The Perceived Value of Pension Funding: Evidence from Border House Prices*

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Abstract

We study the effect of state pension windfalls on property prices near state borders, where theory suggests real estate should reflect marginal residents' perceived value of additional public funds that relax the economic burden of substantial pension shortfalls. We find that one dollar of plausibly exogenous variation in pension asset returns increases border house prices by approximately two dollars. This implies that residents anticipate considerable value-enhancing public spending or reduced distortionary taxation driven by these windfalls. Our analysis of government expenditures suggests windfall-induced differences in the current provision of public goods plays a central role in our findings.

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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]

Underfunded public pensions in the U.S. represent an implicit household liability larger than auto loans, student debt, and credit card balances combined,[2] but little is known about how residents perceive the economic burden of these fiscal deficits. There are several channels that could affect this perception. Pension shortfalls could lead to reductions in the provision of public goods and services as governments balance their budgets, which the public is likely to notice and may also view as a harbinger of future problems.[3] Alternatively, residents may view net fiscal liabilities as a disciplining mechanism for politicians, preventing them from misusing the marginal funds made available by better funded pensions (e.g., Jensen, 1986; Stulz, 1990). In the absence of declines in the quality of government services, the public may not even be aware of the local government's financial condition.[4] Indeed, pension obligations are long-dated, uncertain, hard to measure, and driven by a complex political process that may be overlooked by more present-biased (e.g., Laibson, 1997) or boundedly rational (e.g., Rauh, 2016) residents. Regardless of which channels are at play, the perceived value residents place on the condition of government finances is likely an important driver of current household responses and future political reactions to fiscal deficits.

To estimate the perceived value of pension funds, we implement a novel empirical strategy motivated by a theoretical model of fiscal deficits in the presence of financing or spending inef-

[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] State and local pensions reported $1.38 trillion in 2015 unfunded liabilities, but according to Rauh (2016), who discounts cash flows reflecting the low risk of pension promises, the accumulated deficit is $3.85 trillion. According to the Federal Reserve Bank of New York, as of the fourth quarter of 2020, outstanding student loan, auto loan, and credit card debt are $1.55, $1.37, and $0.82 trillion, respectively, totaling $3.74 trillion.

[3] Shoag (2010, 2013) demonstrates large and immediate government spending responses to pension asset returns.

[4] Evidence from web searches suggests that at least some residents are interested in this issue. Figure 1 shows a correlation of 0.65 between state pension underfunding and state-level Google search intensity for "public pension" and "pension crisis."

ficiencies. In an open economy, where capital and labor are mobile but real estate is not, house prices reflect the marginal residents' perceived value of pension "windfalls".[5] This perceived value could come from reductions in the economic burden caused by inefficiencies or deadweight loss that would have otherwise been generated in honoring these obligations (e.g., Oates, 1969; Bradford, 1978; Kotlikoff and Summers, 1987; Harberger, 1995). Conversely, if all forms of capital face a high cost of relocation, then the marginal economic burden is unclear and the price of any individual asset is unlikely to reflect the total perceived 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. This approach is similar, in spirit, to papers looking at school district borders and house prices to estimate the value of school spending (e.g., Bayer, Blair, and Whaley, 2020). Since the level of pension funding may be affected by local economic conditions and the endogenous response to past contributions or investment decisions, we combine our analysis of state borders with plausibly exogenous variation in pension asset returns that cause windfalls (e.g., Shoag, 2010, 2013) to identify causal estimates of the perceived value of pension funding.

Our main findings illustrate a consistent pattern: changes in pension deficits pass through to house prices in border counties and reflect substantial perceived benefits of exogenous improvements in the funding status of public pension plans. We estimate that the marginal home buyer is willing to pay approximately two dollars for each net dollar of additional pension funds per property. For comparison, a broad existing literature has tried to estimate what has been called the "marginal value of public funds" (MVPF) – which looks at the aggregate willingness to pay for one dollar of net cost of investment for a *particular policy*. In a meta study of 133 of these policies, Hendren and Sprung-Keyser (2020) find that average MVPFs are typically between 0.5 and

[5] We define pension windfalls as exogenous reductions in net shortfalls that are *not* driven by differential contributions that reduce other accounts and neither require spending reductions nor higher taxes.

2, but exceed 2 for high-value projects. We observe a very high willingness to pay for improved pension-related fiscal conditions, consistent with other highly valued projects such as education, healthcare or infrastructure (e.g., Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020). This is intuitive because pension windfalls represent non-earmarked fiscal improvements that may be utilized on the highest marginal value projects.

Does this result imply awareness of pension funding status on the part of households? Not necessarily. Even if residents have no knowledge of state pension health, plan asset returns may be observable through their effect on government spending.[6] Accounting for the different government spending across high- and low-windfall states reduces the marginal value per dollar by approximately 50%, consistent with the majority of our effect coming from residents responding to the direct effects of different government spending trajectories. Therefore, results may largely reflect perceptions of the value of easily observable spending differentials (e.g., building a school) and does not require hyper-rational agents constantly aware of or monitoring current pension funding levels.

We face two major empirical challenges to establish a causal link between shocks to pension assets and their perceived value. First, where businesses and individuals cannot easily move, the benefits of spending or the cost of increased taxes associated with pension funding shocks 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 much lower cost of relocating to a state with a smaller pension shortfall.[7] This allows us to measure the perceived value of pension funds in areas where landowners are expected to bear the brunt of any changes

[6]Similar to Shoag (2013), we find meaningful differences in spending related to pension windfalls.

[7]According to Rauh (2016), state pension plans account for $4.05 trillion (84%) of the $4.80 trillion total reported pension liability, so our analysis captures most of the U.S. public pension burden. City and county borders would also be natural settings if not for a lack of data on local government pensions, except for the largest municipalities, that precludes our empirical strategy which requires information on both sides of borders.

to taxes or the provision of government services.

Our focus on state borders is one of the key features that distinguishes our paper from other 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; Howard, 2020). 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 value of pension funding. 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.[8]

Second, pension funding 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 generous 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 pension funds, 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 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 benchmark returns implied by asset allocations are all associated with increased house prices. To quantify the effect of pension shortfalls, we calculate cumulative dollar pension returns based on 2001 pension assets and find a

[8] In an analysis of 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 various forms of capital in a way that depends on their relative mobility and elasticities and is likely to vary across regions.

pass-through of approximately two – for each dollar of pension asset returns, house prices increase by approximately two dollars. Nonparametric 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, we consider asset returns between 2002 and a property's sale year, instead of using the same return horizon for all houses, and find a similar pass-through of approximately two. The benefit of this approach is that it 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 estimate. This is not surprising as requiring repeat sales on a property moves the average transaction date forward in time, leaving less time on average between 2002 and the sale. 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 has little effect on the overall pass-through in the repeat sales sample, which suggests that unobservable time-invariant factors are not biasing our estimates.

To support our focus on windfalls from pension returns instead of the level of pension shortfalls, we provide evidence that pension investment returns "crowd out" pension fund contributions. Specifically, we show within our research design that each dollar of excess pension returns is associated with a 55 cent reduction in contributions. This estimate is very similar to the 52 cent reduction in long-run contributions estimated by Shoag (2013), who also finds similarly large long-run reductions in government spending. This may explain the view reflected in Anzia (2022) that "local governments are not responding to rising pension costs by increasing revenue; they are instead shrinking their workforces and reducing capital outlays." These findings highlight both the benefits of our empirical strategy, which would be biased by a focus on the level of shortfalls, as well as the increases in spending facilitated by improvements in pension funding.[9]

Municipalities often face constraints on increasing taxes, which not only appear to affect their spending (e.g., Rodden and Wibbels, 2010), but also explain how state pension fund losses could increase financial constraints. The latter effect is reflected in the aforementioned spending responses as well as increases in municipal bond credit spreads (Novy-Marx and Rauh, 2012). Not surprisingly then, we find that our effects are concentrated in financially constrained municipalities. This concentration is consistent with an interpretation of such locales having what are expected to be high-value projects once they are able to obtain additional funds. These results, and evidence of a similar fiscal multiplier out of windfalls as out of other non-military plausibly exogenous spending (Shoag, 2013), might even suggest a more general applicability of our findings among locales with severe fiscal deficits, even outside of pensions.[10]

To shed light on the economic channels driving our results, we turn our attention to the quality of government amenities. As a first step, we observe 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?

We address this question by adding time-varying local rental prices, which should reflect the quality of current but not future amenities, to the set of control variables. This addition reduces

[9]This is also illustrated by a quote from the Philadelphia Inquirer (5/2/2018) featured in Shoag (2013) and noting that "high [pension fund] returns enabled [the Pennsylvania State Employees' Retirement System] to slow the expected increase in the state's payroll contribution [to their pensions]".

[10]Importantly, these results also highlight that the willingness to pay for exogenously better funded pensions does *not* imply that endogenous increases in pension funding would be value-maximizing. Residents of municipalities in poor fiscal condition are not necessarily better off if they reduce pension shortfalls by cutting already underfunded school spending. In this respect, while our work is consistent with findings that household financial decisions and real estate values are associated with pension salience or reforms, since these are associated with municipalities cutting spending to reduce pension deficits (Fan, 2020; Zhang, 2021), such designs are unlikely to recover our primitive of interest. For example, while exogenous windfalls are likely to increase potentially valuable spending, endogenous shortfall reductions via increased contributions would be expected to do the opposite.

the pass-through estimates only slightly when windfalls are measured over the entire sample period. This suggests that the economic burden of pension shortfalls primarily reflects expectations about *future* increases in taxes or decreases in the quality of amenities rather than a lower perceived value of current amenities. As an alternative lens on this issue, we add controls for actual government spending and find that this reduces our coefficient of interest by approximately 50%. Combined with the rental price result above, these findings suggest that homeowners are sensitive to the trajectory of public good provision and extrapolate from changes in spending even if the quality of amenities is not immediately impacted.

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, 2014), the political economy of pension funding (Brinkman, Coen-Pirani, and Sieg, 2018; Myers, 2021), and the impact of pension funding on municipal borrowing costs (Novy-Marx and Rauh, 2012; Boyer, 2020), the precautionary savings of households (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 perceived cost of this future economic burden.

Finally, our results have implications for the political economy of public sector employee compensation. Fitzpatrick (2015) estimates that public school employees in Illinois are willing to pay 20 cents on average for a one dollar increase in the present value of retirement benefits. We find that households perceive substantial deadweight loss associated with addressing pension shortfalls. In combination, these findings question the efficiency of governments promising generous retirement benefits to employees who do not value them and imposing the burden of funding those promises on households who view them as costly.

The remainder of the paper is organized as follows. Section 1 presents a model of the marginal value of pension funding and real estate values in a small open economy to motivate our empirical analysis. Section 2 describes our data on public pension funding and house transaction prices. Section 3 explains our identification strategy. Section 4 presents the main results. Section 5 concludes.

1 Theoretical Framework

In this section, we present a theoretical framework that motivates our research question and identification strategy. We first show that in an open economy, under certain conditions, landowners of a state providing a net marginal spending (subsidy) to a domestically mobile factor fully reap the benefit of this net spending, motivating our empirical design.[11] 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.

1.1 Incidence of net marginal spending in an open economy

Using similar arguments as Harberger (1962), one can show that in a closed economy, unsubsidized factors always reap some benefit of the net marginal spending if the subsidized factor's supply (demand) is not perfectly inelastic (elastic).[12] Relaxing the closed-economy assumption, most studies argue that in an open economy, immobile factors reap most, if not all, of the long-

[11]We show our analysis for a net marginal change in spending, but identical results hold in the case of the burden of imposing a marginal tax (a net marginal change in revenue).

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

run benefits of a net marginal spending in the economy due to capital mobility across borders.[13] Thus, it is critical for our empirical design to focus on an 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 must be equalized across states: a net marginal spending provided to capital owners in a state is not fully reaped by the capital initially located in the state providing this spending. In contrast, landowners in the two states are differentially impacted: there is a gain of rental income in the state providing the net marginal spending to capital and a loss in the other state.

If the spending-providing state is small and capital is perfectly mobile in a one-good economy (or under alternative assumptions discussed in Appendix A.2), we can summarize the main takeaway of the open-economy model in Proposition 1.

Proposition 1. *In an open economy, the immobile factor in a state reaps the entire benefit of any net increase in marginal spending that the state provides, even if it is on the domestically mobile factor.*

Proof. See Appendix A.3. □

Imagine, for example, if marginal public spending were increased for previously underfunded schools, generating a substantial surplus for the area. In a closed economy, some of this spending

[13]Notable examples that study tax incidence in open economies 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.

might lead to an increase in equilibrium teachers' wages, depending on their negotiating power, or implicitly the relative elasticities of labor demand and supply relative to, say, land. In a small open economy, e.g., just across the border from another state, however, labor market forces would force teachers' wages to be equal to those just across the border in the other state, where wages did not move. Since teachers could not "capture" any surplus by getting higher wages, the benefits would pass through entirely to homeowners. One would expect the value of these homes to rise, reflecting the perceived marginal value of increased school quality, without any of that "negated" by a rise in the cost of teachers' wages. While it is outside the model, such "negation" or "dispersion" of the surplus in a closed economy would become even more difficult to account for with many forms of immobile factors, highlighting our need to focus on real estate and settings where most factors are much more mobile than land, such as near state borders.

1.2 Pension shortfalls, economic burden, and property values

The previous section establishes that an open economy is the appropriate setting for our empirical analysis. In this section, we study the capitalization of future net 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 reduction in net public spending is affected by changes in asset prices due to the discounted present values of future changes in public revenues and expenditures. 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-price approach to tax burden presented in Poterba (1984). The key component of the burden is the price change for existing real estate due to the change in the present value of future reduction in net spending 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. $R(H)$ represents the marginal benefit of housing services.

Households consume housing services until the marginal value of these services equals their marginal cost. We assume all houses incur depreciation at a constant rate δ per period, maintenance costs equal to a fraction κ of the current value, and property taxes at a rate μ. All households face a marginal income tax rate τ, can deduct property taxes from taxable income, and can borrow and lend at the nominal interest rate r. The 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 net cost on each household that takes the form of a lump-sum payment to cover the unfunded pension liability L_t in period t. The government reduces net spending (by raising revenues Y_t and/or reducing expenses E_t) to cover the pension liability.[14] We assume the reduction in net spending induces a deadweight loss,[15]

$$-(E_t - Y_t) = L_t + f(L_t), \tag{2}$$

[14] For instance, raising revenues can be in the form of imposing taxes and cutting expenses can be in the form of reducing the public provision of goods, services, and other amenities.

[15] For example, if reduction in net spending is through raising revenues, this could represent the distortionary effect of taxation. If it is through cutting expenditures it could be an inefficient reduction in valuable public investments.

where $f(\cdot)$ is an increasing and convex function, representing the deadweight loss.[16] This means that to fund each additional dollar of pension liability in period t, the state has to raise more than one dollar net revenues.

Because houses are durable assets, future costs can still depress prices today. In each period when the net cost is imposed, the equilibrium condition (1) becomes

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

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

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

Iterating Equation (4) forward and applying the no-bubble condition,[17] the assumption of distortions from net spending reduction in (2) gives

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

The second term in Equation (5) is the present value of current and future net costs imposed 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 value of pension obligations will have lower house prices today.

[16] For example, one can assume a quadratic functional form for $f(\cdot)$ to represent the distortion from raising revenues (e.g., Lucas and Zeldes, 2009).

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

If the stock of housing is fixed[18] (i.e., $H_{t+j} = H_t$ for all j), then from Equation (5) we can determine the impact of an unfunded liability j periods ahead on house prices today:

$$\frac{dq_{H,t}}{dL_{t+j}} = -\frac{1 + f'\left(L_{t+j}\right)}{(1+\eta)^{j+1}} < 0. \tag{6}$$

With reasonable parameter values for income and property tax rates, depreciation, and maintenance 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 is and how far in the future the tax is imposed. We summarize the main message in Proposition 2.

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

Proof. See Appendix A.3. □

2 Data

2.1 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 the state level and 76 administered locally. This sample covers 95% of public pension membership

[18]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 housing stock adjustments can mitigate the effect of taxes on house prices. See Poterba (1984) for more details.

and assets nationwide.[19] 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 the plan-level pension shortfall defined as the actuarial accrued liabilities less the market value of 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.

2.2 Detailed investment data by asset class

The PPD includes detailed annual data on each plan's specific asset allocations, returns by asset class, and the associated benchmark returns. The asset classes in the PPD are based on the categories reported by plans. We use these data to calculate the cumulative pension plan returns used as instruments for pension shortfalls.[20]

Table 1 reports descriptive statistics on the PPD data. On average across time and funds, the largest asset holdings were equities and fixed income (53% and 28% of total assets, respectively), followed by real estate and private equity (6% and 5% of total assets, respectively). The value of assets is 79% of the actuarial value of liabilities for the mean observation, indicative of substantial underfunding. Appendix Figure A.1 shows that the average ratio of pension assets to liabilities declined from just above 100% in 2001 to 76.4% in 2019, reflecting an increase in underfunding over the period we study.

As discussed in Novy-Marx and Rauh (2011) and Rauh (2016), public pension liabilities are ef-

[19] 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.

[20] The pension return data in the PPD have been used in academic research by Lu, Pritsker, Zlate, Anadu, and Bohn (2019), among others.

fectively risk-free, so the appropriate discount rate for valuing them is the yield on a zero-coupon Treasury bond with the same duration. To discount pension liabilities using Treasury rates, we need to calculate the duration and convexity of each plan. Unfortunately, the information necessary for this calculation is unavailable in the PPD database prior to changes in pension reporting standards in 2014.[21] Therefore, to adjust the liability discount rate we use the aggregate adjustment factor in Rauh (2016) and inflate unfunded liabilities by a constant factor of 2.86.[22] While we acknowledge this is an imperfect adjustment method, any resulting bias would affect only our analysis of shortfalls and not our main analysis that exploits variation in pension asset returns.

2.3 Zillow transaction and assessment database

We obtain property-level data from 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 (2019), among others.

We filter the Zillow data in three ways. First we retain only residential property transactions for which the price of the transaction is verified by the closing documents as being between the typical home values in the the bottom and top market tiers as calculated at the county-month level

[21]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.

[22]In fiscal year 2014, the state and local pension systems in the United States reported aggregate unfunded pension liabilities of $1.19 trillion under GASB 67. Rauh (2016) applies a correction on a plan-by-plan basis that results in aggregate unfunded accumulated benefits of $3.41 trillion under Treasury yield discounting. This implies an average adjustment factor of 3.412/1.191 = 2.864.

by the Zillow Home Value Index (ZHVI).[23] Second, we focus only on single-family residences. Third, in our primary empirical analysis we restrict attention to properties located in counties sharing a border with an adjacent state and are located within 50 miles of the border.

3 Empirical Methodology

We examine the impact on real estate values near state borders because according to theory, they should reflect the perceived economic burden of shortfalls. Since real estate is effectively immobile, as detailed in Section 1, property bears the full brunt of inefficiencies in public capital raising in settings where other capital, consumers, and labor can easily move, such as near state borders. While prior studies have looked at the correlation between pension underfunding 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 economic burden of pension shortfalls. In addition, these earlier studies suffer from endogeneity in the determinants of shortfalls, which preclude a causal interpretation.

Therefore, 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:

$$PropertyValue_{it} = \beta\, PensionShortfallPerProperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}, \qquad (7)$$

[23]The ZHVI provides separate time series for the bottom market tier (33rd percentile and below of home values) and for the top market tier (67th percentile and above of home values), representing typical home values in these tiers. We impose an additional floor of \$30,000 on the bottom tier and an additional ceiling of \$2,000,000 on the top tier to avoid data quality issues. 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.

where $PropertyValue_{it}$ is the transaction price of house i and $PensionShortfallPerProperty_{st}$ is the estimated pension shortfall per property in state s, in thousands of dollars, in year t. γ_{bt} are border county pairs interacted with time fixed effects that allow us to compare properties transacting in physically adjacent regions, just across the state border from each other, in the same time period. This approximates the empirical design suggested by our theoretical framework for an open economy. D_i is the distance to the state border from the property's centroid. 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_l are location fixed effects that capture time-invariant differences by region and region interacted with property characteristics. These include zip code, zip code interacted with property characteristics, and property fixed effects. 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. Finally, $\boldsymbol{X_{lt}}$ is a vector of time-varying continuous economic controls at the state-year or county-year level.

Figure A.2 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.

Our theoretical framework suggests that the BDD on shortfalls is an improvement over existing work because of its focus on border regions. However, we still face endogeneity concerns similar 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, shocks to pension shortfalls that allow us to compare real estate transactions before and after the shocks. We therefore focus our analysis on pension asset returns, which cause immediate changes in unfunded pension liabilities driven by factors that are plausibly exogenous to state expenditures. We implement the same empirical design as Equation (7), substituting pension shortfalls with asset performance "windfalls:"

$$PropertyValue_{it} = \beta\, PensionWindfallPerProperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}, \qquad (8)$$

where $PensionWindfallPerProperty_{st}$ is the compounded cumulative return for the pension plans of state s from the beginning of the sample (2002) to the transaction date or interim period of interest (as explained in Section 4.1) 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 over that period. The regression coefficient β represents the economic burden, which is equal to one plus the deadweight loss, α, from our theoretical model in Equation (2). The economic interpretation is consistent with the pass-through in our theoretical motivation because one dollar lower windfall per property implies one dollar of additional pension shortfall per property.

We also consider two-stage least squares (2SLS) designs that recover the economic burden of pension underfunding while alleviating some remaining identification concerns. While our focus on asset returns in border counties reduces many concerns about endogeneity, it is still possible that pension funds' home or familiarity biases (Hochberg and Rauh, 2013) could induce mechanical relations between pension returns and local economic conditions. First, pension managers may buy shares in local firms so that when the local economy does well, both the pension assets and home prices appreciate (home bias). Second, pension managers may overallocate to industries or asset classes that are relatively abundant in a state, inducing a positive correlation between those industries, local economic conditions, and pension returns (familiarity bias). Conversely, pension funds may be used to hedge a state's fundamental risks, 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 estimate the following 2SLS regression:

$$\begin{aligned} PropertyValue_{it} &= \beta\, \widehat{WindfallPerProperty}_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}, \\ WindfallPerProperty_{st} &= \kappa\, ExWindfallPerProperty_{st} + \eta_{bt} + \psi D_i + \mu_l + \boldsymbol{\phi' X_{lt}} + \upsilon_{it}, \end{aligned} \tag{9}$$

where $ExWindfallPerProperty_{st}$ is an instrumental variable that exploits plausibly exogenous variation in pension asset performance. First, we instrument for pension returns using returns in excess of listed benchmarks, which mitigates the familiarity bias concern about the asset category composition of the pension portfolio. However, this first approach leaves open the possibility of home bias where outperformance of local firms drives excess pension returns and provides spoils for the entire state. To alleviate concerns of home bias, we instrument for pension returns using the returns of benchmark assets. To address both concerns simultaneously, we multiply allocations to asset classes that have less potential to be localized (i.e., bonds and equities, and funds that invest in them, rather than commodities, private debt, and real estate) by the relevant benchmark returns from all pensions in the country.[24] In this setup, returns should be unrelated to both local economic conditions and state governance.

4 Results

4.1 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. Our analysis follows the baseline regression in Equation (8), including border county group by year fixed effects that effectively compare the property value at sale of houses in adjacent counties transacting in the same year but in states with different pension windfalls. The group by time fixed effects absorb local trends in economic activity. We control for income per capita at the state level to further alleviate concerns that differential trends in economic activity across the state border affect our estimates. Lastly, we include a continuous measure of distance to the border and a set of fixed effects that controls flexibly 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 property transaction prices from 2015 to 2018 occurring near state borders where one state had higher pension asset returns from 2002 to 2014 than the other. We focus on this specification for two reasons. First, unless homebuyers are perfectly rational and pay close attention to the evolution of pension funding ratios, short-term variation in asset values is unlikely to impact home prices.[25] Second, to the ex-

[24] Appendix Table A.1 details the asset classes reported in the PPD and delineates which are included in the restricted benchmark return calculations.

[25] Bayer, Blair, and Whaley (2020) find it can take several years for property values to reflect the underprovision of educational public goods.

tent that observable degradation or improvement in public amenities reduces perceived value of living in an area or that it operates as a signal about the financial position of the state government or the trajectory of the quality of life from residing there, these effects would likely accumulate over long periods of time.

In addition to residents observing and valuing direct effects of variation in government spending, the pass-through of pension performance to house prices could also reflect some amount of home buyer awareness of pension funding directly. However, it is only necessary that a subset of residents be aware of pension funding for it to have an impact on the housing market equilibrium. In support of this prerequisite, Figure 1 presents Google Trends data showing that internet search volume related to public pensions is higher in states with larger pension shortfalls. In particular, there is a correlation of 0.65 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 at some homeowners may be aware of the financial problems plaguing their state governments, especially in states with the largest shortfalls.

Table 3 presents formal evidence of how such concerns are reflected in property values. 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. We construct the independent variable of interest as the product of the cumulative pension portfolio return from 2002 to 2014 and the 2001 pension assets per property, which represents the dollar windfall per property. Column (1) reports a positive and statistically significant coefficient of 2.42, which suggests a rise of about two dollars in property values for each dollar of additional pension funding caused by state pension investment outperformance.

Our theoretical framework shows that the the coefficient on pension asset returns can be mapped directly to the perceived marginal value of additional pension funds. For instance, a coefficient of 2.42 suggests that the ceteris paribus marginal value of one dollar more in net pension funding is $2.42, implying a deadweight loss or inefficiency of $1.42. This is also equivalent to an implied economic burden or cost of $2.42 that is relaxed by $1 of additional exogenous pension funds. As discussed in Section 1.2, a pass-through larger than one is not surprising. In a related context, the effect of investment in public education on house prices is also estimated to be large, implying a pass-through in our framework of between one and two (Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020). In this light, our pass-through estimates are consistent with an underprovision of public goods or services, perhaps driven by severe underfunding of pensions, which is relaxed by higher returns on pension investments.

4.2 Addressing identification concerns

This section examines potential biases in the estimate presented above. As noted previously, the relative performance of pension assets still has the potential to be endogenously related to state-level outcomes due to familiarity or home bias. We work to alleviate these concerns by restricting variation in pension returns using an instrumental variables framework. An alternative concern with the above approach is that it relies on a single measure of pension windfalls for each state, which could be correlated with unobservable time-invariant state characteristics. We address this concern by constructing a time-varying measure of pension returns and employing property fixed effects. Finally, we present nonparametric estimates of the border discontinuity to ensure the results are not an artifact of our linear regression specification.

In the case of familiarity bias, invested asset composition could be driven by familiarity with the sectors prevalent in a region (e.g., timber in Minnesota), inducing a correlation between pension returns and local economic outcomes. Column (2) of Table 3 includes the same sample and control variables as column (1) but incorporates an instrumental variable for the pension windfall in the 2SLS specification of Equation (9) 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 restricts variation to relative outperformance within each asset class, rather than variation in allocation across asset classes or sectors. If familiarity bias were driving our results, then using excess returns should eliminate any composition effect on portfolio returns as long as the benchmarks are well specified. Column (2) reports a similar estimate for the economic burden (2.53) that is statistically significant with a strong first stage. This suggests that familiarity bias is unlikely to drive our findings.

However, this still leaves the possibility that home bias could be affecting our estimates. In this case, even within an asset class a pension fund might be more likely to invest in local firms (e.g., Minnesota equities in the Minnesota pension fund). To address this possibility, column (3) takes the pension portfolio composition and applies the benchmark returns of each asset class to calculate implied portfolio returns and reports a similar estimate of the economic burden (2.39). To simultaneously shut down the familiarity channel, in column (4) we collapse the benchmarks into major categories and omit niche asset classes to form our Restricted Benchmarks. Specifically, we restrict attention to assets that have less potential to be localized (i.e., bonds and equities, and funds that invest in them, rather than commodities, private debt, and real estate). Again, we find a similar estimate of the economic burden (2.39), suggesting little evidence of home bias in our primary specification.

One remaining concern with the evidence presented thus far is that it relies on purely cross-sectional variation, so any time-invariant differences across state borders that correlate with pension asset performance could confound identification. To help alleviate this concern, we adjust the independent variable of interest to be the cumulative return between 2002 and the transaction date of the property. This specification allows us to control for unobservable time-invariant confounds, but has a downside relative to our baseline model. Since the sample includes transactions with a shorter window over which pension returns are measured, the regression estimates could be attenuated if it takes time for pension performance to be reflected in property values. This is especially true when we require a house to have repeat sales, which mechanically tilts the sample towards earlier observations.

The first column of Table 4 replicates the regression in column (1) of Table 3 using the rolling measure of cumulative pension returns. This specification yields a positive and significant coefficient of 2.16, quantitatively similar to our baseline estimate. The point estimate is slightly lower in this setup, perhaps reflecting the attenuation bias discussed above.

After establishing similar findings with the rolling measure of cumulative returns, we explore whether time-invariant confounds are biasing our estimates. One possibility is that property values are correlated with 2001 pension assets in a manner unrelated to pension shortfalls (e.g., generous pensions are associated with better or worse public amenities). To address this, we instrument for windfalls using only the public benchmark returns (not multiplied by initial assets per property) from our most restrictive specification in Table 3 (i.e., the first stage is a regression of dollars on returns). Column (2) of Table 4 reports a coefficient estimate based on this approach that is slightly lower (1.83) but similar to column (1).

Next, we restrict attention to properties with repeat sales and add property fixed effects to rule out the possibility that other unobservable time-invariant local factors affect our results. In column (3), we focus on the sub-sample of properties with repeat transactions during our sample period, requiring at least four years between transactions. Unsurprisingly, since this sample allows even less time for property values to reflect pension performance, the coefficient estimates are lower than the full-sample estimates. More importantly, we obtain nearly identical estimates after adding property fixed effects in column (4), which suggests that time-invariant omitted variables at the state, local, and property level do not bias our estimates of the economic burden.

Finally, we show that our findings are driven by neither the construction of windfalls per property nor the functional form of the BDD. In Appendix Tables A.2 and A.3, we present coefficients with the same sign and statistical significance using a simpler specification that focuses on cumulative pension returns without scaling by 2001 pension assets.

We apply this simple form of variation to confirm our main result in a nonparametric border discontinuity design. For each border pair, we determine 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, restricting attention to properties within 20 miles of the border. We estimate the following regression to obtain a vector of coefficients that reflect the total sales price increase for a house that trades in each one-mile bucket on either side of the border:

$$HousePrice_{it} = \sum_{k=1}^{20} \beta_k \, Treated_{st} \times \mathbf{1}(Miles_i = k) + \gamma_{bt} + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}. \tag{10}$$

Figure 2 plots the coefficients for five miles on either side of the border. Circular dots represent the β coefficient estimates, diamonds are the differences between the treated and untreated coefficients, and 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 outperformance. This is consistent with our predictions and suggests that our findings are not driven by the functional form assumptions of the BDD.

4.3 External validity

Since our analysis restricts attention to a subset of the housing market near state borders, it is worthwhile to assess whether our estimates are likely to apply more generally. As explained above, we focus on state borders because theory suggests that the burden of addressing pension shortfalls should accrue to real estate when labor and physical capital can be relocated to another state at low cost. In contrast to prior work on pensions and house prices, our primitive of interest is the economic burden of pension shortfalls, not a more general average effect on house prices that can be observed across all counties. As we move further away from state borders, the cost of moving other types of capital increases, which disperses the pension burden among other forms of capital and precludes us from making clear predictions about the effect on house prices.

Along these lines, Appendix Table A.4 reports a much smaller, but statistically significant, coefficient when applying our main specification to interior counties.[26] Since we cannot recover the coefficient of interest directly in interior counties, we evaluate whether there is something different about border counties by comparing the observable characteristics of interior and border counties. Our estimates reflect the deadweight loss associated with raising funds or cutting amenities to address pension shortfalls, so we focus our comparison on differences in local government finances and costs of fundraising across these regions. Appendix Table A.5 shows that border counties are similar to interior counties on these dimensions. This analysis uses local government financial data aggregated to the county level by the U.S. Census Bureau for fiscal years 2007 and 2012. We make statistical comparisons for 15 different financial measures in these two

[26]We use a linear specification in column (1) of Appendix Table A.4 to reveal a statistically significant decline in the coefficient of interest based on distance to the border, suggesting a diffusion of the burden across other forms of capital that precludes identification in interior regions. We also show a larger economic burden when separately estimating effects in border (column 2) relative to interior counties (column 3) with the same specification. For counties internal to a state, we impute the county border group to which it belongs by finding the county border group of the county whose centroid is closest to its own centroid.

years and find that only four out of 30 differences are statistically significant at the 10% level, none of which hold across both observation years for a given ratio. This suggests that border counties are fairly representative in terms of their financial position.

Nevertheless, to examine whether the observed differences in county characteristics are correlated with the estimated economic burden, columns (1)-(5) of Appendix Table A.6 reproduce our main specification using weighted least squares regressions in which the weights are chosen such that border counties match interior counties on each characteristic.[27] The results of this approach are identical to those of column (1) in Table 3. Additionally, in column (6) of Appendix Table A.6 we replace the county border group structure of the data with a matched county pair structure wherein each county is matched to it's nearest neighbor (with replacement) along a pair-wise Mahalanobis distance calculation utilizing our 15 different financial ratios plus the population of that county. In addition to providing another flexible control for any potential differences across counties, the similarity of our findings with this approach also reduces concerns about any possible substantial bias induced by "reflection" problems which could potentially arise if there are spillovers across borders. In sum, the evidence in Tables A.5 and A.6 suggests that our estimates of the economic burden are likely to apply more generally.

Although modeling the general equilibrium implications of our findings is beyond the scope of this paper, a simple linear aggregation highlights the overall magnitude of the economic burden imposed by pension underfunding. As noted in the introduction, Rauh (2016) estimates that the unfunded portion of U.S. state and local pension promises exceeds $3.8 trillion. Our estimated economic burden of approximately two implies a deadweight loss of approximately one dollar per dollar of shortfall. Since there are about 121 million households in the United States,

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

the 95% confidence interval around the estimate from column (1) of Table 3 corresponds to an average deadweight loss of between \$25,611 and \$63,422 per household, or between 37% and 92% of median household income.[28]

4.4 Crowding out contributions: The shortfall of shortfalls

Although we have motivated the use of a border discontinuity design, we have not fully explained why we use windfalls from variation in pension returns rather than the level of pension shortfalls as the explanatory variable of interest. As a starting point, it is important to note that there is an inverse relation between windfalls and shortfalls that must hold instantaneously. By definition, an additional dollar of assets reduces the net pension shortfall by one dollar. However, at longer horizons the change in the pension funding ratio in response to an exogenous one dollar windfall depends on whether the state reduces pension contributions in response. This "crowding out" between windfalls and contributions would lead observed shortfalls to fall by less than one dollar after a one dollar windfall in equilibrium, since the state responds by contributing less to the pension fund than it otherwise would have.

For direct evidence that the observed pension shortfall is an equilibrium outcome, Appendix Table A.7 shows that pension shortfalls are positively correlated with contributions to the pension system by both the state and its employees. If pension fund outperformance leads to a reduction in contributions and a shift in government spending to value-improving projects, then even a 2SLS regression that instruments for shortfalls would understate the effects of pension funding. On the other hand, if such expenditures are value-destroying, the same regression would be biased upwards. Ultimately, this is an empirical question that demands variation in pension funding that is unaffected by the substitution between pension contributions and local government expenditures

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

and the relative value of those expenditures.

While it does not recover the economic primitive of interest, we can learn something interesting about crowding out and the benefits of our empirical design by considering windfalls as an instrumental variable for the observed level of pension shortfalls in the following 2SLS regression:

$$
\begin{aligned}
PropertyValue_{it} &= \theta \, \widehat{ShortfallPerProperty}_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}, \\
ShortfallPerProperty_{st} &= \phi \, WindfallPerProperty_{st} + \eta_{bt} + \psi D_i + \mu_l + \boldsymbol{\rho' X_{lt}} + \upsilon_{it}.
\end{aligned}
\tag{11}
$$

Relating this system of equations to the system in Equation (7), the economic interpretation of the first-stage regression here is that $1 - \phi = 1 - \beta/\theta$ represents the crowding out per dollar of windfall. If there is no crowding out, then $\phi = 1$ and $\beta = \theta$, and the second-stage estimates are equal whether we use the windfalls or shortfalls as the explanatory variable of interest.

Table 5 presents estimates of Equation (11). Column (2) reports the first-stage regression, in which the endogenous variable is the observed net shortfall per property and the instrumental variable is windfall per property coming from pension asset returns. The coefficient of −0.45 indicates that each dollar of windfall causes the equilibrium shortfall to fall by about 45 cents. Since the shortfall must fall instantaneously by one dollar, this means that pension contributions are reduced by 55 cents for each dollar of windfall. This estimate of the crowding out is similar to those found in Shoag (2013).

While this result is interesting on its own, the comparison between columns (1) and (3) is more important for understanding our empirical strategy. For ease of comparison, column (1) reproduces the same estimate of Equation (8) reported in Table 4, which is based on pension windfalls due to asset returns. Column (3) presents the second-stage coefficient from Equation (11), based on the level of pension shortfalls. The respective coefficient estimates of 2.16 and 4.82 would correspond to vastly different implications for perceived economic burden of pension funding, but the latter estimate is contaminated by the crowding out effect documented above. Mechanically, the ratio of these estimates is equal to the first-stage estimate from column (2), which means the bias from using the level of shortfalls in this analysis is increasing in the degree of crowding out. This shows that even if we instrument for the level of shortfalls using plausibly exogenous variation due to windfalls, we would obtain an upward-biased estimate of the economic burden with this (incorrect) approach because states contribute less to their pension funds when the funds' investments are performing well.

4.5 Drivers of the perceived marginal value

4.5.1 Current versus future benefits

In the preceding analysis, we estimate a pass-through of approximately two between the public pension windfall per property and the value of houses near state borders. Since we measure pension asset returns over long horizons – for the properties transacting at the end of the sample, a 16-year period – the channel through which pension windfalls affect house prices is ambiguous. After a long period of disappointing returns, states with large shortfalls may raise taxes or shift the allocation of tax revenues away from public projects to make pension contributions. If this substitution were at play, then the estimated pass-through could reflect a combination of worse current amenities and higher future liabilities or worse amenities.

Columns (1) through (2) and (4) through (5) of Table 6 attempt to distinguish between the present and future channels by controlling for local rental prices. Renters have the same ability as homeowners to enjoy public amenities, but they are unaffected by the capitalization of future costs and benefits accruing after the term of the rental. If the estimated effect of pension shortfalls on house prices is driven by worse current amenities, then pension shortfalls should also affect rental prices. If instead the effect is driven by expectations about the future costs of addressing shortfalls, then current rental prices should not show the same effect.

Column (1) of Table 6 reproduces our main result and column (2) adds a control for rental prices for similar properties in the same county in the same year.[29] The estimated economic burden is 23% lower after we control for rental prices. According to the logic outlined above, this implies that 23% of the estimate is due to worse current amenities and the other 77% is due to expectations of worse future net benefits associated with addressing pension shortfalls. This could reflect reductions in future spending or declines in current spending that may not accrue till the future (e.g., many forms of school spending). The role of amenities is consistent with the evidence on public service provision in Appendix Table A.7.

In contrast, we find virtually no difference in the estimated burden between columns (4) and (5), which add property fixed effects to control for unobservable time-invariant factors. This analysis restricts attention to the sample of repeat sales, which as we note above, allows for less time over which shortfalls can accumulate to impact housing prices. Together with the first two columns, these findings are consistent with pension shortfalls driving changes in current amenities, but these changes occurring relatively slowly. In sum, our findings suggest that the bulk of the perceived burden of pension shortfalls will be realized in the future, but current amenities are deteriorating in the later part of our sample period.

We next consider the extent to which pension windfalls are reflected in government spending. First, in unreported results we confirm prior research (e.g., (Shoag, 2013)) that exogenous shocks to pension funding result in changes to government spending. In Column (3) of Table 6, we

[29]It is worth pointing out that monthly rental rates are a "bad control" in the estimation of the economic burden. As we argue above, we expect some amount of the burden to pass through into current rental rates, especially over long intervals, which means the coefficient on pension windfalls would change. While the inclusion of rental rates is helpful in distinguishing the effect of windfalls on current and future (dis)amenities, it is unlikely to help us recover a more accurate estimate of the economic burden.

include deciles of total expenditures per capita at the county level in 2012 in our regression specification.[30] Our estimate in column (1) reflects the perceived value of improved pension funding from the perspective of residents. When column (3) controls for differential government expenditure patterns across counties, the estimate of this perceived value of pension funding drops substantially. Combined with the rental result above, we conclude that changes in spending do not immediately impact the current amenity "flow" of public good provision, but their trajectory impacts homebuyer's willingness to pay in a way that captures a substantial portion of the perceived value of pension windfalls. Or put differently homeowners are more concerned about deteriorating quality of public services/goods (e.g., schools) and what it means for the future, then short-term renters.

4.5.2 Role of financial constraints

As discussed above, our estimates reflect the perceived value of marginal net spending caused by improved exogenous increases in pension funds. If this is correct, then no matter how differential shortfalls are met, one would expect them to be most consequential for more financially constrained locales. These locales would be the most likely to have under-provision of high-value government spending on schools, healthcare, etc, as well as the most difficulty in raising revenue. In columns (6) through (8) of Table 6 we find exactly this. Large economic burdens of pensions shortfalls are concentrated in municipalities with high bond spreads (6), as well as evidence of ability and therefore issuance of long-term debt overall (7) and relative to county salaries (8).[31] These are consistent with effects being driven by locales with more constrained access to finance. Not only that, but the evidence presented previously that pension windfalls relax overall

[30] These data are county and municipal finances aggregated to the county level by the U.S. Census Bureau.

[31] We examine interactions with municipal financial constraints as early as possible in the sample to avoid any potential contamination from direct effects of pension shortfalls on fiscal conditions.

budget constraints and increase fiscal spending, when combined with direct evidence of effects concentrated in more financially constrained locales, could suggest that our findings might be even more broadly applicable. In particular, even though it is not the primary purpose of our paper, our findings would be consistent with high perceived marginal value to improvements in not just pensions, but local fiscal conditions more generally.

5 Conclusion

This paper estimates the perceived marginal residents' value of pension windfalls or equivalently an exogenous reduction in the economic burden of the trillions of dollars in pension shortfalls. We use plausibly exogenous variation in state pension funding based on excess asset performance to show that a one dollar reduction in the public pension shortfall per property causes an approximately two dollar rise 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 perceived value of the marginal improvement in pension funding. The consistency of our estimates across a variety of instrumental variables specifications supports a causal interpretation, while our evidence of concentrations in financial constrained locales points to the importance of fiscal conditions in significant economic burdens. Although we show evidence of some current reduced public investment in states with larger pension shortfalls, our analysis of rental rates suggests that the house price effects are mostly driven by future costs (taxes or lost amenities) rather than reductions in current amenities. Our findings are consistent with models of perceived inefficient taxation or the underprovision of high-value future public goods, services, or other expenditures. The estimated economic burden of pension shortfalls is comparable to previous estimates of the effect of public spending on infrastructure and public teacher salaries (e.g., Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020).

Finally, our results have implications for the political economy of public sector labor markets and local government finances. In light of prior evidence on public workers' low willingness to pay for retirement benefits (Fitzpatrick, 2015), our findings raise questions about the efficiency of public sector compensation schemes. Why do state governments offer generous retirement benefits to employees who do not value them while imposing the funding burden on households who view it as costly? We quantify the latter dimension of this problem by estimating a deadweight loss associated with addressing pension shortfalls of between \$25,611 and \$63,422 per household. However, our analysis takes the degree of pension underfunding as given. Though it is beyond the scope of this paper to explain the origins of the public pension crisis, we look forward to future research on this topic.

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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), private equity (PE), hedge fund (HF), commodities (Comd), cash, miscellaneous alternative assets (AltMisc), and other assets are shown in percentage terms.

Variable	Obs.	Mean	Std. Dev	Q5	Q25	Q50	Q75	Q95
Asset return	624	0.056	0.076	-0.086	0.010	0.060	0.115	0.163
Actuarial assets ($m)	624	13,947	13,469	1,445	5,135	8,612	18,082	44,358
Actuarial liabilities ($m)	624	17,577	15,908	2,217	6,522	11,880	24,174	52,007
Actuarial funded ratio	624	0.787	0.144	0.555	0.697	0.776	0.890	1.010
Equity share	624	0.529	0.094	0.360	0.472	0.538	0.598	0.663
FI share	624	0.279	0.077	0.180	0.226	0.268	0.317	0.412
RE share	624	0.055	0.038	0.000	0.022	0.059	0.081	0.110
PE share	624	0.053	0.051	0.000	0.007	0.041	0.082	0.146
HF share	624	0.040	0.056	0.000	0.000	0.013	0.064	0.154
Comd share	624	0.014	0.022	0.000	0.000	0.000	0.021	0.064
Cash share	624	0.017	0.019	0.000	0.004	0.012	0.023	0.053
AltMisc share	624	0.009	0.026	0.000	0.000	0.000	0.000	0.076
Other share	624	0.004	0.022	0.000	0.000	0.000	0.000	0.017

Table 2

Housing Transactions Summary Statistics

This table presents summary statistics for our sample of properties that merges 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 rates for single family residences with the same number of bedrooms as 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 property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI.

Variable	Obs.	Mean	Std. Dev	Q5	Q25	Q50	Q75	Q95
Sales Price ($ '000s)	712, 505	260	135	105	162	228	325	525
Transaction Month	712, 505	07/2010	54 Mos	11/2003	06/2006	06/2010	05/2014	08/2017
Border Dist (mi)	712, 505	16.9	10.7	2	8	16	26	35
Building Age (yrs)	629, 379	7.7	5.5	2	3	6	12	19
Sq Ft	603, 693	1, 813	793	800	1, 290	1, 655	2, 110	3, 800
Lot Sq Ft	657, 004	19, 020	77, 226	2, 000	4, 500	7, 500	14, 500	62, 000
# Bedrooms	492, 118	3.28	0.71	2	3	3	4	4
# Bathrooms	554, 187	4.31	1.30	2	4	4	5	6
Shortfall/Prop ($ '000s)	712, 505	21.28	16.10	0.54	9.19	17.68	30.71	46.84
02-14 Cum. Port. Ret. (%)	129, 940	145	23	90	137	141	161	167
02-14 Cum. Excess Ret. (%)	129, 940	12	16	−2	1	4	28	40
'02-14 Cum. Port. Ret. × '01 Assets/Prop($ '000s)	129, 940	27.02	22.16	11.48	16.19	16.19	25.64	78.23
Rental Price ($)	712, 505	1, 328	345	800	1, 073	1, 318	1, 567	1, 898
State-Year Income PC ($)	712, 505	44, 607	9, 195	32, 996	38, 103	41, 862	50, 035	63, 598

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 the sales price, in thousands of dollars, of a residential property. The explanatory variable of interest is based on invested assets' cumulative performance from 2002-2014 in the pension plans associated with the state in which the focal property is located, 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 property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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 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 baseline regression 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) instruments for Windfall using the initial assets per property in 2001 multiplied by the cumulative pension fund performance from 2002-2014 in excess of the benchmark performance for each asset class the fund is invested in. Column (3) instruments for Windfall using the initial assets per property in 2001 multiplied by the cumulative pension fund performance from 2002-2014 that would have occurred based on the fund's asset allocations, had it earned the benchmark performance for each asset class. Column (4) is the same as column (3), but restricts attention to assets that have less potential to be localized (i.e., bonds and equities, and funds that invest in them, rather than commodities, private debt, and real estate). Where applicable, we report the Kleibergen-Paap F-test for weak identification. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)			
	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
2002-2014 Windfall Per Property $('000s)	2.418***	2.529***	2.391***	2.389***
	(8.10)	(9.23)	(7.81)	(7.79)
Border Distance	X	X	X	X
State-Year Income PC	X	X	X	X
Border Group-Tran Year FE	X	X	X	X
6 Prop Chars FE	X	X	X	X
Instrumental Variable	—	Excess Ret. Windfall Per Prop	Bnchm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop
Observations	129,940	129,940	129,940	129,940
Adj. R^2	0.813			
Weak ID KP F Stat		312.5	83,047	68,209

Table 4

Rolling Pension Windfall Regressions and Repeat Sales

This table presents estimates from a state border discontinuity design model where the dependent variable is the sales price, in thousands of dollars, 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 assets per property as of 2001 (Windfall). The sample is restricted to property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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 and six interacted property characteristic fixed effect cells that control for property type (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 baseline regression described above. Column (2) instruments for Windfall using the cumulative pension fund performance from 2002 to the sale of the property that would have occurred based on the fund's asset allocations, had it earned the benchmark performance for each asset class, but restricting attention to securities that have lessened potential to be localized (i.e., bonds and equities rather than commodities, private debt, real estate) and funds investing in them. Column (3) is the same as column (1) but restricts to properties with repeat sales in the sample. 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, identification is based on within-property variation over time coming from repeat sales. Where applicable, we report the Kleibergen-Paap F-test for weak identification. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, 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)	2.162***	1.831***	1.481***	1.425***
	(8.01)	(4.26)	(5.95)	(5.15)
Border Distance	X	X	X	
State-Year Income PC	X	X	X	X
Border Group-Tran Year FE	X	X	X	X
6 Prop Chars FE	X	X	X	
Repeat Sales Sample			X	X
Property FE				X
Instrumental Variable		Restr. Bm. Return		
Observations	712,505	712,505	54,882	54,882
Adj. R^2	0.857		0.853	0.913
Weak ID KP F Stat		43.03		

Table 5

The Shortfall of Shortfalls

This table presents estimates from a state border discontinuity design model where the dependent variable is the sales price, in thousands of dollars, 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 assets per property as of 2001 (Windfall). The sample is restricted to property transactions involving single-family residences in counties sharing a border with an adjacent state with differential pension funding that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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 baseline regression described above and replicates column (1) of Table 4. Column (2) is the first stage of the 2SLS regression detailed in Equation (11), where the endogenous variable is the observed net shortfall per property and the instrumental variable is windfall per property coming from pension asset performance. This regression demonstrates the the crowding-out effect of pension performance on fund contributions. Column (3) is the specification in Equation (11) and demonstrates that, because states contribute less to their pensions when they earn high returns, using equilibrium shortfalls leads to a biased estimate of the economic burden, even if shortfalls are instrumented with plausibly exogenous windfalls. Where applicable, we report the Kleibergen-Paap F-test for weak identification. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)	Shortfall Per Prop $('000s)	Sales Price $('000s)
	OLS	OLS	2SLS
	(1)	(2)	(3)
2002-Sale Windfall Per Property $('000s)	2.162***	-0.449***	
	(8.01)	(-7.98)	
Shortfall Per Property $('000s)			-4.816***
			(-5.73)
Border Distance	X	X	X
State-Year Income PC	X	X	X
Border Group-Tran Year FE	X	X	X
6 Prop Chars FE	X	X	X
Instrumental Variable	—	—	Windfall Per Prop
Observations	712,505	712,505	712,505
Adj. R^2	0.857	0.913	
Weak ID KP F Stat			63.7

Table 6

Pension Windfalls: Drivers of the Economic Burden

This table presents estimates from a state border discontinuity design model where the dependent variable is the sales price, in thousands of dollars, of a residential property. Columns (2) and (5) compare baseline estimates to the estimates after controlling for time-varying rental rates. Column (3) controls for 2012 county-level expenditures, per capita, flexibly as decile fixed effects. Columns (6) through (8) examine how the effect of pension funding on house prices varies with the difficulty of raising additional funds, as proxied by various measures related to municipal bonds. 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 for columns (1) through (3) and only for the years prior to the transaction since 2002 for columns (4) through (8), 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 property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. In columns (1) through (3) transactions are further restricted to those in the years 2015-2018. 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 throughout. Columns (1) through (3) also include a covariate for the distance to the state border and six interacted property characteristic fixed effect cells that control for property type (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Columns (4) through (8) include a property-level fixed effect to exploit within-property variation over time. Column (1) replicates column (1) of Table 3. Column (2) is the same as column (1) but adds a control for time-varying rental rates. Column (3) is the same as column (1) but uses 2012 county-level expenditure per capita decile fixed effects to control for cross-sectional government spending differences. Column (4) replicates column (4) of Table 4. Column (5) is the same as column (4) but adds a control for time-varying rental rates. In columns (6) through (8), the windfall is interacted with indicators for above median municpal bond spreads in the time period 2001-2003, the per-capita outstanding municipal bond volumes in 2007, and the per salary dollar outstanding municipal bond volumes in 2007, respectively, all at the county level. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2002-2014 Windfall Per Property $('000s)	2.418***	1.853***	1.051***					
	(8.10)	(7.50)	(3.80)					
2002-Sale Windfall Per Property $('000s)				1.425***	1.353***	0.737***	0.520**	0.585***
				(5.15)	(4.87)	(3.13)	(1.99)	(2.63)
Est. Mo Rent ($)		0.124***			0.0603***			
		(6.98)			(2.86)			
2002-Sale Windfall Per Prop $('000s) × 2001-2003 County Municipal Bond Spread, Above Med.						1.562***		
						(5.23)		
2002-Sale Windfall Per Prop $('000s) × 2007 County Long-Term Municipal Bond Outstanding Per-Capita, Above Med.							1.452***	
							(4.69)	
2002-Sale Windfall Per Prop $('000s) × 2007 County Long-Term Muni. Bond Outstanding Scaled by 2002 County Salaries, Above Med.								1.604***
								(5.74)
Border Distance	X	X	X					
State-Year Income PC	X	X	X	X	X	X	X	X
Border Group-Tran Year FE	X	X	X	X	X	X	X	X
6 Prop Chars FE	X	X	X					
2012 County Per Capita Expenditures, Decile FE			X					
Property FE				X	X	X	X	X
Observations	129,940	129,940	129,940	54,882	54,882	51,261	54,882	54,882
Adj. R^2	0.813	0.814	0.814	0.913	0.914	0.918	0.914	0.915

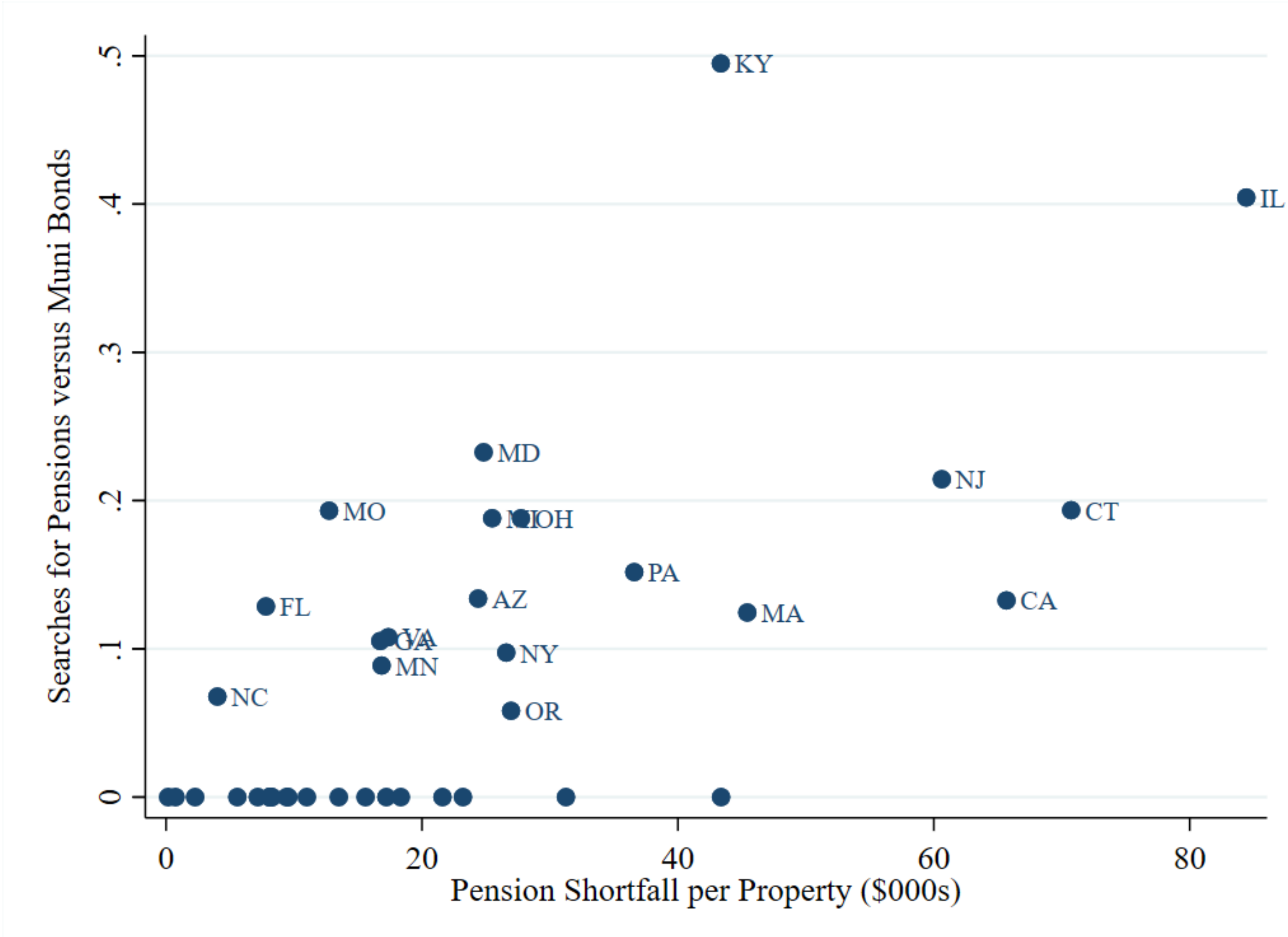

Figure 1. Popular Interest in Public Pensions from Google Search Trends This figure presents evidence on popular interest in the condition of public pensions using data from Google Trends. For each state in our regression sample, depicted in Figure A.2, we obtain monthly series of search trends for the terms "pension crisis" and "public pension" over the period January 2004 to December 2020. Google 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 0.0038 (t = 4.93) and the R^2 is 0.43.

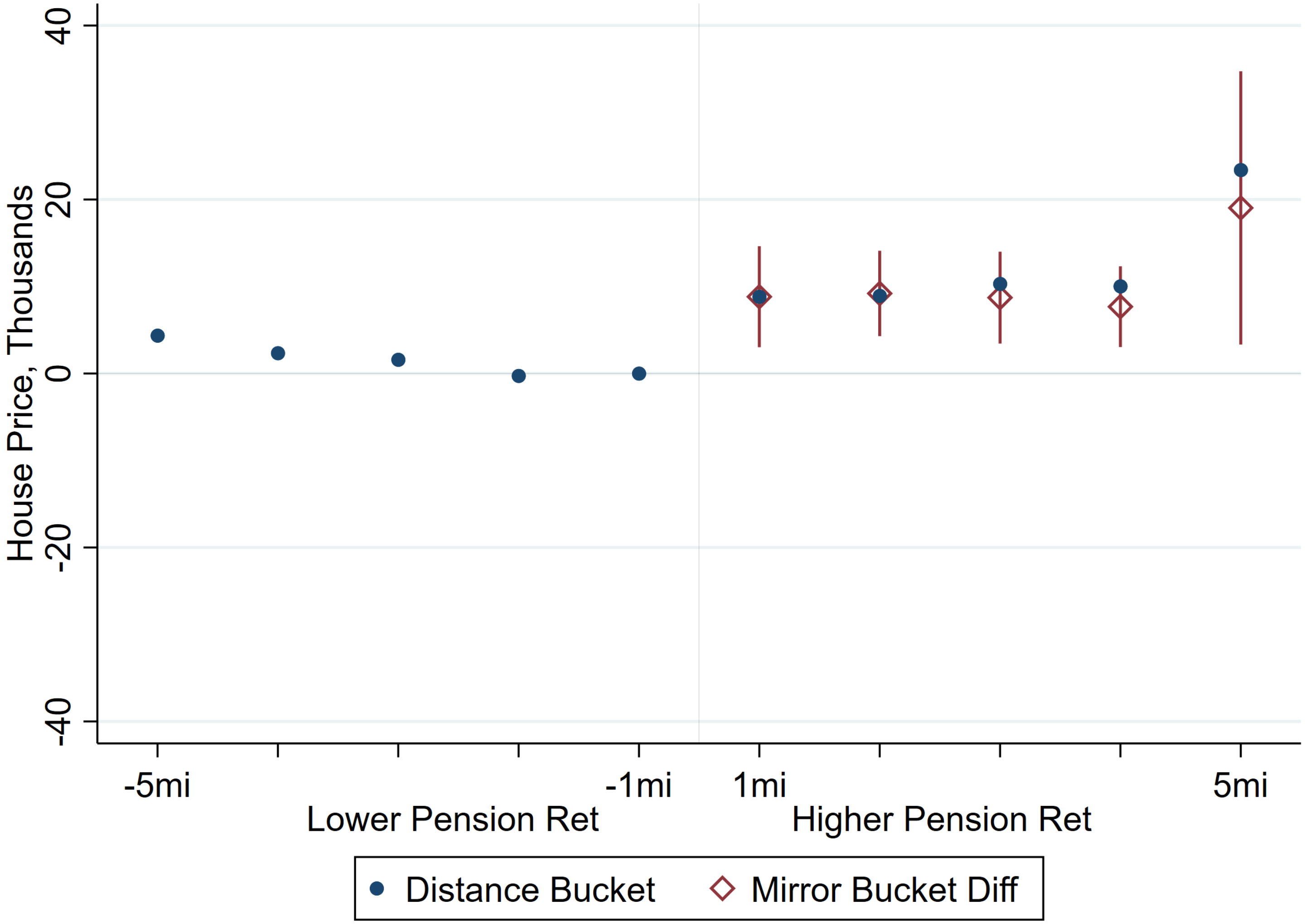

Figure 2. Pension Return Discontinuity in House Prices This figure presents nonparametric estimates of a border discontinuity design for house values near the borders of states with differing pension asset performance between 2002 and the sale date of the property. We plot the coefficients for the five miles surrounding each border in our sample, with blue dots representing the primary coefficient of interest in Equation (10). Red diamonds denote the difference between the coefficient estimates for properties in better performing states minus those for equidistant from the border properties in worse performing states. Red lines denote 95% confidence intervals for these estimates.

Appendix

A Details of the Model

In this section, we study the incidence a net marginal spending (subsidy) provided in closed and open economies. We show our analysis for the benefits of a net marginal spending, but identical results hold for the burden of a tax (net marginal revenue).

A.1 Incidence of a net marginal spending in a closed economy

Consider a closed economy in general equilibrium 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 $F_K > 0$, where subscripts K and L denote partial derivatives with respect to capital and labor, respectively. Suppose that the supply of capital, K, is perfectly inelastic in the short run, but the 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 $F_L(K, L(W/P)) = W/P$.

First consider the incidence of a net marginal spending at rate s on the rental rate of capital.

The left hand side of (A.2) becomes

$$PF_K = r(1-s).$$

Since it is perfectly inelastic in supply, capital reaps the full benefit of the net spending: its real rental rate r/P rises from F_K to $F_K/(1-s)$.

The results are different in the case of a net marginal spending at rate s provided to the elastically-supplied labor. Producers equate the marginal revenue product of labor to the cost of hiring labor after subsidy,

$$PF_L = W(1-s). \tag{A.3}$$

Equating supply and demand for labor in the subsidy equilibrium and taking the derivative with respect to s, we find that the percentage change in real wage W/P from an increase in s, evaluated at $s = 0$, is given by

$$\frac{\partial(W/P)/(W/P)}{\partial s} = -\frac{\eta^D}{\eta^S - \eta^D}, \tag{A.4}$$

where η^S is the positive elasticity of labor supply, and η^D is the negative elasticity of labor demand ($F_{LL} < 0$). The marginal increases of rents to labor, $(\partial(W/P)/\partial s)\,L$, and to capital, $(\partial(r/P)/\partial s)\,K$, as a ratio of the marginal subsidy expense, $(W/P)L$, can be written as

$$\frac{\frac{\partial(W/P)}{\partial s}L}{(W/P)L} = -\frac{\eta^D}{\eta^S - \eta^D}, \qquad \frac{\frac{\partial(r/P)}{\partial s}K}{(W/P)L} = -\frac{\eta^S}{\eta^D - \eta^S}. \tag{A.5}$$

Note that two expressions in (A.5) sum to +1: the full benefit of the net marginal spending accrues to 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 reaps the full benefit of the net marginal spending, i.e., the right hand sides of the expressions in (A.5) are equal to +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 reaps the full benefit of the net marginal spending. Importantly, although the spending is provided to labor, from Equation (A.5), capital always reaps some benefit of the subsidy if $\eta^S \neq 0$ and $\eta^D \neq \infty$. The larger (smaller) the supply (demand) elasticity of labor, the larger is the share of benefits accrued to capital.

A.2 Incidence of a net marginal spending 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 s is the net marginal spending (subsidy) to capital in state A, we have

$$F_K^A(K_A) = r - s; \qquad\qquad F_K^B(K_B) = r. \tag{A.6}$$

Using Equation (A.6) 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 subsidy expense, dsK_A, calculates at $s = 0$ equilibrium, is given by

$$\frac{(dr/ds)\,\overline{K}}{K_A} = \frac{\eta_A \overline{K}}{\eta_B K_B + \eta_A K_A} \geq 0, \tag{A.7}$$

where η_A and η_B are the nonnegative 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.7) equals +1 and countrywide capital, $\overline{K}$, reaps the full marginal benefit of the net marginal spending 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 reaps more than 100%

of the net marginal spending's benefit. At the opposite extreme, if capital demand is perfectly elastic in B ($\eta_B = \infty$) or in perfectly inelastic demand in A ($\eta_A = 0$), $\overline{K}$ reaps none of the benefit of the net marginal spending.

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

$$R_A = F^A(K_A) - (r - s)K_A; \qquad R_B = F^B(K_B) - rK_B, \tag{A.8}$$

implying[32]

$$\frac{dR_A/ds}{K_A} = \frac{\eta_B K_B}{\eta_B K_B + \eta_A K_A} \geq 0, \tag{A.9}$$

$$\frac{dR_B/ds}{K_A} = -\frac{\eta_A K_B}{\eta_B K_B + \eta_A K_A} \leq 0. \tag{A.10}$$

The intuition from Equations (A.9) and (A.10) is that landowners in state A providing the net spending gain rental income, while B's landowners lose. Note that the three subsidy benefits in Equations (A.7), (A.9), and (A.10) sum to +1. With identical production functions, landowners in state A (B) gain (lose) rents equal to half of the marginal subsidy expenses.

In special cases, the entire incidence of the net marginal spending will be reaped by landowners in the state providing the net spending. If the state providing the spending is small ($K_A \longrightarrow 0$) and capital is perfectly mobile in this one-good economy, landowners in A reap 100 percent of the net marginal spending, i.e., $\frac{dR_A/ds}{K_A} = +1$. Similarly, when the demand for capital is perfectly inelastic in A ($\eta_A = 0$) or is perfectly elastic in B ($\eta_B = \infty$), landowners in A reap the entire marginal benefit of the net marginal spending, while B's land and capital owners see no change in their

[32] Differentiating (A.8) with respect to s, we get

$$\frac{dR_A}{ds} = F_K^A(K_A)\frac{dK_A}{ds} - (r - s)\frac{dK_A}{ds} - K_A\left(1 + \frac{dr}{ds}\right); \qquad \frac{dR_B}{ds} = F_K^B(K_B)\frac{dK_B}{ds} - r\frac{dK_B}{ds} - K_B\frac{dr}{ds}.$$

From (A.6), the first two terms in each expression above cancel out and using (A.7), we get the expressions in (A.9) and (A.10).

rents. Therefore, in this model, a state within a country is likely to reap a significant portion of the benefit of a net marginal spending it provides to a domestically mobile factor.

A.3 Proofs

A.3.1 Proof of Proposition 1

Proof. The proof directly follows from the discussion in Appendix A.2. Equations (A.9) and (A.10) imply that landowners in state A providing the net marginal spending on capital within its border gain rental income, while state B's landowners lose. In this model, the immobile factor (land) in a state is likely to reap a significant portion of the benefit of a net marginal spending the state provides to a domestically mobile factor.

As mentioned in Appendix A.2, when the subsidy-providing state is small or has a perfectly inelastic demand for capital, or the other state's capital demand is perfectly elastic, the entire benefit of the net marginal spending will be reaped by landowners in the state providing the spending. □

A.3.2 Proof of Proposition 2

Proof. From Equation (6), the magnitude of the marginal decline in current house prices ($q_{H,t}$) from an additional dollar of pension shortfall j periods ahead (L_{t+j}) depends on how large the distortion is and low far in the future the tax is imposed.

With reasonable parameter values for income and property tax rates, depreciation, and maintenance costs, the capitalization of future pension liabilities in house prices today can have a magnitude of less or greater than one. □

Table A.1

Asset Class Detail

The PPD provides detailed breakdowns of the various asset classes invested in by public pensions. This table reports summary statistics for the allocations of the 616 state-year pension plan observations available. The average allocation and the standard deviation of the allocation across pension years are reported, as well as the percent of state-years that had a non-zero allocation to that asset class (short positions are also reported and accounted for in the below). Also reported is whether the asset class is included in our Restricted Benchmark measure. See https://publicplansdata.org/wp-content/uploads/2013/12/Investment-Codebook.xlsx for definitions of Asset Classes.

Asset Class	Obs.	Average Allocation	Std. Dev Allocation	Percent of State-Years with non-zero Allocation	Included in Restricted Benchmark
AbsRtrn	616	0.0079	0.0212	0.2419	Yes
AltInflation	616	0.0009	0.0057	0.0357	Yes
AltMisc	616	0.0094	0.0277	0.1932	Yes
Cash	616	0.0169	0.0214	0.8506	Yes
Commod	616	0.0023	0.0092	0.1899	No
CoveredCall	616	0.0000	0.0001	0.0065	Yes
CreditOpp	616	0.0052	0.0216	0.0990	Yes
DistrssedDebt	616	0.0000	0.0004	0.0032	No
EQCore	616	0.0002	0.0025	0.0065	Yes
EQDomesticLarge	616	0.0200	0.0599	0.2565	Yes
EQDomesticMid	616	0.0006	0.0031	0.0503	Yes
EQDomesticMisc	616	0.2519	0.1696	0.8506	Yes
EQDomesticSmall	616	0.0074	0.0246	0.2565	Yes
EQGlobal	616	0.0083	0.0340	0.1802	Yes
EQGlobalGrowth	616	0.0000	0.0006	0.0065	Yes
EQIntlActv	616	0.0001	0.0016	0.0097	Yes
EQIntlDev	616	0.0125	0.0397	0.1380	Yes
EQIntlEmerg	616	0.0071	0.0193	0.2143	Yes
EQIntlMisc	616	0.1225	0.0840	0.8669	Yes
EQIntlPass	616	0.0008	0.0079	0.0114	Yes
EQLarge	616	0.0002	0.0038	0.0016	Yes
EQMicro	616	0.0001	0.0011	0.0081	Yes
EQMisc	616	0.1002	0.1924	0.3669	Yes
EQPrivate	616	0.0570	0.0575	0.7938	Yes
EQSecLend	616	0.0004	0.0021	0.0568	Yes
EQSmall	616	0.0001	0.0008	0.0065	Yes
Farm	616	0.0000	0.0004	0.0114	No
FIAlt	616	0.0109	0.0638	0.0422	Yes
FIBelowInvestGrd	616	0.0005	0.0050	0.0097	Yes
FIConv	616	0.0005	0.0037	0.0471	Yes
FICore	616	0.0171	0.0447	0.2419	Yes
FICorpBonds	616	0.0008	0.0059	0.0455	Yes
FIDomestic	616	0.0384	0.0960	0.3425	Yes
FIEmerg	616	0.0023	0.0102	0.0763	Yes
FIETI	616	0.0000	0.0001	0.0146	Yes

Asset Class	Obs.	Average Fund Allocation	Std. Dev Fund Allocation	Percent of State-Years with non-zero Allocation	Included in Restricted Benchmark
FIFundsFunds	616	0.0000	0.0000	0.0081	Yes
FIGlobal	616	0.0022	0.0134	0.0909	Yes
FIHighYield	616	0.0062	0.0151	0.2403	Yes
FIIntl	616	0.0040	0.0144	0.1981	Yes
FIInvestGrd	616	0.0035	0.0233	0.0471	Yes
FILoans	616	0.0001	0.0014	0.0211	Yes
FIMisc	616	0.1772	0.1230	0.7825	Yes
FIMortgage	616	0.0011	0.0058	0.0974	Yes
FINominal	616	0.0001	0.0011	0.0081	Yes
FIOpp	616	0.0001	0.0006	0.0227	Yes
FIStructured	616	0.0001	0.0012	0.0130	Yes
FITIPS	616	0.0096	0.0294	0.2630	Yes
FITreasury	616	0.0006	0.0108	0.0227	Yes
GTAA	616	0.0047	0.0229	0.1315	No
Hedge	616	0.0098	0.0258	0.2890	Yes
HedgeEQ	616	0.0008	0.0069	0.0519	Yes
Infrast	616	0.0012	0.0066	0.1185	No
MLP	616	0.0010	0.0049	0.0909	No
MultiClass	616	0.0037	0.0121	0.1510	No
NatResources	616	0.0004	0.0036	0.0146	No
Opp	616	0.0009	0.0048	0.1445	No
OppDebt	616	0.0005	0.0047	0.0146	Yes
OppEQ	616	0.0002	0.0014	0.0162	Yes
Other	616	0.0020	0.0072	0.7192	Yes
PrivateDebt	616	0.0011	0.0069	0.0519	No
PrivatePlacement	616	0.0009	0.0053	0.0519	No
PrivRealEstate	616	0.0008	0.0061	0.0633	No
RealAssets	616	0.0051	0.0155	0.2159	No
RECore	616	0.0002	0.0032	0.0049	No
REIT	616	0.0004	0.0019	0.0844	Yes
RelativeRtrn	616	0.0000	0.0001	0.0162	Yes
REMisc	616	0.0534	0.0385	0.8377	Yes
RENonCore	616	0.0002	0.0029	0.0049	No
RiskParity	616	0.0017	0.0112	0.0568	Yes
Timber	616	0.0019	0.0072	0.1347	No

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 the logarithm of the sales price, in thousands of dollars, 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 property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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 baseline regression described above where the dependent variable is the logarithm of the sales price, in thousands of dollars, for transactions 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 replaces the primary variable of interest with the cumulative pension fund performance from 2002-2014 in excess of the benchmark performance for each asset class the fund is invested in. Column (3) is the same as column (1), but replaces the primary variable of interest with the cumulative pension fund performance from 2002-2014 that would have occurred based on the fund's asset allocations, had it earned the benchmark performance for each asset class. Column (4) is the same as column (3), but restricts attention to assets that have less potential to be localized (i.e., bonds and equities, and funds that invest in them, rather than commodities, private debt, and real estate). Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, 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.	0.323***			
	(6.87)			
2002-2014 Cum. Excess Ret.		0.604***		
		(4.93)		
2002-2014 Cum. BenchMk Ret.			0.311***	
			(4.26)	
2002-2014 Cum. (Restr.) BenchMk Ret.				0.304***
				(4.07)
Border Distance	X	X	X	X
State-Year Income PC	X	X	X	X
Border Group-Tran Year FE	X	X	X	X
6 Prop Chars FE	X	X	X	X
Observations	129,940	129,940	129,940	129,940
Adj. R^2	0.864	0.863	0.863	0.863

Table A.3

Rolling Pension Returns and House Prices

This table presents estimates from a state border discontinuity design model where the dependent variable is the logarithm of the sales price, in thousands of dollars, 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 property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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 baseline regression 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 replaces the primary variable of interest with the cumulative pension fund performance from 2002 until the year prior to that particular transaction in excess of the benchmark performance for each asset class the fund is invested in. Column (3) is the same as column (1), but replaces the primary variable of interest with the cumulative pension fund performance from 2002 until the year prior to that particular transaction that would have occurred based on the fund's asset allocations, had it earned the benchmark performance for each asset class. Column (4) is the same as column (3), but restricts attention to assets that have less potential to be localized (i.e., bonds and equities, and funds that invest in them, rather than commodities, private debt, and real estate). Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, 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.	0.222***			
	(4.67)			
2002-Sale Cum. Excess Ret.		0.363***		
		(5.04)		
2002-Sale Cum. BenchMk Ret.			0.173***	
			(2.70)	
2002-Sale Cum. (Restr.) BenchMk Ret.				0.167***
				(2.61)
Border Distance	X	X	X	X
State-Year Income PC	X	X	X	X
Border Group-Tran Year FE	X	X	X	X
6 Prop Chars FE	X	X	X	X
Observations	712,505	712,505	712,505	712,505
Adj. R^2	0.879	0.878	0.878	0.878

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 the sales price, in thousands of dollars, 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 property transactions involving single-family residences that have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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 (column (1) only) and 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). Columns (1) and (2) restrict the sample to properties located in counties sharing a border with an adjacent state that are within 50 miles of that border. Column (3) restricts the sample to properties that do not meet the definition of being in a border county (i.e., only counties in the interior of the state). Column (2) differs from column (1) of Table 3 only in the exclusion of a measure of distance to the state border. Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)		
	(1)	(2)	(3)
2002-2014 Windfall Per Property $('000s)	3.662***	2.510***	0.920***
	(6.65)	(8.46)	(3.82)
2002-2014 Windfall Per Prop $('000s) × Border Distance (mi)	-0.113***		
	(-2.81)		
Border Distance	X		
State-Year Income PC	X	X	X
Border Group-Tran Year FE	X	X	X
6 Prop Chars FE	X	X	X
Sample	Border	Border	Interior
Observations	129,940	129,940	769,004
Adj. R^2	0.816	0.813	0.717

Table A.5

County-Level Municipal Finances: Border vs. Interior Counties

This table presents county-level regressions of various financial outcomes on an indicator for whether the county is on a state border. The sample includes counties in states that qualify for our regression sample, depicted in Figure A.2. These specifications include state fixed effects to account for differences in financial ratios across states. Information regarding the finances of local governments (counties, cities, and other local municipalities) is aggregated to the county level by the U.S. Census Bureau and available for the years 2007 and 2012. We estimate separate regressions for these two reporting years. The estimates suggest that border counties are comparable to counties on the interior of their state with respect to the financial health of local governments. Reported *t*-statistics in parentheses are heteroskedasticity-robust. ***, **, 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	0.04	0.17*	Total Expenditures	0.01	0.19*
Per Capita	(0.13)	(1.87)	Per Capita	(0.05)	(1.96)
Revenues From Federal Govt	0.00	0.01	Capital Expenditures	-0.03	0.01
Per Capita	(0.11)	(0.96)	Per Capita	(-0.87)	(0.31)
Revenues From State Govt	0.04	0.07***	Education Expenditures	-0.06	0.00
Per Capita	(0.50)	(3.34)	Per Capita	(-0.64)	(-0.11)
Total Taxes	-0.06	-0.03	Safety Expenditures	-0.01	0.00
Per Capita	(-0.62)	(-0.78)	Per Capita	(-0.31)	(0.56)
Property Taxes	-0.05	-0.04	Utility Expenditures	0.08	0.05
Per Capita	(-0.75)	(-1.20)	Per Capita	(1.31)	(0.92)
Sales Taxes	0.00	0.00	Short-Term Debt	-0.01	0.00
Per Capita	(0.09)	(0.62)	Per Capita	(-0.95)	(0.53)
Income Taxes	-0.02	0.00	Long-Term Debt	0.82	0.65*
Per Capita	(-0.85)	(0.29)	Per Capita	(1.39)	(1.87)
Other Taxes	0.00	0.00			
Per Capita	(0.12)	(0.43)			

Table A.6

Pension Windfalls and House Prices in Border Counties External Validity and Matching

This table presents estimates from a state border discontinuity design model where the dependent variable is based on the sales price, in thousands of dollars, 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 property transactions involving single-family residences in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI. 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. Columns (1)-(5) utilize a weighted least squares specification with weights chosen such that these border counties match interior counties on the specified dimension(s) and utilize fixed effects for the county border group of the property interacted with the calendar year of the transaction. Columns (1)-(4) use weights chosen to match the four variables in Table A.5 with statistically significant differences between border and interior counties. Column (5) uses weights chosen to match all four variables jointly. Column (6) replaces the county border group structure of the data with a matched county pair structure wherein each county is matched to it's nearest neighbor (with replacement) along a pair-wise Mahalanobis distance calculation utilizing the 15 county-level municipal finance variables for 2012 (listed in Table A.5) plus the population of that county. A fixed effect is then included for the matched county pair by calendar year of the transaction. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)					
	(1)	(2)	(3)	(4)	(5)	(6)
2002-2014 Windfall Per	2.413***	2.414***	2.414***	2.414***	2.407***	2.158***
Property $('000s)	(8.27)	(8.35)	(8.30)	(8.30)	(8.43)	(3.36)
Border Distance	X	X	X	X	X	X
State-Year Income PC	X	X	X	X	X	X
Border Group-Tran Year FE	X	X	X	X	X	
Matched County Pair-Tran Year FE						X
6 Prop Chars FE	X	X	X	X	X	X
2012 Balance Variable(s)	Total Revenues, PC	Revenues From State Govt, PC	Total Expenditures, PC	Long-Term Debt, PC	Cols. (1)-(4)	
Matched County Sample						X
Observations	129,940	129,940	129,940	129,940	129,940	192,615
Adj. R^2	0.820	0.822	0.821	0.821	0.825	0.916

Table A.7

State Responses to Shortfalls

This table presents regressions of various economic outcomes on lagged state pension shortfalls. Observations are at the state-year level. 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 annual changes 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Employer Pension Contribution	Employee Pension Contribution	Secondary Education Appropriation	Change in Percent of Rural Roads in Poor Condition	Change in Percent of Urban Roads in Poor Condition
	(1)	(2)	(3)	(4)	(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	X	X	X	X	X
Observations	806	806	450	383	393
Adj. R^2	0.606	0.802	0.942	0.046	-0.045

Table A.8

Differences in the Perceived Value: Commercial vs. Residential Properties

This table presents estimates from a state border discontinuity design model where the dependent variable is the sales price, in thousands of dollars, of a property. The sample has been expanded to include commercial properties with a single property per parcel, subject to the availability and coverage of commercial properties in the ZTRAX dataset. Columns (1), (2), and (3) replicate Column (1) of Table 3 and Columns (1) and (4) of Table 4, respectively, whilst including an interaction of the relevant windfall variable with an indicator for whether or not the property was a single-family residential property (and therefore in our main sample). 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 for column (1) and only for the years prior to the transaction since 2002 for columns (2) and (3), 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 property transactions in counties sharing a border with an adjacent state that are within 50 miles of that border and have a transaction price between the typical home values in the the bottom and top market tiers as calculated at the county-month level by the ZHVI for the residential properties and, for the commercial properties, between the lowest and highest observed sales prices in the residential sample. In column (1) transactions are further restricted to those in the years 2015-2018. 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 throughout. Columns (1) and (2) also include a covariate for the distance to the state border and six interacted property characteristic fixed effect cells that control for property type (square footage of structure, square footage of lot, age of building, number of bedrooms, number of bathrooms, and number of stories). Column (3) includes a property-level fixed effect to exploit within-property variation over time.Reported *t*-statistics in parentheses are heteroskedasticity-robust and clustered at both the zip and transaction month level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)		
	(1)	(2)	(3)
2002-2014 Windfall Per Property $('000s)	2.165*		
	(1.81)		
2002-Sale Windfall Per Property $('000s)		2.127***	1.825***
		(2.95)	(3.43)
Residential Property Indicator	-74.52**	-76.30***	114.4***
	(-2.03)	(-5.02)	(8.32)
2002-2014 Windfall Per Prop $('000s) × Residential Property Indicator	0.698		
	(0.63)		
2002-Sale Windfall Per Prop $('000s) × Residential Property Indicator		0.289	-0.587
		(0.44)	(-1.31)
Border Distance	X	X	
State-Year Income PC	X	X	X
Border Group-Tran Year FE	X	X	X
6 Prop Chars FE	X	X	
Property FE			X
Observations	135,140	761,371	76,776
Adj. R^2	0.737	0.769	0.801

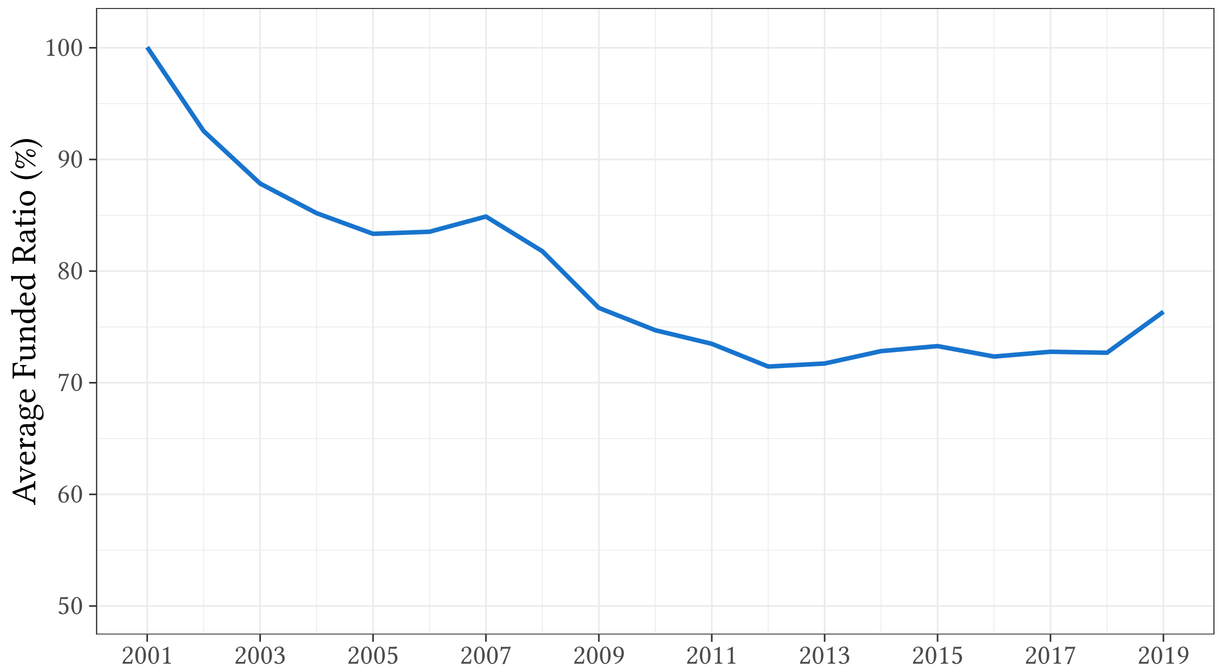

Figure A.1. Average Funded Ratio This figure presents the time-series of average ratio of pension assets to liabilities, the actuarial funded ratio, at the state-year level for the Public Plans Data (PPD) database provide by the Center for Retirement Research (CRR) at Boston College. Actuarial funded ratio is given by ActFundedRatio_GASB, which is ActAssets_GASB divided by ActLiabilities_GASB in the database.

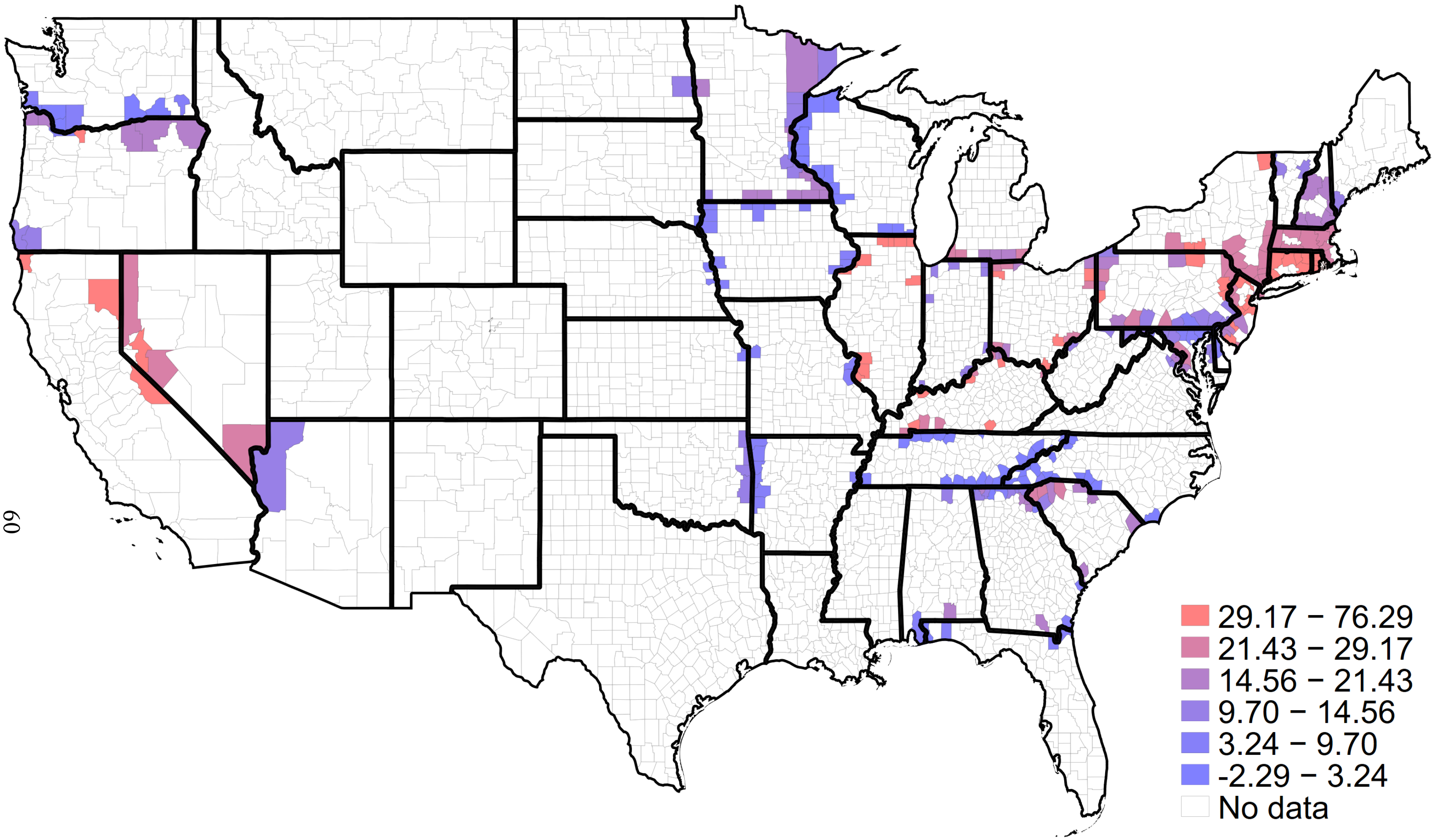

Figure A.2. State-Level Shortfalls by County This figure presents the state-level pension shortfall, in thousands of dollars, averaged over properties in each county in our sample. The sample includes all transacting properties that qualify for the regressions in Table 4 and covers the full sample period from 2002 to 2018. Note that pension shortfalls only vary at the state-year level, but since the number of transactions per county is not constant over time, there is within-state variation in shortfalls due to differences in the implicit time-varying weights across counties.