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ABSTRACT

We study effects of state pension windfalls on property prices near state borders, where theory suggests real estate reflects the value of additional public resources. Windfalls have grown to half the size of total state tax revenues and provide plausibly well-identified variation in fiscal conditions. We find one dollar of exogenous variation in pension asset returns increases border house prices by approximately two dollars. These estimates suggest governments, rather than wasting incremental resources, allocate additional funds towards high value projects or tax abatement. Evidence of larger effects in financially constrained municipalities highlight how fiscal resources amplify welfare effects of economic shocks.

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Are marginal municipal resources wasted or allocated efficiently? Are economic shocks amplified by local public finances? If so, what are the welfare implications of these budget fluctuations?

Central to answering these questions is an understanding of how individuals value an additional dollar of public wealth—which is directly tied to the GDP consequences of fiscal spending, a subject long debated in academic and political discourse (see, e.g., Samuelson, 1954; Ballard and Fullerton, 1992; Uhlig, 2010). Existing literature documents a wide range of estimates for the marginal value of public spending on specific projects, with education, health care, and tax abatement topping the list (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020). Yet little is known about the value of — or willingness to pay for — the entire basket of projects undertaken due to an infusion of fiscal resources, which could either be wasted or allocated efficiently.

Utilizing state pension plans as a laboratory, we provide an estimate for this key parameter in public finance, which we call the marginal value of public wealth (MVPW). For each incremental dollar of plausibly exogenous pension windfall we estimate an increased willingness-to-pay (WTP) for real estate prices of \$1.95 for properties near neighboring states, where theory suggests that at these borders this reflects the MVPW. To check the plausibility of our estimate, we construct a benchmark MVPW range by combining the observed composition of spending out of pension windfalls (Shoag, 2010) with WTP measurements in these categories (Hendren and Sprung-Keyser, 2020). The resulting interval spans estimates that indicate wasted fiscal resources (<\$1 at 25th percentile), pure pass-throughs (\$1), and extraordinarily valuable allocations (>\$3 at 75th percentile). Our estimate of near \$2 (25th-75th percentiles of \$1.8-\$2.1) is reasonable, and demonstrates efficient utilization of marginal public funds at the local level but is inconsistent with all marginal projects being of the highest possible value. Moreover, an MVPW of approximately \$2 implies significant welfare ramifications of shocks to public finances.[1]

Estimating the MVPW is empirically challenging for two primary reasons: i) because spending and revenues are jointly determined, most changes in public wealth are not exogenous, and ii) estimating the "value" component would require summing the prices of all affected assets in an

[1]See Section 4.2 for a more detailed discussion.

area, which we do not observe. We overcome these challenges using a two pronged approach.

First, we focus on differences in fiscal condition driven by variation in U.S. states' pension fund performance ("windfalls"). Not only does this provide plausibly exogenous and substantial variation in public wealth and spending consistent with other fiscal multipliers (e.g., Shoag, 2010, 2013; Nakamura and Steinsson, 2014; Adelino, Cunha, and Ferreira, 2017), the effects of pension funding are important to understand in of themselves. Underfunded U.S. public pensions represent an implicit household liability larger than auto loans, student debt, and credit card balances combined[2], while pension asset returns have grown to become the single largest driver of state revenue (and are half the size of total tax revenue and three-quarters the size of transfers from the federal government in 2017).[3]

Second, instead of measuring the change in value for all assets exposed to pension windfall shocks, we implement a novel empirical strategy motivated by a theoretical model of fiscal deficits in the presence of financing or spending inefficiencies. In an open economy, where capital and labor are mobile but real estate is not, property prices reflect the marginal value of fiscal "windfalls", even if housing supply is elastic (e.g., Harberger, 1962; Oates, 1969; Bradford, 1978; Kotlikoff and Summers, 1987; Harberger, 1995).[4] Homebuyers' willingness to pay for such windfalls comes from either reductions in deadweight losses that would have otherwise been generated in honoring these obligations or in the provision of high value public goods.[5] Conversely, if all forms of capital face a high cost of relocation, then the willingness to pay is unclear since the price of any individual asset is unlikely to reflect the pro-rata total cost or benefit of capital fluctuations. Motivated by this insight, we focus our analysis on locations near state borders where real estate, as the immobile

[2]According to Rauh (2016), state and local pensions had unfunded liabilities of $3.85 trillion as of 2015. 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]Based on the most recent complete survey of state and local government finances by the U.S. Census Bureau (www.census.gov/programs-surveys/gov-finances.html).

[4]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. We also show that as long as housing stock is immobile and housing markets are integrated near state borders, the relative impact of windfalls on current house prices remains unaffected by the elasticities of housing supply and demand.

[5]Note that our analysis does not require that homeowners are aware of pension funding in their locale per-se. Agents may be responding to differential policies (e.g., spending and tax rates) already undertaken or expected to be undertaken due to differences in fiscal conditions.

asset, should bear the burden and thus reflect the implied value of pension windfalls.[6]

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 pension 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[7] and find a pass-through of approximately two. For each additional dollar of pension asset returns on one side of a border, house prices increase by approximately two dollars.[8] Non-parametric analysis within a regression discontinuity design (Hahn, Todd, and Van der Klaauw, 2001) at state borders confirms evidence of a clear increase in prices when moving from a low-return state to a high-return state.

Our estimates survive a number of identification concerns and are robust to alternative specifications. First, we refine our source of exogenous returns 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. We also show that pension performance is not predicted by house values nor correlated with measures of public convictions per capita, and is only associated with future house values if pension assets per property are substantial, all indicating our findings are not driven by omitted variable bias coming from political mismanagement/corruption.

Second, we examine 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 for the inclusion of property fixed effects amongst 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

[6]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 requiring information on both sides of borders.

[7]To account for incremental contributions, we apply the average contribution rate across the U.S. to each state.

[8]By contrast, estimates are substantially lower in an analysis of properties in the interior of the state. This is consistent with states distributing windfalls across regions according to their size. In such settings, the value should be split locally among various forms of capital in a way that depends on their relative mobility and elasticities. This contrasts with a state-wide per-capita tax increase/reduction which would imply the same, lower, coefficient on property throughout the state.

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 that it can take several years for spending, even on things like revitalization projects and public schools, to be fully realized into house prices (e.g., Rossi-Hansberg, Sarte, and Owens III, 2010; 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.

Municipalities often face constraints on increasing taxes, which appear to not only affect their spending (e.g., Rodden and Wibbels, 2010), but also explain how state pension fund losses could increase financial constraints. Spending also responds to municipal credit spread changes (Novy-Marx and Rauh, 2012), so we posit that the effect of pension windfalls will be most beneficial to municipalities facing borrowing or taxation constraints. We find evidence consistent with this hypothesis: in the cross-section, our effects are concentrated in financially constrained municipalities. Constrained communities would like to undertake more value enhancing projects or tax abatement, but cannot. Residents place a large value on alleviating these constraints. These results, and evidence of a similar fiscal multiplier out of windfalls from other non-military plausibly exogenous spending (Shoag, 2013), suggest a more general applicability of our findings among locales with severe fiscal deficits.[9] One implication of our finding is that fiscal health is very valuable, consistent with persistent regional differences in economic growth following local shocks (e.g., Amior and Manning, 2018; Babina, Bernstein, and Mezzanotti, 2023).[10]

Importantly, our findings are *not* a statement about the optimal "size of government", since

[9]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.

[10]Though this may also be reflected in persistently lower land values in some Midwestern U.S. cities (e.g., Detroit), it is beyond this paper to decompose the role of fiscal constraints from other drivers, such as urban construction coordination problems (e.g., Owens III, Rossi-Hansberg, and Sarte, 2020).

marginal policies may include both high value additional spending as well as reductions in distortionary taxation.[11] In that sense, our paper could tie together seemingly opposing views of fiscal spending. Conservative estimates of the "fiscal multiplier" due to government spending are low, and even possibly negative (Uhlig, 2010), implying high benefits from tax reduction. However, as noted, micro studies estimate high WTP for certain projects. We find that marginal public wealth carries a similar value as high WTP projects, *including* opportunistic tax abatement policies. Thus, our findings are consistent with an equilibrium where marginal public projects are of high value, but funding these projects is equally distortionary.

In this way, our findings connect to a literature in macroeconomics examining the effects of the sources and uses of fiscal funds. These include papers (e.g., Cohen, Coval, and Malloy, 2011; Chodorow-Reich, Feiveson, Liscow, and Woolston, 2012; Nakamura and Steinsson, 2014; Suárez Serrato and Wingender, 2016; Farhi and Werning, 2016) estimating fiscal multipliers, and especially those that study exogenous transfer or spending multipliers. Particularly relevant are those that consider local regional shocks coming from national transfers without a need to balance the local budget.[12] These papers do not examine the marginal value of the spending induced by these shocks, but still our findings of large value effects are consistent with large spending multipliers observed in many of these settings. Our findings are also consistent with significant changes in employment and provision of critical public services when faced with external financing shocks (e.g., Adelino, Cunha, and Ferreira, 2017; Agrawal and Kim, 2021).

This paper also 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

[11] Why distortion-free/low lump sum or land taxes are not the source of all public financing is an important and interesting question that falls outside the scope of this paper. In practice we marginal funds are raised to balance fiscal budgets by cutting expenditures and/or raising taxes, not just lump/land taxes.

[12] See Ramey (2011) for a more complete review of the literature on government spending multipliers.

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 measure marginal value of additional public (pension) wealth.

Our focus on 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. Our approach is more similar, in spirit, to papers looking at house prices near school district borders to estimate the value of school spending (e.g., Black, 1999; Bayer, Ferreira, and McMillan, 2007; Bayer, Blair, and Whaley, 2020) and across municipal borders to examine the value of land use regulations (e.g., Turner, Haughwout, and Van Der Klaauw, 2014). That said, where the capitalization of pensions into house prices is itself of interest, our framework and findings provide evidence on where and when you should expect it to be either larger or smaller, and may explain a unifying framework for some of the observed variation in house price estimated effects in prior work.

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 that motivates 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 Foundation

In this section, we present a theoretical foundation that motivates our empirical design. Our framework, detailed in Appendix A, builds off the tax incidence literature (see e.g., Harberger, 1962) in a few simple ways and yields the following insights. In a small open economy landowners reap the entire benefit or cost of an exogenous shock to public funds, without regard to the intended beneficiaries (Proposition 1). In a stark setting with fixed housing supply, house prices fully reflect the willingness to pay for (to avoid) (in)efficiencies in the public provision of goods and capital raising from net spending (Proposition 2). We further show that, even if housing supply is not fixed, the net cost/benefit of additional public funds can still be fully captured by looking at the *relative price* of real estate between two integrated markets (Proposition 3). This last result, expressed in Equation (A.15) in the appendix, guides our empirical strategy of examining the price wedge between properties in adjacent counties, but across state lines where pension assets differ.

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).[13] Relaxing the closed-economy assumption, most studies argue that in an open economy, immobile factors reap most, if not all, of the long-run benefits of a net marginal spending in the economy due to capital mobility across borders.[14]

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.

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

[14] 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.

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. *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), 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.4. □

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 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 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. So far, we have focused on capital mobility and the elasticity of demand. In this section, we introduce a role for asset prices by studying the capitalization of net pension liabilities (or equivalently net negative pension windfalls) into house prices. The economic burden of a reduction in net public spending is affected by changes in asset prices due to changes in public revenues and expenditures. The magnitude of the marginal increase in house prices from an additional dollar of pension windfalls, reflects households' willingness-to-pay (WTP) for a one dollar reduction in net public spending (including tax abatement). A WTP of \$0, <\$1, \$1, and >\$1 reflect, respectively, completely wasted allocations, distortionary allocations, pure pass-throughs, and efficient allocation. Where it falls is theoretically ambiguous and therefore an empirical question.

The argument presented here is based on the asset pricing 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 value of reductions in net spending associated with the asset. We present the details of the model in Appendix A.3.

Proposition 2. *The magnitude of the marginal decrease (increase) in house prices from an additional exogenous dollar of pension shortfalls (windfalls) is theoretically ambiguous and reflects the WTP for this fiscal deterioration (improvement).*

Proof. See Appendix A.4. □

Even with endogenous housing supply, as long as housing is immobile across state borders and neighboring states face the same housing stock and demand, we show that the relative drop in housing prices between the neighboring states in response to pension-induced shocks will not be impacted by the elasticities of housing demand or supply. We summarize the main message in Proposition 3.

Proposition 3. *If housing is considered immobile and assuming the neighboring states face the same linear housing supply and demand curves, when the housing stock is endogenous, the relative magnitude of the marginal decrease in current house prices from an additional dollar of pension shortfalls remains unaffected by the elasticities of housing demand and supply.*

Proof. See Appendix A.4. □

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 (CRR) at Boston College. The PPD contains annual plan-level data from 2001 through 2019 for 190 pension plans: 114 administered at the state level and 76 administered locally. We aggregate these data to the state level. This sample covers 95% of public pension membership and assets nationwide.[15] 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.

[15]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.

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.[16]

The majority of pension plans report performance net of fees, but a small fraction of plans still disclose gross performance. Our measure of returns is based on asset class level performance data and does not allow us to clearly distinguish between the two cases. We demonstrate robustness to an alternate return calculation in Section 4.3.4.

Table 1 reports descriptive statistics on the PPD data. On average across time and states, the largest asset holdings were equities and fixed income (53% and 28% of total assets, respectively), followed by real estate and private equity (5% of total assets, each). The value of assets is 78% of the actuarial value of liabilities for the mean observation, indicative of substantial underfunding. Appendix Figure C.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.

Novy-Marx and Rauh (2011) and Rauh (2016) suggest that the appropriate discount rate for public pension liabilities is the yield on a zero-coupon Treasury bond with the same duration. To discount pension liabilities using Treasury rates, we would 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.[17] 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.[18] While we acknowledge this is an imperfect

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

[17]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 ($\text{TPL}_{r+1\%}$) and 100 bps lower ($\text{TPL}_{r-1\%}$). However, this information is only available starting in fiscal year 2014, when GASB 67 became effective.

[18]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 $3.412/1.191 = 2.864$.

adjustment method, any resulting bias would affect only our analysis of shortfalls in Appendix B and not our main analysis of windfalls throughout the paper 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 by the Zillow Home Value Index (ZHVI).[19] Second, we focus only on single-family residences.[20] 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. Our sample contains 70.5% of counties in disclosure states that lay on a border with a different disclosure state by count. Weighting counties by the number of housing transactions in the full ZTRAX sample over our sample period that are within 50 miles of the county border, our sample contains 95.1% of eligible counties. Table 2 reports descriptive statistics for the observations utilized in our main regression samples.

[19]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.

[20]Previously circulated versions of this paper containing very similar empirical results utilizing a different manner of conditioning on single-family residences within the ZTRAX database, and resulted in sample sizes of approximately a quarter of that currently used in our main specifications.

3 Empirical Methodology

Our theory suggests an empirical framework that focuses on state borders where real estate prices should reflect the economic burden of shortfalls. As detailed in Section 1, because real estate is effectively immobile property will bear the full brunt of inefficiencies surrounding the raising of public capital 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 exogenous variation in pension assets per property, all else equal, translates into variation in property values in regions near state borders. Consider the following border discontinuity design (BDD) regression:

$$PropertyValue_{it} = \beta\, PensionShortfallPerProperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}, \qquad (1)$$

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.[21] 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 property

[21] Total properties are based on all lots, including residential, commercial, and industrial, since all this land is immobile. While we look primarily residential transactions for which we have data on many more sales, this should not affect our estimates as long as our findings are similar across lot type - which indeed we show is the case for a subset of commercial transactions.

characteristics that capture time-invariant differences in property values. 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.

Appendix Figure C.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 may even recover 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 that are driven by factors that are plausibly exogenous to state expenditures. We implement the same empirical design as Equation (1), substituting pension shortfalls with asset performance "windfalls:"

$$PropertyValue_{it} = \beta\, PensionWindfallPerProperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}, \tag{2}$$

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.[22] This can be interpreted as the additional pension assets available per property that are caused by performance of that state's investment portfolio over that period. The regression coefficient β represents the MVPW. The economic interpretation is consistent with the pass-through in our theoretical motivation because a 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 over-allocate 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:

$$PropertyValue_{it} = \beta\, Windf\widehat{allPerP}roperty_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it},$$

$$WindfallPerProperty_{st} = \kappa\, ExWindfallPerProperty_{st} + \eta_{bt} + \psi D_i + \mu_l + \boldsymbol{\phi' X_{lt}} + \upsilon_{it}, \quad (3)$$

[22]Additionally, we account for interim contributions by growing individual plan assets annually at the growth rate of pension assets in the entire observed pension system that are due to contributions in that particular year. These contribution dollars then serve as an increase in basis when calculating future windfalls.

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.[23] In this setup, returns should be unrelated to both local economic conditions and state governance.

It is important to note here, that it is critical to *avoid* using observed shortfalls as the endogenous variable in the 2SLS framework. Exogenous windfalls are immediate net reductions in shortfalls and can recover the MVPW, while observed equilibrium shortfalls can induce a substantial bias in such estimates. As we note in more detail in Appendix B, in an extreme case where 99.9 cents out of every dollar of windfalls is spent on a \$1 value non-distortionary tax refund, the MVPW (and what you recover using exogenous windfalls) is \$1, but with shortfalls as the endogenous variable instrumented with windfalls you would estimate a (substantially biased) MVPW of \$1,000, since the reduced form is \$1 but the first-stage is 0.001. This is a bias that occurs anytime there is spending due to windfalls, which we illustrate in more detail in Appendix B.

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

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 (2), 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 be fully reflected in home prices.[24] Second, to the extent that observable degradation or improvement in public amenities reduces residents' 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. Though not necessary for our findings, some residents may also become directly aware of the pension effects on fiscal condition. For example, there is a 65% correlation between state level pension shortfalls per household and Google search activity for "pension crisis" and "public pension". It would only necessary that a subset of residents be aware of pension funding

[24]Prior work has found it can take several years for property prices to reflect the value of revitalization projects (Rossi-Hansberg, Sarte, and Owens III, 2010) and the provision of educational public goods (Bayer, Blair, and Whaley, 2020).

for it to have an impact on the housing market equilibrium.

Table 3 presents formal evidence of how such concerns or realized differences in spending or taxes due to fiscal condition 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 including typical annual nationwide contributions and the 2001 pension assets per property, which represents the dollar windfall per property. Column (1) reports a positive and statistically significant coefficient of 1.95, 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 marginal value of additional public pension wealth. For instance, a coefficient of 1.95 suggests that the ceteris paribus marginal value of one dollar more in net pension funding is \$1.95, implying a deadweight loss or inefficiency of \$0.95. This is also equivalent to an implied economic burden or cost of \$1.95 that is relaxed by \$1 of additional exogenous pension funds. An estimate larger than one is not surprising, but does suggest a high marginal value of public wealth and therefore allocation to high value policies. For example, the effect of investment in public education on house prices is also estimated to be of a similar magnitude (Cellini, Ferreira, and Rothstein, 2010; Bayer, Blair, and Whaley, 2020). For a more detailed discussion of this magnitude and comparison with estimates based on the marginal value of public policies see Section 4.2.

We also show that our findings are driven by neither the construction of windfalls per property nor the functional form of the BDD. In columns (1) through (4) of Appendix Table C.2, 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 non-parametric border discontinuity design. For each border pair, we determine the state that has the larger pension asset return between 2002 and 2014 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:

$$HousePrice_{it} = \sum_{k=1}^{20} \beta_k Treated_{st} \times \mathbf{1}(Miles_i = k) + \gamma_{bt} + \lambda_l + \boldsymbol{\delta' X_{lt}} + \epsilon_{it}. \quad (4)$$

Figure 1 plots the coefficients recovered from this specification 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.

4.2 Discussion: Placing our estimate in the literature

The MVPW we estimate in this paper can be thought of as the dollar-spent weighted average of the policies undertaken due to an additional dollar of pension windfalls. While a vast existing literature estimates the WTP for $1 of upfront cost for public projects (Hendren and Sprung-Keyser, 2020), these papers consider separately each type of spending. Our focus is the WTP for the aggregate basket of projects undertaken with a marginal dollar of public funds. Separately, Shoag (2010) estimates the categorical spending response to plausibly exogenous fluctuations in pension assets. Prior work has not combined these strands of research to estimate the marginal value of public wealth. In this section we combine the WTP estimates with an empirically motivated basket of incremental projects to supply a plausible benchmark against which we validate our findings.

Hendren and Sprung-Keyser (2020) examines the WTP for 106 policies ranging from those that would actually actively destroy pre-existing value (<0), completely waste resources given ($0), worth less than the cost (<1), non-distortionary pass-throughs (≈1), and those whose value exceeds those costs (>1).[25] Some categories stand out as having high WTP estimates: the interquartile range for education is 1.01 to 7.03 and health care is 1.0 to 1.95, while an aggregate of all remaining categories has an interquartile range of 0.88 to 1.17. If the marginal projects favor education and health spending, MVPW estimated in this way may end up quite high.

To calculate a benchmark MVPW we match individual WTPs to estimates of spending response coefficients estimated in Shoag (2010). That work examines broad categories of spending after plausibly exogenous public pension windfalls. For each $1 of windfall-induced spending, they document an additional 33 cents spent on education, 14 cents on health, and the remaining 53 cents spent on a basket of programs, of which none appear significantly identified. Approximately half of the spending involves high WTP projects. To pin down our benchmark, we match each category to their respective WTP distributions with the 53% unclassified matched to a generic basket of projects excluding health and education. We calculate a weighted average WTP (a potential MVPW benchmark) 25-75th percentile (depending on the specific programs within each category) range from $0.9-3.2 with a median of 1.8.

While this benchmark approach validates the plausibility of our estimate of 1.95 (25th-75th percentile in our estimate of $1.8-2.1), our empirical design has multiple advantages and important implications.

First, the methodology we utilize in this paper provides a directly measured willingness-to-pay estimate for a dollar of public funds from an asset that should reflect this value in its entirety. In doing so we eliminate multiple sources of estimation error, and account for time series or regional fluctuation on how the marginal dollars from pension windfalls are spent.

[25] We focus here on WTP, rather than the marginal value of public funds (MVPFs) for these programs since there is a direct mapping as the weighted average to our MVPW estimate. On the other hand, MVPFs are WTP divided by the net long-term cost to the government. So for some programs these end up as infinity, since eventually they bring in more revenue than they cost. While a useful policy tool, MVPF estimates do not map well into our setting. In practice, this nuance only impacts extreme projects as Hendren and Sprung-Keyser (2020) documents that only 32% of MVPFs are >2. Utilizing MVPFs, the highest value programs are those focused on children or tax abatements.

Second, our estimated distribution is much tighter than the imputed benchmark — perhaps due in part to the decreased measurement error. These distributional differences are economically important: the interquartile range of our benchmark imputed MVPWs based on WTPs range from wasteful (MVPWs less than 1) to wildly beneficial (MVPWs greater than 3). Thus we can reject both wasteful marginal spending and a world where the marginal government project is of exceptionally high value. The latter might arise if governments were so fiscally or politically constrained that they could not even undertake projects with WTPs greater than three.

Lastly, this imputed approach to identifying the MVPW likely understates the true range of possible estimates. There is uncertainty in the estimated spending within categories, how marginal dollars are allocated for programs within categories, what the actual WTP are for those programs at the study level, and whether such programs are representative of the marginal policy opportunity set available. These may even be within policies that, while often available to policymakers, are not necessarily ones frequently studied by academic researchers. While we still think it is a helpful exercise to understand some idea of how our findings relate to the existing evidence on WTP across programs, there are substantial challenges and uncertainty in trying to use that approach to obtain MVPWs directly. Our approach does not rely on this strong set of assumptions to establish an estimate of the MVPW and the associated confidence bounds.

4.3 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 provide evidence that the relationship we estimate between pension returns and house values is not driven by the potential for political corruption to influence both.

4.3.1 Home/familiarity bias

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 (3) 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 including average national annual contributions. 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 (2.37) 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 (1.95). To simultaneously shut down both the home and familiarity channels, in column (4) we collapse the benchmarks into major categories and omit niche asset classes to form our Restricted Benchmark. 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 marginal value (1.95), suggesting little evidence of home bias in our primary specification.

4.3.2 Time-invariant unobservables

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 returns in 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 1.77, 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 similar to column (1), 1.90.

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.

4.3.3 Political mismanagement

A remaining concern is that political mismanagement or corruption could cause omitted variable bias if it leads to both the appointment of pension fund managers who underperform and poor economic outcomes. Any such omitted variables bias is less likely given that we obtain very similar estimates using all variation in returns, returns just driven by asset class allocation, returns holding asset class allocation constant and using just performance within asset class, and across-time variation in returns. Therefore any proposed bias would need to be similar across those differing sources of variation. While this might be somewhat unlikely, it is of course still possible, and so something we examine and explore more directly.

First, we show in Table 5 that our initial estimates in columns (1), (3), and (5) are virtually unchanged in columns (2), (4), and (6) after the inclusion of the most common proxy used in political science and public economics for local political mismanagement/corruption based on public convictions per capita.[26] Second, we show in Appendix Table C.3 that not only do the inclusion of these controls not change our results, but looking at leads/lags 5 years before and after we see no consistent statistically significant relationship at all between pension performance and corruption. Third, we show in Appendix Table C.2 column (5) that not only are pension returns more strongly related to house values when initial pension assets per property are high, but as

[26]We follow a large literature (Fredriksson, List, and Millimet, 2003; Glaeser and Saks, 2006; Butler, Fauver, and Mortal, 2009; Campante and Do, 2014; Cordis and Warren, 2014; Smith, 2016; Ellis, Smith, and White, 2020; Huang and Yuan, 2021; Aggarwal and Litov, 2023) that measures corruption by looking at Department of Justice (DOJ) public corruption convictions. We aggregate federal court district level convictions at the year level to the state-year level to construct our measures of *2002-2014 Public Corruption Convictions per Million Residents* and *2002-Sale Public Corruption Convictions per Million Residents*. Appendix Tables C.4 and C.5 consider robustness to the specific state-level measure of corruption (also constructed with DOJ public corruption data) utilized in Campante and Do (2014), as well as to two alternate measures of corruption that are unrelated to public corruption convictions found in Saiz and Simonsohn (2013) and Boylan and Long (2003).

implied by the coefficient without the interaction in that column, effects are not even statistically significant when assets per property are very small. In other words, just as would be expected if it is going through the economic burden, pension performance does not matter for house values if there is little asset value in those funds to affect governmental finances. It is not, however, consistent with an omitted variable such as political corruption which we would expect to cause pension funds to underperform and economic conditions to worse even if the amount of assets in those funds are lower. Finally, we show in that same Table C.2 in column (7) that house values do not predict future lower pension fund performance. If it is really about persistently worse managed governments appointment poor pension managers we would expect such a relationship and we do not find any evidence of that. Again, this suggests it is unlikely political mismanagement is driving our results and points to the causal interpretation presented.

4.3.4 Robustness

Our results are robust to a wide variety of alternative specifications. Table C.7 reports robustness to our Table 3 column (4) specification where we instrument for windfalls using the Restricted Benchmark windfall per property, which is replicated as column (1). These estimates are statistically significant whether clustering at the zip code (column 2), transaction month (column 3) or double clustering at both levels (column 4). We also find similar results even excluding property-level characteristic fixed effects (column 5) and annual state income per capita controls (column 6). Both results are consistent with integrated housing and labor markets that allow us to recover the economic burden from house values. Results are also robust to alternative method of computing pension asset returns (column 7).[27] Finally, in Table C.8 we show evidence of similar estimates

[27]While the majority of pension plans report performance net of fees, a small fraction of plans still disclose gross performance. Our data does not allow us to clearly distinguish between the two cases. Andonov and Rauh (2021) provide an alternate method of calculating plan returns, but one only available at the total plan level (not at the individual asset class level). Their method utilizes fields that the CRR label as being strictly related to net performance, but also differs along other dimensions (such as its ability to break returns out at an asset class level and the way it handles performance on mid-year contributions). The correlation between the return calculated following their methodology and ours is high, 83%. Importantly, our analyses focusing on the Benchmark and Restricted Benchmark return series are not subject to concerns regarding gross vs. net performance reporting, and column (7) of Table C.7 demonstrates that our effect is robust to utilizing a pension windfall instrument that is calculated using this alternate method of measuring pension plan returns.

among commercial properties indicating that are findings are not driven by of our focus on residential houses.

4.4 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 C.6 reports a smaller, but statistically significant, coefficient when applying our main specification to interior counties.[28] 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 C.9 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 years and find that only four out of 30

[28]We use a linear specification in column (1) of Appendix Table C.6 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.

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 C.10 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.[29] The results of this approach are identical to those of column (1) in Table 3. Finally in column (6) we drop border group by transaction year fixed effects and keep only the time fixed effects. This changes our comparison group from those just across the same state borders just next to each other, to those on any border anywhere in the country. While this does not control for effects of differential housing supply responses, and thus is not our preferred specification, finding similar estimates in this setting does reduce concerns about "reflection" problems which could potentially arise if there are spillovers across borders. In sum, the evidence in Tables C.9 and C.10 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, the 95% confidence interval around the estimate from column (1) of Table 3 corresponds to an average deadweight loss of between \$11,184 and \$48,393 per household, or between 16% and 70% of median household income.[30]

[29]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.

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

4.5 Role of municipal financial constraints

As discussed above, our estimates reflect the residents' value of marginal net spending caused by exogenous increases in public pension wealth. 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 without distortive taxation. In Table 6, we find exactly this. Large economic burdens of pensions shortfalls are concentrated in municipalities with high bond spreads (column 1), as well as evidence of ability and therefore issuance of long-term debt overall (column 2) and relative to county salaries (column 3).[31] These are consistent with effects being driven by locales with more constrained access to finance. Not only that, but in Appendix B, we provide evidence in-line with prior work that pension windfalls relax overall budget constraints and increase fiscal spending. When combined with direct evidence of effects concentrated in more financially constrained locales, this suggests that our findings might be even more broadly applicable. In particular, our findings would be consistent with high marginal value to improvements in not just pensions, but local fiscal conditions more generally. While there could be "flypaper effects" (e.g., Hines and Thaler, 1995) leading to a dependence on exactly what part of the budget fiscal improvements arise, similar local fiscal multipliers, especially across spending categories, for pension wealth shocks to the rest of the literature suggests again a likely general applicability (e.g., Shoag, 2010, 2013).

5 Conclusion

This paper provides an estimate of what we argue to be a key parameter in public finance—the marginal value of public wealth. To do this, we focus on residents' marginal value of pension windfalls or, equivalently, their marginal value of an external reduction in the economic burden of the trillions of dollars in state public pension shortfalls. We use plausibly exogenous variation in

[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.

state pension funding stemming from excess asset performance and show that a one dollar reduction in the public pension shortfall per property causes an approximately two dollar increase 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 value of the marginal improvement in pension funding. We obtain similar estimates using investment performance only in excess of benchmarks, returns driven just by allocations to those benchmarks, and repeat sales. Our findings are robust to a wide range of controls, including those related to corruption, supporting a causal interpretation of our findings. We also find that effects are concentrated in financially constrained locales, pointing to the importance of fiscal conditions in the presence of significant economic burdens.

Our findings indicate that governments allocate marginal fiscal resources towards policies with a high marginal value (e.g., reducing inefficient taxation and/or the underprovision of high-value future public goods or services), with important implications for our understanding of public financing and allocation. While this MVPW estimate does not inform the optimal size of government, it does suggest that marginal changes in municipal resources can have large welfare consequences. Our estimates imply that governments respond to shocks to the local economy that deprive areas of marginal resources by cutting high value projects. This behavior is not only critical for understanding government allocation decisions, but also speaks to the amplifying effects of economic shocks from fiscal conditions.

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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	616	0.056	0.075	-0.074	0.010	0.061	0.113	0.163
Actuarial assets ($m)	616	13,875	13,521	1,491	4,783	8,445	17,969	44,553
Actuarial liabilities ($m)	616	17,496	15,986	2,296	6,295	11,601	23,992	52,215
Actuarial funded ratio	616	0.784	0.143	0.554	0.695	0.773	0.883	1.007
Equity share	616	0.528	0.094	0.360	0.472	0.537	0.597	0.663
FI share	616	0.279	0.077	0.180	0.226	0.267	0.316	0.412
RE share	616	0.054	0.038	0.000	0.020	0.056	0.081	0.110
PE share	616	0.053	0.051	0.000	0.009	0.042	0.082	0.146
HF share	616	0.041	0.056	0.000	0.000	0.016	0.064	0.154
Comd share	616	0.013	0.021	0.000	0.000	0.000	0.021	0.061
Cash share	616	0.017	0.019	0.000	0.004	0.013	0.023	0.054
AltMisc share	616	0.010	0.028	0.000	0.000	0.000	0.000	0.081
Other share	616	0.005	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 and state-level annual income per capita. 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)	3,023,415	242	130	93	152	211	299	500
Transaction Month	3,023,415	02/2010	56 Mos	04/2003	11/2005	11/2009	03/2014	07/2017
Border Dist (mi)	3,023,415	16.0	11.2	2	6	14	24	36
Building Age (yrs)	2,248,709	31.2	26.0	2	9	23	49	84
Sq Ft	2,243,188	1,900	869	845	1,310	1,690	2,210	4,250
Lot Sq Ft	2,579,288	23,650	127,656	2,500	5,000	8,500	14,500	60,500
# Bedrooms	1,862,434	3.29	0.73	2	3	3	4	4
# Bathrooms	2,233,187	4.42	1.37	2	4	4	5	7
Shortfall/Prop ($ '000s)	3,023,415	18.25	17.75	−1.63	5.24	14.29	26.15	56.39
02-14 Cum. Port. Ret.	531,695	142%	29%	55%	137%	143%	163%	184%
02-14 Cum. Excess Ret.	531,695	−2%	2%	−8%	−3%	−2%	−1%	1%
'02-14 Cum. Port. Ret. × '01 Assets/Prop($ '000s)	531,695	21.66	15.47	7.01	13.92	16.71	26.75	79.59
State-Year Income PC ($)	3,023,415	42,071	8,051	31,370	36,301	40,259	46,412	57,377

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 and typical system-wide annual contributions, 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. 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. Columns (4) and (5) are the same as column (3), but 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). Column (5) differs from column (4) in that it restricts attention only to pension plans run at a state level. Where applicable, we report either the Kleibergen-Paap F-test for weak identification or the adjusted R^2. 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	2SLS
	(1)	(2)	(3)	(4)	(5)
2002-2014 Windfall Per Property $('000s)	1.949***	2.372***	1.947***	1.948***	2.213***
	(6.63)	(8.61)	(6.59)	(6.61)	(4.98)
Border Distance	X	X	X	X	X
State-Year Income PC	X	X	X	X	X
Border Group-Tran Year FE	X	X	X	X	X
6 Prop Chars FE	X	X	X	X	X
Instrumental Variable	—	Excess Ret. Windfall Per Prop	Bnchm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop
Windfall Plan Inclusion	State and Local	State and Local	State and Local	State and Local	State Only
Observations	531,695	531,695	531,695	531,695	531,695
Adj. R^2	0.831				
Weak ID KP F Stat		151.5	25,766	19,196	5,837

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 and typical system-wide annual contributions, 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 either the Kleibergen-Paap F-test for weak identification or the adjusted R^2. 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)	1.772***	1.899***	1.617***	1.579***
	(9.69)	(8.93)	(9.38)	(9.37)
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	3,023,415	3,023,415	359,291	359,291
Adj. R^2	0.861		0.852	0.924
Weak ID KP F Stat		110.4		

Table 5

Political Mismanagement, Pension Windfalls and House Prices

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 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 (4) 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 a baseline regression conditioning on transactions in the years 2015-2018 where 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 and typical system-wide annual contributions, multiplied by initial assets per property in 2001 to create a measure of additional pension shortfall per property due to asset performance (Windfall). Column (3) is a baseline regression conditioning on transactions in the years 2015-2018 where 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 and typical system-wide annual contributions, but only in the years prior to the transaction since 2002, multiplied by the pension assets per property as of 2001 (Windfall). Column (5) is a baseline regression similar to column (3) but replaces the interacted property characteristic fixed effects and the distance to state-border covariate with a property-level fixed effect, and restricts to properties with repeat sales in the sample. Columns (2), (4), and (6) are specifications building off of columns (1), (3), and (5), respectively, but include a control for the number of public corruption convictions per million residents either over the period from 2002-2014 for column (2) or over the period from 2002 until the sale of the property for columns (4) and (6). 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 Property $('000s)	1.949***	1.913***				
	(6.63)	(6.70)				
2002-Sale Windfall Per Property $('000s)			1.772***	1.797***	1.579***	1.594***
			(9.69)	(10.03)	(9.37)	(9.44)
2002-2014 Public Corruption Convictions Per Million Residents		-0.130				
		(-0.75)				
2002-Sale Public Corruption Convictions Per Million Residents				0.0946		0.0563
				(0.86)		(0.34)
Border Distance	X	X	X	X		
State-Year Income PC	X	X	X	X	X	X
Border Group-Tran Year FE	X	X	X	X	X	X
6 Prop Chars FE	X	X	X	X		
Property FE					X	X
Observations	531,695	531,695	3,023,415	3,023,415	359,291	359,291
Adj. R^2	0.834	0.834	0.861	0.861	0.924	0.924

Table 6

Municipal Financial Constraints, Pension Windfalls, and House Prices

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 and examines 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 for the years prior to the transaction since 2002 and typical system-wide annual contributions, 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 a covariate for the income per capita at the state-year level, are included throughout. Also included is a property-level fixed effect to exploit within-property variation over time. The windfall measure is interacted with indicators for above median municipal bond spreads in the time period 2001-2003 (column 1), the per-capita outstanding municipal bond volumes in 2007 (column 2), and the per salary dollar outstanding municipal bond volumes in 2007 (column 3), 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)
2002-Sale Windfall Per Property $('000s)	0.644***	0.829***	0.772***
	(3.78)	(4.73)	(5.23)
2002-Sale Windfall Per Prop $('000s) × 2001-2003 County Municipal Bond Spread, Above Med.	1.308***		
	(7.43)		
2002-Sale Windfall Per Prop $('000s) × 2007 County Long-Term Municipal Bond Outstanding Per-Capita, Above Med.		0.724***	
		(5.07)	
2002-Sale Windfall Per Prop $('000s) × 2007 County Long-Term Muni. Bond Outstanding Scaled by 2002 County Salaries, Above Med.			0.878***
			(6.11)
State-Year Income PC	X	X	X
Border Group-Tran Year FE	X	X	X
Property FE	X	X	X
Observations	346,975	359,291	359,291
Adj. R^2	0.926	0.924	0.925

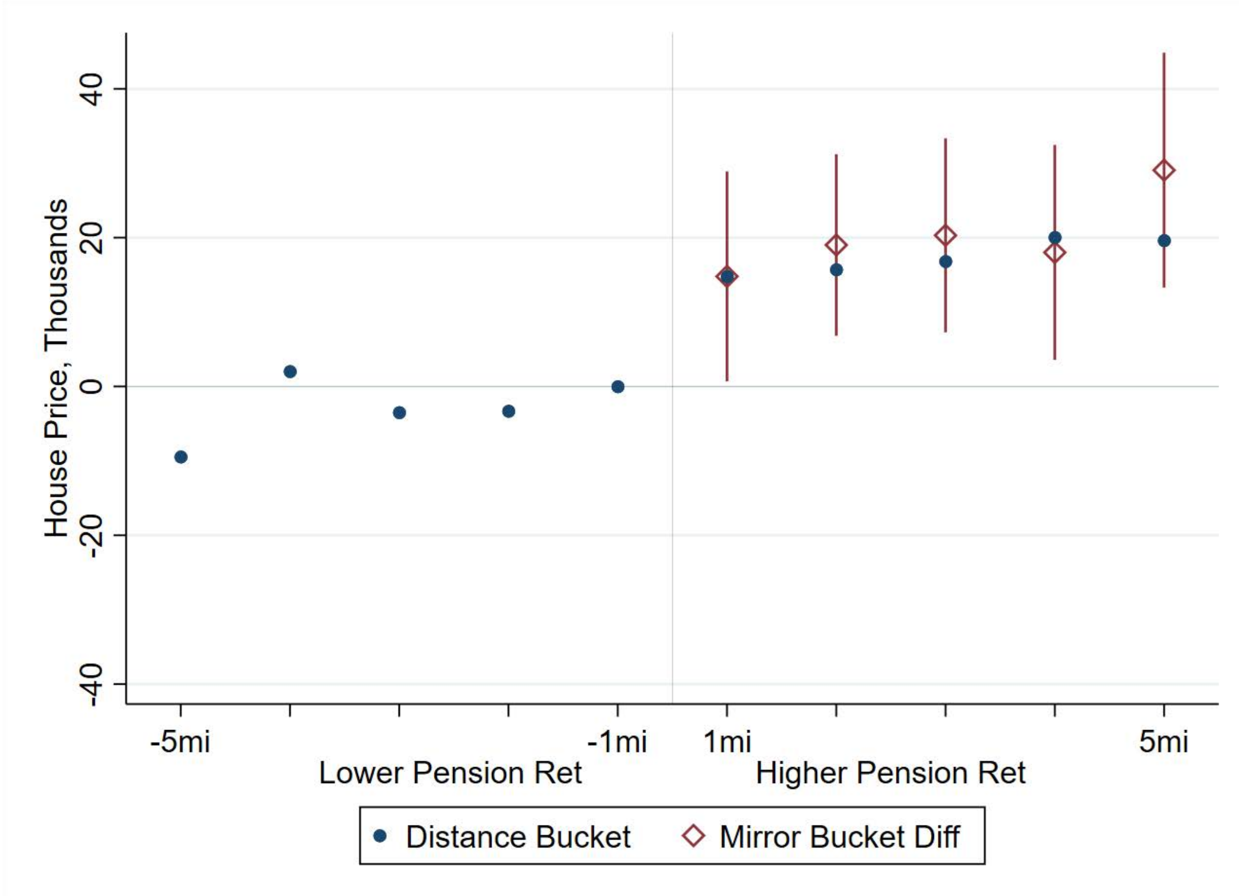

Figure 1. Pension Return Discontinuity in House Prices This figure presents nonparametric estimates of a border discontinuity design for house values related to transactions between 2015 and 2018 near the borders of states with differing pension asset performance between 2002 and 2014. We plot the coefficients for the five miles surrounding each border in our sample, with blue dots representing the primary coefficient of interest in Equation (4). 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 Theoretical Framework

In this section, we first 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).

Then in a stark setting with fixed housing supply, we show that house prices fully reflect the willingness to pay for (to avoid) (in)efficiencies in the public provision of goods and capital raising from net spending. We further show that, even if housing supply is not fixed, the net cost/benefit of additional public funds can still be fully captured by looking at the relative price of real estate between two integrated markets.

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, calculated 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 ($K_A \to 0$) 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 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.

[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).

A.3 Details of the model in Section 1.2

The model presented here is based on a slight modification of the asset pricing approach to tax incidence presented in Poterba (1984). The key component of the burden is the price change for existing real estate due to the change in the value of reductions in net spending associated with the asset. 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 generated by a housing stock H.

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:

$$R(H_t) = \eta \, q_{H,t} - \left(q_{H,t+1} - q_{H,t}\right), \tag{A.11}$$

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 change unfunded pension liability ΔL_t in period t. The government reduces net spending (by raising revenues Y_t and/or reducing expenses E_t) to cover the change in pension liability.[33] We assume the reduction in net spending induces a deadweight loss,[34]

$$-\left(E_t - Y_t\right) = \Delta L_t + f\left(\Delta L_t\right), \tag{A.12}$$

[33]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.

[34]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.[35] 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 (A.11) becomes

$$R(H_t) + (E_t - Y_t) = \eta \, q_{H,t} - \left(q_{H,t+1} - q_{H,t}\right). \tag{A.13}$$

Since $q_{H,t+1}$ is unknown at time t, we can solve the price $q_{H,t}$ forward by rewriting (A.13) as

$$q_{H,t} = \frac{R(H_t) + (E_t - Y_t) + q_{H,t+1}}{1+\eta}. \tag{A.14}$$

Iterating Equation (A.14) forward and applying the no-bubble condition,[36] the assumption of distortions from net spending reduction in (A.12) gives

$$q_{H,t} = \sum_{j=0}^{\infty} \frac{R(H_{t+j})}{(1+\eta)^{j+1}} - \sum_{j=0}^{\infty} \frac{\Delta L_{t+j} + f\left(\Delta L_{t+j}\right)}{(1+\eta)^{j+1}}. \tag{A.15}$$

The second term in Equation (A.15) is the present value of current and future net costs imposed to cover pension liabilities. For two neighboring states with integrated housing markets (i.e., facing identical supply and demand curves), the second term in Equation (A.15) captures the difference in real estate prices when only one of them experiences an exogenous shock to public pension funding.

[35]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).

[36]The transversality (no-bubble) condition in our setting is $\lim_{j\to\infty} \frac{q_{H,t+j}}{(1+\eta)^{j+1}} = 0$, which rules out exploding house prices. This condition is consistent with Giglio, Maggiori, and Stroebel (2016), who 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.

A.3.1 Fixed housing stock

If the stock of housing is fixed (i.e., $H_{t+j} = H_t$ for all j), then from Equation (A.15) we can determine the impact of an unfunded liability flow j periods ahead on house prices today:

$$\frac{dq_{H,t}}{d\Delta L_{t+j}} = -\frac{1 + f'\left(\Delta L_{t+j}\right)}{(1+\eta)^{j+1}} < 0. \tag{A.16}$$

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.

A.3.2 Endogenous housing stock

In this section, we demonstrate that even when the housing stock can change, the decrease in house prices induced by a one-dollar increase in pension shortfall in a particular state, compared to a neighboring state without the shortfall, will not be influenced by the elasticities of housing demand and supply if housing is considered immobile.

When the housing stock is endogenous, changes in future net cost induced by future pension liabilities will also affect current and future investment in housing construction and the stock of housing $\{H_t, H_{t+1}, \ldots\}$. In general, the effect of changing housing stock $\{H_{t+j}\}_{j=0}^{\infty}$ can offset the immediate effect of net costs on today's house prices. Let I_t denote gross construction of new housing. When prices decline due to increases in net costs, housing construction will decline. But this will raise the rental value of a unit of housing services helping to raise prices.

Assume that the home-building industry is perfectly competitive and the supply function for new construction is $I_t = S\left(q_{H,t}\right)$, where $S' > 0$. Then the net change in the housing stock is given by

$$H_{t+1} - H_t = S\left(q_{H,t}\right) - \delta H_t. \tag{A.17}$$

We can rewrite Equation (A.13) as

$$q_{H,t+1} - q_{H,t} = \eta q_{H,t} - R(H_t) + (L_t + f(L_t)) \,. \tag{A.18}$$

Equations (A.17) and (A.18) define the system of difference equations in (q_H, H). We can use these to analyze how the value of $q_{H,t}$ responds to a shock to $\{L_t\}$. A long-run steady-state where both q_H and H are constant (i.e., $\dot{q}_H = 0$ and $\dot{H} = 0$) is defined by:

$$S(q_H) = \delta H, \quad \text{and} \quad \eta q_H = R(H) - (L + f(L)) \,. \tag{A.19}$$

Figure A.1 illustrates the loci along which the housing stock is constant (i.e., $dH/dt = \dot{H} = 0$) and there are no capital gains ($\dot{q}_H = 0$). Point A is the housing equilibrium before the net cost shock (and also in the neighboring state that does not experience a cost shock). In the state with the net cost shock, the $\dot{q}_H$ curve shifts to the left, leading to a lower housing demand at every price q_H. If the housing stock is fixed at $H^\star$, the equilibrium in the state with a pension-induced cost shock moves to point C. Thus, the length of the line segment AC measures the housing price decline in the state with a net cost shock relative to the neighboring state that does not experience the shock. Here, we assume that both neighboring states face the same housing supply and demand.

With endogenous housing stock, when the system is out of equilibrium due to a net cost shock, the $\dot{q}_H$ curve shifts to the left, leading to a lower housing demand at every price q_H. House prices and the quantity of housing thus decrease, leading to the new steady-state point B in the state experiencing the pension-induced cost shock. Given that the neighboring state without the shock faces the same housing stock, the equilibrium in that state moves from point A to point D.[37] Therefore, the housing price decline is the length of the segment BD. Assuming linear supply and demand curves, the size of the drop is similar to the case where the housing supply is fixed (i.e., $AC = BD$). Crucially, as long as housing is immobile across state borders and neighboring

[37] As shown in Poterba (1984), the equilibrium exhibits saddle-point stability: following the shock, there is a unique downward-sloping stable path that leads to an equilibrium point. See Appendix A.5 for more details.

states face the same housing stock and demand, the relative drop in housing prices between the neighboring states in response to pension-induced shocks will not be impacted by the elasticities of housing demand or supply.

A.4 Proofs

A.4.1 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.4.2 Proof of Proposition 2

Proof. From Equation (A.16), the magnitude of the marginal decline in current house prices ($q_{H,t}$) from an additional dollar of pension shortfall j periods ahead (ΔL_{t+j}) depends on how large the distortion is and low far in the future the net revenue is raised. Conversely, the magnitude of the marginal increase in house prices from an additional dollar of pension windfalls, reflects households' WTP for a one dollar reduction in net public spending.

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. Therefore, the magnitude of the WTF is theoretically ambiguous. □

A.4.3 Proof of Proposition 3

Proof. The proof directly follows the graphical argument provided in Section A.3.2 from Figure A.1. □

A.5 Stability of the equilibrium with endogenous housing stock

Figure A.2 illustrates the loci along which the housing stock is constant ($\dot{H} = 0$) and there are no capital gains ($\dot{q}_H = 0$). We can use this figure to analyze the effects of a pension-induced net cost shock on the steady-state. Point A is an initial steady-state at $\left(H^\star, q_H^\star\right)$. When the system is out of equilibrium due to a net cost shock, the $\dot{q}_H$ curve shifts to the left, leading to a lower housing demand at every price q_H. House prices and the quantity of housing thus decrease, leading to the new steady-state point B. The equilibrium exhibits "saddle-point stability": there is a unique downward-sloping stable path (depicted in Figure A.2 as the path $B'B'$) that leads to the equilibrium new point B.[38] Conditional on a value of H, there is only one value of q_H that will result in the system evolving back to the equilibrium. This "stable arm" is the only path that satisfies the transversality condition. The housing stock when the shock arrives is fixed at $H^\star$, so the price must adjust to reach the stable arm at point C with $\left(H^\star, \hat{q}_H\right)$. From this point, as the system moves along the stable arm $B'B'$ to point B, the housing construction will decline, and the house price will rise.[39]

[38]For any level of the housing stock, unless the price of housing lies on $B'B'$ it will either become infinite or reach zero and cannot be on the equilibrium path. See Sheffrin (1996) for more details.

[39]The speed of convergence to the new equilibrium is faster when the $\dot{H} = 0$ curve becomes flatter. This can happen due to a lower depreciation rate or greater responsiveness of new housing production to the price of housing. Convergence will also be faster when $\dot{q}_H$ locus becomes steeper. This locus will tend to be steeper the greater the excess supply caused by an increase in the stock of housing and the smaller the excess supply created by the rise in the price of housing.

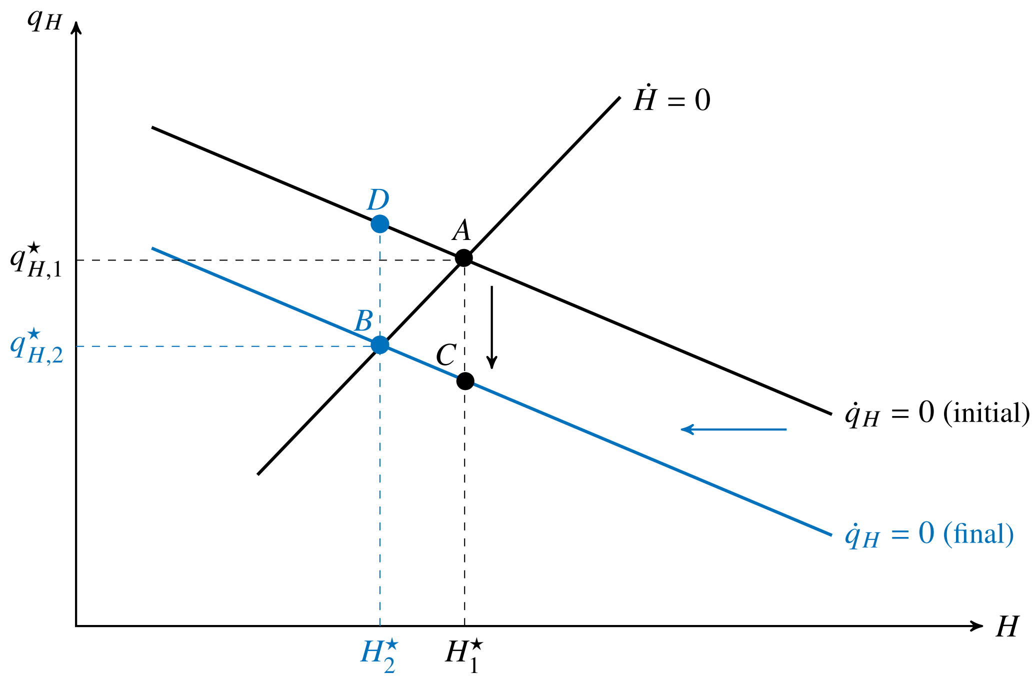

Figure A.1. Effect of a pension-induced cost shock.

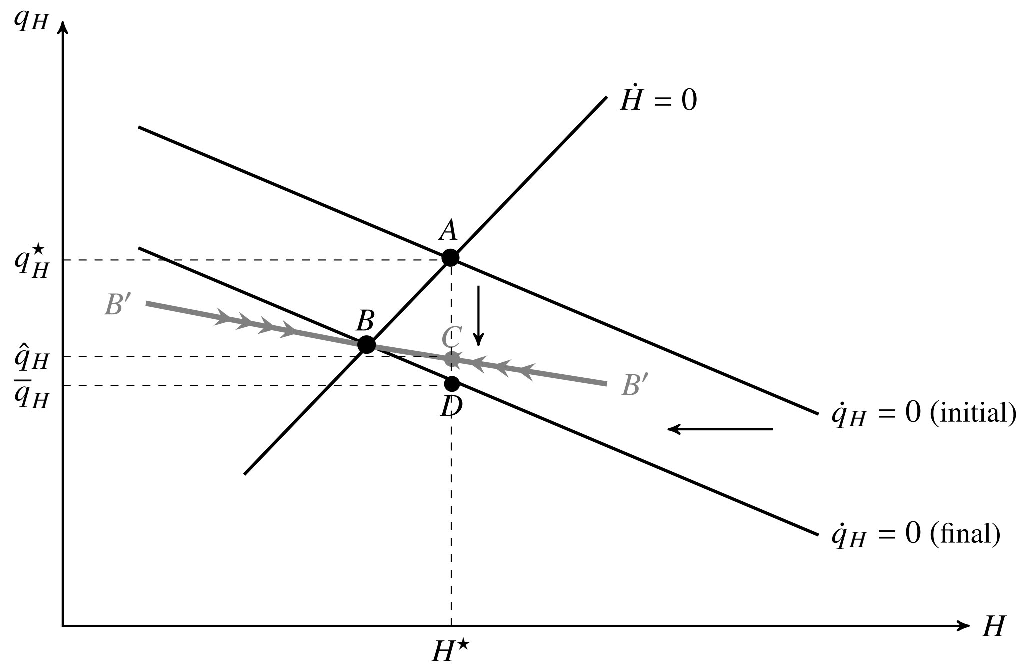

Figure A.2. Effect of a pension-induced cost shock: stability analysis.

B 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 C.11 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 and the relative value of those expenditures.

As an extreme example to illustrate this point, consider if 99.9 cents out of every dollar of windfall is immediately spent on a non-distortionary lump sum tax rebate with a WTP of $1 for every dollar spent. The correct MVPW would be $1 and that is what you would find regressing exogenous windfalls on values. If instead, however, shortfalls were erroneously used as the LHS variable, the reduced form would stay the same ($1), but the first stage would be 0.001, leading to a SLS (and implied MVPW) of 1/0.001 = $1,000! As the spending out of windfalls rises the 2SLS estimate becomes more and more baised upwards.

So, 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:

$$PropertyValue_{it} = \theta\,\widehat{ShortfallPerProperty}_{st} + \gamma_{bt} + \omega D_i + \lambda_l + \boldsymbol{\delta}'\boldsymbol{X_{lt}} + \epsilon_{it},$$
$$ShortfallPerProperty_{st} = \phi\,WindfallPerProperty_{st} + \eta_{bt} + \psi D_i + \mu_l + \boldsymbol{\rho}'\boldsymbol{X_{lt}} + \upsilon_{it}. \quad \text{(B.1)}$$

Relating this system of equations to the system in Equation (1), 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 B.1 presents estimates of Equation (B.1). 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 -0.37 indicates that each dollar of windfall causes the equilibrium shortfall to fall by about 37 cents. Since the shortfall must fall instantaneously by one dollar, this means that pension contributions are reduced by 63 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 (2) reported in Table 4, which is based on pension windfalls due to asset returns. Column (3) presents the second-stage coefficient from Equation (B.1), based on the level of pension shortfalls. The respective coefficient estimates of 1.77 and 4.80 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.

Table B.1

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 and typical system-wide annual contributions, 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 (B.1), where the endogenous variable is the observed net shortfall per property and the instrumental variable is windfall per property coming from pension asset performance. Column (3) is the specification in Equation (B.1) 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 either the Kleibergen-Paap F-test for weak identification or the adjusted R^2. 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)	1.772***	-0.369***	
	(9.69)	(-8.04)	
Shortfall Per Property $('000s)			-4.803***
			(-6.48)
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	3,023,415	3,023,415	3,023,415
Adj. R^2	0.861	0.934	
Weak ID KP F Stat			64.71

C Additional Tables and Figures

Table C.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	0.0081	0.0212	0.2549	Yes
AltInflation	616	0.0009	0.0057	0.0357	Yes
AltMisc	616	0.0133	0.0374	0.2127	Yes
Cash	616	0.0171	0.0214	0.8506	Yes
Commod	616	0.0023	0.0092	0.1802	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	0.0197	0.0600	0.2338	Yes
EQDomesticMid	616	0.0006	0.0031	0.0503	Yes
EQDomesticMisc	616	0.2530	0.1703	0.8506	Yes
EQDomesticSmall	616	0.0074	0.0246	0.2338	Yes
EQGlobal	616	0.0082	0.0339	0.1786	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	0.0072	0.0193	0.2208	Yes
EQIntlMisc	616	0.1216	0.0832	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	0.1004	0.1923	0.3782	Yes
EQPrivate	616	0.0574	0.0573	0.8198	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	0.0103	0.0636	0.0341	Yes
FIBelowInvestGrd	616	0.0005	0.0050	0.0097	Yes
FICash	616	0.0004	0.0039	0.0114	Yes
FIConv	616	0.0005	0.0037	0.0471	Yes
FICore	616	0.0171	0.0447	0.2435	Yes
FICorpBonds	616	0.0008	0.0059	0.0503	Yes
FIDomestic	616	0.0413	0.0971	0.3425	Yes
FIEmerg	616	0.0023	0.0102	0.0763	Yes
FIETI	616	0.0000	0.0001	0.0146	Yes
FIFundsFunds	616	0.0000	0.0001	0.0179	Yes

Asset Class	Obs.	Average Fund Allocation	Std. Dev Fund Allocation	Percent of State-Years with non-zero Allocation	Included in Restricted Benchmark
FIGlobal	616	0.0022	0.0134	0.0909	Yes
FIHighYield	616	0.0062	0.0151	0.2419	Yes
FIIntl	616	0.0052	0.0158	0.2208	Yes
FIInvestGrd	616	0.0035	0.0233	0.0471	Yes
FILoans	616	0.0001	0.0014	0.0211	Yes
FIMisc	616	0.1727	0.1241	0.7808	Yes
FIMortgage	616	0.0011	0.0058	0.0974	Yes
FINominal	616	0.0001	0.0011	0.0081	Yes
FINonCore	616	0.0000	0.0001	0.0016	Yes
FIOpp	616	0.0001	0.0006	0.0227	Yes
FIStructured	616	0.0001	0.0012	0.0130	Yes
FITIPS	616	0.0092	0.0288	0.2581	Yes
FITreasury	616	0.0006	0.0108	0.0227	Yes
FIValue	616	0.0016	0.0109	0.0260	Yes
GTAA	616	0.0050	0.0230	0.1461	No
Hedge	616	0.0099	0.0258	0.3052	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	0.1526	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	0.7289	Yes
PrivateDebt	616	0.0011	0.0069	0.0519	No
PrivatePlacement	616	0.0002	0.0010	0.0325	No
PrivRealEstate	616	0.0008	0.0061	0.0633	No
RealAssets	616	0.0041	0.0126	0.2143	No
RECore	616	0.0002	0.0032	0.0049	No
REIT	616	0.0004	0.0019	0.0877	Yes
RelativeRtrn	616	0.0000	0.0001	0.0162	Yes
REMisc	616	0.0516	0.0390	0.8328	Yes
RENonCore	616	0.0002	0.0029	0.0049	No
RiskParity	616	0.0017	0.0112	0.0584	Yes
Timber	616	0.0019	0.0072	0.1234	No

Table C.2

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 and typical system-wide annual contributions, 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 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). Column (5) is the same as column (1) but includes a control for level of 2001 pension assets as well as its interaction with cumulative returns. Column (6) is a placebo that regresses transaction prices occurring in the years 2003 through 2006 onto the pension portfolio return realized from 2015 to 2018. 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)	(5)	(6)
2002-Sale Cum. Port. Ret.	0.143***				0.0264	
	(5.91)				(1.34)	
2002-Sale Cum. Excess Ret.		0.137***				
		(3.34)				
2002-Sale Cum. BenchMk Ret.			0.142***			
			(5.76)			
2002-Sale Cum. (Restr.) BenchMk Ret.				0.133***		
				(5.47)		
2002-Sale Cum. Port. Ret. × 2001 Assets per HH					0.00311***	
					(6.92)	
2015-2018 Cum. Port. Ret.						-0.551
						(-1.43)
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	X
6 Prop Chars FE	X	X	X	X	X	X
2001 Assets Per HH					X	
Sample						2003-2006 Transactions
Observations	3,023,415	3,023,415	3,023,415	3,023,415	3,023,415	807,444
Adj. R^2	0.852	0.851	0.852	0.852	0.852	0.875

Table C.3

Political Mismanagement and Pension Asset Returns

This table presents estimates from regressions at the state-year level relating forward-looking cumulative portfolio returns (one through five years across columns) to backwards looking cumulative public corruption convictions (one through five years down rows). Panel A reports coefficients on public corruptions convictions with no fixed effects and no clustering, while Panel B includes both a state-level fixed effect as well as state-level clustering. As an example, row 3 column 3 regresses for, i.e., the focal observation of Alaska in 2012, regresses the portfolio return for Alaska's pension plans over the years 2013-2015 on the number of public corruption convictions in the state of Alaska from 2010-2012.

Panel A: No Fixed Effects, No Clustering

Public Corruption Convictions, Years Prior	Cumulative Portfolio Return, Years Forward 1	2	3	4	5
1	-0.0001	-0.0001	-0.0000	-0.0000	0.0001
	(-0.20)	(-0.21)	(-0.02)	(-0.03)	(0.07)
2	-0.0000	-0.0000	-0.0001	-0.0000	-0.0000
	(-0.08)	(-0.15)	(-0.17)	(-0.05)	(-0.01)
3	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001
	(-0.40)	(-0.47)	(-0.27)	(-0.21)	(-0.23)
4	-0.0000	-0.0000	-0.0000	-0.0000	-0.0001
	(-0.18)	(-0.24)	(-0.13)	(-0.18)	(-0.34)
5	-0.0000	-0.0000	-0.0000	-0.0001	-0.0001
	(-0.23)	(-0.27)	(-0.21)	(-0.41)	(-0.31)

Panel B: State Fixed Effects, State Clustering

Public Corruption Convictions, Years Prior	Cumulative Portfolio Return, Years Forward 1	2	3	4	5
1	-0.0002	-0.0004	-0.0002	0.0003	0.0011
	(-0.64)	(-0.92)	(-0.15)	(0.19)	(0.69)
2	-0.0000	-0.0002	-0.0000	0.0009	0.0022
	(-0.11)	(-0.38)	(0.00)	(0.60)	(1.18)
3	-0.0001	-0.0002	0.0001	0.0014	0.0023
	(-0.45)	(-0.72)	(0.20)	(1.18)	(1.54)
4	0.0000	-0.0001	0.0007	0.0019*	0.0026
	(0.17)	(-0.33)	(1.31)	(1.70)	(1.64)
5	0.0001	0.0000	0.0008	0.0017	0.0024
	(0.67)	(0.24)	(1.31)	(1.45)	(1.37)

Table C.4

Corruption Measure Robustness

This table replicates, in columns (1) and (2), columns (1) and (2) of Table 5. Columns (3), (4), and (5) substitute, for our measure of public corruption convictions, alternate measures following Campante and Do (2014), Saiz and Simonsohn (2013) and Boylan and Long (2003), respectively.

	Sales Price $('000s)				
	(1)	(2)	(3)	(4)	(5)
2002-2014 Windfall Per Property $('000s)	1.949*** (6.63)	1.913*** (6.70)	1.922*** (6.70)	2.007*** (7.21)	1.943*** (6.59)
2002-2014 Public Corruption Convictions Per Million Residents		-0.130 (-0.75)			
Corruption Robustness Measure Campante-Do, State-level			18.48 (0.94)		
Corruption Robustness Measure Saiz-Simonsohn, State-level				3,748 (0.47)	
Corruption Robustness Measure Boylan-Long, State-level					-3.963 (-0.86)
Border Distance	X	X	X	X	X
State-Year Income PC	X	X	X	X	X
Border Group-Tran Year FE	X	X	X	X	X
6 Prop Chars FE	X	X	X	X	X
Observations	531,695	531,695	531,695	531,695	531,695
Adj. R^2	0.834	0.834	0.834	0.834	0.834

Table C.5

Corruption Measure Robustness - Rolling Returns

This table replicates, in columns (1) and (2), columns (3) and (4) of Table 5. Columns (3), (4), and (5) substitute, for our measure of public corruption convictions, alternate measures following Campante and Do (2014), Saiz and Simonsohn (2013) and Boylan and Long (2003), respectively.

	Sales Price $('000s)				
	(1)	(2)	(3)	(4)	(5)
2002-Sale Windfall Per Property $('000s)	1.772***	1.797***	1.734***	1.764***	1.764***
	(9.69)	(10.03)	(9.81)	(11.00)	(9.61)
2002-Sale Public Corruption Convictions Per Million Residents		0.0946			
		(0.86)			
Corruption Robustness Measure Campante-Do, State-level			45.58***		
			(2.61)		
Corruption Robustness Measure Saiz-Simonsohn, State-level				-877.9	
				(-0.14)	
Corruption Robustness Measure Boylan-Long, State-level					-5.336
					(-1.31)
Border Distance	X	X	X	X	X
State-Year Income PC	X	X	X	X	X
Border Group-Tran Year FE	X	X	X	X	X
6 Prop Chars FE	X	X	X	X	X
Observations	3,023,415	3,023,415	3,023,415	3,023,415	3,023,415
Adj. R^2	0.861	0.861	0.861	0.861	0.861

Table C.6

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 and typical system-wide annual contributions 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)	2.429***	1.954***	1.182***
	(6.10)	(6.60)	(5.75)
2002-2014 Windfall Per Prop $('000s) × Border Distance (mi)	-0.0493**		
	(-2.29)		
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	531,695	531,695	2,808,664
Adj. R^2	0.835	0.834	0.730

Table C.7

Pension Windfalls and House Prices in Border Counties
Robustness

This table presents estimates similar to Column (4) of Table 3, where column (1) replicates this column, columns (2)-(4) vary the clustering, columns (5) and (6) vary the fixed effects and covariate structure, and column (7) varies the pension return calculation method in the instrument. Specifically, column (2) drops transaction month clustering, column (3) drops zip clustering, column (4) substitutes county clustering for zip clustering, column (5) drops the six interacted property characteristic fixed effects, column (6) drops the state by year per capita income control, and column (8) instruments with windfall per property utilizing pension returns calculated under the Andonov-Rauh methodology. We report the Kleibergen-Paap F-test for weak identification. Reported t-statistics in parentheses are heteroskedasticity-robust and clustered at the level indicated. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales Price $('000s)						
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2002-2014 Windfall Per Property $('000s)	1.948***	1.948***	1.948***	1.948***	1.856***	1.897***	1.818***
	(6.61)	(6.85)	(19.60)	(3.77)	(9.58)	(6.49)	(6.38)
Border Distance	X	X	X	X	X	X	X
State-Year Income PC	X	X	X	X	X		X
6 Prop Chars FE	X	X	X	X		X	X
Border Group-Tran Year FE	X	X	X	X	X	X	X
Transaction Month Clustering	X		X	X	X	X	X
Zip Clustering	X	X			X	X	X
County Clustering				X			
Instrumental Variable	Restr. Bm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop	Restr. Bm. Ret. Windfall Per Prop	Andonov-Rauh Ret. Windfall Per Prop
Observations	531,695	531,695	534,683	534,683	609,971	532,509	531,695
Weak ID KP F Stat	19,196	19,363	361,402	3,402	54,680	17,973	1,295

Table C.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 and typical system-wide annual contributions 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)	1.745*		
	(1.69)		
2002-Sale Windfall Per Property $('000s)		2.259***	2.127***
		(3.70)	(4.60)
Residential Property Indicator	-90.62***	-44.01***	5.688
	(-3.65)	(-4.17)	(0.11)
2002-2014 Windfall Per Prop $('000s) × Residential Property Indicator	0.421		
	(0.41)		
2002-Sale Windfall Per Prop $('000s) × Residential Property Indicator		-0.429	-0.590
		(-0.71)	(-1.38)
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	536,818	3,071,456	382,379
Adj. R^2	0.800	0.829	0.869

Table C.9

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 C.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	0.01	0.16*	Total Expenditures	-0.01	0.18*
Per Capita	(0.03)	(1.77)	Per Capita	(-0.04)	(1.91)
Revenues From Federal Govt	0.00	0.01	Capital Expenditures	-0.03	0.01
Per Capita	(0.05)	(0.80)	Per Capita	(-0.81)	(0.49)
Revenues From State Govt	0.04	0.09***	Education Expenditures	ro	0.00
Per Capita	(0.60)	(3.50)	Per Capita	(-0.68)	(0.08)
Total Taxes	-0.08	-0.03	Safety Expenditures	-0.01	0.00
Per Capita	(-0.79)	(-0.84)	Per Capita	(-0.33)	(0.67)
Property Taxes	-0.07	-0.04	Utility Expenditures	0.08	0.05
Per Capita	(-0.93)	(-1.26)	Per Capita	(1.17)	(0.90)
Sales Taxes	0.00	0.00	Short-Term Debt	-0.01	0.00
Per Capita	(-0.08)	(0.48)	Per Capita	(-1.20)	(-0.03)
Income Taxes	-0.02	0.00	Long-Term Debt	0.83	0.62*
Per Capita	(-0.85)	(0.27)	Per Capita	(1.40)	(1.72)
Other Taxes	0.00	0.00			
Per Capita	(0.00)	(0.46)			

Table C.10

Pension Windfalls and House Prices in Border Counties External Validity

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 and typical system-wide annual contributions 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 C.9 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 by year fixed effect with just a fixed effect for the 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 Property $('000s)	1.899***	1.911***	1.896***	1.915***	1.898***	1.847***
	(6.71)	(6.87)	(6.74)	(6.75)	(6.94)	(4.25)
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	
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)	
Observations	531,695	531,695	531,695	531,695	531,695	531,695
Adj. R^2	0.836	0.837	0.836	0.836	0.838	0.752

Table C.11

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	

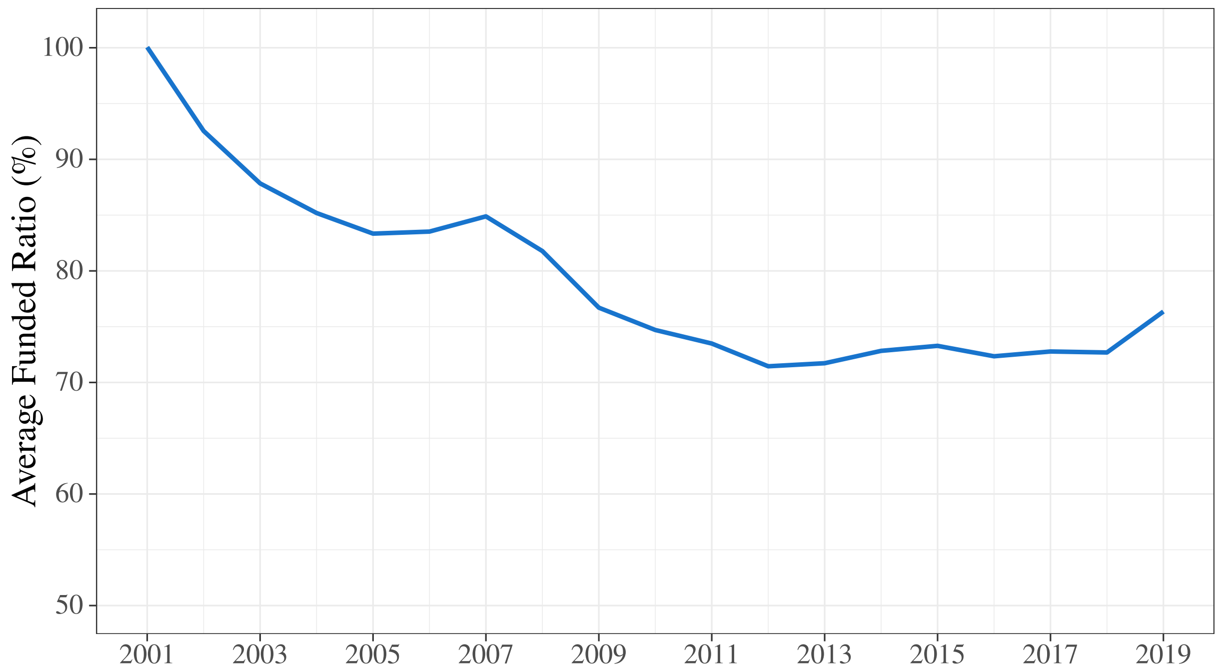

Figure C.1. Average Funded Ratio This figure presents the time-series of average ratio of pension assets to liabilities, the actuarial funded ratio, at the state-year level for the Public Plans Data (PPD) database provide by the Center for Retirement Research (CRR) at Boston College. Actuarial funded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

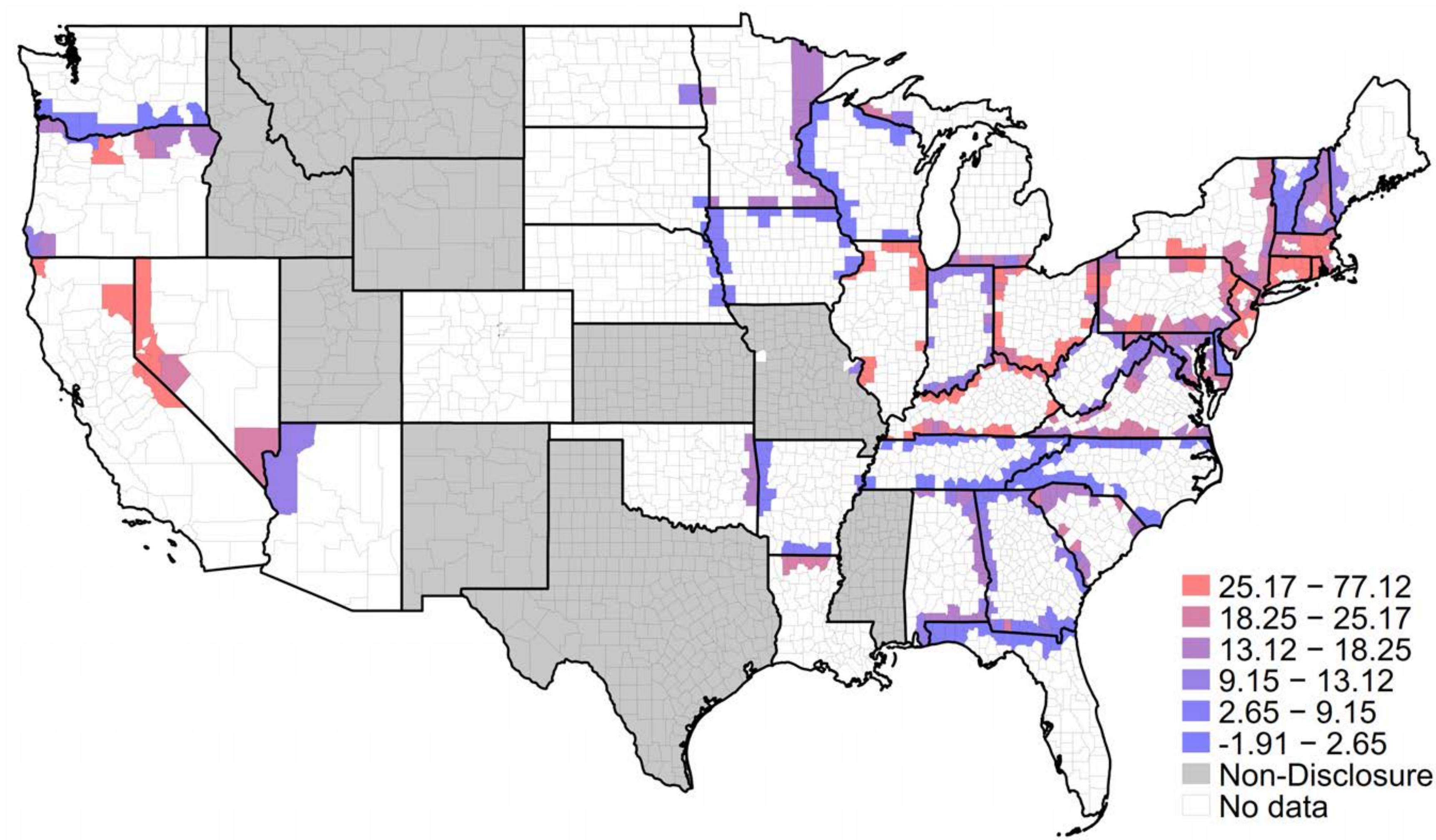

Figure C.2. State-Level Shortfalls by County This figure presents the state-level pension shortfall, in thousands of dollars, averaged over properties in each county in our sample. The sample includes all transacting properties that qualify for the regressions in Table 4 and covers the full sample period 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. Gray states (or counties, in Missouri) are non-disclosure and do not report public transaction price information. Our sample contains 70.5% of counties in disclosure states that lay on a border with a different disclosure state by count. Weighting counties by the number of housing transactions in the full ZTRAX sample over our sample period that are within 50 miles of the county border, our sample contains 95.1% of eligible counties.