing Wealth}_{c,t} + \log\text{ (Plant Workforce Size)} + \text{County Controls} \\ + \gamma_{k,i} + \delta_{s,\tau} + \epsilon_{i,c,t} \end{aligned} \quad (3)$$

where $Y_{k,i,c,t}$ is the log of one plus the *pollution intensity* ($\frac{\text{total pollution}}{\text{the normalized production level}}$) level for a given chemical k, released by plant i in year t, located in county c. The natural logarithm of the plants' workforce size is denoted as the plant level control. County level control variables include the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant $\times$ Chemical and State $\times$ Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity.

Table 6 explores the impact of median housing wealth on the volume of toxic chemical releases from plants on a year-to-year basis, broken down by both the location and medium of release. The Ordinary Least Squares (OLS) regression results, presented in Column 1 of Panel A, indicate a significant negative association between median housing wealth and total scaled toxic releases. For each unit increase in median housing wealth, total scaled toxic releases decrease by 0.021 units, as denoted by the coefficient of -0.021 (t-statistic = -3.55, p ¡ 0.01). 2SLS regression analyses are reported in the remaining columns. Panel A column 2 shows that, holding production constant, we see that total pollution intensity reduce by about 4% relative to their standard deviation, for a one std. deviation increase in contemporaneous median housing wealth in the county. In columns 2 & 3, the results reveal that an increase in median housing wealth is significantly associated with a decrease in both on-site and off-site toxic chemical releases (coefficients of -0.042 and -0.035 respectively, with corresponding t-statistics of -4.28 and -2.11, and significant at 1% and 5% respectively). A similar pattern is observed with regard to the medium of release in Panel B. Increases in median housing wealth are significantly associated with decreases in toxic releases into the air, water, and underground, with coefficients of -0.029, -0.045, -0.061, and -0.043 respectively (t-statistics of -2.67, -2.25, -6.91, and -5.82, all significant at the 5% level or better). Most of the reduction in air pollution intensity results from a reduction in air fugitive emissions.

Table IA.4 reports the intricate relationship between median housing wealth and the volume of toxic chemical releases from industrial plants, stratified over three distinct time periods., using the exact same specification as in (3). In the period spanning 1991-1999, Panel A reveals a robust inverse relationship between median home value and the volume of toxic chemical releases across all mediums. For every standard deviation increase in median home value, total scaled toxic releases decreased by 0.075 standard deviations. This significant correlation extends to both on-site and off-site releases, and toxic air releases, with coefficients of -0.069, -0.037, and -0.068 respectively, all significant at the 1% level. Moving to the 2000-2011 period in Panel B, this inverse relationship is not only maintained but also intensified. A one standard deviation increase in median home value led to a 0.124 std.deviation decrease in total pollution intensity, a substantial increase from the previous period. This increase is observed across toxic releases both by location (onsite & offsite releases) and medium (air, water and underground) of release. Additionally, the decrease in water releases became statistically significant during this period. This is consistent with the findings in **table IA.1**, that the relationship between housing wealth and enforcement intensified in the

2000-2011 period.

However, the trends observed in the earlier periods undergo an intriguing transformation in the 2012-2019 period, as depicted in Panel C. The once robust and significant inverse relationship between median home value and various types of toxic releases appears to diminish and even reverse for on-site releases. This is line with the results in table IA.1, which showed that the relationship between housing wealth and enforcement outcomes appears to diminish in the 2012-2019 period. The relationship was significant only for informal enforcement, suggesting a potential shift in enforcement strategies over time. This interplay appears to be dynamic over time, suggesting the influence of temporal and possibly unobserved factors.

Table 7 provides insights into the influence of median housing wealth on one-year-ahead toxic chemical releases at the plant-year level. Looking at the coefficients associated with median housing wealth, we see a consistently negative relationship between housing wealth and toxic releases across different types of releases. This suggests that an increase in median housing wealth is generally associated with a decrease in toxic releases at the aggregate plant level. Specifically, a unit increase in median housing wealth is associated with a 0.051 unit decrease in total toxic releases (significant at 5% level), a 0.038 unit decrease in on-site releases (significant at 5% level), a 0.030 unit decrease in toxic air releases (significant at 10% level), and a 0.038 unit decrease in air fugitive releases (significant at 10% level). For the remaining types of releases (stack air, water, and underground), the relationship is not statistically significant. The control variables also present interesting findings. For example, the diversity index has a strong negative relationship with all types of releases, implying that more diverse counties tend to have lower toxic releases. On the other hand, plant workforce size has a positive relationship with all types of releases, suggesting that larger plants are associated with greater toxic releases. The log of population shows mixed results, with significant negative relationships with air stack releases and a significant positive relationship with water releases. Unemployment rate is negatively associated with most types of releases, suggesting that areas with higher unemployment rates may have lower toxic releases. The income level does not have a significant relationship with total toxic releases, but it does have a significant positive relationship with air fugitive releases and a significant negative relationship with water releases. In summary, these results underscore the complex dynamics between socio-economic factors and toxic releases.

In **Table 8**, we present the results from an analysis investigating the impact of median housing wealth on emissions of criteria air pollutants (CAP) and greenhouse gas emissions at the plant-year level. The NEI dataset includes emissions of carbon monoxide, ammonia, nitrogen oxides, particle pollution, sulfur dioxide and other volatile organic compounds, starting from 2008. The GHGRP dataset reports carbon dioxide emissions at the plant level, starting from 2010. A unit increase in median housing wealth is associated with a 0.109 standard deviation decrease in total CAP releases. This finding, significant at the 1% level, suggests that wealthier counties (as measured by median housing wealth) tend to have fewer total CAP releases. Median housing wealth has a similarly significant negative impact on carbon monoxide emissions, with a unit increase in median housing wealth leading to a 0.105 standard deviation decrease in these emissions. A unit increase in median housing wealth leads to a 0.097 standard deviation decrease in nitrogen oxide emissions, significant at the 1% level. For the other pollutants, namely ammonia, particulate matter, sulfur dioxide, volatile organic compounds, and carbon dioxide, the relationship with median housing wealth is not statistically significant.

The results in **table 5** highlighted that both median housing wealth and social capital, along with the party of the state governor, play significant roles in influencing enforcement outcomes under the Clean Air Act. The introduction of the interaction terms between median housing wealth and social capital, and between median housing wealth and the party of the governor, emphasized how social and political contexts can modify the impact of housing wealth on enforcement outcomes. Motivated by this cross-sectional evidence on variation in enforcement outcomes that depend on social & political factors, we employ a similar analysis for toxic chemical releases at the plant-year level.

Table IA.5 demonstrates the role of the county specific social capital in influencing the impact of changes in median housing wealth on toxic chemical releases of local polluting plants. The interaction term median home value × social capital index provides insight into how the relationship between housing wealth and emissions changes with different levels of social capital. The negative and statistically significant coefficients in columns (1), (3), and (5) suggest that as social capital increases, the negative impact of housing wealth on total toxic releases, offsite releases, and CAP releases becomes more pronounced. However, for onsite releases and toxic air releases, the interaction term is not significant, suggesting that social capital does not modify the relationship between housing wealth and these emissions. Similarly, in **table IA.6**, the interaction term between 'Median Home Value' and 'Party of the Governor' is included to examine whether the impact of median housing wealth on toxic chemical releases is affected by the political party of the governor. For total Toxic Releases, onsite Releases, offsite Releases, and total Toxic Air Releases, the interaction term is statistically insignificant. This indicates that the effect of median housing wealth on these types of toxic releases is not significantly different based on the party of the governor. For CAP Releases, the interaction term is statistically significant and negative (estimate = -0.048, t=-2.63). This suggests that the negative effect of median housing wealth on CAP releases is stronger when the governor belongs to the Democratic party. For CO2, the interaction term is negative but not statistically significant (estimate = -0.018, t=-1.63). This indicates that, while the point estimate suggests a stronger negative effect of median housing wealth on CO2 releases in areas with a democrat governor, the effect is not statistically distinguishable from zero at conventional levels.

4.3 Effect on Plant Level Abatement and Waste Generation Activities

In light of the empirical evidence presented in preceding sections, which illustrate the influence of alterations in median county-level housing wealth on toxic emissions, it becomes indispensable to pose the following inquiry: Through what mechanisms do local pollution-producing facilities modify their decisions concerning toxic emissions and attain a heightened intensity of pollution?

Figure 8 illustrates the EPA's waste management hierarchy for reducing emissions from industrial facilities. The figure depicts a pyramid, with the most preferred waste management methods at the bottom and the least preferred methods at the top. At the bottom of the pyramid is "Source Reduction", which refers to pollution prevention activities that reduce the amount of waste generated in the first place. This is the most preferred method according to the EPA. The next level up shows "Recycling", which involves processing waste materials back into usable products, reducing the need for new raw materials. The third level is "Energy Recovery", which includes methods like combusting waste to generate electricity. This extracts value from waste that can't be recycled. The fourth level is "Treatment", which refers to processes like incineration that change the form of waste to be less hazardous before disposal. At the top of the pyramid is "Disposal/Release", which includes any direct disposal or release of waste into the environment through methods like landfilling or air emissions. This is considered the least preferred method by the EPA. The width of the pyramid illustrates that in practice, the intensity of waste management is highest for Disposal/Release, followed by Treatment, then Recovery, Recycling and finally Source Reduction. In summary, this EPA pyramid reinforces the preferred hierarchy for waste management methods, with an emphasis on source reduction and recycling over disposal and release of hazardous materials into the environment. The widths show the practical reality does not yet match the ideal hierarchy, since pollution abatement is costly for polluting firms.

Following Li et al. (2021), we first focus on pollution abatement activities of local polluting plants. Plants' pollution abatement activities under two major categories: pollution prevention (also referred to as source reduction) and post-production process. Pollution prevention reduces or eliminates pollutants generated during the production process through practices such as modifying production processes, promoting the use of nontoxic or less toxic substances, and implementing conservation techniques. Post-production activities, including treatment, recycling, and disposal, are used to manage pollutant after their generation by the production process. **Table 9** explores the impact of median housing wealth on various aspects of plant operations and waste management, including abatement activities, total waste generated, and the percentage of toxic waste processed through different methods (released, recovered, recycled, and treated).

In column 1, the dependent variable represents an indicator of whether a plant has reported engaging in activities aimed at reducing pollution. The positive and statistically significant coefficient for median housing wealth (0.022, significant at the 1% level) suggests that an increase in housing wealth is associated with a higher likelihood of plants reporting pollution reduction activities at the plant-chemical level. In column 2, the dependent variable is the log of one plus the number of abatement activities at the plant-chemical level. Here again, the positive and statistically significant coefficient for median housing wealth (0.061, significant at the 1% level) indicates that an increase in housing wealth is associated with a higher number of abatement activities undertaken by the plant.

In column 3, the dependent variable is the total amount of waste generated at the plant-chemical level. The negative coefficient for median housing wealth (-0.122, significant at the 1% level) suggests that as housing wealth increases, the total amount of waste generated at the plant decreases. For columns 4 through 7, the dependent variables measure the percentage of toxic waste processed by plants through different means—releases, recovery, recycling, and treatment. An increase in median housing wealth is associated with a significant decrease in the percentage of waste released (-1.832, significant at the 1% level), a decrease in the percentage of waste recovered (-0.452, significant at the 1% level), an increase in the percentage of waste recycled (1.949, significant at the 1% level), and an increase in the percentage of waste treated (0.324, significant at the 10% level). In conclusion, the table suggests that median housing wealth can have significant impacts on plant operations and waste management practices. Specifically, higher median housing wealth is associated with increased abatement activities, reduced waste generation, lower waste releases, lower waste recovery, and higher waste recycling. The relationship with waste treatment is positive but not statistically significant.

Table IA.7 shows interesting variations of the impact of median housing wealth on local plants' waste management decisions across time, suggesting intertemporal heterogeneity in the effect of housing wealth on these environmental outcomes. In Panel A, which covers the time period 1991-1999, the median housing wealth exhibits a negative relationship with total waste generation (-0.018, significant at the 1% level) and the percentage of waste released (-1.143, significant at the 1% level). At the same time, the positive coefficients for the percentage of waste recovered (0.369, significant at the 1% level) and treated (0.356, significant at the 10% level) imply that higher housing wealth was associated with a greater emphasis on these waste management practices. In Panel B, which spans the time period 2000-2011, the negative relationship between housing wealth and total waste generation (-0.149, significant at the 1% level) and releases (-4.233, significant at the 1% level) becomes more pronounced, consistent with stronger enforcement and lower toxic chemical release during this sub-period. Additionally, the positive associations between housing wealth and the percentage of waste recycled (2.441, significant at the 1% level) and treated (1.661, significant at the 1% level) also strengthen. In Panel C, which covers the time period 2012-2019, the negative association between housing wealth and total waste generation (-0.134, significant at the 1% level) and releases (-4.433, significant at the 5% level) persist. The positive relationship between housing wealth and the percentage of waste recycled (3.949, significant at the 1% level) is also maintained. However, the coefficients for the percentage of waste recovered and treated are not statistically significant, suggesting no clear relationship between housing wealth and these waste management practices during this period.

Next, we focus on other potential channels, other than enforcement, through which an increase in the median housing wealth of the county could lead to reductions in toxic releases of polluting plants. One possible mechanism is through reductions in the cumulative production levels of toxic chemicals. To test this mechanism, we rely on the analysis in Akey and Appel (2019). The authors use the production ratio from the TRI database for this analysis. The production ratio is defined as the total output quantity for a particular year normalized by the output quantity of the preceding year for each reported chemical. This variable provides the advantage of being a quantity-centric output measure. Nonetheless, it does possess the drawback of measuring the growth in production, rather than its absolute level. We utilize the production ratio to ascertain normalized production levels for our analysis. The proxy for total production, Cumulative Production$_{p,c,t}$, is computed by setting the production to one in the initial year a chemical is reported in the TRI database and subsequently 'multiplying forward' each year by the reported production ratio for each plant-chemical observation set.[18] Precisely, our normalized production measure is computed using the

[18] Any missing observations are replaced by one

following formula:

$$\text{Cumulative Production}_{p,c,t} = \prod_{\tau=1}^{t} \left(1 \times \frac{\text{Quantity Produced}_{p,c,\tau}}{\text{Quantity Produced}_{p,c,\tau-1}}\right) = \prod_{\tau=1}^{t} (1 \times \text{Production Ratio}_{p,c,\tau})$$

Panel A of Table 10 investigates the relationship between variations in local housing wealth and changes in the production ratio, using 2SLS regressions. The analysis is disaggregated into different time periods to uncover potential temporal variations in these relationships. Across the full sample, the coefficient for median housing wealth is negative but statistically insignificant (-0.004), suggesting no strong association between housing wealth and production ratio. However, when the sample is divided into different time periods, the relationship becomes significant and varies over time. For the period 2000-2011, the coefficient is negative and significant at the 1% level (-0.041), indicating a decrease in production ratio with an increase in housing wealth. In contrast, for the period 2012-2019, the coefficient is positive and significant at the 1% level (0.066), indicating an increase in the production ratio with higher housing wealth.

Another potential mechanism by which rising housing wealth could influence the production and emission decisions of polluting plants, beyond the effect of heightened enforcement, is through escalating manufacturing costs for the chemicals produced. This escalation could occur if worker salaries in counties experiencing increased housing wealth also rise. Indeed, if wealth shocks affect the cost of providing local labor and capital (real estate) - which are factors of production, they could then change the prices of inputs and hence the plant's production function. To investigate this channel, we need to be able to show that the marginal costs of manufacturing goods do not change with an increase/ decrease in house prices.

Table 10, panel B scrutinizes the correlation between median housing wealth and the mean wage in the utilities, manufacturing, and construction industries. When considering the entire sample, there is no statistically notable linkage between housing wealth and the average wage (0.006). The correlation, however, shifts across distinct timeframes. During the 1991-1999 period, an elevation in housing wealth correlates with a significant decline in the average wage (-0.091, significant at the 5% level). Conversely, between 2000-2011, the association is positive and marginally significant (0.129, significant at the 10% level), indicating that augmented housing wealth correlates with a rise in average wage. But in the period from 2012-2019, the coefficient is not statistically significant (0.025), implying no evident correlation between housing wealth and average wage during this timeframe. Taken as a whole, the results in **table 10** suggest that housing wealth's influence on production and wages is subject to changes across different time periods, emphasizing the necessity to account for temporal variations in these relationships. This could be attributed to evolving economic conditions, policy landscapes, or other unobservable factors that vary over time.

5 Discussion of Results and Prospects for Future Research

The findings from the preceding sections imply that an augmentation in housing wealth correlates with an increase in enforcement actions. Consequently, local pollutant-generating plants, faced with a heightened marginal cost of pollution, are prompted to decrease their pollution intensity and allocate more resources towards pollution abatement technologies. The causality narrative within the paper flows from changes in median housing wealth to enforcement actions, and finally to toxic chemical discharges. However, it's also conceivable that alterations in house prices could yield similar effects by influencing the collateral value of firms' real estate. Given that the majority of the pollutant-generating plants examined in this paper are part of tradable industries, it seems unlikely that local house prices would have any significant impact on plant-level employment or production decisions through a local demand channel.

Nonetheless, if these plants possess local real estate, an escalation in house prices could alleviate financial constraints, thereby augmenting the firm's investments in pollution abatement activities. In fact, Xu and Kim (2022) provide evidence that financial constraints amplify firms' toxic emissions, as firms consciously balance abatement costs against potential legal liabilities. However, Mian and Sufi (2014) and Giroud and Mueller (2019) suggest that house price changes had a minimal impact on local employment of firms in tradable industries, although their sample period only covers the housing bust phase from 2006-09. The hypothesis proposed by Giroud and Mueller (2019) is only applicable to financially constrained firms, whereas the regulatory-based channel (our hypothesis) is relevant to both financially constrained and unconstrained firms. Financially unconstrained firms might find it more cost-effective to pay fines in response to increased enforcement stringency, while constrained firms might find it cheaper to decrease production or engage in within-firm pollution shifting (Bartram et al. (2022)). As our argument is predicated on an enforcement-based channel, this implies that financially unconstrained firms in tradable industries should also be influenced by changes in local house prices.

A pertinent topic for further investigation is the issue of non-linearity of enforcement actions in relation to output. If house prices correlate positively with plant production, then any non-linearity could confound our estimates. We must ascertain that the marginal costs of manufacturing goods remain stable in the face of an increase or decrease in house prices. However, it's not just the costs of goods that we need to consider, but also the quantity of output produced, since any non-linear relationship between output and enforcement action could pose a threat to our identification. Even if output or production fluctuates in parallel with local house price changes, we need to identify those plants that demonstrate the least sensitivity of output to local house price changes (the bottom quartile). The next step would then be to demonstrate that enforcement actions still fluctuate for this subset of firms.

6 Conclusion

This paper provides novel evidence on the link between local housing wealth and the stringency of environmental regulation enforcement. Using county-level variation in median home values driven by exogenous mortgage rate shocks, we demonstrate a causal relationship whereby increases in local housing wealth lead regulators to significantly strengthen enforcement of clean air standards under the Clean Air Act. A one standard deviation rise in instrumented median home values causes enforcement actions and penalties against polluting firms to increase by 8-19% at the county-industry level. The results are robust to various identification strategies and hold when analyzing enforcement at the individual plant level.

Additional analyses indicate that in regions with higher levels of social capital and under the leadership of Democratic governors, enforcement actions show a greater sensitivity to changes in housing wealth. This aligns with observations made in prior research. Exploiting the panel structure of emissions data, we then show heightened enforcement compels polluting plants to reduce future toxic releases by 3-6%. Local firms particularly cut more hazardous emissions in response to enforcement pressure induced by housing wealth gains. Plants concurrently invest more in abatement technologies like recycling when county home values rise.

This study makes three key contributions. First, it establishes the importance of local community preferences in shaping the intensity of environmental regulation enforcement. Second, it highlights how distributional impacts can arise in decentralized environmental policy if enforcement stringency correlates with economic resources. Lastly, it demonstrates that exogenous changes in local housing wealth have tangible effects on polluting behavior of plants through the regulatory channel.

While we focused specifically on clean air regulation, future work could examine enforcement and firm responses across other environmental domains like water pollution. It would also be fruitful to explore other dimensions of inequality, such as race and income, in determining uneven enforcement incentives for regulators. Overall, this paper deepens our understanding of the political economy forces that influence real environmental outcomes in the presence of decentralized policy and enforcement.

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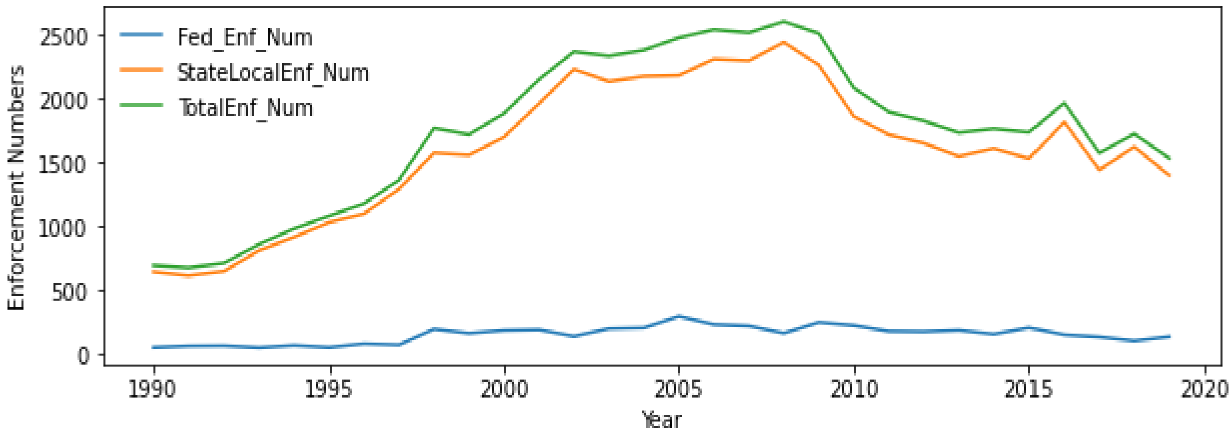

Figure 1.1: This figure plots the total civil enforcement activity in the US under the Clean Air Act, by either the EPA or State & Local agencies, in each year starting from 1990 to 2020. Civil enforcement include the following three categories- judicial, administrative formal and administrative informal enforcement actions.

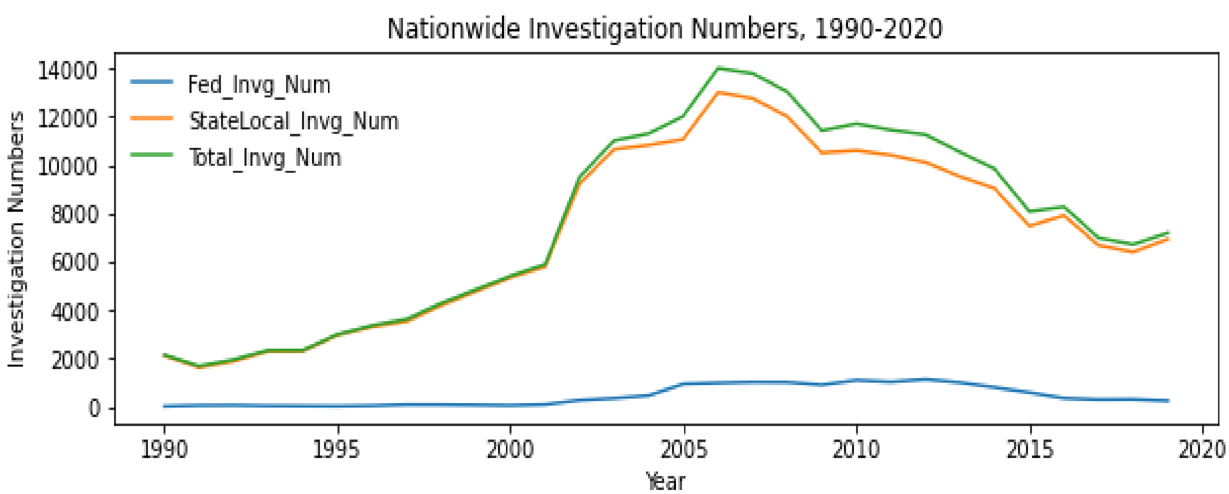

Figure 1.2: This figure plots the total number of investigations in the US, conducted under the Clean Air Act, by either the EPA or State & Local agencies, in each year starting from 1990 to 2020. Investigations include on-site and off-site full/partial compliance evaluations (FCE and PCE). We use records in Integrated Compliance Information System (ICIS)-AIR FCE & PCE database to count facility-year level investigations.

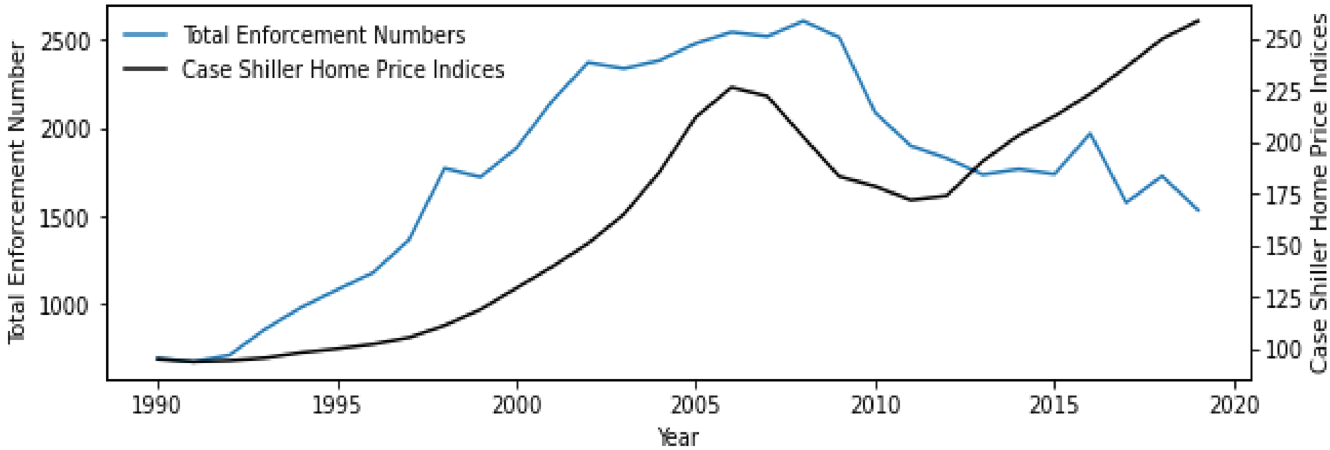

Figure 2: This figure plots the real S&P/Case-Shiller national house price index and total civil enforcement activity in the US under the Clean Air Act.

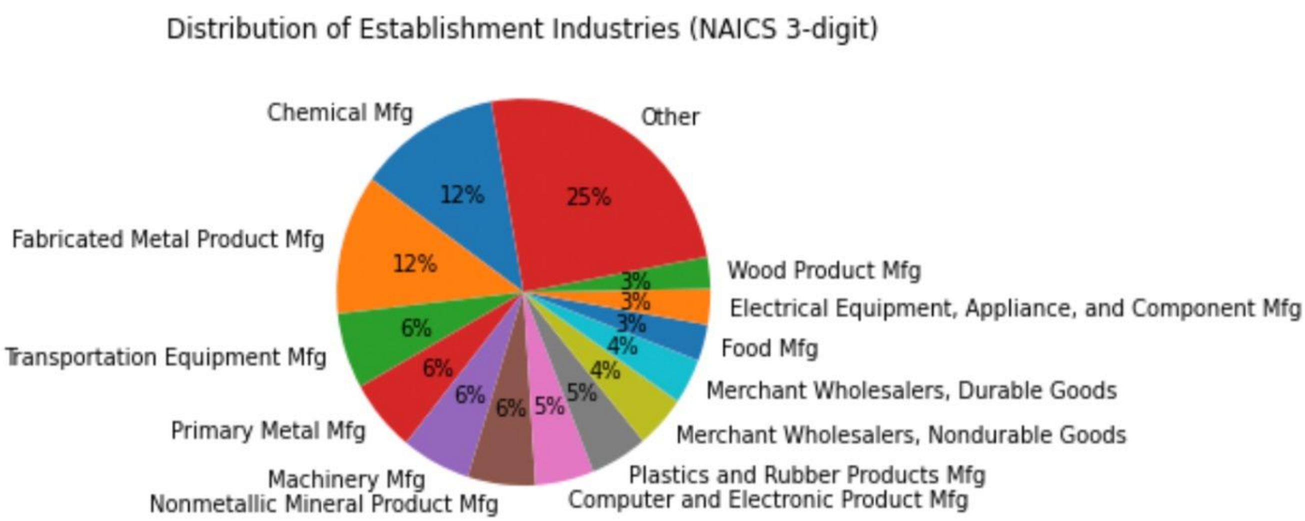

Figure 3: This figure shows the distribution of industries (defined at the three-digit NAICS) for all polluting plants in the Toxic Releases Inventory (TRI) database that are included in our sample.

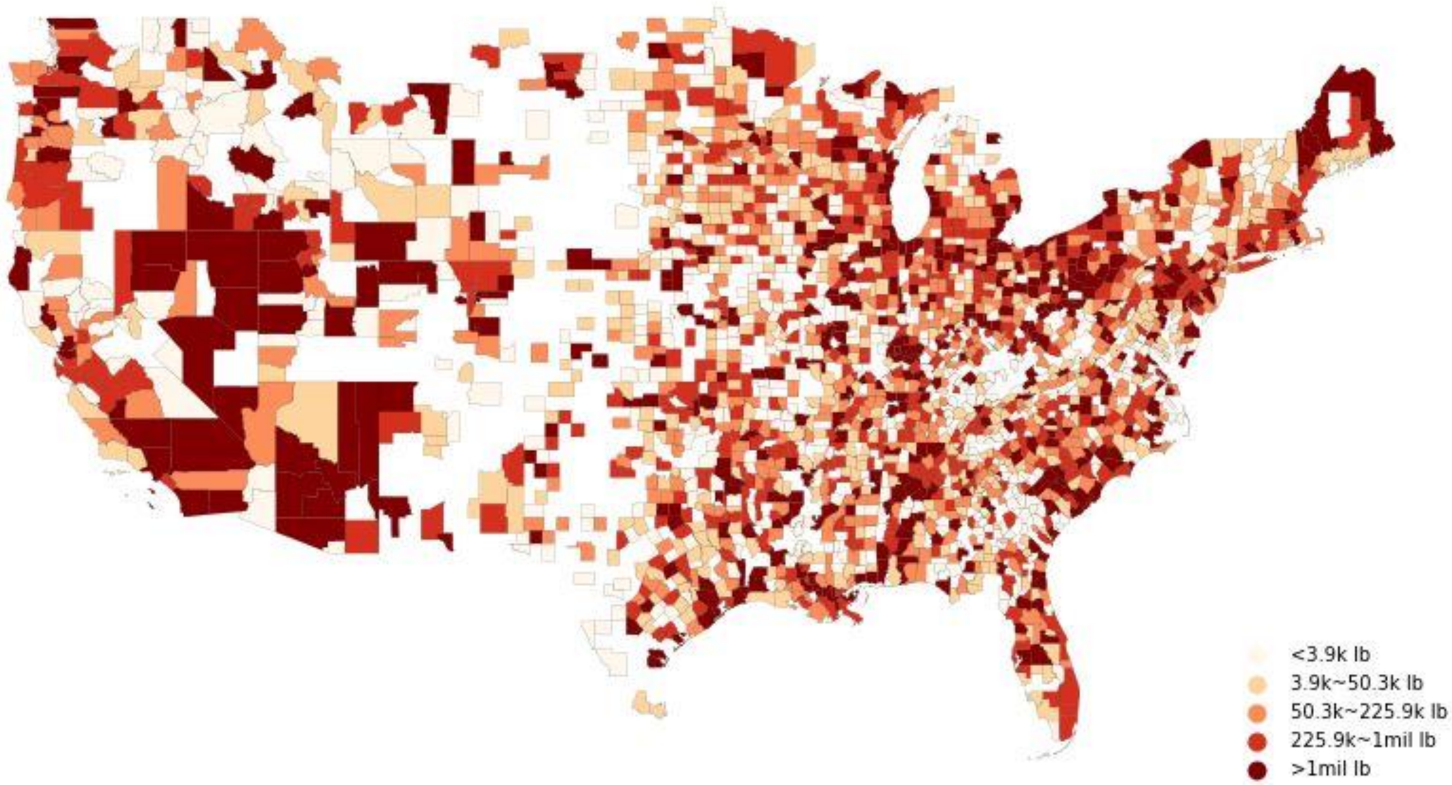

Figure 4: This figure displays the county level total toxic emission in pounds for the year 2007.

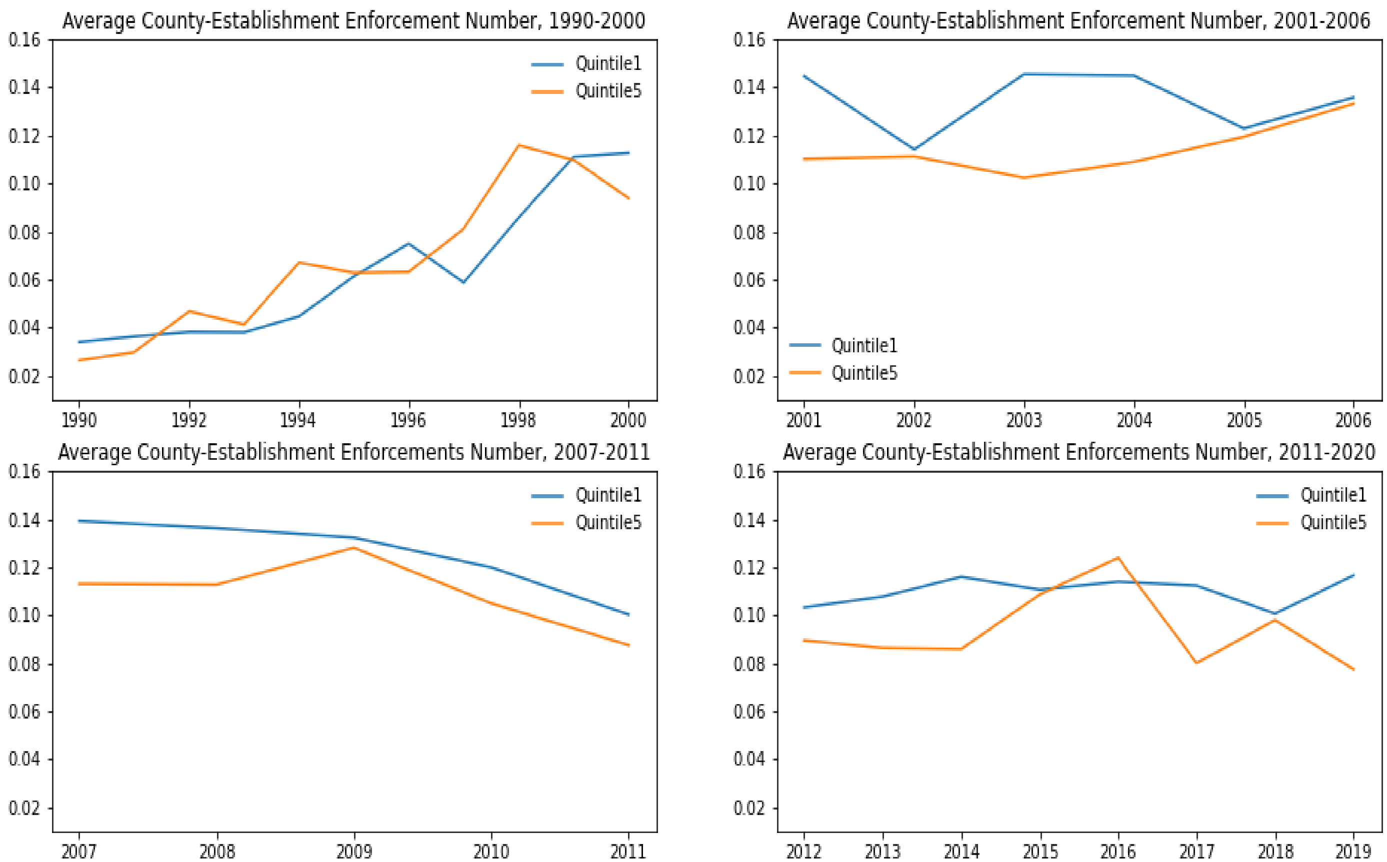

Figure 5: This figure plots the county-level average plant enforcement activity under the Clean Air Act in the top quintile (quintile 5) and bottom quintile (quintile 1) of median housing wealth, across four separate time periods.

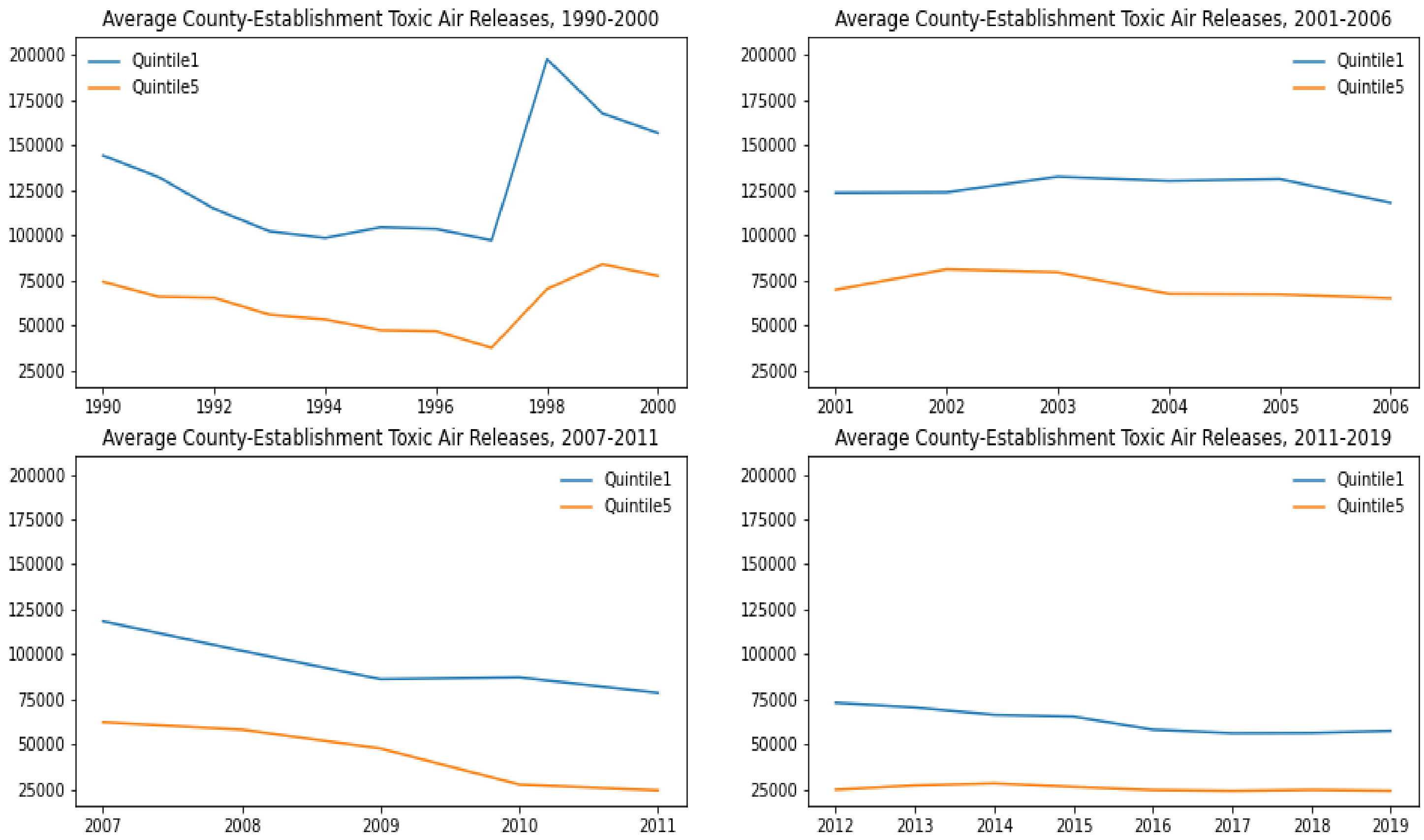

Figure 6: This figure plots the county-level average plant toxic air emissions (in pounds) in the top quintile (quintile 5) and bottom quintile (quintile 1) of median housing wealth, across four separate time periods.

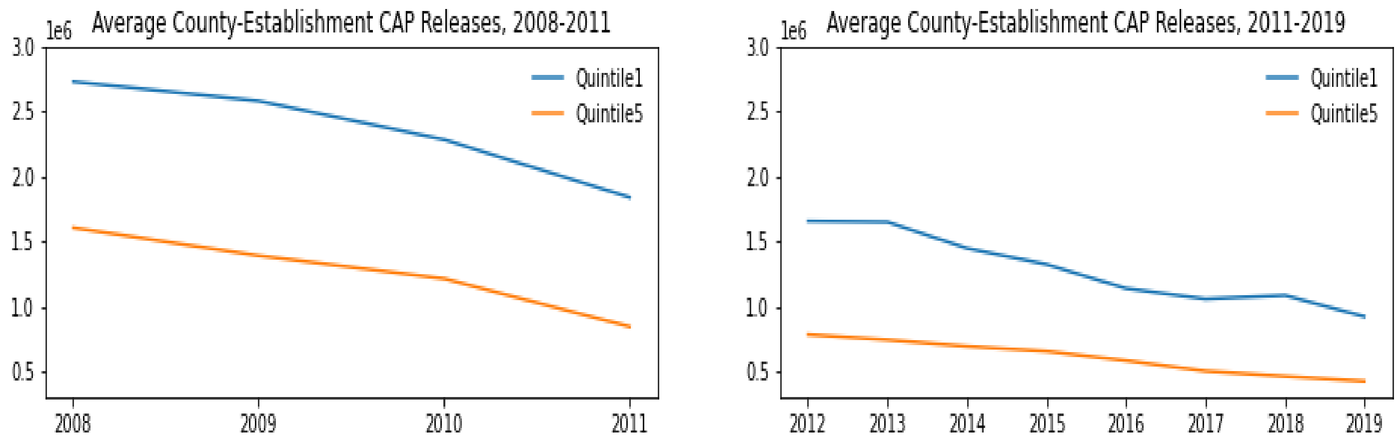

Figure 7.1: This figure shows the county-level average emissions of criteria air pollutants (in millions of pounds) in the top quintile (quintile 5) and bottom quintile (quintile 1) of median housing wealth, across two separate time periods. Source– National Emissions Inventory (NEI)

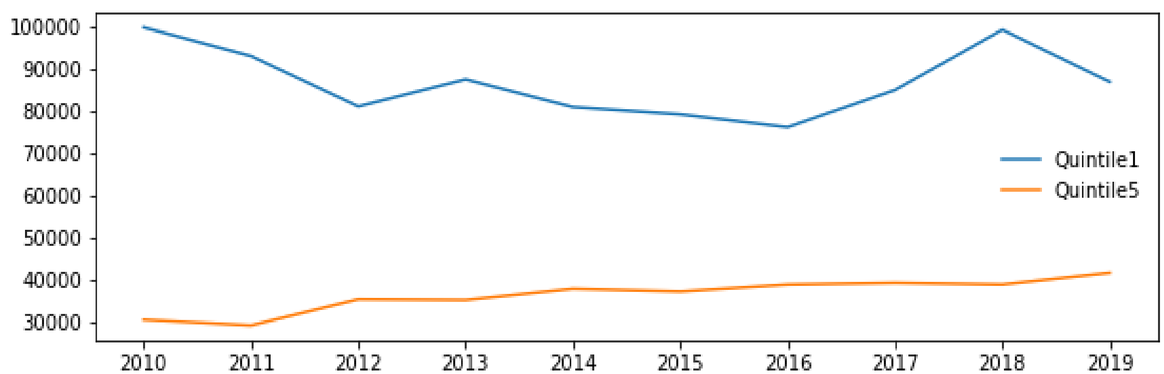

Figure 7.2: This figure plots the county-level average plant combined greenhouse gas emissions (in millions of metric tons) of carbon dioxide, methane, nitrous oxide and fluorinated gases, in the top quintile (quintile 5) and bottom quintile (quintile 1) of median housing wealth, across two separate time periods. Source – Greenhouse Gas Reporting Program (GHGRP)

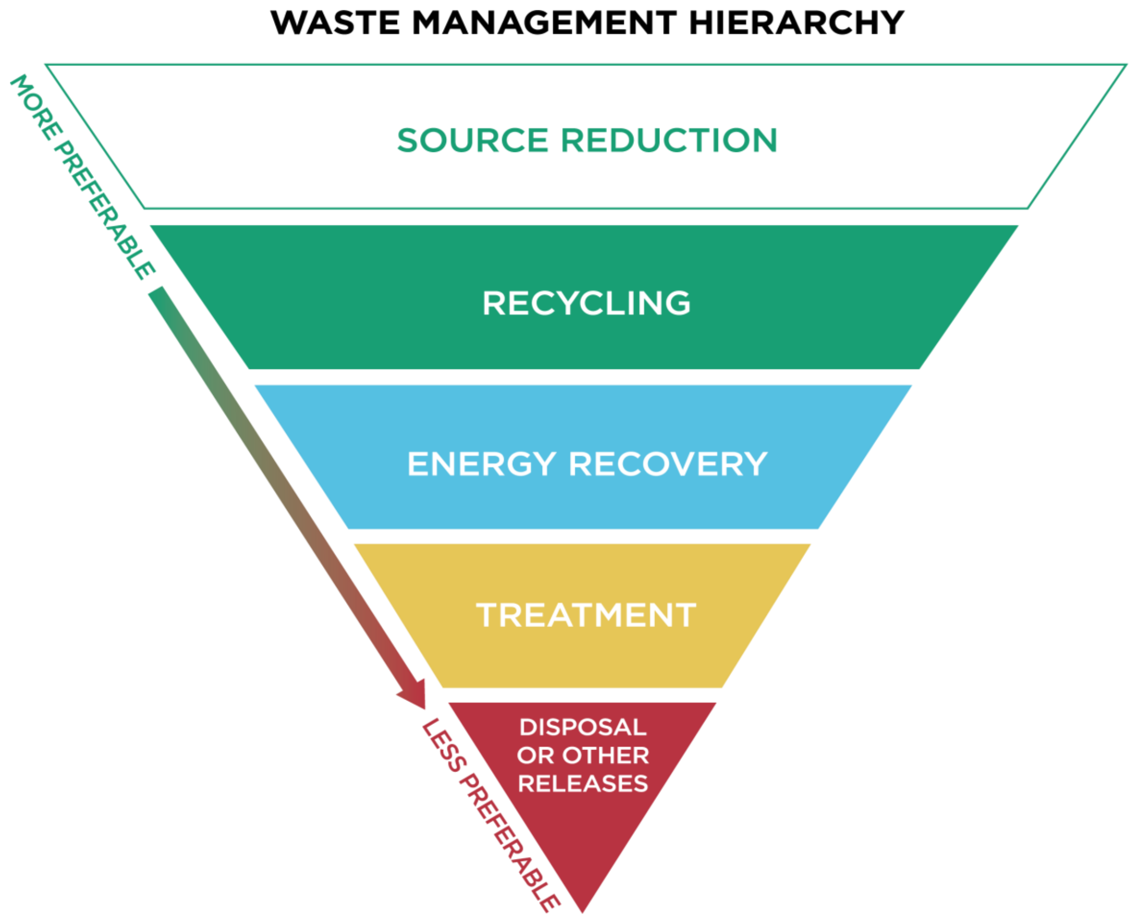

Figure 8: Waste Management Hierarchy and Intensity
This figure shows EPA's waste management hierarchy in reducing emission. Waste management activities are ranked from the most to the least environmentally preferred by EPA's preference. The most preferred approach includes source reduction (pollution prevention) activities that directly reduce the generation of toxic releases, followed by post-product processes such as recycling, energy recovery, and treatment. The least preferred approach is disposal or other release into the environment.

Table 1

This table presents summary statistics for the main variables in the paper. Emissions are annual toxic chemical releases (in pounds) at the plant-year level over the period 1991-2019, available on the U.S. Environmental Protection Agency's (EPA) website. Environmental regulation compliance data including inspections, enforcement actions, and penalty amounts (in thousands of dollars), are from the EPA's Enforcement and Compliance History Online (ECHO) dataset over the period 1991-2019. Plant workforce size is the employment at the plant level, which is obtained from the National Establishment Time Series- TRI dataset.

	Observations	MEAN	SD
Total Investigations	552,583	0.288	0.800
Total Enforcement	552,583	0.0577	0.276
Total Penalty Amount	552,583	49.53	578.7
Total Toxic Releases	552,583	52767	200321
Onsite Releases	552,583	39831	160845
Offsite Releases	552,583	4710	21816
Plant Workforce Size	552,583	179.3	259.0

Table 2

This table presents the results of the OLS regressions of enforcement outcomes under the Clean Air Act on contemporaneous county level variables between 1991 and 2019 across counties in the US. The dependent variable denotes the natural logarithm of one plus the enforcement outcomes at the county-industry-year level (aggregated across all plants within each industry in a county that emit toxic releases), divided by the total number of toxic-waste emitting plants within each industry. The industry sector is identified by the first two digits of North American Industry Classification System (NAICS) codes. All the dependent variables along with the median housing wealth and salient manmade spills are standardized each year so as to have zero mean and unit standard deviation. The natural logarithm of one plus the average toxic waste per plant within each industry in the county is included as a control. Other control variables include the logarithm of the per capita income and population level of the county, the unemployment rate, diversity index and a dummy variable that equals one if the county is designated as a nonattainment county. All regressions include State × Industry × Year fixed effects and County fixed effects. Standard errors are clustered at the county level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively. Data Source: - EPA ECHO and TRI Databases

	Log (1 + Enforcement Outcomes/ Number of Plants)				
Dependent Variable	Total Enforcement	Formal Enforcement	Informal Enforcement	Penalty Numbers	Penalty Amount
	(1)	(2)	(3)	(4)	(5)
Median Housing Wealth	0.034**	0.007	0.057***	0.066***	0.042**
	(2.24)	(0.87)	(2.96)	(3.03)	(2.45)
Salient Manmade Spills	0.123	0.118	0.066	0.080	0.174**
	(1.37)	(1.32)	(0.82)	(0.81)	(1.98)
Log (Toxic Air Releases)	0.032***	0.023***	0.026***	0.022***	0.030***
	(22.72)	(16.64)	(19.76)	(15.31)	(22.55)
Log (Income)	0.065	0.145**	-0.026	0.143**	0.134*
	(0.82)	(2.00)	(-0.34)	(1.98)	(1.79)
Log (Population)	-0.079	0.015	-0.126**	-0.001	-0.050
	(-1.33)	(0.25)	(-2.37)	(-0.01)	(-0.85)
Unemployment Rate	0.000	0.001	-0.001	0.001	0.006*
	(0.08)	(0.23)	(-0.17)	(0.16)	(1.72)
Diversity Index	0.470*	0.498*	0.279	0.440	0.441
	(1.79)	(1.93)	(1.14)	(1.51)	(1.63)
Nonattainment County	0.032	0.042**	0.012	0.046***	0.088***
	(1.24)	(2.06)	(0.41)	(2.62)	(3.26)
Observations	162,524	162,524	162,524	162,524	162,524
R-squared	0.251	0.224	0.230	0.215	0.209
County FE	Yes	Yes	Yes	Yes	Yes
State × Industry × Year FE	Yes	Yes	Yes	Yes	Yes

Table 3

This table presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act on contemporaneous county level variables between 1991 and 2019 across counties in the US. We instrument the median housing wealth with the interaction between the land unavailability (LU) measure and the national mortgage interest rate (NIMR). Column 1 reports the results of the first-stage regression. All regressions include State × Industry × Year fixed effects and County fixed effects. Standard errors are clustered at the county level and are robust to heteroscedasticity. We further report the Kleibergen-Paap F-statistic for the weak instrument test. T statistics are reported in brackets, and ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Median Housing Wealth	Total Enforcement	Formal Enforcement	Informal Enforcement	Penalty Numbers	Penalty Amount
	(1)	(2)	(3)	(4)	(5)	(6)
LU × NMIR	-0.00175***					
	(-6.68)					
Median $\widehat{\text{Housing}}$ Wealth		0.081***	0.020	0.147***	0.190*	0.126**
		(2.66)	(0.73)	(2.69)	(1.91)	(2.49)
Salient Manmade Spills		0.145	0.143	0.080	0.100	0.192**
		(1.49)	(1.51)	(0.91)	(0.99)	(2.12)
Log (Toxic Air Releases)	-0.000901**	0.035***	0.026***	0.028***	0.024***	0.037***
	(-2.27)	(23.82)	(17.67)	(21.94)	(16.01)	(24.00)
Log (Income)	1.612***	0.066	0.157	-0.039	0.129	0.098*
	(8.00)	(1.27)	(1.31)	(-0.86)	(1.26)	(1.78)
Log (Population)	1.100***	-0.194	-0.021	-0.267**	-0.078	-0.145
	(12.94)	(-1.40)	(-0.14)	(-2.21)	(-0.47)	(-1.05)
Unemployment Rate	-0.0311***	-0.003	-0.000	-0.005	-0.002	0.003
	(-4.82)	(-0.62)	(-0.02)	(-0.99)	(-0.31)	(0.72)
Diversity Index	1.559***	0.257	0.426	0.028	0.324	0.330
	(3.17)	(0.82)	(1.44)	(0.10)	(0.98)	(1.06)
Nonattainment County	0.0840***	0.023	0.039*	0.001	0.041**	0.087***
	(4.80)	(0.83)	(1.87)	(0.02)	(2.06)	(3.08)
Kleibergen-Paap Wald F statistic	44.60					
Observations		161,776	161,776	161,776	161,776	161,776
County FE		Yes	Yes	Yes	Yes	Yes
State × Industry × Year FE		Yes	Yes	Yes	Yes	Yes

Table 4

This table presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act at the plant level on contemporaneous county level variables. The dependent variable denotes the natural logarithm of one plus the enforcement outcomes at the plant-year level. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. The natural logarithm of one plus the average toxic waste emitted by the plant, and the natural logarithm of the plants' workforce size are included as additional controls. Other control variables include the logarithm of the per capita income and population level of the county, the unemployment rate, diversity index and a dummy variable that equals one if the county is designated as a nonattainment county. All regressions include State × Year fixed effects and County fixed effects. Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Median Home Value (1)	Total Enforcement (2)	Formal Enforcement (3)	Informal Enforcement (4)	Penalty Numbers (5)	Penalty Amount (6)
LU × NMIR	-0.000518***					
	(-5.85)					
Median $\widehat{\text{Housing}}$ Wealth		0.104***	0.081*	0.112***	0.119***	0.109**
		(2.59)	(1.86)	(2.73)	(2.82)	(2.55)
Log (Toxic Air Releases)	-0.000432***	0.008***	0.005***	0.007***	0.005***	0.006***
	(-1.06)	(13.29)	(8.96)	(11.25)	(9.44)	(9.91)
Log (Income)	1.062***	-0.103***	-0.051*	-0.101***	-0.058*	-0.051
	(20.12)	(-3.41)	(-1.66)	(-3.17)	(-1.96)	(-1.57)
Log (Population)	0.401***	-0.008	-0.001	-0.014	0.004	0.004
	(13.95)	(-0.67)	(-0.08)	(-1.25)	(0.32)	(0.36)
Unemployment Rate	0.0184***	-0.007***	-0.005***	-0.005**	-0.007***	-0.004*
	(11.94)	(-3.73)	(-2.60)	(-2.46)	(-3.35)	(-1.93)
Diversity Index	-0.791***	0.004	0.065	-0.046	-0.036	-0.030
	(-4.65)	(0.05)	(0.82)	(-0.57)	(-0.46)	(-0.36)
Log (Plant Workforce Size)	-0.00152***	-0.010***	-0.012***	-0.006**	-0.016***	-0.013***
	(-0.72)	(-3.67)	(-4.04)	(-2.15)	(-5.18)	(-4.38)
F statistic	34.20					
Observations	552,583	552,583	552,583	552,583	552,583	552,583
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 5

This table presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act at the plant level on contemporaneous county variables. The dependent variable denotes the natural logarithm of one plus the enforcement outcomes at the plant-year level. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. In columns 1, 2, and 3, we include the interaction between median housing wealth and the social capital index of Rupasingha, Goetz & Freshwater (2006). In columns 4, 5 and 6, we include the interaction between median housing wealth and the party of the governor of the state in which the county is located. The natural logarithm of one plus the average toxic waste emitted by the plant, and the natural logarithm of the plants' workforce size are included as additional controls. Other county level control variables include the logarithm of the per capita income and population level of the county, the unemployment rate, diversity index and a dummy variable that equals one if the county is designated as a nonattainment county. All regressions include State × Year fixed effects and County fixed effects. Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total Enforcement (1)	Penalty Numbers (2)	Penalty Amount (3)	Total Enforcement (4)	Penalty Numbers (5)	Penalty Amount (6)
Median $\widehat{\text{Housing}}$ Wealth	0.111***	0.127***	0.113***	0.093**	0.115***	0.097**
	(2.87)	(3.15)	(2.78)	(2.34)	(2.67)	(2.26)
Median $\widehat{\text{Housing}}$ Wealth × Social Capital Index	0.033*	0.043**	0.037**			
	(1.90)	(2.39)	(2.01)			
Median $\widehat{\text{Housing}}$ Wealth × Party of the Governor				0.049***	0.058***	0.052***
				(2.88)	(4.19)	(3.65)
F statistic	21.94			17.92		
Observations	552,583	552,583	552,583	552,583	552,583	552,583
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Plant Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table 6

This table reports the impact of median housing wealth on one year ahead toxic chemical releases at the plant-chemical level. Column 1 of Panel A reports the results of the OLS regression, whereas the rest of the columns report the results of 2SLS regressions. Panel A reports toxic chemical releases by the location of release, i.e., on-site and off-site while panel B reports toxic chemical releases disaggregated by the medium of release, i.e., air and water. The dependent variable is the log of one plus the total pollution/ the normalized production level for a given plant-chemical observation. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. The natural logarithm of the plants' workforce size is denoted as the plant level control. County level control variables include the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant × Chemical and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively. Data Source: - EPA's TRI and Pollution Prevention (P2) Databases

PANEL A

Dependent Variable	Total Scaled Toxic Releases (1)	Total Scaled Toxic Releases (2)	Onsite Releases (3)	Offsite Releases (4)
Median Housing Wealth	-0.021***			
	(-3.55)			
Median $\widehat{\text{Housing}}$ Wealth		-0.042***	-0.035**	-0.026*
		(-4.28)	(-2.11)	(-1.81)
Observations	3,692,863	3,692,863	3,692,863	3,692,863
Plant Level Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Plant × Chemical Fixed Effects	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes

PANEL B

Dependent Variable	Toxic Air Releases (1)	Air Stack Releases (2)	Air Fugitive Releases (3)	Water Releases (4)	Underground Releases (5)
Median $\widehat{\text{Housing}}$ Wealth	-0.029***	-0.017	-0.045**	-0.061***	-0.043***
	(-2.67)	(-0.32)	(-2.25)	(-6.91)	(-5.82)
Observations	3,692,863	3,692,863	3,692,863	3,692,863	3,692,863
Plant Level Controls	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes
Plant × Chemical FEs	Yes	Yes	Yes	Yes	Yes
State × Year FEs	Yes	Yes	Yes	Yes	Yes

Table 7

This table reports the impact of median housing wealth on one year ahead toxic chemical releases at the plant-year level. The dependent variable is the log of one plus the total pollution level (summed across all toxic chemical releases for a given plant) for a given plant-year observation. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total Toxic Releases	Onsite Releases	Offsite Releases	Toxic Air Releases	Air Fugitive Releases	Air Stack Releases	Water Releases	Underground Releases
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Median $\widehat{\text{Housing}}$ Wealth	-0.051**	-0.038**	-0.033	-0.030*	-0.038*	-0.011	-0.010	-0.023
	(-2.35)	(-2.47)	(-1.51)	(-1.79)	(-1.73)	(-0.05)	(-0.49)	(-1.04)
Log (Income)	-0.001	0.021	-0.001	0.027	0.055***	0.028	-0.063***	0.005
	(-0.03)	(1.21)	(-0.05)	(1.56)	(2.83)	(1.63)	(-3.82)	(0.33)
Log (Population)	-0.002	-0.008	-0.003	-0.014**	-0.002	-0.017***	0.018**	-0.005***
	(-0.31)	(-1.32)	(-0.43)	(-2.17)	(-0.24)	(-2.62)	(2.56)	(-2.76)
Unemployment Rate	-0.009***	-0.011***	-0.001	-0.010***	-0.009***	-0.008***	-0.005***	-0.002
	(-7.05)	(-9.63)	(-0.47)	(-8.95)	(-7.32)	(-6.74)	(-4.42)	(-1.54)
Diversity Index	-0.165***	-0.189***	-0.066	-0.137***	-0.345***	-0.034	-0.069	-0.074**
	(-3.39)	(-4.09)	(-1.23)	(-2.94)	(-6.71)	(-0.74)	(-1.51)	(-2.27)
Log (Plant Workforce Size)	0.027***	0.028***	0.017***	0.029***	0.023***	0.033***	0.010***	0.016***
	(17.77)	(18.74)	(9.40)	(18.99)	(13.88)	(22.22)	(6.31)	(10.43)
Observations	525,703	525,703	525,703	525,703	525,703	525,703	525,703	525,703
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8

This table reports the impact of median housing wealth on one year ahead emissions of criteria air pollutants (CAP) and greenhouse gas emissions at the plant-year level between 2008 and 2019 across counties in the US. The NEI (National Emissions Inventory) dataset includes emissions of carbon monoxide, ammonia, nitrogen oxides, particle pollution, sulfur dioxide and other volatile organic compounds. The GHGRP (Greenhouse Gas Reporting Program) dataset reports carbon dioxide emissions at the plant level, starting from 2010. The dependent variable is the log of one plus the total pollution level for a given plant-year observation. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and $***$, $**$, $*$ denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total CAP Releases (1)	Carbon monoxide (2)	Ammonia (3)	Nitrogen Oxide (4)	Particulate matter<2.5 μm (6)	Sulphur Dioxide (7)	Volatile organic compounds (8)	Carbon dioxide (9)
Median $\widehat{\text{Housing}}$ Wealth	-0.109***	-0.105***	-0.048	-0.097***	0.038	-0.019	-0.039	0.025
	(-2.97)	(-3.22)	(-1.11)	(-3.17)	(1.14)	(-0.68)	(-1.02)	(0.90)
Log (Income)	0.022	0.029	0.014	0.023	0.023	0.042**	-0.003	0.060***
	(0.88)	(1.33)	(0.44)	(1.03)	(0.98)	(2.16)	(-0.10)	(4.24)
Log (Population)	0.014	0.001	-0.001	0.010	0.005	0.010	0.017	-0.010***
	(1.42)	(0.07)	(-0.12)	(1.22)	(0.56)	(1.29)	(1.64)	(-3.77)
Unemployment Rate	-0.001	-0.005***	0.004**	-0.005***	-0.000	-0.006***	-0.004***	-0.001
	(-0.56)	(-3.77)	(2.39)	(-4.10)	(-0.33)	(-5.27)	(-2.61)	(-1.10)
Diversity Index	-0.046	-0.161**	0.051	-0.128*	-0.065	-0.097	-0.081	0.131***
	(-0.56)	(-2.08)	(0.59)	(-1.78)	(-0.89)	(-1.49)	(-0.95)	(4.05)
Log (Plant Workforce)	0.012***	0.004	0.006	0.008***	0.011***	0.013***	0.010***	0.005**
	(3.78)	(1.40)	(1.59)	(2.86)	(3.67)	(4.91)	(3.18)	(2.36)
Observations	202,334	202,334	202,334	202,334	202,334	202,334	202,334	165,259
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9

This table reports the impact of median housing wealth on plants' one year ahead source reduction abatement activities, total waste generation at the plant-chemical level, and waste management through recycling, energy recovery, treatment, and releases. In column 1, the dependent variable is an indicator that takes the value of one for if a plant reported engaging in activities designed to reduce pollution for a given plant-chemical observation. In column 2, the dependent variable is the log of one plus the number of abatement activities at the plant-chemical level. In column 3, the dependent variable is the amount of total waste generated at the plant-chemical level. The dependent variables in columns 4, 5, 6 and 7 measure the percentage of toxic waste processed by plants through recycling, energy recovery, treatment, and releases. Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively. Data Source: - EPA's Pollution Prevention (P2) Databases

Dependent Variable	Abatement	Abatement Numbers	Total Waste Generated	% Released	% Recovered	% Recycled	% Treated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Median $\widehat{\text{Housing}}$ Wealth	0.022***	0.061***	-0.122***	-1.832***	-0.452***	1.949***	0.324
	(2.99)	(3.27)	(-3.01)	(-6.25)	(-4.19)	(11.10)	(1.64)
Observations	3,690,481	3,690,481	3,681,291	3,681,291	3,681,291	3,681,291	3,681,291
Plant Level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant-Chemical Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10

This table uses 2SLS regressions to examine how production and local wages in manufacturing, utilities and construction sectors change with variations in local housing wealth. The dependent variable in Panel A, is the cumulative production ratio. The cumulative production ratio is constructed from the TRI database and is defined as the natural logarithm of the normalized production level – the production level relative to the first year in the sample. Fixed effects are indicated in the table. Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. Standard errors are clustered at the plant level and are robust to heteroscedasticity. The dependent variable in Panel B is the logarithm of the average wage in one of the three industry sectors (grouped by NAICS codes) – utilities, manufacturing and construction. All regressions include Industry × Year and State × Year fixed effects. Control variables include the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. Standard errors are clustered at the county level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively.

PANEL A				
	Full Sample	1991 - 1999	2000 - 2011	2012 - 2019
Dependent Variable		Cumulative Production Ratio		
	(1)	(2)	(3)	(4)
$Median\ \widehat{Housing}\ Wealth$	-0.004	-0.003	-0.041***	0.066***
	(-0.51)	(-0.57)	(-4.07)	(2.96)
Observations	3,692,863	1,159,285	1,615,935	899,399
Plant Level Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Plant-Chemical Fixed Effects	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes

PANEL B				
	Full Sample	1991 - 1999	2000 - 2011	2012 - 2019
Dependent Variable		Log (Average Pay)		
	(1)	(2)	(3)	(4)
$Median\ \widehat{Housing}\ Wealth$	0.006	-0.091**	0.129*	0.025
	(0.15)	(-2.17)	(1.78)	(0.14)
Observations	431,866	135,050	178,999	117,811
County Fixed Effects	Yes	Yes	Yes	Yes
Industry × Year Fixed Effects	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes

INTERNET APPENDIX

Table IA.1

This table presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act on contemporaneous county level variables, across three separate time periods between 1991 and 2019. We instrument the median housing wealth with the interaction between the land unavailability (LU) measure and the national mortgage interest rate (NIMR). All regressions include State × Industry × Year fixed effects and County fixed effects. Standard errors are clustered at the county level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total Enforcement	Formal Enforcement	Informal Enforcement	Penalty Numbers	Penalty Amount
PANEL A: Time Period (1991 - 1999)	(1)	(2)	(3)	(4)	(5)
Median $\widehat{\text{Home}}$ Value	0.093*	0.003	0.091*	0.157	0.155
	(1.76)	(0.61)	(1.67)	(1.26)	(0.94)
Salient Manmade Spills	0.133**	0.096**	0.060*	0.178	0.176
	(2.37)	(2.21)	(1.67)	(1.51)	(1.31)
Observations	41,434	41,434	41,434	41,434	41,434
County Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
State × Industry × Year FE	Yes	Yes	Yes	Yes	Yes
PANEL B: Time Period (2000 - 2011)					
Median $\widehat{\text{Home}}$ Value	0.201***	0.021**	0.256***	0.250**	0.143*
	(2.83)	(2.27)	(2.58)	(2.29)	(1.77)
Salient Manmade Spills	0.016	0.053	0.002	0.080	0.022
	(0.91)	(1.33)	(0.58)	(1.47)	(0.19)
Observations	72,885	72,885	72,885	72,885	72,885
County Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
State × Industry × Year FE	Yes	Yes	Yes	Yes	Yes
PANEL C: Time Period (2012 - 2019)					
Median $\widehat{\text{Home}}$ Value	0.083	-0.054	0.109*	0.042	0.077
	(1.17)	(-0.39)	(1.91)	(0.30)	(0.69)
Salient Manmade Spills	0.312**	0.306**	0.149	0.242**	0.293***
	(2.25)	(2.08)	(1.63)	(2.04)	(2.62)
Observations	47,457	47,457	47,457	47,457	47,457
County Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
State × Industry × Year FE	Yes	Yes	Yes	Yes	Yes

TABLE IA.2

This table presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act on contemporaneous county level variables between 1991 and 2019 across counties in the US. We replace the median housing wealth with the county level homeownership rate interacted with the interaction of local house price sensitivity to regional house price cycles from Guren et al. (2020a), and the median housing wealth at the census-region level. All regressions include State × Industry × Year fixed effects and County fixed effects. The natural logarithm of one plus the average toxic waste per plant within each industry in the county is included as a control. Other control variables include the logarithm of the per capita income and population level of the county, the unemployment rate, diversity index and a dummy variable that equals one if the county is designated as a nonattainment county. Standard errors are clustered at the county level and are robust to heteroscedasticity. T statistics are reported in brackets, and $***$, $**$, $*$ denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total Enforcement (1)	Formal Enforcement (2)	Informal Enforcement (3)	Penalty Numbers (4)	Penalty Amount (5)
Homeownership rate × Local sensitivity × Regional House Prices	0.069***	0.013	0.076***	0.078***	0.050***
	(4.32)	(0.97)	(4.95)	(4.97)	(3.38)
Log (Toxic Air Releases)	0.007***	0.005***	0.007***	0.005***	0.006***
	(11.90)	(8.52)	(9.69)	(9.09)	(9.40)
Log (Income)	-0.112***	-0.081**	-0.090**	-0.105***	-0.109***
	(-3.06)	(-2.14)	(-2.33)	(-2.83)	(-2.69)
Log (Population)	-0.025*	-0.011	-0.034**	0.001	0.005
	(-1.83)	(-0.73)	(-2.49)	(0.06)	(0.30)
Unemployment Rate	-0.013***	-0.011***	-0.007**	-0.014***	-0.011***
	(-4.79)	(-3.89)	(-2.55)	(-5.06)	(-4.04)
Diversity Index	0.063	0.039	0.057	-0.055	-0.043
	(0.68)	(0.42)	(0.59)	(-0.60)	(-0.44)
Log (Plant Workforce Size)	-0.014***	-0.016***	-0.010***	-0.018***	-0.015***
	(-4.32)	(-4.52)	(-2.72)	(-5.10)	(-4.13)
Observations	423,837	423,837	423,837	423,837	423,837
R-squared	0.336	0.305	0.260	0.300	0.234
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes
State * Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

TABLE IA.3

This table presents the estimation results of the two-stage-least-square regressions of enforcement outcomes under the Clean Air Act at the federal and state & local level, on contemporaneous county level variables, between 1991 and 2019 across counties in the US. In panel A, the dependent variable is the log of one plus the federal level enforcement outcomes at the plant-year level. In panel B, the dependent variable is the log of one plus the state & local level enforcement outcomes at the plant-year level. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. All regressions include State × Year fixed effects and County fixed effects. All regressions include State × Year and County fixed effects. Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

PANEL A

Dependent Variable	Total Federal Enforcement	Federal Formal Enforcement	Federal Informal Enforcement	Federal Penalty Numbers	Federal Penalty Amount
	(1)	(2)	(3)	(4)	(5)
Median $\widehat{\text{Home}}$ Value	0.016	0.083*	-0.094**	0.105**	0.095**
	(0.40)	(1.95)	(-2.42)	(2.28)	(1.99)
Log (Toxic Air Releases)	0.004***	0.003***	0.003***	0.003***	0.003***
	(7.06)	(5.38)	(4.84)	(5.14)	(5.40)
Log (Plant Workforce Size)	-0.000	-0.005	0.007**	-0.006	-0.005
	(-0.08)	(-1.30)	(2.24)	(-1.52)	(-1.54)
Observations	552,583	552,583	552,583	552,583	552,583
County Controls	Yes	Yes	Yes	Yes	Yes
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes
State * Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

PANEL B

Dependent Variable	Total S&L Enforcement	S&L Formal Enforcement	S&L Informal Enforcement	S&L Penalty Numbers	S&L Penalty Amount
	(1)	(2)	(3)	(4)	(5)
Median $\widehat{\text{Home}}$ Value	0.103**	0.057	0.134***	0.099**	0.104**
	(2.52)	(1.30)	(3.24)	(2.35)	(2.45)
Log (Toxic Air Releases)	0.007***	0.004***	0.006***	0.005***	0.005***
	(12.03)	(7.68)	(10.53)	(8.52)	(8.85)
Log (Plant Workforce Size)	-0.011***	-0.012***	-0.008***	-0.015***	-0.013***
	(-3.84)	(-3.85)	(-2.65)	(-5.05)	(-4.18)
Observations	552,583	552,583	552,583	552,583	552,583
County Controls	Yes	Yes	Yes	Yes	Yes
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

Table IA.4

This table reports the impact of median housing wealth on one year ahead toxic chemical releases at the plant-chemical-year level, across three separate time periods. The dependent variable is the log of one plus the total pollution/ the normalized production level for a given plant-chemical observation. All the dependent variables along with the median housing wealth are standardized each year so as to have zero mean and unit standard deviation. The natural logarithm of the plants' workforce size is denoted as the plant level control. County level control variables include the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant × Chemical and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively.

PANEL A: Time Period (1991 - 1999)						
Dependent Variable	Total Scaled Toxic Releases	Onsite Releases	Offsite Releases	Toxic Air Releases	Water Releases	Underground Releases
	(1)	(2)	(3)	(4)	(5)	(6)
Median $\widehat{\text{Home}}$ Value	-0.075***	-0.069***	-0.037***	-0.068***	-0.006	0.011*
	(-12.01)	(-11.26)	(-5.90)	(-11.45)	(-0.77)	(1.81)
Observations	1,159,285	1,159,285	1,159,285	1,159,285	1,159,285	1,159,285
PANEL B: Time Period (2000 - 2011)						
Median $\widehat{\text{Home}}$ Value	-0.124***	-0.114***	-0.061***	-0.092***	-0.039**	-0.109***
	(-8.52)	(-7.96)	(-4.00)	(-6.67)	(-2.42)	(-8.36)
Observations	1,615,935	1,615,935	1,615,935	1,615,935	1,615,935	1,615,935
PANEL C: Time Period (2012 - 2019)						
Median $\widehat{\text{Home}}$ Value	0.050	0.071**	-0.023	0.043	-0.106***	-0.028
	(1.34)	(2.04)	(-0.53)	(1.21)	(-2.76)	(-1.40)
Observations	899,399	899,399	899,399	899,399	899,399	899,399
Plant Level Controls	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Plant × Chemical FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.5

This table reports the impact of median housing wealth on one year ahead toxic chemical releases at the plant-year level. The dependent variable is the log of one plus the total pollution level (summed across all toxic chemical releases for a given plant) for a given plant-year observation. Along with the median housing wealth, we include its interaction with the social capital index of Rupasingha, Goetz & Freshwater (2006). Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ∗∗∗, ∗∗, ∗ denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total Toxic Releases	Onsite Releases	Offsite Releases	Toxic Air Releases	CAP Releases	CO2 GHGRP
	(1)	(2)	(3)	(4)	(5)	(6)
Median $\widehat{\text{Home}}$ Value	-0.067***	-0.048**	-0.019	-0.040*	-0.116***	0.017
	(-2.97)	(-2.23)	(-1.06)	(-1.86)	(-3.09)	(0.58)
Median $\widehat{\text{Home}}$ Value ×	-0.018**	-0.005	-0.021**	-0.000	-0.031**	-0.026**
Social Capital Index	(-2.02)	(-0.64)	(-2.03)	(-0.03)	(-2.05)	(-2.08)
Log (Income)	0.005	0.026	0.008	0.032*	0.007	0.060***
	(0.28)	(1.41)	(0.37)	(1.72)	(0.26)	(4.05)
Log (Population)	-0.005	-0.016**	-0.001	-0.021***	0.017*	-0.010***
	(-0.72)	(-2.37)	(-0.13)	(-3.14)	(1.76)	(-3.68)
Unemployment Rate	-0.008***	-0.011***	0.000	-0.010***	-0.001	-0.001
	(-6.09)	(-8.63)	(0.02)	(-8.17)	(-0.72)	(-0.99)
Diversity Index	-0.156***	-0.176***	-0.064	-0.123**	-0.018	0.139***
	(-2.97)	(-3.55)	(-1.10)	(-2.47)	(-0.21)	(4.09)
Log (Plant Workforce Size)	0.025***	0.026***	0.014***	0.027***	0.007**	0.005**
	(15.38)	(16.47)	(7.49)	(16.52)	(2.11)	(2.02)
Observations	525,703	525,703	525,703	525,703	202,334	165,259
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.6

This table reports the impact of median housing wealth on one year ahead toxic chemical releases at the plant-year level. The dependent variable is the log of one plus the total pollution level (summed across all toxic chemical releases for a given plant) for a given plant-year observation. Along with the median housing wealth, we include its interaction with the party of the governor. Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and $***$, $**$, $*$ denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Total Toxic Releases (1)	Onsite Releases (2)	Offsite Releases (3)	Total Toxic Air Releases (4)	CAP Releases (5)	CO2 GHGRP (6)
Median $\widehat{\text{Home}}$ Value	-0.050**	-0.046**	-0.034***	-0.040*	-0.074*	0.049*
	(-2.16)	(-2.06)	(2.65)	(-1.78)	(-1.81)	(1.67)
Median $\widehat{\text{Home}}$ Value × Party of the Governor	-0.004	0.011	-0.017	0.010	-0.048***	-0.018
	(-0.35)	(0.98)	(-1.32)	(0.95)	(-2.63)	(-1.63)
Log (Income)	0.007	0.028	-0.003	0.030	0.037	0.069***
	(0.37)	(1.50)	(-0.14)	(1.57)	(1.35)	(4.42)
Log (Population)	-0.007	-0.015**	-0.005	-0.022***	0.018	-0.010***
	(-0.93)	(-2.09)	(-0.63)	(-3.03)	(1.64)	(-3.13)
Unemployment Rate	-0.009***	-0.011***	-0.000	-0.011***	-0.001	-0.002*
	(-6.65)	(-9.30)	(-0.32)	(-8.79)	(-0.33)	(-1.85)
Diversity Index	-0.150***	-0.147***	-0.111*	-0.090*	-0.055	0.120***
	(-2.79)	(-2.87)	(-1.86)	(-1.73)	(-0.59)	(3.38)
Log (Plant Workforce Size)	0.025***	0.026***	0.014***	0.027***	0.007*	0.003
	(15.06)	(16.12)	(7.20)	(16.33)	(1.93)	(1.53)
Observations	525,703	525,703	525,703	525,703	202,334	165,259
Plant Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.7

This table reports the impact of median housing wealth on plants' one year ahead total waste generation (at the chemical level), and waste management through recycling, energy recovery, treatment, and releases, across three separate time periods. In column 1, the dependent variable is the amount of total waste generated at the plant-chemical level. The dependent variables in columns 2, 3, 4 and 5 measure the percentage of toxic waste processed by plants through recycling, energy recovery, treatment, and releases. Control variables include the natural logarithm of the plants' workforce size, the logarithm of the per capita income and population level of the county, the unemployment rate and the diversity index of the county. All regressions include Plant and State × Year fixed effects and are estimated using two-stage-least-square regressions (2SLS). Standard errors are clustered at the plant level and are robust to heteroscedasticity. T statistics are reported in brackets, and ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. Data Source: - EPA's Pollution Prevention (P2) Databases

Dependent Variable	Total Waste Generated	% Released	% Recovered	% Recycled	% Treated
	(1)	(2)	(3)	(4)	(5)
PANEL A: Time Period (1991 - 1999)					
Median $\widehat{\text{Home}}$ Value	-0.018***	-1.143***	0.369***	0.170	0.356*
	(-2.66)	(-3.78)	(3.64)	(1.08)	(1.88)
Observations	1,151,298	1,151,298	1,151,298	1,151,298	1,151,298
PANEL B: Time Period (2000 - 2011)					
Median $\widehat{\text{Home}}$ Value	-0.149***	-4.233***	0.032	2.441***	1.661***
	(-10.30)	(-6.39)	(0.13)	(6.09)	(4.04)
Observations	1,612,926	1,612,926	1,612,926	1,612,926	1,612,926
PANEL C: Time Period (2012 - 2019)					
Median $\widehat{\text{Home}}$ Value	-0.134***	-4.433**	0.251	3.949***	0.003
	(-3.58)	(-2.48)	(0.41)	(3.24)	(0.00)
Observations	898,797	898,797	898,797	898,797	898,797
Plant Level Controls	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes
Plant-Chemical Fixed Effects	Yes	Yes	Yes	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes