The Economic Burden of Pension Shortfalls: Evidence from House Prices*

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Abstract

U.S. state pensions are underfunded by trillions of dollars, but their economic burden is unclear. In a model of inefficient taxation, real estate fully reflects the cost of these shortfalls when it is the only form of immobile capital. Thus, we study the effect of pension shortfalls on real estate values at state borders, where labor and physical capital can easily relocate to a state with a smaller shortfall. Using plausibly exogenous variation driven by pension asset returns, we find that one dollar of pension underfunding reduces house prices near state borders by approximately two dollars. Controlling for county-level rental rates as a proxy for current housing consumption does not affect our estimates, which suggests that house prices are affected by future costs rather than the current quality of public services. Our estimates imply a deadweight loss associated with addressing pension shortfalls that is consistent with prior research in settings with high returns to public spending and costs of taxation.

Keywords: Public finance, pensions, deadweight loss, real estate.

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"Moody's Investors Service estimates state and local pensions have unfunded liabilities of about $4 trillion, roughly equal to the economy of Germany, the world's fourth-largest economy."
— The Wall Street Journal, July 30, 2018[1]

1 Introduction

Underfunded public pensions in the U.S. represent a net implicit household liability larger than auto loans, student debt, and credit card balances combined.[2] However, little is known about the economic burden imposed by these shortfalls despite awareness and concerns about them.[3] Their temporally distant and uncertain nature makes it challenging to identify the implied cost of pension shortfalls. Moreover, without theoretical guidance, the appropriate empirical design to estimate the economic burden of pension underfunding is unclear.

A model of inefficient taxation motivates our empirical strategy to identify the economic burden of pension shortfalls. In an open economy, where capital and labor are mobile but real estate is not, house prices reflect the total cost of pension shortfalls including any inefficiencies or dead-weight loss generated in honoring the obligations. Conversely, if all forms of capital face a high cost of relocation, then the burden is unclear and the price on any individual asset is unlikely to reflect the true implied cost. Motivated by this insight, we focus our analysis on locations near state borders where immobile factors, such as real estate, should bear the burden and thus reflect the implied cost of pension shortfalls (e.g., Oates, 1969; Bradford, 1978; Kotlikoff and Summers,

[1]"The Pension Hole for U.S. Cities and States is the Size of Germany's Economy", available from: https://www.wsj.com/articles/the-pension-hole-for-u-s-cities-and-states-is-the-size-of-japans-economy-1532972501.

[2]The state and local pension systems in the U.S. reported $1.378 trillion in unfunded liabilities in FY 2015, but, according to Rauh (2016), using the Treasury curve to discount instead, the accumulated deficit is $3.846 trillion. According to the Federal Reserve Bank of New York, as of the fourth quarter of 2020, outstanding amounts of student loan, auto loan, and credit card debt are $1.55, $1.37, and $0.82 trillion, respectively, totaling to $3.74 trillion.

[3]Figure 1 shows a correlation of 0.72 between state pension underfunding and state-level Google search intensity for "public pension" and "pension crisis".

1987; Harberger, 1995). Since the level of pension underfunding may be affected by local economic conditions and the endogenous response to past fund contributions and investment decisions, we use plausibly exogenous variation in pension asset returns or "windfalls" to identify the causal effect of pension shortfalls on house prices.

Our main findings illustrate a consistent pattern: changes in pension funding pass-through to house prices in settings where they reflect the economic burden of shortfalls. We estimate that the marginal home buyer is willing to pay as much as two dollars for each dollar of additional pension funding per property. This house price pass-through is comparable to the estimated impact of public spending on infrastructure and school salaries (e.g., Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020). Our theoretical framework shows that this pass-through can be interpreted as a tax multiplier with respect to the funding of future liabilities – for each additional dollar that states will have to raise through future taxes or amenity reductions, households perceive an economic burden of between one and two dollars. Controlling for rental prices does not meaningfully change our pass-through estimates, which suggests that pension shortfalls affect house prices through the capitalization of future costs and not through the current quality of amenities.

We face two major challenges in our empirical analysis. First, where businesses and individuals cannot easily avoid the future taxation associated with pension shortfalls, the burden of taxes will be spread across all assets (i.e., human and physical capital as well as real estate). Thus, we conduct our analysis at state borders, where businesses and individuals face a low cost of relocating to a state with a smaller pension shortfall.[4] This allows us to measure the economic cost

[4] According to Rauh (2016), state pension plans account for $4.05 trillion out of $4.80 trillion (84%) of total pension liabilities, so our analysis captures most of the U.S. public pension burden. City and county borders might also be natural sub-setting, but while data exist on large municipalities, comprehensive data spanning both sides of municipal boundaries are not available. Therefore, such data are better suited for estimated the general effect of pension shortfalls on house prices (e.g., Howard, 2020) rather than the implied economic burden, which is the focus of this paper.

of pension shortfalls in areas where landowners are expected to bear the brunt of any changes to taxes and the provision of government services.

This focus on state borders is one of the key features that distinguishes our paper from earlier studies on the relation between pension funding and house prices in individual cities or states (e.g., Epple and Schipper, 1981; Leeds, 1985; Hur, 2008; Albrecht, 2012; MacKay, 2014; Stadelmann and Eichenberger, 2014). These studies do not focus on border areas where real estate is the only immobile asset, so their estimates do not reflect the full economic cost of pension shortfalls. Thus, we answer a fundamentally different question from these earlier papers, using housing markets as a laboratory to measure an economic primitive rather than as the outcome of interest.[5]

Second, pension underfunding is likely to be correlated with omitted variables. If shortfalls were accrued in efforts to improve amenities for state residents, then states with high shortfalls would provide better services than states with low shortfalls. Alternatively, if high pensions are the result of overinvestment in poorly performing projects, shortfalls may be associated with worse quality of life. Our question requires exogenous variation to identify the causal effect of shortfalls, so we focus on the effect of pension asset returns on house prices. We refine this approach to account for potential "home bias" or "familiarity bias" in pension investments by restricting attention to benchmark returns that depend only on broad asset allocations or unexpected excess returns over these benchmarks.

In our baseline analysis, we compare the pension asset returns in the early part of our sample (2002–2014) with home prices thereafter for properties in county clusters across state borders. We find that increases in raw returns, excess returns, and implied returns from asset allocations are all associated with increased house prices. To quantify the effect of pension shortfalls, we cal-

[5]In fact, in a sub-analysis when we rerun our properties in the interior of the state, we find disperse and inconsistent estimates, which is exactly what we would expect. In such settings the relative burden should be split among a broad set of actors and assets in a way that depends on their relative mobility and elasticises and is likely to vary across regions.

culate cumulative dollar pension returns based on 2001 pension assets and find a pass-through of approximately two—for each dollar of pension asset returns, house prices increase by approximately two dollars. Non-parametric analysis of the border discontinuity reveals a clear increase in prices when moving from a low-return state to a high-return state.

Our estimates are robust to a battery of alternative specifications. First, if we consider asset returns between 2002 and a property's sale year instead of using the same return horizon for all houses and we find a similar pass-through of approximately 2. The benefit of this approach is that it then allows us to include property fixed effects among properties with repeat sales. This alleviates the concern that our findings could be driven by time-invariant factors at the state, local, or property level. Focusing on the sub-sample of repeat sales provides a slightly lower pass-through. This is not surprising since less time has passed between transactions, and prior work has shown it can take several years for even things like public spending on schools to be fully realized in house prices (e.g., Bayer, Blair, and Whaley, 2020). More importantly, the inclusion of property fixed effects, after conditioning on the repeat sales sample, has little effect on the overall pass-through, suggesting time-invariant factors play little role in our observed findings. For example, this suggests that unobservable time-invariant differences in states or property characteristics across treated areas cannot explain our results.

To shed light on the economic channels driving our results, we turn our attention to the quality of government amenities and find that higher state-level pension shortfalls correlate with less educational spending and poorer road quality. This raises a question: do our estimates reflect worse current amenities in places with low pension asset returns, or an expectation of future costs that are capitalized in housing prices? To address this, we add time-varying local rental prices to the set of control variables and find similar results. While reaffirming that our results are not endogenously related to economic conditions, this last test also narrows the interpretation of our pass-through estimates. If pension asset returns impact property prices through changes in the current amenity set, rental properties should also benefit from positive asset shocks. Moreover, we find little to no direct effect of pension windfalls on rental prices, suggesting the house price pass-through reflects the capitalization of future changes in taxes and the quality of government services.[6]

This paper contributes to the literature on the real effects of public finance. An emerging segment of this literature focuses on the condition of state and local pensions in the United States. Earlier work in this area has focused on the measurement of the pension underfunding (Brown and Wilcox, 2009; Novy-Marx and Rauh, 2011; Novy-Marx and Rauh, 2014), the political economy of pension funding (Brinkman, Coen-Pirani, and Sieg, 2018; Myers, 2020), the impact of pension funding on municipal borrowing costs (Novy-Marx and Rauh, 2012; Boyer, 2020), the precautionary savings of households in response to pension deficits (Zhang, 2021), and the economic recovery after the financial crisis (Shoag, 2013). We complement this work by estimating the effect of pension shortfalls on house prices near state borders to quantify the current economic impact of this future burden.

Our theoretical model highlights the analogy between our results and earlier studies of tax burden. In equilibrium, the effect of an exogenous increase in pension assets is equal to the present value of the tax multiplier, which means the pass-through of pension shortfalls to house prices is comparable to other estimates of the economic burden of raising taxes. Consistent with our estimates, Cellini, Ferreira, and Rothstein (2010) and Bayer, Blair, and Whaley (2020) find

[6]An implication of this finding is that marginal home buyers are aware of the condition of local government finances to the extent that they anticipate future tax hikes or reduced service provision. Since prices are based on common signals, such as comparable recent property transactions, this does not require that most households are aware, but just that marginal ones are. While we can't provide direct evidence on the signal transmission, under-provision of maintenance (e.g., poor roads) might provide little disamenity in the present, but provide an indication to marginal buyers about future local conditions. As we suggest in our analysis of Google search trends and pension funding, news coverage might also be an even more straight forward explanation for information transmission among marginal buyers.

economic costs between one and two dollars per dollar of tax revenue in the context of school spending.

The remainder of the paper is organized as follows. Section 2 presents a model of tax burden to motivate our empirical analysis. Section 3 describes our data on public pension funding and house transaction prices. Section 4 explains our identification strategy. Section 5 presents the main results. Section 6 concludes.

2 Theoretical Framework

In this section, we first show that in an open economy, landowners of a state within a country are likely to bear the burden of a tax levied on a domestically mobile factor, motivating our empirical design. We then show that the pass-through of pension shortfalls to house prices is theoretically ambiguous, and therefore an empirical question that reflects inefficiency in the public provision of goods and capital raising.

2.1 Tax burden in an open economy

Harberger (1962) argues that in a closed economy, the burden of the corporate income tax tends to fall entirely on physical capital. Importantly, in a closed economy, untaxed factors always bear some burden of the tax if the taxed factor's supply (demand) is not perfectly inelastic (elastic).[7] Relaxing the closed economy assumption, most studies argue that in an open economy, immobile factors bear most, if not all, of the long-run burden of the tax in the economy due to capital mobility across borders.[8] Thus, it is critical for our empirical design to focus on an

[7] In Appendix A.1, we presents a simple closed-economy framework to illustrate this point.

[8] Notable examples include Bradford (1978), Kotlikoff and Summers (1987), Mutti and Grubert (1985), Harberger (1995), and Gravelle and Smetters (2001). See Gravelle (2013) for a review of tax burden in general equilibrium.

open-economy setting at state borders to measure the burden of pension shortfalls.

In Appendix A.2, we provide a simple framework based on Kotlikoff and Summers (1987) to illustrate this point. There are two factors of production for the single good in the economy: capital and land. Following Harberger (1962), we assume perfect competition and a fixed national capital stock that is perfectly mobile within the country. For simplicity, we assume that the factor complementary to capital, here labeled land, is supplied inelastically and is immobile. Since capital is mobile, rental rates on capital need to be equalized across states: a tax on capital imposed by one state on income earned by capital in that state is not fully borne by the capital initially in the state imposing the tax. In contrast, landowners in the two states are differentially impacted: there is a loss of rental income in the state imposing the tax on capital and a gain in the other. We summarize the main takeaway of the open economy model in Remark 1.

Remark 1. *In an open economy, the immobile factor in a state is likely to bear a significant portion of the burden of a tax it levies on a domestically mobile factor.*

2.2 Pension shortfalls, tax distortions, and property values

The previous section establishes that an open economy is the appropriate setting for our empirical analysis. In this section, we study capitalization of future pension liabilities in current house prices. Whereas the previous section focused on capital mobility and the elasticity of demand, this section introduces a role for asset prices. The economic burden of a tax is affected by changes in asset prices due to the discounted present values of future tax and public expenditure changes. We argue that the magnitude of the marginal decrease in house prices from an additional dollar of pension shortfalls is theoretically ambiguous and therefore an empirical question.

The model presented here is based on a slight modification of the asset-market approach of tax burden presented in Poterba (1984). The key component of the burden is the price change for existing owner-occupied homes due to the change in the present discounted value of taxes associated with the asset. The stock of houses is assumed to be fixed in the short run, so the equilibrium rental rate equates the demanded quantity with the existing housing services flow. Denote the market clearing rental rate by $R(H)$ with $R' < 0$, where R is the inverse demand function for housing services.

Households consume housing services until the marginal value of these services equals their cost. We assume all houses face a constant depreciation rate δ per period and maintenance costs equal to fraction κ of current value. Houses incur property tax liabilities at a rate μ. All households face a marginal tax rate τ, and may deduct property taxes from taxable income and borrow and lend at a nominal interest rate r. The investor cost also includes any capital gain or loss of holding the asset. Let $q_{H,t}$ be the house price at the start of period t. So $(q_{H,t+1} - q_{H,t})$ represents the capital gain or loss during period t. In equilibrium, homeowners equalize the marginal cost and marginal benefit of housing services:

$$R(H_t) = \eta\, q_{H,t} - (q_{H,t+1} - q_{H,t})\,, \tag{1}$$

where $\eta \equiv \delta + \kappa + (1 - \tau)(r + \mu)$.

Consider a tax on each house that takes the form of a payment T_t to cover the unfunded pension liabilities L_t in period t. We assume taxes induce a deadweight loss,

$$T_t = (1 + \alpha)L_t, \tag{2}$$

where $\alpha > 0$. This means that to fund each additional dollar of pension liability in period t, the state has to raise more than one dollar in taxes. Parameter α is meant to capture the cost of raising revenues that we later estimate empirically.

Because houses are durable assets, future taxes can still depress prices today. In each period when the tax is levied, the equilibrium condition (1) becomes

$$R(H_t) - T_t = \eta\, q_{H,t} - (q_{H,t+1} - q_{H,t})\,. \tag{3}$$

Since $q_{H,t+1}$ is unknown at time t, we can solve the price $q_{H,t}$ forward by rewriting (3) as

$$q_{H,t} = \frac{R(H_t) - T_t + q_{H,t+1}}{1+\eta}. \tag{4}$$

Iterating Equation (4) forward, applying the no-bubble condition,[9] and the distortionary tax assumption in (2) gives

$$q_{H,t} = \sum_{j=0}^{\infty} \frac{R(H_{t+j})}{(1+\eta)^{j+1}} - \sum_{j=0}^{\infty} \frac{(1+\alpha)L_{t+j}}{(1+\eta)^{j+1}}. \tag{5}$$

The second term in Equation (5) is the present discounted value of current and future tax payments to cover pension liabilities. It is clear from (5) that if two states face the same housing demand curves but have different levels of liabilities L, all else equal, the one with a higher present discounted value of pension obligations will have lower house prices today.

If the stock of housing is fixed,[10] i.e., $H_{t+j} = H_t$ for all j, then from Equation (5), we can determine the impact of unfunded liability j periods ahead on house prices today:

$$\frac{dq_{H,t}}{dL_{t+j}} = -\frac{1+\alpha}{(1+\eta)^{j+1}} < 0. \tag{6}$$

With reasonable parameter values for income and property tax rates, depreciation, and main-

[9]The transversality (no-bubble) condition in our setting is $\lim_{J\to\infty} \frac{q_{H,t+J}}{(1+\eta)^{J+1}} = 0$, which rules out exploding house prices. This condition is consistent with Giglio, Maggiori, and Stroebel (2016) who fond no evidence of violations of the transversality condition in housing market in the U.K. and Singapore even during periods when housing bubbles were thought to be present.

[10]With an endogenous housing stock, changes in future taxes induced by future pension liabilities will also affect current and future investment in housing construction and the stock of housing $\{H_{t+j}\}_{j=0}^{\infty}$. In general, the effect of changing housing stock can mitigate the effect of taxes on house prices. See Poterba (1984) for more details.

tenance costs, the capitalization of future pension liabilities in house prices today can have a magnitude of less or greater than one. It depends on how large the distortion α is and how far in the future the tax is imposed. We summarize the main message in Remark 2.

Remark 2. *The magnitude of the marginal decrease in house prices from an additional dollar of pension shortfalls is ambiguous; it can be smaller or larger than one.*

3 Data

3.1 State and local public pension plans database

We obtain accounting and actuarial data for state and local pension plans from the Public Plans Database (PPD) from the Center for Retirement Research at Boston College. PPD contains annual plan-level data from 2001 through 2019 for 190 pension plans: 114 administered at a state level and 76 administered locally. This sample covers 95% of public pension membership and assets nationwide.[11] The PPD is updated each spring from data available in the most recent Comprehensive Annual Financial Reports (CAFRs) and Actuarial Valuations (AVs). Intermediate updates may occur when new variables are added or data errors are corrected.

We use the PPD data to calculate plan-level pension shortfall defined as actuarial accrued liabilities less market assets. Actuarial accrued liabilities, measured under traditional Governmental Accounting Standards Board (GASB) 25 standards, are equal to the present value of future benefits, discounted using the plan's assumed long term investment return.[12] We also obtain the

[11]The PPD sample is carried over from the Public Fund Survey (PFS), which was constructed with an emphasis on the largest state-administered plans in each state, but also includes some large local plans such as New York City ERS and Chicago Teachers. See https://publicplansdata.org/ for more details.

[12]Recent GASB statements, known as GASB 67, require new disclosures by public pension systems that shed additional light on the extent of these promises and the rate at which they are growing. See Rauh (2016) for more details.

long-term investment return assumption for public pension plans used to discount actuarial liabilities.

3.2 Detailed investment data by asset class

Each plan also reports detailed annual data on specific asset classes it invests in and their associated benchmarks as well as returns for each asset class and benchmark. The major asset classes presented in the PPD are generated from the specific asset classes that plans report. We use these data to calculate the cumulative pension plan return used as an instrument for pension shortfall.[13]

Descriptive statistics on the PPD data are contained in Table 1. On average across time and funds, the largest asset holdings were equities and fixed income (53% and 28% of total assets, respectively), followed by private equity and real estate (5% each). The value of assets is only 79% of the actuarial value of liabilities for the mean observation, indicative of substantial underfunding.[14]

As discussed in Novy-Marx and Rauh (2011) and Rauh (2016), the appropriate discount rate for valuing pension liabilities is the yield on a zero-coupon Treasury bond with the same duration. To discount pension liabilities at the appropriate Treasury rates, we need to calculate plan duration and convexity. Under new GASB 67 guidelines, plans are required to disclose their total pension liabilities (TPL) under alternative scenarios of the discount rate being 100 bps higher ($TPL_{r+1\%}$) and 100 bps lower ($TPL_{r-1\%}$). However, this information is only available starting in fiscal year 2014, when GASB 67 became effective. To adjust the liability discount rate in our full sample, we use the aggregate adjustment factor in Rauh (2016) and inflate unfunded liabilities by a constant

[13]These data have been used recently in the asset pricing literature (e.g., Lu, Pritsker, Zlate, Anadu, and Bohn, 2019).

[14]The average ratio of pension assets to liabilities, the funded ratio, declined from 100% in 2001 to 76.3% in 2019.

factor of 2.86.[15]

3.3 Zillow transaction and assessment database

We obtain property-level data from the real estate assessor and transaction datasets in the Zillow Transaction and Assessment Dataset (ZTRAX). ZTRAX is, to the best of our knowledge, the largest national real estate database with information on more than 374 million detailed public records across 2,750 U.S. counties. It also includes detailed assessor data including property characteristics, geographic information, and valuations on over 200 million parcels in over 3,100 counties. These data have been used by Bernstein, Gustafson, and Lewis (2018), among others.

We filter the Zillow data in four ways. First, we retain only transactions of residential properties for which the price of the transaction is verified by the closing documents as being between $30,000 and $2,000,000. Given that Zillow obtains prices from a variety of third-party sources and anecdotal evidence suggests that these prices are occasionally incorrect, this filter improves the quality of our data. Second, we only keep transactions in bordering counties across neighboring states. Third, we focus only on single-family residences. Finally, in our primary empirical analysis we restrict attention to properties in a border county and within 50 miles of a state border.

4 Empirical Methodology

The objective of this paper is to estimate the economic burden of temporally distant and uncertain public liabilities. We focus on state pension shortfalls because of the growing concern

[15]In FY 2014, in aggregate, the state and local pension systems in the United States reported unfunded pension liabilities of $1.19 trillion under GASB 67. Rauh (2016) applies a correction on a plan-by-plan basis that results in unfunded accumulated benefits of $3.41 trillion under Treasury yield discounting. This means an average adjustment factor of 3.412/1.191 = 2.864. Any error in this adjustment could affect our analysis of pension shortfalls, but not the analysis that exploits variation in pension asset returns.

about their magnitude (Rauh, 2016) and their impact on real estate because according to the theory, it should reflect the perceived economic burden of such shortfalls.

First, property values provide a parsimonious and direct measure of the perceived discounted present value of all future costs/benefits for homeowners. Unlike many assets, long-run discount rates in housing tend to be quite low, increasing the plausibility that temporally distant costs could significantly impact current prices (Giglio, Maggiori, and Stroebel, 2014). Also, for many households their home is the largest financial investment, and prices are likely to reflect perceptions when the stakes are highest.

Second, real estate is effectively immobile. As detailed in Section 2, in settings where other capital, consumers, and labor can easily move, such as near state borders, property bears the full economic burden of the current perceived burden of these shortfalls. While prior studies have looked at the correlation between pension shortfalls and house prices (e.g., Leeds, 1985; Hur, 2008; Albrecht, 2012; MacKay, 2014; Stadelmann and Eichenberger, 2014; Brinkman, Coen-Pirani, and Sieg, 2018), none focus on border regions. We argue that this is critical for properly measuring the burden. In addition, these earlier studies suffer from endogeneity problems in the determinants of shortfalls, which preclude a causal interpretation.

Therefore, in our analysis we investigate how variation in net pension liabilities per capita, all else equal, translates into variation in property values in regions near state borders. Consider the following border discontinuity design (BDD) regression:

$$PropertyValue_{it} = \beta\, PensionShortfallPerProperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \delta_{it}\boldsymbol{X_{lt}} + \epsilon_{it}, \tag{7}$$

where $PropertyValue$ is the transaction price of house i and $PensionShortfallPerProperty$ is the estimated pension shortfall per property in state s, in thousands of dollars, in year t. γ are county-border-pairs interacted with time fixed effects that allow us to compare properties transacting in physically adjacent regions in the same period, just across the state border from each other. This approximates the empirical design suggested by our theoretical framework for an open economy. D is a distance-to-state-border measure from the centroid of a given property. If the pension burden is reflected in property values, we would expect prices to jump suddenly at the state border, when shortfalls also jump, even after the inclusion of this distance control. λ are location fixed effects that capture time-invariant differences by region and region interacted with property characteristics. Therefore, we obtain identification not only from cross-sectional differences across state borders, but from variation in state pension funding status and house prices over time in a border county relative to an adjacent county across the border. These include zip code, zip code interacted with property characteristics, and property fixed effects. For the property fixed effects specification, we can also exploit the subset of properties that transact multiple times over the sample period. Finally, $\boldsymbol{X}$ is a set of time-varying continuous economic controls at the state-year or county-year level.

Figure A.1 illustrates the counties involved in the discontinuity design along with the average shortfall throughout the sample. Our analysis requires sufficient population density to have contemporaneous transactions on either side of the border among comparable property types. As such, we are limited mostly to the East Coast and Midwest regions.

As motivated by theory, because of its focus on border regions the BDD on shortfalls is already an improvement over existing work, but still faces similar endogeneity concerns to those present in the prior literature. Suppose a state chose to increase local spending on public services instead of funding its pension plans. These sorts of expenditures have been shown to raise property values (e.g., Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020) and would mechanically increase net pension liabilities per capita. In this case, the estimated pass-through between shortfalls and house prices would understate the economic burden borne by households and could even have the wrong sign. Conversely, if shortfalls are the result of poorly performing expenditures that have negative economic consequences for the state, then the estimated burden may be biased upward.

An ideal empirical setting supplies exogenous, as good as random, changes in pension shortfalls, allowing us to compare real estate transactions before and after the shocks. We therefore focus our analysis on pension asset performance. Variation in pension portfolio returns causes immediate changes in net pension liabilities driven by factors that are plausibly exogenous to state expenditures. We thus implement the same empirical design as Equation (7), but substitute pension shortfalls with asset performance "windfalls":

$$PropertyValue_{it} = \beta\, PensionWindfallPerProperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \delta_{it} \boldsymbol{X_{lt}} + \epsilon_{it}, \tag{8}$$

where $PensionWindfallPerProperty$ are the compounded cumulative returns for the pension plans of state s from the beginning of the sample, 2002, to the transaction date or period of interest t (as discussed in more detail later in Section 5.1) multiplied by the assets per property in that state at the beginning of the sample. This can be interpreted as the additional pension assets available per property caused by performance of that state's investment portfolio relative to other states over that period. This provides an economic meaning consistent with the pass-through discussed in our theoretical motivation. β recovers an estimate of the economic burden, which is equal to one plus the deadweight loss, α, from our theoretical model in Equation (2). This is because a one dollar lower windfall per property in a given state implies a one dollar higher net pension shortfall per property.

It is important to note that this inverse relationship between windfalls and shortfalls must be true by definition instantaneously. One dollar more of assets, by definition reduces the net pension shortfall by one dollar. On the other hand, in equilibrium the endogenous choice of funding by the state in response to an exogenous dollar of additional windfall depends on whether it reduces pension contributions in response. This sort of "crowding out" between windfalls and contributions, would lead equilibrium observed shortfalls to fall by less than a dollar for each dollar of windfalls, since the state responds by contributing less than they would have otherwise to the pension fund. While it does not recover our primary economic primitive of interest, we can learn something interesting about crowding out and the importance of our empirical design by considering windfalls as an instrumental variable for observed equilibrium shortfalls within the following two-stage least squares (2SLS) regression:

$$\begin{aligned} PropertyValue_{it} = \theta\,(ShortfallPerProperty_{it} = \phi\, WindfallPerProperty_{st}) + \gamma_{bt} \\ + \omega D_i + \lambda_l + \delta_{it}\boldsymbol{X_{lt}} + \epsilon_{it}. \end{aligned} \tag{9}$$

We can then compare θ to our estimates from Equation (7) and an OLS regression without using windfalls as an instrumental variable. Since $1 - \phi$ is the crowding out per dollar of windfall, so is $1 - \beta/\theta$. In other words, if the true economic burden were 2 and the crowd-out was about 33 cents per dollar of windfall, then we would expect to obtain an estimate for θ of around 3. This is because, for each dollar of exogenous windfall received, the state would spend some of it and the net shortfall observed in equilibrium would only decrease by 66 cents, and yet residents could still benefit from the incremental 33 cents spent.[16] This approach therefore highlights the importance in our empirical design of examining windfalls, rather than observed shortfalls, when trying to

[16] In an extreme example, a state could reduce pension funding contributions by 99 cents for each dollar of pension asset performance. In that case, even if each marginal dollar raised property values by one dollar, so the true economic burden was 1, the estimated effects in this empirical design would be 100, since it would appear that 1 cent less of observed equilibrium shortfall raised values by one dollar.

recover an estimate of the economic burden.[17]

We also consider 2SLS designs that work to recover our primary economic primitive of interest, the economic burden of pension underfunding, while alleviating potential remaining identification concerns. While our focus on asset returns in border counties reduces many concerns about endogeneity, it is still possible that investor home or familiarity bias could cause problems (Hochberg and Rauh, 2013). These biases may mechanically relate pension returns to state level economic conditions in two ways. First, pension managers may buy shares in local firms so that when the local economy does well, both the pension portfolio and home prices go up (home bias). Second, pension managers may over-allocate to industries or asset classes that are relatively abundant in a state, inducing correlation between those industries, local economic conditions, and pension returns (familiarity bias). Conversely, pension funds may be used to hedge the innate risk of a state, thus resulting in a negative correlation between state economic activity and returns. For example, Texas-based managers with home bias (hedging concerns) might overweight (underweight) both Texan firms and energy-related assets generally.

To alleviate these concerns, we run the following 2SLS regression:

$$\begin{aligned} PropertyValue_{it} &= \beta\,(WindfallPerProperty_{it} = \kappa\,ExWindfallPerProperty_{st}) + \gamma_{bt} \\ &\quad + \omega D_i + \lambda_l + \delta_{it}\boldsymbol{X_{lt}} + \epsilon_{it}, \end{aligned} \tag{10}$$

where $ExWindfallPerProperty$ are variables that exploit alternative plausibly exogenous variation in pension asset performance as instrumental variables for $WindfallPerProperty$ as the endogenous variable of interest. First, instead of raw pension returns, we consider an instrumental

[17] Another benefit of this approach, is that we can easily compare θ with the estimate obtained regressing property values on shortfalls per property within the same design, but without an instrumental variable. This again helps to reveal benefits of our research design, since any difference between those reflects biases due to omitted variables or reverse causality driven by the time-varying funding decisions of those states.

variable which calculates windfalls per property based on returns in excess of listed benchmarks. Doing so addresses the familiarity bias concern that the asset category composition of the pension portfolio drives our results but leaves open the possibility of home bias where outperformance of local firms drives excess pension returns and provides spoils for the entire state. To address concerns of home bias we focus on the returns of benchmark assets. To address both simultaneously, we employ a novel technique and multiply the broad category allocations (e.g., "equities" or "bonds") in each state by the relevant benchmark returns from all pensions in the country. Here, the returns should be both unrelated to local firms and uncorrelated with the state's organizational composition. Finally, since windfalls based on excess returns vs. those based on benchmark returns rely on differing sources of variation, and different fundamental assumptions, we can include both instrumental variables simultaneously. The presence of two instrumental variables exploiting differing sources of variation for just one endogenous variable, allows us to run an overidentification test, to jointly test the exclusion restriction of both simultaneously.

5 Results

5.1 Pension windfalls and property values near state borders

In this section we exploit variation in pension funding coming from windfalls caused by the realized performance of invested pension assets. All else equal, an additional dollar per property in pension asset value due to invested portfolio outperformance causes a dollar reduction in shortfalls per property. Unlike the direct observation of shortfalls, this measure does not depend on whether outperformance causes a change in valuable (or not) expenditures, overcoming the endogeneity problem discussed previously.

Our analysis follows the baseline regression in Equation (8). In particular, we include border group by year fixed effects, allowing us to compare the property value at sale of houses in adjacent counties, but in states with differing pension windfalls. In addition to allowing us to examine just those settings where our theoretical framework suggests the economic burden should be reflected in property values, it also allows us to control for time-varying local economic conditions by including controls for annual state-level income per capita. To take advantage of the BDD we include controls for distance to the border and to avoid concerns about systematic changes in property type we include flexible controls for property characteristics. Within this framework, we begin by using cross-sectional variation in pension asset performance over most of the sample period. In particular, we compare home values in states whose pension assets have outperformed benchmarks over a long horizon to those that have not. We focus on this specification for two reasons. First, unless homebuyers are perfectly rational and pay close attention to the evolution of pension assets, small, short-term variation in asset values is unlikely to impact home prices—Bayer, Blair, and Whaley, 2020 find it can take several years for property values to reflect the underprovision of educational public goods. Second, to the extent that observable degradation or improvement in public amenities operates as a signal about the asset position of the state, these effects would likely accumulate over long periods of time.

Of course all of this requires at least some awareness by marginal buyers of pension funding. It is only necessary that a subset of residents be aware of pension underfunding for it to have an impact on the housing market equilibrium. Nevertheless, Figure 1 presents Google Trends data showing that internet search volume related to public pensions is higher in states with higher pension shortfalls. In particular, there a correlation of 72% between state level pension shortfalls per household and Google search activity for "pension crisis" and "public pension". States like Illinois, Kentucky, and New Jersey have some of the worst-funded pensions and the most local interest in this issue. This suggests that homeowners are likely aware of the financial problems plaguing their state governments, especially in states with the largest shortfalls.

Given this, we formally test how such concerns are reflected in property values in Table 3. We estimate a BDD that compares house values in adjacent regions just across state borders with varying levels of pension funding caused by pension asset performance from 2002 to 2014. Since the independent variable of interest is 2002–2014 cumulative pension portfolio returns × 2001 pension asset per property, this can be interpreted as the dollar windfall per property. Column (1) reports a positive and significant coefficient of 2.07, which suggests a rise of about two dollars in property values for each dollar of additional pension funding caused by state pension asset investment outperformance, relative to counties just across state borders. As we note in our theoretical framework, in a perfectly rational world, the coefficient on pension asset returns can be mapped directly to the cost of raising revenues in our model, denoted by α. For instance, a coefficient of 2.07 suggests that the marginal cost of raising one dollar to pay back pension obligations is $2.07 of total economic burden. As discussed in Section 2.2, a pass-through larger than one is not surprising. The net present value of public investment is also estimated to be large, implying a pass-through in our setting of between one and two (Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020). Another way to think about the estimated effects of these windfalls is that they suggest an inefficient underprovision of public goods or services, perhaps driven by severe underfunding of pensions, which are relaxed by the outperformance of pension assets.

5.2 Addressing identification concerns

As noted previously, while findings in column (1) of Table 3 are suggestive, the relative performance of pension assets still have the potential to be endogenously related to state-level outcomes due to familiarity or home bias. We work to alleviate these concerns in columns (2)–(5) of Table 3. In the case of familiarity bias, invested asset composition could be driven by familiarity with the sorts of sectors prevalent in that region (e.g., timber in Minnesota), which could also be driving local economic outcomes. Column (2) includes the same set of controls and sample as in column (1) but we construct an instrument variable for the primary variable of interest in a 2SLS specification using the initial assets per property in 2001 multiplied by the cumulative pension fund performance from 2002–2014 in excess of the mean benchmark performance for each asset class. This follows the 2SLS specification previously described in Equation (10) in our empirical methodology section. By constructing the instrumental variable in this way, variation is driven by relative outperformance within each asset class, not the variation in allocation across asset classes or sectors. If familiarity bias were driving our results, then, as long as the investor benchmarks are well specified, the excess return should eliminate any composition effect on portfolio returns. Column (2) reports a similar estimate for the economic burden (2.14) that is statistically significant with a strong first stage. This suggests that familiarity bias is unlikely to drive our prior findings.

However, this still leaves the possibility that home bias could be affecting our estimates. In this case, even within a given asset class the fund might be more likely to invest in local firms (e.g., Minnesota equities by the Minnesota pension fund). To address this possibility, we take the composition of the portfolio and use the returns on each benchmark nationally to calculate implied portfolio returns (column 3). To simultaneously shut down the familiarity channel, in column (4), we also collapse the benchmarks into major categories and throw out niche asset classes (e.g., we compare allocations to stocks, bonds and real estate, but drop allocations to commodities as they may be more closely related to state outcomes). Again, we find similar estimates of the economic burden of just over 2, suggesting little evidence of home bias in our primary specification.

An additional feature of these approaches is that excess returns and benchmark returns decompose the overall pension asset performance into different sources of variation with different identifying assumptions. Since we have two instrumental variables with differing sources of variation, but only one endogenous variable, we can jointly test for the plausibility of the exclusion restriction of each via an overidentification test. We carry this out in column (5) using both instrumental variables and fail to reject that they yield the same estimated economic burden. This provides further evidence that neither familiarity nor home bias are significant drivers of our overall findings and supports the conclusion that our empirical framework provides an unbiased estimate of the economic burden of person funding.

One remaining concern with the evidence presented so far is that it relies on purely cross-sectional variation, so any time-invariant differences across state borders that correlate with the dollar value of pension asset performance per property could confound identification. To help alleviate these concerns, we consider a panel regression setup for windfalls similar to the exercise carried out in Table 3. The dependent variable remains the same, but the independent variable of interest becomes the cumulative return between 2002 and the transaction date of the property. This specification faces a number of tradeoffs as compared with our baseline model. In particular, this sample includes transactions with a shorter window over which we compound returns, likely attenuating estimates if it takes time for asset performance to be reflected in property values. This is especially true when we require a house to have repeat sales, given the reduced time between observations. However, this also provides us with an opportunity to control for potential time-invariant confounds.

Table 4 column (1) replicates the exact analysis as in Table 3 but in a panel setting. To flexibly control for time variation in economic conditions across the border, we interact the border pair fixed effect with time. Again, we find a positive and significant coefficient, though just slightly lower than two, perhaps reflecting the shorter period over which property values can price movements in pension asset performance.

After establishing similar findings within this panel framework, we then provide evidence that time-invariant confounds are unlikely to be a concern in our setting. First of all, in Table 4 column (2), we consider an alternative version of what is perhaps our most restrictive instrumental variable based on broad benchmark returns from Table 3 column (4). In order to eliminate concerns that any characteristics that happen to be correlated with initial pension assets per property could be driving our findings, in this specification we do not multiply the broad benchmark returns by initial assets property and find similar estimates to the rest of the paper. Of course, it is still possible that other local differences correlate with future pension performance. To alleviate even this concern, in Table 4 columns (3) and (4) we take advantage of the panel setting to look at only properties with repeat sales. In column (3), we first restrict to the sub-sample of properties with repeat transactions during our sample period. Not surprisingly, since this sample allows even less time for property values to reflect pension asset movements, the coefficient estimates are lower than the full sample estimates. More importantly, the estimates in column (4) are very similar to those in column (3), which suggests that time-invariant omitted variables at the state, local, and even property-level do not significantly bias our estimates of the economic burden.

Finally, we show that our findings are also neither an artifact of our choice for the functional form of the BDD nor our construction of windfalls per property. In Tables A.2 and A.3 in the Appendix, we present similar findings using a simple parsimonious specification that focuses on pension asset returns. Figure 2 also uses this simple form of variation and depicts the main results of our paper within a non-parametric border discontinuity design. For each border pair, we calculate the state that has the larger pension asset return between 2002 and the sale date of the property and label this a "treated" state, with $Treated_{st}$ taking a value of 1 for treated states and -1 for non-treated states, and focus attention to properties within 30 miles of the state border. We then estimate the following regression to obtain a vector of coefficients that reflect the total sales price increase for a house that trades in the corresponding mile bucket on either side of the border:

$$HousePrice_{it} = \beta\, Treated_{st} \times Miles_i + \gamma_{bt} + \lambda_l + \delta_{it}\boldsymbol{X_{lt}} + \epsilon_{it}. \tag{11}$$

We plot the coefficients for five miles on either side of the border in Figure 2. Circular dots represent the β estimates while the diamonds are the difference between the estimate and the corresponding untreated bucket and the lines are the 95% confidence intervals for the differences.

Two distinct patterns are visible. First, for properties very close to the border, we observe a fairly stable premium in states with higher pension returns. Second, as we move across the border there is a sudden jump in the value of the properties in states with higher pension out-performance. This is exactly what we would expect and shows that our findings are not driven by specific functional form assumptions for the BDD nor any peculiarities with mapping into an economic burden that matches our theoretical model.

5.3 External validity

When considering external validity, the critical question for our paper is whether our empirical estimate of the economic burden is likely to apply more generally, not whether the effect of pension funding on house prices is generalizable.

While the latter is certainly interesting, it is also a very different research question. At state borders, the model's prediction is clear: the preponderance of the pension shortfalls should accrue to real estate since it cannot be relocated. However, as we move further away from the border, the cost of moving other types of capital increases, which disperses the potential burden away from real estate and toward a range of assets including non-tradable goods, wages, profits, etc.[18] As such, if we apply our analysis to non-border counties, we no longer have a clear prediction about the coefficient. This is confirmed in Table A.4 in the Appendix, which contrasts our main result with the same regression estimated in interior counties.[19] In contrast to counties near state borders (column 1), for non-border counties (column 2) we estimate a much smaller and statistically insignificant coefficient, highlighting the benefits of our identification strategy. This is consistent with the predictions of our theoretical model — house price effects are uncertain and unlikely to recover an estimate of the economic burden as we focus on interior counties. It also emphasizes that unlike prior work, our primitive of interest is the economic burden of pension shortfalls, not a more general average effect on house prices.

Therefore, when considering the external validity of our findings, what matters is whether border counties differ systematically from those in the interior, and whether that difference is likely to result in a different average economic burden. Since, as we have shown theoretically, this burden is a reflection of the deadweight loss associated with raising additional funds, the most plausible concern would be differences in government finances and costs of fundraising across these regions. In Table A.5 in the Appendix, we show that such concerns are unlikely to be particularly relevant. Comparing municipal finances for border relative to interior counties across 15 variables in 2007 and 2012, we only find four instances of potentially statistically significant differences across all 30 comparisons. In other words, when it comes to municipal finances, border counties are fairly representative of what we would expect in most counties nationwide. Still,

[18]Moreover, focusing on state borders allows us to control for the local economy using border county group fixed effects. Within a state, we are unable to account for this source of variation because geographic fixed effects would absorb variation in pension shortfalls.

[19]For consistency across specifications within this table we drop the distance-to-border controls as well as border county group fixed effects.

even if it happened by chance, we do observe some small differences between these counties. To address concerns that these might be especially relevant for our estimated average economic burden, in Table A.6, we re-run our analysis but run weighted least squares regressions where the weights are chosen, so that border counties match interior ones.[20] Whether we choose weights to match the mean of any particular one of these variable-years (columns 1–4) or all of them jointly (column 5), we again find estimates of about two across all specifications. These findings, therefore, support the conclusion that our main estimates for the economic burden are likely to apply more generally.

Modelling the full general equilibrium implications of our estimates for the economic burden are likely outside the scope of our paper, but a simple linear aggregation highlights just how substantial such findings could be, given their generalizability. As we noted previously, estimates of just the underfunded portion of state and local pension systems in the United States run in excess of $3.8 trillion (Rauh, 2016). Our estimate of an economic burden of about two, implies one dollar of excess burden, or deadweight loss, for each dollar of shortfall. Since there are about 121 million households in the United States, this would suggest an average deadweight loss of around $31,000 for every household in the country, or approximately 45% of median household income.[21]

5.4 The shortfall of shortfalls and importance of our empirical design

Our theoretical framework makes it clear that in regions with high mobility of non-housing factors, property values are more likely to reflect an estimate of the economic burden. This frame-

[20]In particular, we follow prior work (e.g., Jacob, Michaely, and Müller, 2018) in using an entropy balancing method developed by Hainmueller, 2012 that allows us to find weights that would set the weighted average of the interior counties to be the same as those in the interior for multiple variables.

[21]Based on 2019 median household income, available from: https://www.census.gov/content/dam/Census/library/publications/2020/demo/p60-270.pdf.

work, does not, however, clearly explain why it is critical to use windfalls, rather than shortfalls as our endogenous variable of interest. In this section, we explain and provide evidence on why this is necessary.

As noted in our empirical methodology in Section 4, a challenge with looking at shortfalls directly is that they represent an equilibrium endogenous decision of the state in response to funding shocks and overall gubernatorial objectives. Appendix Table A.1 shows that high pension shortfalls at the state level are correlated with changes in the contributions into the pension system by both the state government and the employees, providing direct evidence that the observed shortfall is an equilibrium outcome. If pension fund outperformance leads to a reduction in contributions, with spending shifted to value-improving projects, then we could be severely understating the effects of pension funding. On the other hand, such expenditures could be value-destroying, in which case an analysis based on observed shortfalls would be upward biased. Ultimately, this is an empirical question that demands variation in pension funding that is unaffected by the marginal propensity to spend out of pension funding and the relative value of those expenditures.

These problems are shown explicitly in Table 5. In column (2), we run the first stage of the 2SLS regression detailed in Equation (9), where the endogenous variable is the observed net shortfall per property and the instrumental variable is windfall per property coming from pension asset outperformance. The coefficient of -0.408 indicates that each dollar of windfall causes the equilibrium shortfall to fall by about 41 cents. Since instantaneously the shortfall must fall by one dollar, this means that there is 59 cents of offsetting crowding out that reduces pension contributions for each dollar of windfall. This is not shocking, but provides the first estimate we are aware of for the crowding out effects of pension performance on funding contributions.

While this is an interesting parameter to estimate, it also highlights why using shortfalls as an endogenous variable, even with windfalls as an instrumental variable, would lead to a biased estimate of the economic burden. This is made clear in columns (1) and (3). The OLS estimate from looking at windfalls yields a coefficient of 1.91, while the 2SLS estimation results in 4.69. This shouldn't be surprising though since the first stage estimate of -0.408 is exactly $1.913/-4.686$. In other words, because states contribute less to their pensions if they are performing well, looking at equilibrium shortfalls, even if instrumented with plausibly exogenous changes due to windfalls, leads to a systematic bias in the estimate of the economic burden.

5.5 Drivers of the economic burden

5.5.1 Current versus future benefits

In the preceding analysis, we estimate a pass-through between pension shortfall per property and house prices in border regions of approximately two. Since we measure asset returns over long horizons — for the properties transacting at the end of the sample, a 16 year period — the channel through which pension asset shortfalls affect house prices is ambiguous. With gains building up over a long period, states with large shortfalls may raise taxes or reduce the allocation of tax revenues to public projects. If this substitution were at play, the coefficient we estimate could reflect a combination of worse current amenities and higher future liabilities.

In Table 6 we attempt to distinguish between the present and future channels by controlling for local rental prices. If the effect of past pension shortfalls is capitalized through current amenities, renters have an equal opportunity to enjoy those amenities as owners. However, if the estimated effect reflects expectations about the relatively distant future (more than one or two years later), then current rental prices will not incorporate these benefits.

In our primary specification looking at windfalls over more than a decade, we find a statistically significant and economically important pass-through, even after controlling for estimated rents in column (2) of Table 6.[22] Perhaps more interestingly, we also find that our point estimate falls by just over 34% from column (1) to column (2). By contrast, we find virtually no change between columns (3) and (4), when focusing on the sample of repeat sales. Such contrasting findings are entirely consistent with shortfalls driving changes in current amenities, but this process occurring relatively slowly. This is because, in columns (1) and (2), windfalls occur over more than decade, while in columns (3) and (4), the panel nature of our measure combined with the focus on repeat sales will naturally shrink the period over which windfalls occur substantially.

Putting all of this together, our findings suggest that the majority of the perceived burden of pension shortfalls, or the benefit of windfalls, will be enjoyed in the future, but some of the effects of decades prior pension funding shocks are affecting local amenities already.

5.5.2 Economic burden and tax distortions

As noted above, our estimates are also bounded by the perceived deadweight loss associated with improving pension funding through changes in future taxes. For example, the finding of a pass-through larger than one suggests that substantial expected costs would be associated with raising taxes to improve pension funding (or even perhaps the political infeasibility of doing so). If this interpretation carries weight, we might expect to see larger effects of pension shortfalls in states where the distortionary effects of raising additional funds are larger. Income taxes are typically considered distortionary, while property taxes, especially on land, are not. Along these lines, we show evidence of heterogeneity in our estimated elasticities for states with high marginal income tax rates in Table 7. In particular, we find that in a state with a one-standard deviation

[22] It is worth pointing out that monthly rental rates are a "bad control" when considering the estimate for the economic burden. As we argue above, we expect some amount of the burden, especially over long intervals, to pass through into current rents, and therefore for the coefficient on windfalls to change. So while the inclusion of rental rates is helpful in considering the role of windfalls in current vs future (dis)amenities, it is unlikely to help us recover a more accurate estimate for the economic burden.

higher top marginal income tax rate, the economic burden is 0.465 to 0.532 larger, consistent with larger distortions of raising tax revenue. This could represent the marginal cost associated of raising funds via income taxes, but more likely reflects the high marginal value of a dollar of additional funds among this subset of states that choose to have high marginal income tax rates despite their distortionary effects.

6 Conclusion

This paper uses plausibly exogenous variation in state pension funding based on excess asset performance to show that a one dollar increase in the public pension shortfall per property causes a two dollar reduction in property values near state borders. We motivate this research design with a parsimonious theoretical framework showing that due to its relative immobility, real estate on state borders should reflect the economic burden of pension obligation shortfalls. Our use of excess asset performance, relative to benchmarks, are driven by as-if-random asset allocation, supports a causal interpretation of our findings. These findings are not driven by differential property characteristics, even restricting the sample to repeat sales, nor proxies for coincident economic conditions, such as rental rates. This suggests that price effects are likely driven by future costs, consistent with the expected timing for the burden of such shortfalls. The estimated willingness to pay on the part of marginal homebuyers is also comparable in value with extant estimates for public spending on infrastructure and public teacher salaries, highlighting the perceived importance of these obligations. Our findings are consistent with models of inefficient taxation or the underprovision of public goods and services.

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Table 1

Public Plans Data Summary Statistics

Data are from the Public Plans Data (PPD) database provided by the Center for Retirement Research (CRR) at Boston College and are reported at the state-year level. Asset return is average annual portfolio return. Actuarial assets and Actuarial liabilities are ActAssets_GASB and ActLiabilities_GASB in the dataset in millions of dollars. Actuarial funded ratio is given by ActFundedRatio_GASB, which is ActAssets_GASB divided by ActLiabilities_GASB in the dataset. Allocation of pension portfolios to equities, fixed-income (FI), real estate (RE), cash, private equity (PE), hedge fund (HF), commodities (Comd) and other alternative assets (AltMisc) are shown in percentage terms.

Variable	Obs.	Mean	Std. Dev	Q5	Q25	Q50	Q75	Q95
Asset return	496	0.057	0.077	−0.078	0.010	0.061	0.115	0.168
Actuarial assets ($m)	496	15, 209	13, 838	1, 190	5, 871	10, 037	20, 186	46, 550
Actuarial liabilities ($m)	496	19, 032	16, 258	1, 823	7, 469	13, 601	27, 940	52, 492
Actuarial funded ratio	496	0.792	0.148	0.539	0.705	0.778	0.896	1.016
Equity share	496	0.527	0.100	0.349	0.467	0.540	0.603	0.664
FI share	496	0.285	0.079	0.190	0.232	0.272	0.324	0.435
RE share	496	0.054	0.037	0.000	0.021	0.057	0.080	0.110
PE share	496	0.053	0.053	0.000	0.007	0.038	0.081	0.159
HF share	496	0.038	0.058	0.000	0.000	0.008	0.053	0.177
Comd share	496	0.014	0.022	0.000	0.000	0.000	0.021	0.065
Cash share	496	0.016	0.019	0.000	0.004	0.012	0.021	0.053
Other share	496	0.005	0.025	0.000	0.000	0.000	0.000	0.018

Table 2

Housing Transactions Summary Statistics

This table depicts summary statistics for the sample of properties used in our analysis that are a merge of ZTRAX (Zillow's Transaction and Assessment Dataset) with state-level annual pension performance/shortfalls, local annual rental rates, and state-level annual income per capita. Rental rates are based on fair market rental rates for single family residences with the number of bedrooms matching those of the transaction property (or 3 bedrooms if the number of bedrooms is missing for the transacting property) at the county-year level from the Department of Housing and Urban Development. The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state with differential pension funding, are within 50 miles of that border, are associated with single family residences, and had a transaction price between thirty thousand and two million dollars.

Variable	Obs.	Mean	Std. Dev	Q5	Q25	Q50	Q75	Q95
Sales Price ($ '000s)	872, 872	243	190	58	120	186	312	597
Transaction Month	872, 872	05/2009	54 Mos	09/2003	08/2005	12/2007	04/2013	07/2017
Border Dist (mi)	872, 872	12.0	8.6	1	5	10	17	29
Building Age (yrs)	723, 907	9.5	6.0	1	4	9	14	20
Sq Ft	632, 252	1, 729	1, 041	800	1, 020	1, 340	2, 015	4, 250
Lot Sq Ft	758, 605	57, 763	320, 212	1, 500	2, 500	7, 000	15, 500	113, 500
# Bedrooms	351, 632	3.08	0.75	2	3	3	3	4
# Bathrooms	521, 006	3.79	1.58	2	2	4	5	7
Shortfall/Prop ($ '000s)	872, 872	15.13	17.23	−1.00	3.37	9.73	23.12	45.55
02-14 Cum. Port. Ret. (%)	132, 735	153	28	90	147	161	165	187
02-14 Cum. Excess Ret. (%)	132, 735	20	15	2	6	14	28	37
02-14 Windfall Per Property ($ '000s)	132, 735	39.03	27.84	6.83	15.91	31.09	78.23	78.23
Est. Mo Rent ($)	872, 872	1, 169	354	686	920	1, 115	1, 339	1, 904
State-Year Income PC ($)	872, 872	42, 885	8, 313	31, 456	36, 823	41, 512	47, 586	59, 162

Table 3

Pension Windfalls and House Prices in Border Counties

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located from 2002-2014, multiplied by initial assets per property in 2001 to create a measure of additional pension shortfall per property due to asset performance (Windfall). The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border and the income per capita at the state-year level, are included throughout. These specifications also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Column (1) is the same as the baseline described above where the dependent variable is the sales price (in thousands of dollars), for transactions in the years 2015-2018 and the primary variable of interest is the Windfall per Property over the 2002-2014 period. Column (2) is the same as column (1) but instruments the primary variable of interest in a 2SLS specification using the initial assets per property in 2001 multiplied by the cumulative pension fund performance from 2002-2014 in excess of what is expected given the average benchmark performance for each asset class they are invested in, for each asset class across all funds. Column (3) is the same as column (2), but where the instrument is initial assets per property in 2001 multiplied by the cumulative pension fund performance from 2002-2014 that would have occurred based on the funds' historical asset class allocations, had they earned the average benchmark performance for each asset class they are invested in, for each asset class across all funds. Column (4) is the same as column (3), but limited to only broad asset class definitions, such as bonds or equities. Column (5) is the same as column (2) but utilizes two instruments—those of columns (2) and (3). Also reported, where applicable, are the Kleibergen-Paap F Statistic test for weak identification as well as the Hansen J statistic (and associated p-Value) testing for overidentification. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)				
	OLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
2002-2014 Windfall Per Property $('000s)	2.065***	2.144***	2.133***	2.134***	2.134***
	(3.93)	(3.96)	(4.17)	(4.17)	(4.15)
Border Distance	Yes	Yes	Yes	Yes	Yes
State-Year Income PC	Yes	Yes	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	Yes	Yes	Yes
Border Group-Tran Year FE	Yes	Yes	Yes	Yes	Yes
First Instrumental Variable	—	Excess Ret. Windfall Per Prop	Bnchm. Ret. Windfall Per Prop	Brd Bm. Ret. Windfall Per Prop	Excess Ret. Windfall Per Prop
Second Instrumental Variable	—	—	—	—	Bnchm. Ret. Windfall Per Prop
N	132,735	132,735	132,735	132,735	132,735
adj. R^2	0.581	-0.196	-0.196	-0.196	-0.196
Weak ID KP F Stat		3,162	127,710	100,591	93,256
Over ID Hansen J Stat					0.020
p-Value					(0.888)

Table 4

Pension Windfall Panel Regressions and Repeat Sales

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located, but only in the years prior to the transaction since 2002, multiplied by the pension asset per property as of 2001 (Windfall). The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as a covariate for the income per capita at the state-year level, are included. Columns (1) through (3) also include a covariate for the distance to the state border. Columns (1) through (3) control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Column (1) is the same as the baseline described above—the dependent variable is the sales prices, in thousands of dollars, that transact in a given year and the primary variable of interest is the Windfall per Property in that state from 2002 until the year prior to that particular transaction. Column (2) is the same as column (1) but instruments the primary variable of interest in a 2SLS specification using the cumulative pension fund performance from 2002 to the sale of the property that would have occurred based on the funds' historical asset class allocations, had they earned the average benchmark performance for each asset class they are invested in, limited to only broad asset class definitions, such as bonds or equities. Column (3) is the same as column (1), but restricting the sample to repeat sales observations. Column (4) is the same as column (3), but replaces the interacted property characteristic fixed effects and the distance to state-border covariate with a property-level fixed effect. In this case, variation comes from within-property variation over time, coming from repeat sales. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)			
	OLS	2SLS	OLS	OLS
	(1)	(2)	(3)	(4)
2002-Sale Windfall Per Property $('000s)	1.913***	2.368***	1.303***	1.277**
	(4.42)	(5.28)	(2.79)	(2.24)
Border Distance	Yes	Yes	Yes	No
State-Year Income PC	Yes	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	Yes	No
Repeat Sales Sample	No	No	Yes	Yes
Property FE	No	No	No	Yes
Border Group-Tran Year FE	Yes	Yes	Yes	Yes
Instrumental Variable	—	Brd Bm. Return	—	—
N	872,872	872,872	445,209	445,209
adj. R^2	0.658	-0.259	0.691	0.822

Table 5

The Shortfall of Shortfalls

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located, but only in the years prior to the transaction since 2002, multiplied by the pension asset per property as of 2001 (Windfall). The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border and the income per capita at the state-year level, are included throughout. These specifications also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Column (1) is the same as the baseline described above—and replicates column (1) of Table 4. Column (2) represents the first state of the 2SLS regression detailed in Equation (9), where the endogenous variable is the observed net shortfall per property and the instrumental variable is windfall per property coming from pension asset performance, and demonstrates the the crowding out effects of pension performance on funding contributions. Column (3) represents the specification detailed in Equation (9), and demonstrates that, because states contribute less to their pensions if they are performing well, looking at equilibrium shortfalls, even if instrumented with plausibly exogenous changes due to windfalls, leads to systematic bias in the estimate of the economic burden. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s) OLS (1)	Shortfall Per Prop $('000s) OLS (2)	Sales Price $('000s) 2SLS (3)
2002-Sale Windfall Per Property $('000s)	1.913***	-0.408***	
	(4.42)	(-7.56)	
Shortfall Per Property $('000s)			-4.686***
			(-3.74)
Border Distance	Yes	Yes	Yes
State-Year Income PC	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	Yes
Border Group-Tran Year FE	Yes	Yes	Yes
Instrumental Variable	—	—	Windfall Per Prop
N	872,872	872,872	872,872
adj. R^2	0.658	0.892	-0.338
Weak ID KP F Stat			57.2

Table 6

Pension Windfalls
Current Amenities vs. Future Liabilities

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Sales Price $('000s) of a residential property, but compares baseline results after including time-varying controls for rental rates. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located, but only in the years prior to the transaction since 2002, multiplied by the pension asset per property as of 2001 (Windfall). The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border (for columns (1) and (2) only) and the income per capita at the state-year level, are included throughout. Columns (1) and (2) also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories), while columns (3) and (4) include a property-level fixed effect in order to exploit variation that comes from within-property variation over time. Column (1) replicates the results and specification of column (1) of Table 3. Column (2) is the same as column (1), but includes a control for time-varying rental rates. Column (3) replicates the results and specification of column (4) of Table'4. Column (4) is the same as column (3), but includes a control for time-varying rental rates. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)			
	(1)	(2)	(3)	(4)
2002-2014 Windfall Per Property $('000s)	2.065***	1.361**		
	(3.93)	(2.45)		
2002-Sale Windfall Per Property $('000s)			1.277**	1.252**
			(2.24)	(2.20)
Est. Mo Rent ($)		0.268***		0.0920***
		(3.95)		(3.36)
Border Distance	Yes	Yes	No	No
State-Year Income PC	Yes	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	No	No
Property FE	No	No	Yes	Yes
Border Group-Tran Year FE	Yes	Yes	Yes	Yes
N	132,735	132,735	445,209	445,209
adj. R^2	0.581	0.585	0.822	0.822

Table 7

The Economic Burden and Distortionary Taxation

This table depicts how the effects of pension funding on house prices varies with likely difficulty in raising additional funds, as proxied by the highest marginal state income tax rate. The reported specifications are state border discontinuity design models where the dependent variable is based on the Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located, but only in the years prior to the transaction since 2002, multiplied by the pension asset per property as of 2001 (Windfall) interacted with the highest marginal state income tax rate as of 2002 (taxpolicycenter.org). The income tax rate is standardized at the regression observation level to obtain a normalized measure of the effect of the interaction. The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border and the income per capita at the state-year level, are included throughout. Columns (1) and (2) also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories) interacted with a geographic fixed effect of either county or zip, respectively. The inclusion of time variation in the interaction of interest allows for the interaction of the property characteristics fixed effects geographic fixed effects. Column (3) replaces the property characteristic by geography fixed effects and border distance control with property-level fixed effects, so that variation is based only on repeat sales. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)		
	(1)	(2)	(3)
2002-Sale Windfall Per Property $('000s)	0.762*	0.799**	0.624
	(1.81)	(2.15)	(1.00)
2002-Sale Windfall Per Prop $('000s) × Standardized Highest Marginal Income Tax Rate	0.465***	0.465***	0.532***
	(3.82)	(4.09)	(2.69)
Border Distance	Yes	Yes	No
State-Year Income PC	Yes	Yes	Yes
County by 6 Prop Chars FE	Yes	No	No
Zip by 6 Prop Chars FE	No	Yes	No
Property FE	No	No	Yes
Border Group-Tran Year FE	Yes	Yes	Yes
N	870,698	867,470	445,209
adj. R^2	0.704	0.783	0.822

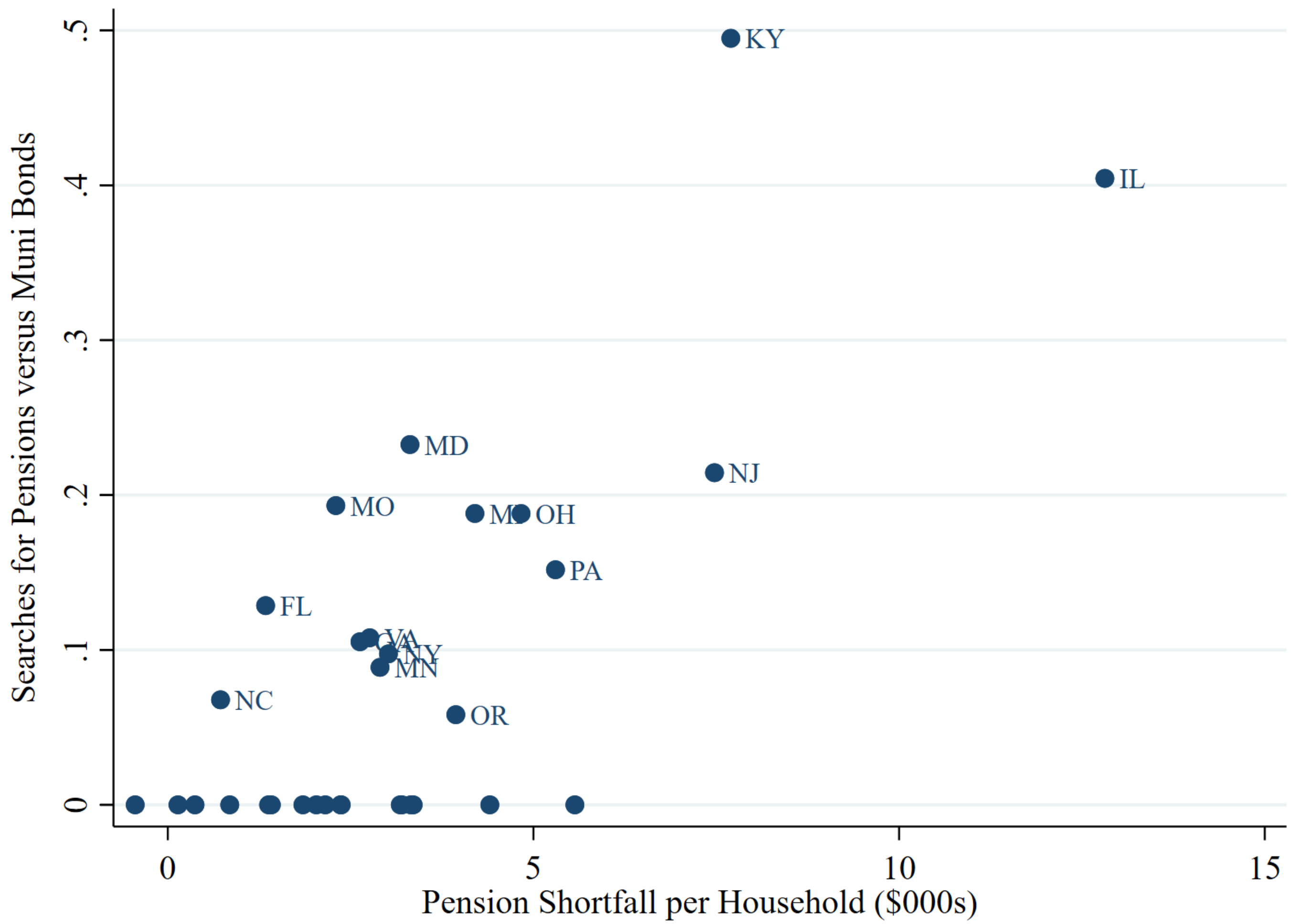

Figure 1. Popular Interest in Public Pensions from Google Search Trends This figure presents evidence on popular interest in the condition of public pensions using data from Google Trends. For each state in our regression sample, depicted in Figure A.1, we obtain monthly series of search trends for the terms "pension crisis" and "public pension" over the period January 2004 to December 2020. Google search trends are computed on a relative basis, so they must be scaled by a common search term to make comparisons across states. We scale the total interest in the two pension-related terms by each state's trend series for the "municipal bond" topic. To match the timing of our estimated house price effects, we take the average ratio of pension search trends to municipal bond search trends from 2015 to 2018, which we plot on the y-axis of the figure. The x-axis of the figure is the average pension shortfall per property, in thousands of dollars, over the same period. The scatter plot reveals a positive relation between pension underfunding and popular interest in the issue. The corresponding regression coefficient is 0.035 (t = 5.40) and the R^2 is 0.52. When states with zero pension-related search are excluded from the sample, the R^2 increases to 0.60.

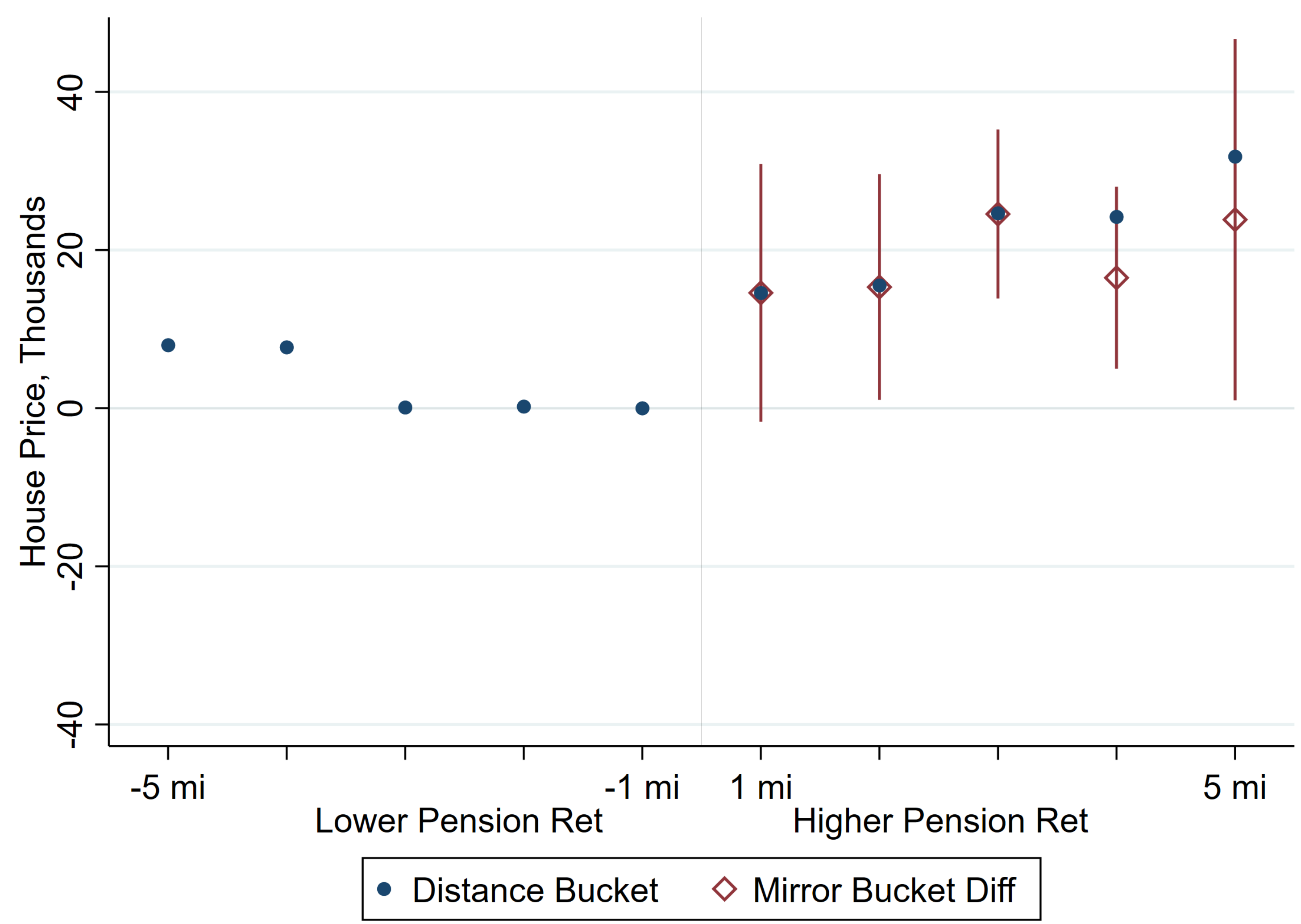

Figure 2. Pension Return Discontinuity in House Prices This figure depicts the results for a border discontinuity design for house values surrounding state borders with differing pension asset performance between 2002 and the sale date of the property, by distance the border non-parametrically. We plot the coefficients for the five miles surrounding each border in our sample, where blue dots represent the primary coefficients of interest in Equation (11). The red diamonds are the difference between the estimates for properties in better performing states minus those for equidistant from the border properties in worse performing states. Red lines depict 95% confidence intervals for these estimates.

Appendix

A Details of the Model

A.1 Tax burden in a one-sector closed economy

Consider a closed economy where labor, L, and capital, K, are used to produce a single good according to a linear homogeneous of degree one production function $F(K, L)$, with $F_L > 0$ and $F_K > 0$. Suppose that capital is inelastically supplied, while labor supply is positively related to the real wage, W/P, where W is the wage rate and P is the price of the economy's single good:

$$L = L(W/P). \tag{A.1}$$

The equilibrium wage rate W and the rental rate on capital r are given by the standard first order conditions:

$$F_K(K, L) = r/P; \qquad F_L(K, L) = W/P \tag{A.2}$$

Using market-clearing in the labor market, we have

$$F_L\left(K, L\left(W/P\right)\right) = W/P. \tag{A.3}$$

Consider the burden of a tax at rate τ imposed on the elastically-supplied labor, we get

$$PF_L = W(1 + \tau) \tag{A.4}$$

Equating supply and demand for labor in the tax equilibrium and taking the derivative with respect to τ, we find that the percentage change in W/P from a change in τ, evaluated at $\tau = 0$, is given by

$$\frac{\partial(W/P)/(W/P)}{\partial\tau} = \frac{\eta^D}{\eta^S - \eta^D}\,, \tag{A.5}$$

where η^S is the positive elasticity of labor supply, and η^D is the negative elasticity of labor demand. The marginal losses of rents to labor, $(\partial(W/P)/\partial\tau)\,L$, and to capital, $(\partial(r/P)/\partial\tau)\,K$, as a ratio of the marginal tax revenue, $(W/P)L$, can be written as

$$\frac{\frac{\partial(W/P)}{\partial\tau}L}{(W/P)L} = \frac{\eta^D}{\eta^S - \eta^D}\,, \tag{A.6}$$

and

$$\frac{\frac{\partial(r/P)}{\partial\tau}K}{(W/P)L} = \frac{\eta^S}{\eta^D - \eta^S}\,. \tag{A.7}$$

Note that Equations (A.6) and (A.7) sum to -1: the full burden of the tax falls on either capital or labor.

If the supply of labor is perfectly inelastic ($\eta^S = 0$) or labor demand is perfectly elastic ($\eta^D = \infty$), labor bears the full burden of the tax, i.e., the right hand sides of (A.6) and (A.7) are -1 and 0, respectively. At the other extreme, if labor supply is perfectly elastic ($\eta^S = \infty$) or the demand for labor is perfectly inelastic ($\eta^D = 0$), capital bears the full burden of the tax. Importantly, although the tax is imposed on labor, from Equation (A.7), capital always bears some burden of the tax if $\eta^S \neq 0$ and $\eta^D \neq \infty$.

A.2 Tax burden in an open economy

Suppose there are two bordering states, A and B, in the country with production functions $F_A(K)$ and $F_B(K)$ used to produce a common consumption good. Let K_A be the capital in state A

and $K_B = \overline{K} - K_A$ be the capital in state B, where $\overline{K}$ is the total countrywide capital. If r is the rental rate on capital, and τ is the tax on capital in state A, we have

$$F'_A(K_A) = r + \tau; \qquad\qquad F'_B(K_B) = r. \tag{A.8}$$

Using Equation (A.8) and the constraint $K_A + K_B = \overline{K}$, we can show that the change in rents to countrywide capital, $dr\overline{K}$, expressed as a ratio of the marginal tax revenue, $d\tau K_A$, evaluated at $\tau = 0$, is given by

$$\frac{(dr/d\tau)\,\overline{K}}{K_A} = -\frac{\eta_A \overline{K}}{\eta_B K_B + \eta_A K_A} \leq 0, \tag{A.9}$$

where η_A and η_B are the non-negative demand elasticities for capital in states A and B, respectively. If A and B have identical production functions, $F_A(\cdot) = F_B(\cdot)$, then $\eta_A = \eta_B$ and $K_A = K_B$ initially. Then the right hand side of Equation (A.9) equals −1 and countrywide capital, $\overline{K}$, bears the full marginal burden of the tax in A. If the demand for capital in B is perfectly inelastic, $\eta_B = 0$, or is perfectly elastic in A, $\eta_A = \infty$, countrywide capital bears more than 100% of the tax. At the opposite extreme, if capital demand is perfectly elastic in B or in perfectly inelastic demand in A, $\overline{K}$ bears none of the burden of the tax.

Land rents in A and B, denoted R_A and R_B, respectively, are given by

$$R_A = F_A(K_A) - (r + \tau)K_A; \qquad\qquad R_B = F_B(K_B) - rK_B, \tag{A.10}$$

implying[23]

$$\frac{dR_A/d\tau}{K_A} = -\frac{\eta_B K_B}{\eta_B K_B + \eta_A K_A} \leq 0; \qquad \frac{dR_B/d\tau}{K_A} = \frac{\eta_A K_B}{\eta_B K_B + \eta_A K_A} \geq 0. \tag{A.11}$$

The intuition from Equation (A.11) is that landowners in state A lose rental income, while B's landowners gain. Note that the three tax burdens in Equations (A.9) and (A.11) sum up to −1. With identical production functions, landowners in state A (B) lose (gain) rents equal to half of the marginal tax revenues. Therefore, in this model, a state within a country is likely to bear a significant portion of the burden of a tax it levies on a domestically mobile factor. Appendix A.3 use the open-economy model to examine the burden of property taxes.

A.3 Burden of the property tax

We can use the tax burden analysis for in open economy in Section 2.1 to study the burden of a property tax. Property taxes are typically levied on both land and capital, so the burden of the tax can be decomposed into that from taxing land and that from taxing capital. As we saw above, a tax on land rents is fully borne by landowners, while the tax on mobile capital may be shifted. Similarly, the economic burden of taxes depend on the supply and demand elasticities.

Consider the two-state (city) model where τ_A is the property tax in state/city A and τ_B the tax in state/city B. Then assuming $F_A(\cdot) = F_B(\cdot) = F(\cdot)$, the capital rental rates in Equation (A.8) become

$$F'(K_A) = r + \tau_A; \qquad F'\left(\overline{K} - K_A\right) = r + \tau_B. \tag{A.12}$$

[23] Differentiating (A.10) with respect to τ, we get

$$\frac{\partial R_A}{\partial \tau} = F_A'(K_A)\frac{\partial K_A}{\partial \tau} - (r+\tau)\frac{\partial K_A}{\partial \tau} - K_A\left(1 + \frac{\partial r}{\partial \tau}\right); \qquad \frac{\partial R_B}{\partial \tau} = F_B'(K_B)\frac{\partial K_B}{\partial \tau} - r\frac{\partial K_B}{\partial \tau} - K_B\frac{\partial r}{\partial \tau}.$$

From (A.8), the first two terms in each expression above cancel out and we can use (A.9) to get the expressions in (A.11).

If $\tau_A = \tau_B$, capital rental rate r declines by the full amount of the tax, and capital bears the full burden of the property taxes levied on capital. If instead $\tau_A > \tau_B$, then $\tau_A - \tau_B$ will reduce land rents in A and increase rents in B. In this case, depending on differences in capital demand elasticities, capital will bear the differential tax in part, in full, or more than in full. To see this, we can replace τ by $\tau_A - \tau_B$ and r by $r + \tau_B$ in Equations (A.8) and (A.10).

Table A.1

State Responses to Shortfalls

This table depicts state-level annual regressions of a variety of outcomes on lagged state pension shortfalls. Column (1) regresses employer pension contributions per property on the prior year's state-level pension shortfall per property after including state fixed effects. Columns (2-5) are the same as column (1), but the dependent variables are employee pension contributions per property, secondary education appropriation per property, and the annual change in the percentages of rural and urban roads in poor condition, respectively. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at the state level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Employer Pension Contribution (1)	Employee Pension Contribution (2)	Secondary Education Appropriation (3)	Change in Percent of Rural Roads in Poor Condition (4)	Change in Percent of Urban Roads in Poor Condition (5)
Lagged Shortfall Per Property	0.0213***	0.00379***	-0.00251***	0.0161*	0.0160*
	(6.12)	(5.39)	(-2.73)	(1.88)	(1.85)
State FE	Yes	Yes	Yes	Yes	Yes
N	806	806	450	383	393
adj. R^2	0.606	0.802	0.942	0.046	-0.045

Table A.2

2002-2014 Pension Returns and 2015-2018 House Prices

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Log Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance over the period 2002-2014 in the pension plans associated with the state in which the focal property is located. The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border and the income per capita at the state-year level, are included throughout. These specifications also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Column (1) is the same as the baseline described above where the dependent variable is the natural logarithm of the sales prices, in thousands of dollars, that transact in the years 2015-2018 and the primary variable of interest is the cumulative pension fund performance from 2002-2014. Column (2) is the same as column (1), but where the primary variable of interest is cumulative pension fund performance from 2002-2014 in excess of what is expected given the average benchmark performance for each asset class they are invested in, for each asset class across all funds. Column (3) is the same as column (1), but where the primary variable of interest is cumulative pension fund performance from 2002-2014 that would have occurred based on the funds' historical asset class allocations, had they earned the average benchmark performance for each asset class they are invested in, for each asset class across all funds. Column (4) is the same as column (3), but limited to only broad asset class definitions, such as bonds or equities. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log Sales Price $('000s)			
	(1)	(2)	(3)	(4)
2002-2014 Cum. Port. Ret.	0.269***			
	(3.64)			
2002-2014 Cum. Excess Ret.		0.564**		
		(2.43)		
2002-2014 Cum. BenchMk Ret.			0.399***	
			(5.10)	
2002-2014 Cum. (Broad) BenchMk Ret.				0.399***
				(5.15)
Border Distance	Yes	Yes	Yes	Yes
State-Year Income PC	Yes	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	Yes	Yes
Border Group-Tran Year FE	Yes	Yes	Yes	Yes
N	132,735	132,735	132,735	132,735
adj. R^2	0.669	0.668	0.671	0.671

Table A.3

2002–Sale Pension Returns and House Prices

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Log Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located, but only in the years prior to the transaction. The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border and the income per capita at the state-year level, are included throughout. These specifications also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Column (1) is the same as the baseline described above where the dependent variable is the natural logarithm of the sales prices, in thousands of dollars, that transact in a given year and the primary variable of interest is the cumulative pension fund performance from 2002 until the year prior to that particular transaction. Column (2) is the same as column (1), but where the primary variable of interest is cumulative pension fund performance from 2002 until the year prior to that particular transaction in excess of what is expected given the average benchmark performance for each asset class they are invested in, for each asset class across all funds. Column (3) is the same as column (1), but where the primary variable of interest is cumulative pension fund performance from 2002 until the year prior to that particular transaction that would have occurred based on the funds' historical asset class allocations, had they earned the average benchmark performance for each asset class they are invested in, for each asset class across all funds. Column (4) is the same as column (3), but limited to only broad asset class definitions, such as bonds or equities. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log Sales Price $('000s)			
	(1)	(2)	(3)	(4)
2002-Sale Cum. Port. Ret.	0.169***			
	(3.02)			
2002-Sale Cum. Excess Ret.		0.346***		
		(3.16)		
2002-Sale Cum. BenchMk Ret.			0.215***	
			(3.73)	
2002-Sale Cum. (Broad) BenchMk Ret.				0.210***
				(3.66)
Border Distance	Yes	Yes	Yes	Yes
State-Year Income PC	Yes	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	Yes	Yes
Border Group-Tran Year FE	Yes	Yes	Yes	Yes
N	872,872	872,872	872,872	872,872
adj. R^2	0.708	0.708	0.708	0.708

Table A.4

House Prices and Pension Windfalls
Border vs. Interior Counties

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the Sales Price $('000s) of a residential property that transacted in 2015-2018. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located from 2002-2014, multiplied by initial assets per property in 2001 to create a measure of additional pension shortfall per property due to asset performance (Windfall). The sample is restricted to transactions on properties that are associated with single family residences and that had a transaction price between thirty thousand and two million dollars. Controls for the income per capita at the state-year level, six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories), and year of the transaction fixed effects are included throughout. Column (1) is the same as the baseline described above focusing on only transactions on properties located in counties sharing a border with an adjacent state and are within 50 miles of that border. Column (2) is the same as column (1), but only includes properties that don't meet that definition of being a border county (i.e. only counties in the interior of the state). Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Border	Interior
	Sales Price $('000s)	
	(1)	(2)
2002-2014 Windfall Per Property $('000s)	2.661***	0.0589
	(5.74)	(0.30)
State-Year Income PC	Yes	Yes
6 Prop Chars FE	Yes	Yes
Tran Year FE	Yes	Yes
N	132,740	1,285,464
adj. R^2	0.545	0.508

Table A.5

County-Level Municipal Finances
Border vs. Interior Counties

This table depicts county-level regressions of a variety of outcomes on an indicator for whether the county is on a state border, for counties in states in our regression sample, depicted in Figure A.1. A fixed effect at the state level is also included. Information regarding municipal finances are aggregated to the county-level and are available for the years 2007 and 2012. Regressions for the 2007 and 2012 values are ran and reported separately. Results suggest that border counties are fairly representative of comparable counties on the interior of their state. Reported *t*-statistics in parentheses are heteroskedasticity-robust. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Border Relative To Interior			Border Relative To Interior	
Variable	2007	2012	Variable	2007	2012
Total Revenues	-0.10	0.19**	Total Expenditures	-0.13	0.17**
Per Capita	(-0.37)	(2.25)	Per Capita	(-0.48)	(2.00)
Revenues From Federal Govt	-0.01	0.00	Capital Expenditures	-0.05	0.00
Per Capita	(-0.85)	(0.07)	Per Capita	(-1.26)	(0.23)
Revenues From State Govt	-0.03	0.05***	Education Expenditures	-0.11	0.00
Per Capita	(-0.45)	(2.63)	Per Capita	(-1.15)	(-0.04)
Total Taxes	-0.09	0.03	Safety Expenditures	-0.03	0.00
Per Capita	(-0.88)	(1.23)	Per Capita	(-1.02)	(0.90)
Property Taxes	-0.06	0.02	Utility Expenditures	0.06	0.04
Per Capita	(-0.93)	(0.87)	Per Capita	(0.99)	(0.71)
Sales Taxes	-0.01	0.00	Short-Term Debt	-0.01	0.00
Per Capita	(-0.43)	(0.51)	Per Capita	(-1.07)	(0.77)
Income Taxes	-0.02	0.00	Long-Term Debt	0.67	0.69*
Per Capita	(-0.87)	(0.23)	Per Capita	(1.13)	(1.90)
Other Taxes	0.00	0.00			
Per Capita	(-0.27)	(1.31)			

Table A.6

Pension Windfalls and House Prices in Border Counties Weighted to Match Interior Counties

This table presents estimates from a weighted least squares state border discontinuity design model where the dependent variable is based on the Sales Price $('000s) of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance in the pension plans associated with the state in which the focal property is located from 2002-2014, multiplied by initial assets per property in 2001 to create a measure of additional pension shortfall per property due to asset performance (Windfall). The sample is restricted to transactions on properties located in counties sharing a border with an adjacent state, are within 50 miles of that border, are associated with single family residences, and that had a transaction price between thirty thousand and two million dollars. Fixed effects for the county border group of the property—interacted with the calendar year of the transaction, as well as covariates for the distance to the state border and the income per capita at the state-year level, are included throughout. These specifications also control for property type by including six interacted property characteristic fixed effect cells (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). These specifications are similar to that of column (1) in Table 3, but have been re-weighted such that these border counties match interior counties on the specified dimension(s). Columns (1)-(4) utilize weights chosen to match the four variables in Table A.5 with statistically significant differences between border and interior counties. Column (5) utilizes weights chosen to match all four variables jointly. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. The symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)				
	(1)	(2)	(3)	(4)	(5)
2002-2014 Windfall Per Property $('000s)	1.997***	2.048***	1.999***	1.983***	2.020***
	(3.75)	(3.89)	(3.75)	(3.69)	(3.80)
Border Distance	Yes	Yes	Yes	Yes	Yes
State-Year Income PC	Yes	Yes	Yes	Yes	Yes
6 Prop Chars FE	Yes	Yes	Yes	Yes	Yes
Border Group-Tran Year FE	Yes	Yes	Yes	Yes	Yes
2012 Balance Variable(s)	Total Revenues, PC	Revenues From State Govt, PC	Total Expenditures, PC	Long-Term Debt, PC	Cols. (1)-(4)
N	132,735	132,735	132,735	132,735	132,735
adj. R^2	0.586	0.585	0.586	0.584	0.584

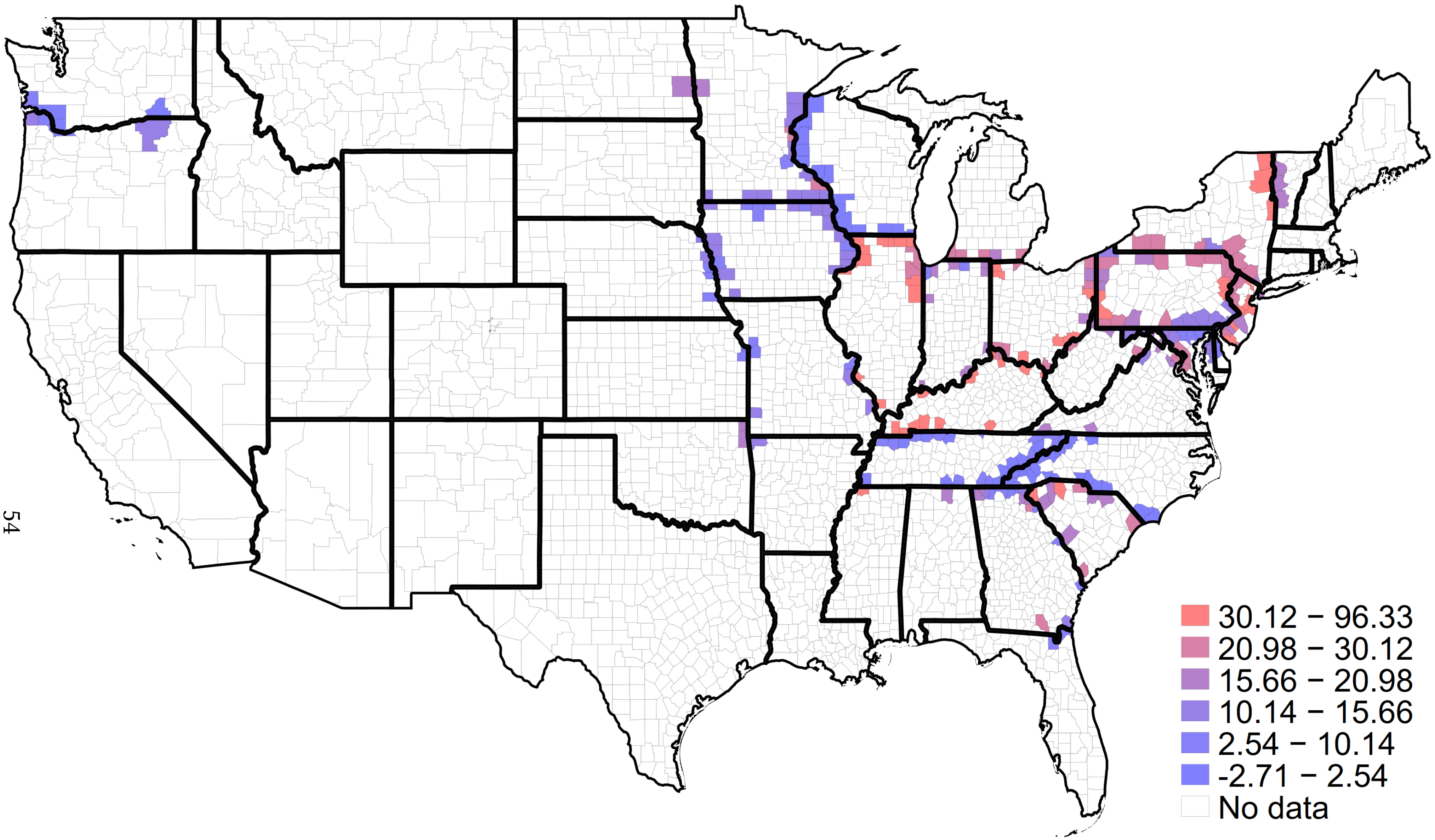

Figure A.1. State-Level Shortfalls by County This figure presents the average (mean) pension shortfall in thousands of dollars per property by county over the whole sample period (2002-2018) for all transacting properties included in our regression specification in Table 4. Note, pension shortfalls only vary at the state-year level, but since the number of transactions per county isn't fixed each year there will be variation even within state in the way in which they are presented in this figure based on the implicit time-varying weights.