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Essays on Financial Economics

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Abstract

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My dissertation has three chapters. In the first chapter, I show that property tax rates among single family homes in the United States are regressive with respect to sale price because tax assessors use flawed valuation models that ignore priced house and neighborhood characteristics. The insight from this chapter is that a wealth tax system that requires the government to value assets that do not have readily available market prices would tend to increase wealth inequality among asset owners. In the second chapter, I show that failure of bond insurance companies during the Global Financial Crisis constrained local municipalities' ability to borrow from the municipal bond market and employ workers. Results from this chapter show why, during the financial crisis, local governments were unable to borrow and spend more to support local economies. In the last chapter, I show that social similarities such as school and ethnic ties between venture capital investors and startup founders increase the likelihood of collaboration and investment success. These results suggest that the type of social traits venture capital investors use to form business partnerships matters for investment outcomes.

Essays on Financial Economics

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Chapter I

Why Are Residential Property Tax Rates Regressive?

1 Introduction

Effective property tax rates – property tax bill as percentage of sale price – among houses that enjoy the same set property tax funded-amenities and pay the same statutory tax rate are regressive with respect to house prices. Figure 1 plots mean scaled effective tax rates for each of twenty sale price bins among houses located in the same tax code area (TCA) in 2016.[1] Each house's effective tax rate is scaled by the median effective tax rate in its TCA. Houses in the bottom decile of the sale price distribution pays an effective tax rate that is, on average, approximately 50% higher than houses in the top decile of the sale price distribution. The wedge between observed effective tax rates and stated statutory tax rates arises from assessment regressivity – inexpensive houses being overappraised relative to expensive houses. This plot uses data from 49 states and the District of Columbia, which shows that this pattern is the norm rather than the exception.

This article has two objectives. The first objective is to explain the source of assessment regressivity. I argue that common valuation methods such as the comparable sales approach and the hedonic pricing method assign appraised values to houses based on observable house characteristics and cause assessment regressivity by systematically ignoring variation in difficult-to-quantify house and neighborhood characteristics. An example of difficult-to-quantify house characteristic is construction quality. Similarly, amenities quality is a difficult-to-measure neighborhood characteristic. To gain some intuition, consider two houses with identical observable house characteristics, but are located in two different neighborhoods. These two houses would be assigned the same appraised value, but have different true market values and realized sale prices. The house located in the worse neighborhood would be overappraised and overtaxed, while the house located in the better neighborhood would be underappraised and undertaxed.

[1] A tax code area is a small geographical area where every house within the perimeter pays the same statutory tax rate and has access to the same set of government services funded by their property tax dollars. Additional institutional details on tax code areas are provided in section 2.

It is important to note that the picture shown in figure 1 does not necessarily show that assessments are regressive. In a world where appraised values are exactly equal to true market values and realized sale prices are noisy, there is no assessment regressivity, but a pattern similar to figure 1 would still appear because of attenuation bias. This article's objective is to provide empirical evidence that is consistent with the flawed valuation method story. By exploiting the data set's large sample size, I show that assessment regressivity is worse in TCA-years where, on top of variation in house characteristics, variation in neighborhood characteristics can explain a substantial proportion of variation in realized sale prices. This result shows that assessment regressivity is worse in instances where house characteristics-based valuation methods perform poorly because they ignore variation in neighborhood characteristics. An exercise that predicts realized sale prices by combining previous sale prices with innovations in zip code-level house price indexes suggests that the flawed valuation method story can explain at least 30% of the observed regressivity. These findings imply that the property tax system unwittingly discriminates against homeowners who sort into houses that are cheap because of latent house and neighborhood characteristics.

Other exaplanations are also considered. I consider the infrequent reappraisal explanation by comparing assessment regressivity among all houses that were sold in 2018 to those that were reappraised and sold in 2018. Eliminating houses with stale appraised values from the sample reduces observed assessment regressivity by less than 10%, which indicates that infrequent reappraisal is a relatively minor contributor. Second, I use appeals data from Cook County, Illinois, to rule out hetergeneous appeal behavior and outcomes as a potential explanation. Within a TCA, owners of relatively more expensive houses are *not* more likely to appeal assigned appraised values, are *not* more likely to win appeals, and do *not* receive larger appraised value discounts upon winning.

This article's second objective is to combine the concept of a TCA and a *nationally* comprehensive property tax data set to quantify the impact that assessment regressivity has on aggregate wealth inequality. For each house, I compute the counterfactual property tax rate that would prevail, if houses were taxed according to their sale prices, instead of their assessed values. Treating these excess tax payments as perpetuities and applying a discount rate of 4% shows that correcting the observed assessment regressivity would increase poor homeowners' wealth by 17%, decrease richest homeowners' wealth by 3%, and reduce the wealth gap between the two groups by 3%.

These calculations suggest that assessment regressivity contributes to the nation's aggregate wealth inequality by transferring housing wealth from poor homeowners to rich homeowners. A key insight from this paper is that a wealth tax system that uses similar valuation methods to appraise thinly traded assets would increase wealth inequality among asset owners.

In the last part of the paper, I merge HMDA data with CoreLogic data to show that assessment gaps between economically disadvantaged households and their wealthier counterparts are by-products of assessment regressivity. In particular, overtaxation of minorities and low-income households arises *mechanically* in a world where assessment regressivity exists because these households sort into cheap houses. Comparing mortgage holders within the same TCA-year price decile, I find that black mortgage holders are proportionately taxed, while Hispanic and low-income mortgage holders are undertaxed, relative to their respective reference groups. These results suggest that tax assessors do not discriminate households on race or income, but, albeit unintentionally, discriminate on house price.

I contribute to the vast literature on assessment regressivity in two ways.[2] The first contribution that this article makes is to provide a general explanation for assessment regressivity and use a nationally comprehensive data set to test it. Existing research on sources of assessment regressivity use city or county-specific data sets to document and explain this phenomenon, which limit their ability to provide a general story that explains this pattern's ubiquitous nature (Paglin and Fogarty, 1972; Eom, 2008; Weber and McMillen, 2010; Ross, 2013, 2012; McMillen, 2013). This article shows that, for the general United States, flawed valuation methods can explain at least 30% of the observed assessment regressivity, while infrequent reappraisal can explain less than 10%. These results are important because critics of the appraisal process often cite infrequent reappraisal as the main source of assessment regressivity (County of Monmouth, 2019). Second, I use the concept of a tax code area to quantify the aggregate effect that assessment regressivity has on wealth inequality. Existing works document assessment regressivity at the city or the county-level, but cannot perform similar counterfactual calculations because it is not reasonable to assume that every house in the city pays the same statutory tax rate and has access to the same set of property tax-funded services (Black, 1977; Smith et al., 2003; Hodge et al., 2017; McMillen and Singh, 2020).

[2] Consult Sirmans et al. (2008) for a literature review.

I add to a growing body of works that studies unintended consequences of algorithms and statistical procedures (Bartlett et al., 2018; Fuster et al., 2018; Kleinberg et al., 2018). I show that mass appraisal methods employed by county assessor's offices produce appraised values that overappraise inexpensive houses and underappraise expensive houses. Since individuals with lower income sort into inexpensive houses, the property tax system ends up overtaxing economically disadvantaged households such as blacks, Hispanics, and low-income households.

Lastly, this article contributes to the property tax literature beyond Avenancio-León and Howard (2019) in several ways. First, I use administrative TCA data, which ensures that I am comparing houses that truly have access to the same set of property-tax funded public amenities. Avenancio-León and Howard (2019) attempt to construct these TCA boundaries by overlaying GIS files. The procedure would produce incorrect taxing boundaries if the GIS files are incomplete or incorrect. Second, this article focuses on quantifying the sources of assessment regressivity and its economic impact on household wealth inequality, while Avenancio-León and Howard (2019) document and explain assessment gaps between racial groups. Lastly, I show that, once I control for the fact that minorities sort into cheap houses, the black and Hispanic assessment gaps that Avenancio-León and Howard (2019) study disappear. This last finding sharpens Avenancio-León and Howard (2019)'s results by ruling out direct discrimination against minorities.

The article is organized as follows. Section 2 reviews important institutional details related to residential property tax in the United States. Section 3 describes key data sets. Section 4 discusses the methodology. Section 5 proposes and tests the flawed valuation methods explanation. Section 6 considers infrequent reappraisals and heterogeneous appeal behavior and outcomes as potential explanations. Section 7 quantifies assessment regressivity's impact on wealth inequality. Section 8 shows that overtaxation of economically disadvantaged groups is a by-product of assessment regressivity. Section 9 concludes.

2 Institutional Details

2.1 Property Tax Basics

Real estate property tax is a form of ad valorem tax where the tax bill is calculated from the property's assessed value (Lincoln Institute of Land Policy, 2014). The tax bill is the product of two components: the house's assesed value, V_i, and the statutory tax rate , τ^s.

$$T_i = \tau^s \times V_i \tag{1}$$

To compute the house's assessed value, the government first assigns an appraised value to the house. The appraised value should, by law, reflect the house's true market value that would result from an arm's length transaction (Lincoln Institute of Land Policy, 2014). The appraisals are periodically done by the county's or city's assessor's office. The assessed value, which is the quantity that the tax rate is to be applied to, is a proportion of the house's appraised value. This proportion, or the assessment ratio, is arbitrarily chosen by a local government entity (Lincoln Institute of Land Policy, 2014). For example, Washington D.C. uses an assessment ratio of one, while the state of Illinois chooses to use an assessment ratio of one third (Lincoln Institute of Land Policy, 2014). This piece of institutional detail adds an additional layer of complexity to the property tax system but has no economic meaning in the following analyses because the assessment ratio is constant within tax code area. To arrive at each house's final assessed value, relevant exemptions are applied. Each local jurisdiction has its own set of idiosyncratic property tax exemptions. For example, Alabama has a homestead exemption that allows homeowners to substract $15,000 from their houses' assessed values.[3]. With an assessed value assigned to each house in its taxing jurisdiction, the taxing entity can calculate the total tax base, which it uses to compute the statutory tax rate that is applied to each house's assessed value.

The statutory tax rate is computed by dividing the taxing entity's total budgetary need for the year by its tax base. The entity's total revenue from property taxes in each year is either decided by a

[3] Ala. Code 6-10-2, 27-14-29

vote at the ballot box or by an elected official (Avenancio-León and Howard, 2019). The property tax bill for a house that is taxed by a single entity is calculated in the following way.

$$T_i = \frac{R}{\sum_{i=1}^{n} V_i} \times V_i = \tau^s \times V_i \tag{2}$$

R is the total revenue that the taxing entity wishes to raise from residential property taxes and $\sum_{j=1}^{n} V_i$ is the entity's total property tax base. Unlike the federal income tax, property taxes under this formulation are uniform, neither regressive nor progressive with respect to the market value of each house.

2.2 Tax Code Areas

In practice, each house is served and taxed by many local government entities, e.g. school districts and local fire departments. Each taxing entity has its own service jurisdiction, which encompasses a certain set of houses. Using assessed value data from the local assessor's office, each taxing entity calculates its total tax base and comes up with its own revenue target and, hence, its own statutory property tax rate. With overlapping service boundaries, each house is assigned to a tax code area (TCA), which is a geographic region that has a unique set of local government entities that serve and tax it. Every house in a TCA pays the same statutory property tax rate, which is the sum of the tax rates imposed by each taxing entity, and, in turn, enjoys the same set of property tax-funded services. In practice, a house's property tax bill is calculated as follows.

$$T_{ik} = \sum_{j=1}^{m} \tau_j^s \times V_{ik} = \tau_k^s \times V_{ik} \tag{3}$$

k is the index for TCAs, j is the index for taxing entities within a TCA. Figure 2 shows a list of all local government entities that collect property taxes from houses in three TCAs in Snohomish County, WA, for the 2020 tax year. First, each TCA has different statutory tax rates. The statutory property tax rate in TCA number 18 is \$11.026 per \$1,000 of assessed value, while the rate in TCA number 20 is \$11.225. The difference in tax rates stems from the fact that houses in each TCA are being served by a different sets of local governments. For example, houses in TCA number 21 pay a higher property tax rate than houses in TCA number 20 because houses in TCA number 21 have access to the Central Puget Sound Regional Transit Authority, which is a network of commuter rails and buses that serve the area. Thus, this additional public amenity comes with an additional cost of 0.23 cents per $1,000 of assessed value.

Figure 3 presents a map of several TCAs in Snohomish County, WA. TCA numbers and boundaries are shown in red. The map contains several TCAs with varying sizes and shapes. For example, TCA number 04110 is small, while TCA number 03992 is large. In particular, TCA number 03992 contains multiple neighborhoods, represented by separate clusters of parcels, which suggests significant variation in neighborhood characteristics within the same TCA.

2.3 Property Tax Rate Uniformity

Within-TCA effective property tax rates across houses are not equal because valuation ratios are not uniform. Define the valuation ratio as $\frac{A_i}{M_i^*}$ where M_i^* denotes house i's true market value and A_i denotes house i's appraised value. If there is a negative relationship between valuation ratios and true market values, then inexpensive houses are relatively overassessed and effective property tax rates are regressive. If there is a positive relationship between valuation ratios and true market values, then inexpensive houses are relatively underassessed and effective property tax rates are progressive. The absence of any correlation between valuation ratios and true market values indicates an equitable effective property tax rates.

Researchers have documented assessment regressivity among houses in the same city and county (Hodge et al., 2017; McMillen and Singh, 2020; Smith et al., 2003). However, without looking within TCAs, these findings do not necessarily show that effective property tax rates are regressive. Cheap houses are likely to be located in an area served by a set of local governments that differs from areas where expensive houses are located. Hence, the comparison of relative valuation ratio disparity between these two groups of houses is not an apples-to-apples comparison. A researcher could find a negative relationship between valuation ratios and house prices among houses in the same city, while there is no such relationship within each TCA. The intra-city assessment regressivity result suggests regressive effective *city* property tax rates, but not necessarily, effective *total* property tax rates, which is the more important economic quantity.

3 Data

The first main data set that the paper uses is the CoreLogic Tax data set, which contains property tax-related data and parcel characteristics for approximately 150 million property parcels in the United States. The data set covers every type of real estate parcels, e.g., residential, commerical, industrial, agricultural, vacant, and tax-exempt. This study focuses on single family residential real estate parcels. For most parcels, the data set contains 10 years of tax data and so this article mainly uses data from 2007 to 2018. Tax-related variables include property tax bill, tax year, appraised value, assessed value, appraisal year, exemption indicators, and tax code areas. Parcel characteristics include land and property information such land area size, total living area, number of bedrooms, number of bathrooms, etc.

A key innovation in this paper is the tax code area (TCA) data. Each parcel is assigned to a TCA, which allows me to control for property tax-funded public services across houses. For example, each house in Snohomish County that appears in the data set is assigned to a TCA numbered similarly to the ones displayed in figure 2.[4] The CoreLogic data set has TCA data for all states, except for Massachusetts. Figure A1 shows that statutory tax rates are uniform within TCAs, which verifies that the TCA data is accurate. Median scaled statutory tax rates, tax bill divided by assessed value, are plotted against within-TCA house price bins for single family homes in 2016. Each house's statutory tax rate is scaled by the TCA's median statutory tax rate. The plot shows that the median house in every price bin pays the same statutory tax rate.[5]

The second main data set that the paper uses is the CoreLogic Deeds data set, which contains

[4]This data differs from Avenancio-León and Howard (2019) because I observe TCA assignments collected from county assessor's offices instead of using GIS area files to construct "taxing jurisdictions" from overlaying taxing boundaries of each local government entity. The latter methodology will likely produce errors if the list of taxing entities is incomplete or the GIS area files are inaccurate.

[5]Medians are plotted instead of means because I observe pre-exemption assessed values and actual tax bills, which includes idiosyncratic exemptions such as exemptions for the elderly. Therefore, plotting the means would not give the same picture because these exemptions introduce deviations in statutory rates around the median.

transaction information on real estate properties in the United States. The transaction information includes sale price, sale date, transaction type, mortgage amount, and lender name. I only use arm's length transactions in my analyses. Both data sets are collected from county governments, which are local government units responsible for administering property taxes and keeping deed records. The CoreLogic Tax data set can be merged with the CoreLogic Deeds data set by using unique county-provided parcel identifiers that link land parcels across data sets.

The 5-year averages of census tract block group characteristics provided by the Census Bureau's American Community Surveys (ACS) are used to construct neighborhood characteristic variables. I follow the urban economics literature and make the implicit assumption is a census tract block group is a neighborhood (Davis et al., 2019). As shown in the previous section, TCAs can be large and contain multiple census tract block groups, which allows me to study within-TCA variation in neighborhood characteristics.

The last data set that I use is the Home Mortgage Disclosure Act (HMDA) data set. The data set contains mortgage applicants' race, ethnicity, and income. These variables are merged into the main CoreLogic data set by matching mortgage grant year, mortgage amount, mortgage type, property census tract, and lender name. For lender name, I perform a fuzzy merge procedure that yields a 3% average error rate. This merging procedure is standard in the real estate literature (Bayer et al., 2017; Avenancio-León and Howard, 2019; McMillen and Singh, 2020).

4 Methodology

This article attempts to explain the origin of assessment regressivity by studying its variation across space and time. To measure assessment regressivity for a certain TCA-year, I run the following within-TCA-year regression.

$$logA_{it} - logM_{it} = \alpha + \beta logM_{it} + \epsilon_{it} \tag{4}$$

A denotes appraised value, M denotes appraised value, i indexex houses, and t indexes years. The log valuation ratio is regressed onto the log of sale price and β captures the degree of assessment regressivity.

$$\beta = \frac{Cov(a-m,m)}{\sigma_m^2} = \frac{Cov(a,m)}{\sigma_m^2} - 1 \tag{5}$$

β is negative if the covariance between log of appraised value and log of sale price is less than the variance of log sale price. It is important to note that this regression is biased towards finding a negative slope coefficient, which suggests that assessments are regressive, while in reality, it may not be (Kochin and Parks, 1982; McMillen and Singh, 2020). Consider the case where appraised values are exactly equal to true market values, but sale price is a noisy proxy of appraised value. Then, mechanically, β is negative, but, by assumption, there is no assessment regressivity. In the subsequent sections, I show empirical evidence that is consistent with a world in which assessment regressivity is produced by local assessors' valuation methods that fail to capture variation in priced house and neighborhood characteristics that are difficult to quantify. In particular, these results rule out the possibility that the observed assessment regressivity is entirely caused by noisy realized sale prices.

5 Flawed Valuation Methods Explanation

In this section, I lay out my arguments for how assessment regressivity could arise from appraisers' flawed valuation methods, which ignore priced house and neighborhood characteristics. I begin by showing that common appraisal methods such as the comparable sales approach and the hedonic pricing method mechanically produce assessment regressivity. Next, I propose predictions from this story and use my national data set of residential property taxes to verify them. Lastly, I quantify the proportion of the aggregate observed assessment regressivity that can be explained by the flawed valuation methods mechanism.

5.1 Intuition

The intuition for the explanation is the following. Consider two houses that have the exact same set of observable structure attributes (e.g., number of bedrooms, number of bathrooms, and living area square footage) and are located in the same TCA. One house is located in a good neighborhood, while the other is located in a bad one. An appraisal method that ignores neighborhood quality would assign the same appraised values to these houses. On the other hand, the market would assign very different prices to these houses because the one in the bad neighborhood would receive a much lower price. Upon sales, the econometrician would observe that β calculated from these two houses is negative. The same intuition applies if the overlooked characteristics are house-related, e.g. construction quality, which is important for house price, but difficult to quantify. I focus on neighborhood characteristics in the rest of the paper becuase I can measure them. The neighborhood characteristics that I have in mind can be thought of as very fine geographical area fixed effects that capture neighborhood quality such as crime rate and pollution. Variation in neighborhood characteristics within a small geogrpahical area can be large. Ananat (2011) shows that neighborhood characteristics can differ significantly over short distances. In the following subsections, I show that common valuation methods used by county assessors tend to yield insufficient covariance between appraised values and realized sale prices.

5.2 Comparable Sales Approach

I first consider the comparable sales approach (CSA). Under the comparable sales approach, the appraiser begins by finding recently transacted houses that have similar characteristics to the house under consideration. These comparable houses should be located in the same neighborhood as the house in question. The definition of a neighborhood or a comparable area is subjectively defined by the appraiser. In the final step, the appraiser calculates the average price per square foot from these comparable sales and use that quantity to assign an appraised value for the house under consideration (FNMA, 2020). The reason that CSA produces assessment regressivity is the coarseness in the degree in which appraisers define comparable areas. For example, figure 4 shows the map of Snohomish County with 2019 benchmark areas drawn with blue boundaries

(Snohomish County Assessor's Office, 2019b). Houses in the same benchmark area are considered to be geographically comparable to each other.[6] Notice that these benchmark areas are much larger than a TCA. Therefore, the mean neighborhood characteristics that are captured in the CSA average price calculation gives rise to insufficient variation in appraised values within a TCA and, thus, insufficient covariation with realized sale prices.

To see this assertion formally, suppose that sale prices reflect true market values and let house i's price per square foot be defined as follows.

$$\frac{M_i}{S_i} := M_i^{SQ} \tag{6}$$

M_i is house i's total sale price and S_i is house i's sqaure footage. To price a certain house j, the appraiser finds several comparable houses and computes the average price per square foot from their observed sale prices. House j's appraised value is as follows.

$$A_j = \overline{M_{i \neq j}^{SQ}} \times S_j \tag{7}$$

$\overline{M_{i \neq j}^{SQ}}$ is the sample mean of price per square foot calculated from chosen comparable houses. The natural log of house j's appraised value is as follows.

$$a_j = \overline{m_{i \neq j}^{SQ}} + s_j \tag{8}$$

Let X be a random variable and $\overline{X}$ be its sample mean. By the result that $Cov(\overline{X}, X) < Cov(X, X)$, it follows that $Cov(a, m) < Cov(m, m) = Var(m)$ because $\overline{m_{i \neq j}^{SQ}}$ are sample means of m.[7] Intuitively, suppose that neighborhood quality varies across census tract block groups, then the CSA would reasonably capture this variation if appraisers computes price per square foot from comparable houses within the same census tract block group. The covariance between appraised values and sale prices decreases as the appraiser computes average price per square foot across larger

[6] http://gis.snoco.org/maps/property2/

[7] Consult the Appendix for additional details on the proof.

geographical areas.

5.3 Hedonic Pricing Method

The hedonic pricing method (HPM) regresses sale prices observed at some time period t onto measurable house and neighborhood characteristics associated with the house observed in the same time period (Rosen, 1974). Coefficients from this regression model are then used to calculate appraised values for all houses. HPM fails to capture relavant variation in neighborhood quality when the appraiser does not include good proxies for neighborhood quality in the regression equation. The International Association of Assessing Officers (IAAO) provides a guideline on which variables should be included in the appraiser's regression model (IAAO, 2014). The guideline suggests that type of dwelling, living area, construction quality, age, secondary areas, land size, available utilities, market area, zone, neighborhood, location amenities, and location nuisances be included in the model. Clearly, variables such as construction quality and location amenities are very difficult to quantify and an appraiser who wishes to build a regression model would likely omit them.

To provide a concrete example of the list of variables that appraisers use in their linear regression model, I turn to Cook County, Illinois, which makes its appraisal data public.[8] The data set has 82 variables and only a few are related to neighborhood characteristics, while the rest are related to house and parcel characteristics. The neighborhood variables are census tract, O'Hare noise indicator, floodplain indicator, near major road indicator, and a location adjustment factor.[9] Although these neighborhood characteristics may contain important pricing information for houses in Cook County, it is clear that the regression model is ignoring many other important neighborhood characteristics.

Formally, if appraised values are predicted sale prices from an OLS regression where log of sale price m is regressed onto an arbitrary vector of house and neighborhood characteristics, then the expression for β can be written as follows.

[8] https://datacatalog.cookcountyil.gov/Property-Taxation/Cook-County-Assessor-s-Residential-Property-Charac/bcnq-qi2z

[9] The location adjustment factor is a constant that is applied to the appraised value to adjust for price variation across different arbitrarily defined areas. The method used to calculate the constant varies across counties.

$$\beta^{HPM} = \frac{Cov(\hat{m}, m)}{\sigma_m^2} = \frac{\sigma_{\hat{m}}}{\sigma_m} \times \rho_{\hat{m},m} - 1 = \sqrt{R_{\hat{m}}^2} \times \sqrt{R_{\hat{m}}^2} - 1 = R_{\hat{m}}^2 - 1 \tag{9}$$

$\hat{m}$ denotes the appraised values. $R_{\hat{m}}^2$ denotes the coefficient of determination from the same regression. The derivation of β above assumes that $\rho_{\hat{m},m} > 0$ and uses the definition of an OLS regression R^2, which can be expressed as (1) the ratio of the explained variance and the total variance of the dependent variable and (2) the square of the Pearson correlation coefficient between the predicted values and the dependent variable. Here, β is always negative except for the knife-edge case where the appraiser's OLS regression model yields an R^2 of 1.[10]

5.4 Testable Predictions

This section presents testable predictions that I can use to verify my proposed explanation. The flawed valuation methods story implies that, in TCAs where house characteristics cannot predict house prices well, assessments are more regressive. Let $\hat{m}(\mathbf{h}^*)$ denote predicted log of sale price from regressing log of sale price m onto a vector of house characteristics and $R^2_{\hat{m}(\mathbf{h}^*)}$ is the coefficient of determination from the same regression. The asterisk highlights the fact that this is an arbitrary vector of house characteristics chosen by the econometrician that is different from the vector of house characteristics in true model of house prices. Then, the prediction is that β should be positivley correlated with $R^2_{\hat{m}(\mathbf{h}^*)}$.

Prediction 1 *Let $R^2_{\hat{m}(\mathbf{h}^*)}$ denote the coefficient of determination calculated from the following TCA-year-level regression.*

$$logM_{it} = \theta + \gamma' \mathbf{h}^*_{\mathbf{it}} + \delta_{it} \tag{10}$$

M_{it} is the observed sale price for house i in period t and $\mathbf{h}^*_{\mathbf{it}}$ is a vector of house characteristics associated with house i in the same time period. Let β be the slope coefficient estimated from the following TCA-year-level regression.

[10] Other appraisal methods commonly used by local tax assessors are discussed in the appendix.

$$logA_{it} - logM_{it} = \alpha + \beta logM_{it} + \epsilon_{it} \tag{11}$$

k is the index for TCAs. Then across TCA-years, $R^2_{\hat{m}(\mathbf{h}^*),kt}$ *should be positively correlated with* β_{kt}.

Note that the positive correlation between β and $R^2_{\hat{m}(\mathbf{h}^*)}$ is not mechanical. This is because I do not know the exact appraisal models that local tax appraisers used to produce appraised values that I observe in the data. The existence of this positive correlation verifies that (1) house characteristics predict appraised values well and (2) assessment regressivity is driven by how well house characteristics serve as predictors of sale prices. Together, these two statements verify that house characteristics-based appraisal methods produce assessment regressivity, which is worse in TCA-years where house characteristics cannot reliably predict realized sale prices.

However, the positive correlation alone does not confirm my story. The finding is also consistent with the noise story, which is where appraised values are exactly equal to true market values, but realized sale prices are noisy. In this world, the correlation between β and $R^2_{\hat{m}(\mathbf{h}^*)}$ is positive because of panel variation in within-TCA-year noise. The second part of the flawed valuation methods story is within-TCA-year variation in neighborhood characteristics is the unobserved component that makes the correlation between appraised values and sale prices low. In other words, β is smaller in TCA-years where variation in neighborhood characteristics can explain a large proportion of the variation in house prices. To fix ideas, suppose that log of sale price m is a linear function of J house characteristics h_j and K neighborhood characteristics n_k.

$$m_i = \sum_{j=1}^{J} \lambda_j^h h_j + \sum_{k=1}^{K} \lambda_k^n n_k \tag{12}$$

λs are arbitrary constants. Let $\hat{m_i}(\mathbf{h}^*, \mathbf{n}^*)$ be the predicted log of sale prices from regressing log of sale price onto a set of house and neighborhood characteristics. The asterisks highlight the fact that this set of house and neighborhood characteristics is not the same as the one shown in equation 12. A measure of the incremental explanatory power that neighborhood characteristics bring to the regression model is the following.

$$\Delta R^2_{kt} = R^2_{\hat{m}(\mathbf{h^*},\mathbf{n^*})} - R^2_{\hat{m}(\mathbf{h^*})} \tag{13}$$

Prediction 2 *Let* $R^2_{\hat{m}(\mathbf{h^*},\mathbf{n^*})}$ *denote the coefficient of determination calculated from the following TCA-year-level regression.*

$$logM_{it} = \theta + \gamma_1' \mathbf{h^*_{it}} + \gamma_2' \mathbf{n^*_{it}} + \delta_{it} \tag{14}$$

$\mathbf{n^*_{it}}$ *is a vector of nieghborhood characteristic associated with house* i *in the same time period and everything else is defined as before.* β *from equation 11 should be negatively correlated with* $\Delta R^2_{kt} = R^2_{\hat{m}(\mathbf{h^*},\mathbf{n^*})} - R^2_{\hat{m}(\mathbf{h^*})}$ *across TCA-years.*

Intuitively, ΔR^2_{kt} is large in places where variation in neighborhood characteristics can offer significant additional explanatory power to the regression model and ΔR^2_{kt} is small when that is not the case. If variation in neighborhood characteristics cannot help explain variation in realized sale prices, then the correlation between β_{kt} and ΔR^2_{kt} would be zero. A negative correlation is consistent with the story that assessments are regressive in places where variation in neighborhood characteristics is important to variation in realized sale prices, over and above variation in house characteristics.

5.5 Testing the Predictions

The previous section proposes that, if assessment regressivity is driven by appraisers ignoring a set of important pricing characteristics, then there should be a positive relationship between β and $R^2_{\hat{m}(\mathbf{h^*})}$ across TCA-years. To test this prediction, I begin by constructing a data set of transacted houses that I observe house characteristics, neighborhood characteristics, sale prices, and appraised values. I am left with approximately 7 million observations. With this data set, I estimate β for each TCA-year by running the regression in equation 11 and I estimate $R^2_{\hat{m}(\mathbf{h^*})}$ by running the regression in equation 10. The house characteristics used are the log of number of bedrooms, number of bathrooms, and living area square footage.

Table 1 presents summary statistics for the estimated parameters. There are 14,478 TCA-years where I have at least 50 transactions. The average β_{kt} is -0.36, which speaks to the fact that, on average, cheap houses are overappraised and expensive houses are underappraised. There is substantial variation across TCA-years. β_{kt} ranges from -0.9 to 0.15. The average $R^2_{\hat{m}(\mathbf{h}^*)}$ is 0.35, which means that the list of house characteristics, on average, explains approximately a third of the variation in house prices within a TCA-year. Similarly to β_{kt}, there is significant variation across TCA-year in $R^2_{\hat{m}(\mathbf{h}^*)}$, which ranges from 0.03 to 0.77. There are TCA-years where house characteristics explain very little of the variation in house prices and those where house characteristics can explain a lot.

Figure 5 presents a binned scatter plot of β_{kt} on $R^2_{\hat{m}(\mathbf{h}^*),kt}$ with county-year fixed effects. Including county-year fixed effects is important because the thought experiment is, holding valuation method constant, does assessment regressivity decrease as house characteristics' ability to explain variation in realized sale prices increases? Figure 5 show that this is the case. There is a linear and positive relationship between β_{kt} on $R^2_{\hat{m}(\mathbf{h}^*),kt}$. I formally test this relationship by regressing β_{kt} on $R^2_{\hat{m}(\mathbf{h}^*),kt}$ with county-year fixed effects. Column 1 of table 2 presents the results. As expected from the plot, there is a positive and significant relationship between β_{kt} and $R^2_{\hat{m}(\mathbf{h}^*),kt}$.[11]

To show that variation in neighborhood characteristics is the unaccounted component that is driving the relationship between β_{kt} on $R^2_{\hat{m}(\mathbf{h}^*),kt}$, I estimate $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$ by estimating regression equation 14. Neighborhood characteristics used are minority share, log of median household income, unemployment rate, percentage of adult with a college degree, percentage of households that participate in SNAP, median gross rent as a percentage of household income, homeownership percentage, home vacancy percentage, percentage of commerical parcels, percentage of industrial parcels, and percentage of agricultural parcels. Neighborhood characteristics are measured at the census tract block group-level.

Table 1 presents summary statistics for $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$ and ΔR^2_{kt}. The average value $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$ is 0.52, which indicates that this set of house and neighborhood characteristics can explain, on

[11] The standard errors are calculated from a bootstrapping procedure that creates 100 random samples from the original data set then estimates β_{ky}, $R^2_{\hat{m}(\mathbf{h}^*),ky}$, and the slope coefficient from regressing β_{ky} onto $R^2_{\hat{m}(\mathbf{h}^*),ky}$ 100 times.

average, half of the variation in realized sale prices. The average value of ΔR^2_{kt} suggests that adding neighborhood characteristics to the linear regression model can help improve its predictive power. There is substantial cross TCA-year variation in ΔR^2_{kt}, which shows that there are TCA-years where neighborhood characteristics are important and those where they are not.

Figure 6 presents a binned scatter plot of β_{kt} on ΔR^2_{kt} with county-year fixed effects. The plot shows a clear negative relatioship between the two. The second column of table 2 shows the estimated OLS coefficient from regressing β_{kt} onto ΔR^2_{kt} with county-year fixed effects. The estimated coefficient is negative and statistically significant, which confirms that omitted neighborhood characteristics are driving the panel variation in assessment regressivity. As a robustness check, column 3 shows the estimated OLS coefficient from regressing β_{kt} onto $R^2_{\hat{m}(\mathbf{n}^*),kt}$ with county-year fixed effects, where $R^2_{\hat{m}(\mathbf{n}^*),kt}$ is the coefficient of determination from regressing log of sale price onto neighborhood characteristics alone. The negative coefficient confirms the same story.[12]

5.6 How Much Do Flawed Valuation Methods Matter?

This section quantifies the proportion of assessment regressivity that can be explained by the flawed valuation method mechanism. I begin by constructing synthetic appraised values for houses sold in 2018. This method follows a similar approach taken by Avenancio-León and Howard (2019) and Bayer et al. (2017). For each house, I grow its previous sale price by a growth factor calculated from the change in its zip code's single family home price index.[13]

$$A^{syn}_{i,2018} = M_{i,t<2018} \times \frac{HPI_{z,2018}}{HPI_{z,t<2018}} \tag{15}$$

$M_{i,t<2018}$ is house i's previous sale price and $\frac{HPI_{z,2018}}{HPI_{z,t<2018}}$ is the change in its zip code's house price index between year t and 2018. Assuming that sale prices equal true market values, house i's previous sale price should capture all of house i's priced house and neighborhood characteristics in year t. The growth factor then accounts for the change in priced neighborhood characteristics

[12] Every result in this section is quantitatively similar when I randomly split the sample in each TCA-year into two equal groups and use one to estimate β_{kt} and the other to estimate $R^2_{\hat{m}(\mathbf{h}^*),kt}$, $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$, and $R^2_{\hat{m}(\mathbf{n}^*),kt}$.

[13] https://www.zillow.com/research/data/

between year t and 2018.

The next step is to construct synthetic valuation ratios from taking the difference between log of house i's synthetic appraised value and log of its sale price. By comparing assessment regressivity that results from the synthetic valuation ratios and assessment regressivity that results from the observed valuation ratios, I can estimate the lower bound for the flawed valuation method story's ability to explain assessment regressivity. This comparison gives the lower bound because errors between the synthetic appraised values and realized sale prices can come from sources related or unrelated to the flawed valuation method story. Reasons related to the flawed valuation methods story include changes in priced house-specific characteristics, such as renovations, and within-zip code variation in priced neighborhood characteristics not captured by the zip code house price indexes. Reasons unrelated to the proposed explanation include pure noise and transactional frictions in the housing market (Giacoletti, 2017). To make this comparison, I run the following two regressions.

$$logA_i - logM_i = \alpha + \beta logM_i + TCA\ FE + \epsilon_i \tag{16}$$

$$logA_i^{syn} - logM_i = \alpha^{syn} + \beta^{syn} logM_i + TCA\ FE + \epsilon_i^{syn} \tag{17}$$

β captures the observed degree of assessment regressivity in the data and β^{syn} captures the degree of assessment regressivity after some priced house and neighborhood characteristics have been accounted for. $1 - \frac{\beta^{syn}}{\beta}$ gives the lower bound of the amount of assessment regressivity that can be explained by flawed valuation methods. Table 3 presents the regression results. The sample for the first two columns includes all houses that were sold in 2018 where I have previous sale price data. The slope coefficient in the second column is -0.088, which is 37% lower than the slope coefficient in the first column. The difference is statistically significant. The third and fourth columns use a subsample of houses that were reappraised in 2018. The effect of infrequent reappraisal is purged from this sample to give the observed appraised values their best chance. For this sample, the reduction in assessment regressivity is 31%. This exercise shows that the flawed valuation methods mechanism can explain a significant portion of the observed assessment regressivity.

6 Other Explanations

6.1 Infrequent Reappraisals

It is a well known fact in the property tax literature that appraised values often lag sale prices (Engle, 1975; Heavey, 1978). In Pennsylvania and New Jersey, counties are not legally bounded to periodically reappraise houses (Lincoln Institute of Land Policy, 2014). Hence, these counties only reappraise houses when forced to do so, e.g. by a court order (Branham, 2017). Infrequent reappraisal can cause assessment regressivity in the following way. Suppose that, initially, appraised values equal sale prices for all houses. Each year, houses experience random i.i.d. mean zero price shocks. Appraisers can perfectly predict these shocks but do not regularly update appraised values to reflect these shocks. The result is low covariance between appraised values and sale prices, which makes assessments regressivity.

To quantify how much of the observed assessment regressivity can be explained by infrequent reappraisal, I run the following regression for all houses sold in 2018 and a subsample of houses that were reappraised and sold in 2018.

$$logA_i - logM_i = \alpha + \beta logM_i + TCA\ FE + \epsilon_i \tag{18}$$

Table 4 presents these regression results. Column 1 shows result for all houses sold in 2018. The estimated slope coefficient is 0.16. Column 2 shows result for a subsample of houses that were reappraised and sold in 2018. The estimated slope coefficient is 0.151 and is statistically different from 0.16. Comparing the two slope coefficients show that removing houses with stale appraised values from the sample decreased the observed regressivity by approximately 5% $(1 - \frac{0.151}{0.16})$. This exercise shows that infrequent reappraisal is a relatively minor contributor.

6.2 Heterogeneous Appeal Behavior and Outcomes

This section discusses and refute the heterogeneous appeal behavior and outcomes explanation. Suppose that individuals who own cheaper houses are less likely to appeal their appraised values, relative to individuals who own more expensive houses. Furthermore, suppose that owners of cheaper homes are also relatively less successful in appeals. These two factors could give rise to assessment regressivity. This story is plausible because individuals sort into cheap or expensive houses according to characteristics such as income and education. Therefore, individuals who own cheaper homes are more likely to be less sophisticated than those who own expensive homes, which could affect their appeal behavior and outcomes in the manner described above.

To explore whether the appeal hypothesis could explain within-TCA assessment regressivity, I use publicly available tax, transaction, and appeal data from Cook County, Illinois.[14] To begin, I use unique parcel identifiers to merge the Cook County transaction data with the Cook County appeal data. Using the same identifers, I merge TCA data from the CoreLogic data set into the merged Cook County data set. The resulting data set has approximately 500,000 transactions that took place between 2007 and 2017. Finally, I assign houses to 1 of 20 price bins within their TCA and year of transaction to explore how appeal behavior and outcomes vary across price bins.

Figure 7 plots average appeal probability against within-TCA-year house price bins. If differences in appeal behavior were to explain the negative relationship between valuation ratio and house price, then there should be a positive relationship between appeal probability and house prices. However, this is not the case. There seems to be a negative relationshop between appeal probability and house price, which indicates that, within a TCA, owners of cheaper houses are more likely to appeal than owners of expensive houses.

Next, I investigate the relationship between win probability and within-TCA house prices. Figure 8 plots average win probability against within-TCA-year house price bins. This sample includes only houses that filed an appeal in the same year that it was sold. If differences in win probability were to explain assessment regressivity, then there should be a positive relationship between win probability and house price. Again, this is not the case. In fact, there is an almost monotonically

[14] https://datacatalog.cookcountyil.gov/

negative relationship between the two variables.

Lastly, I investigate how, conditional on winning, appraisal reduction percentage varies with house price. Figure 9 plots average percentage appraised value reduction against within-TCA-year house price bins. This sample includes only houses that won an appeal in the year that it was sold. If differences in degrees of appeal success were to explain assessment regressivity, then there should be a positive relationship between appraised price reduction and house price. However, the relationship is, overall, negative. I formally test these three sets of correlation by running various versions of the following panel regression.

$$Y_{it} = \alpha + \gamma log M_{it} + TCA \times Year\, FE + \epsilon_{it} \tag{19}$$

Y_{it} is the placeholder for appeal-related outcome variables – appeal indicator, win indicator, and percentage reduction in appraised value. Table 5 presents the results. The slope coefficient on log of sale price is negative in all three columns, which is consistent with the figures discussed above. Together, these results show that heterogeneous appeal behavior and outcomes cannot explain within-TCA assessment regressivity in Cook County, which weakens its potential as an explanation for the national phenomenon.

7 Impact on Wealth Inequality

This section concerns the impact that assessment regressivity has on the wealth distribution of homeowners in the United States. To quantify this impact, I begin by calculating excess tax payments (ETP) for each house that was sold in 2016. Excess tax payment is calculated as the difference between the observed tax bill and the counterfactual tax bill, if these houses were taxed according to their sale prices.

$$ETP_{ik} = \underbrace{T_{ik}}_{\text{Observed Tax Bill}} - \underbrace{\frac{\sum_{i=1}^{n} T_{ik}}{\sum_{i=1}^{n} M_{ik}}}_{\text{Counterfactual Tax Rate}} \times \underbrace{M_{ik}}_{\text{Sale Price}} \tag{20}$$

Within a TCA k, for all houses that were sold, I compute total tax revenue and total sale value. The total tax revenue divided by total sale value gives the counterfactual statutory tax rate. Note that this calculation is analogous to the formula for statutory tax rate, which is the ratio of total property tax revenue raised (sum of all tax bills) and the municipal government's tax base (sum of all assessed values). The counterfactual tax rate is multiplied to each house's sale price to arrive at the counterfactual tax bill. A positive ETP value means that the observed tax bill is too high and a negative value means that it is too low, relative to the sale price-based benchmark. By treating each house's excess tax payment as a perpetuity and assuming that property taxes are fully capitalized into house prices at a discount rate of 4%, these excess tax payments can be converted into changes in home equity (Do and Sirmans, 1994). For example, a $1 excess tax payment per year, if eliminated, would increase home equity by $25.

Table 6 presents the result of these back-of-the-envelope calculations for the average household in each primary home value decile and the average household whose primary home value is in the top 1%. Home value group limits and net worth data are collected from the 2016 Survey of Consumer Finance.[15] On average, households whose primary home values are in the bottom decile pay $684 in excess tax payment per year. This amount of annual tax payment, if eliminated, would increase home equity by $17,100. With an average net worth of $101,052, the change in home equity is equivalent to a 16.9% increase in net worth. Average percentage change in net worth decreases as primary home value increases and turns negative for households in the top decile. The richest homeowners receive an average property tax discount of $29,056 per year, which, if eliminated, would decrease home equity by $726,402. With an average net worth of $22,419,290, the change in home equity is equivalent to a 3.2% decrease in net worth. Overall, correcting assessment regressivity would reduce the wealth gap between the top 1% and the bottom 10% by 3.3%.[16] These calculations show that assessment regressivity increases wealth inequality by transferring housing wealth from poor homeowners to rich homeowners.[17]

[15] https://sda.berkeley.edu/sdaweb/analysis/?dataset=scfcomb2019

[16] Calculation for change in wealth gap is 1 - $\frac{New\,Wealth\,Gap}{Old\,Wealth\,Gap}$ = 1 - $\frac{22{,}419{,}290-726{,}402-101{,}052-17{,}100}{22{,}419{,}290-101{,}052}$ = 1 - 0.967 = 3.3%.

[17] Refer to the appendix for additional details on these calculations.

8 Sorting and Overtaxation of Disadvantaged Households

In this section, I argue that overtaxation of minorities and low-income households is a *by-product* of assessment regressivity. In a world where there is assessment regressivity and no direct discrimination by tax assessors towards any group, i.e., no racism, groups that sort into cheap houses would *mechanically* face unfavorable assessment gaps. Therefore, despite the existence of assessment gaps between racial groups, the only form of discrimination that truly exists in this hypothetical world is discrimination with respect to house price. To make this point, I begin by replicating results from the literature, which documents that minorities and low-income households live in houses that are overappraised relative to non-Hispanic whites and higher-income earners, respectively (Baar, 1981; Avenancio-León and Howard, 2019). Following a standard merging procedure in the literature, I merge race, ethnicity, and income data from HMDA into my main data set and run variants of the following regression.

$$logA_{it} - logM_{it} = \alpha + \gamma Demographic\,Indicator_{it} + TCA \times Year\,FE + \epsilon_{it} \tag{21}$$

Log valuation ratio is regressed onto a demographic indicator variable, along with TCA by year fixed effects. Note that the sample is now mortgage holders in HMDA, rather than all home purchasers. The demographic indicator variables that I use are black, Hispanic, and low-income. Low-income indicator variable equals 1 if the mortgage holder's reported annual income is lower than 80% of the application year's national median household income (HUD, 2018).[18] Table 7 presents the results. In line with the literature, black and Hispanic mortgage holders live in houses that are overappraised relative to non-Hispanic white mortgage holders. Likewise, low-income mortgage holders live in houses that are overappraised relative to other mortgage holders.

Next, I show that minorities and poorer individuals sort into cheap houses. Using 2017 data, table 8 presents average mortgage holder characteristics by TCA price decile. Wealthy mortgage holders with high home equity and household income sort into expensive houses. Not surprisingly, black

[18]Results are quantitatively and qualitatively similar if low income is defined as mortgage applicants whose reported annual income is lower than the median reported income among mortgage applicants in his or her TCA-year.

and Hispanic mortgage holders tend to buy cheaper houses. To investigate whether the assessment gaps shown in table 7 are purely a function of sorting by price, I run variants of the following regression.

$$\begin{aligned} logA_{it} - logM_{it} = \alpha + \gamma Demographic\, Indicator_{it} & \\ + \theta_1 Price\, Decile\, 2_{ijt} + \theta_2 Price\, Decile\, 3_{ijt} + ... & \\ + \lambda_1 Demographic\, Indicator_{it} \times Price\, Decile\, 2_{ijt} + ... & \\ + TCA \times Year\, FE + \epsilon_{it} & \end{aligned} \tag{22}$$

j is the index for TCA. Log valuation ratio is regressed onto a demographic indicator variable, within-TCA-year house price decile indicator variables, and their interaction terms. Table 9 presents the results. Statistical significance tests can be performed on the estimated coefficients to determine whether, conditional on house price, the assessment gaps shown in table 7 remain positive and statistically different from zero.

Table 10 presents t-test results on the assessment gaps in each price decile and F-statistics from joint tests against the null hypothesis that the assessment gap is equal to zero in all ten price deciles. Figures 10, 11, and 12 plot the estimated coefficients relative to the reference group's average log valuation ratios, along with 95% confidence interval bars based on results shown in table 10. Column 1 of table 10 shows that the black assessment gap is absent in all price deciles. The joint test confirms this conclusion. These results show that the black assessment gap shown in table 7 is purely a function of black mortgage holders sorting into overappraised cheap houses and rule out systematic racism against black households. Columns 2 and 3 show that the Hispanic and low-income assessment gaps *reversed*. Although the economic magnitudes are small, conditional on house price, Hispanics mortgage holders' houses are underassessed relative to non-Hispanic white mortgage holders' houses. Similarly, low-income mortgage holders' houses are underassessed relative middle to high-income mortgage holders' houses. This set of results shows that the minority and income assessment gaps are purely a function of the interaction between assessment regressivity and the fashion in which these disadvantaged households sort into relatively cheaper houses. If anything, conditional on house price, there seems to be reverse discrimination against non-Hispanic white households and high-income earners.

9 Conclusion and Discussion

This article documents assessment regressivity among houses that have access to the same set property-tax funded amenities and shows that it contributes to wealth inequality in the United States. Flawed valuation methods, which ignore priced latent characteristics, can explain at least 30% of this phenomenon, which suggests that assessment regressivity could be alleviated by improving appraisal techniques. Since assessment regressivity is difficult to measure, the most conservative interpretation of these results is, regardless of the initial level assessment regressivity, any increase in house price variation that comes from these latent characteristics would cause property taxes to become more regressive. Furthermore, I show that unfavorable assessment gaps that minorities and low-income households face are by-products of the interaction between assessment regressivity and the way in which different types of households sort into differentially priced homes. Hence, policymakers could eliminate these assessment gaps by fixing assessment regressivity.

Lastly, results from this article imply that a general wealth tax system that requires the federal government to value and tax private buisnesses would likely produce an undesirable distribution of tax burdens. For example, a system that uses data on publicly traded companies and a linear regression to estimate market prices of private businesses would overtax small businesses and undertax big businesses. This distribution of tax burdens is, potentially, undesirable because owners of start-ups and young businesses are the main drivers behind job creation (Haltiwanger et al., 2013).

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Figure 1: Mean Scaled Effective Tax Rate by 2016 Within-TCA House Price Bin

Binnned scatter plot of mean scaled effective tax rate for houses in each within-TCA price bin. Tax code areas (TCA) are small geographical areas where every house has access to the same set of property-tax funded government services and pay the same statutory tax rate. Each house's effective tax rate is scaled by the median effective tax rate in its TCA. Houses in each TCA are evenly divided into twenty price bins. The cheapest houses are in the first bin and the most expensive houses are in the twentieth bin. The sample contains houses in 49 states and the District of Columbia that were sold in 2016.

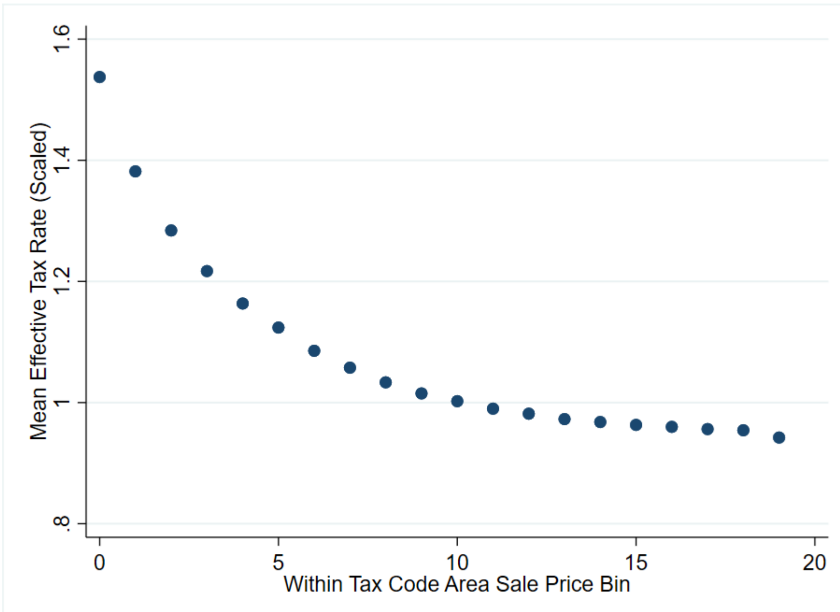


Figure 2: 2020 Tax Code Areas in Snohomish County, WA

List of all local government entities that collect property taxes in three tax code areas (TCA) in Snohomish County, WA. Tax code areas (TCA) are small geographical areas where every house has access to the same set of property-tax funded government services and pay the same statutory tax rate. Levy rates are presented as $1 USD of tax per $1,000 USD of assessed value. The list is for the 2020 tax year.

TAX CODE AREAS & RATES FOR TAX YEAR 2020

"TCA's" (Tax Code Areas) designate a unique set of taxing districts. They appear on tax statements. These columns list the Tax Code Area, district/levy, and regular and excess levy rates within that TCA. All rates are expressed in dollars per thousand dollars of assessed value. Totals are accurate, but may not agree to sum of detail because of rounding.

TCA	District Abbrev.	District/Levy Name	Regular/Excess	Rate
00018	CNT	COUNTY REGULAR	Regular Levy	0.63749375727
	CNT	COUNTY CONSERVATION FUTURES	Regular Levy	0.02796182191
	CTYEVT	EVERETT	Regular Levy	1.90529928265
	CTYEVT	EVERETT EMS PERMANENT 2001-ON	Regular Levy	0.46801912362
	PRTEVT	PORT OF EVERETT MAINTENANCE	Regular Levy	0.23664019462
	SCH002EVT	SCHOOL 002 BONDS	Excess Levy	2.41352285021
	SCH002EVT	SCHOOL 002 CAPITAL PROJECTS	Excess Levy	0.54775496112
	SCH002EVT	SCHOOL 002 ENRICHMENT	Excess Levy	1.92148220699
	STASCH	STATE SCHOOL 1	Regular Levy	1.86415073934
	STASCH	STATE SCHOOL 2	Regular Levy	1.00352181207
			Sum of Regular Levy Rate	6.14308673148
TCA Value:	$250,619		*Sum of Excess Levy Rate*	4.88276001832
			Sum of TCA 00018	***11.02584674980***
00020	CNT	COUNTY REGULAR	Regular Levy	0.63749375727
	CNT	COUNTY CONSERVATION FUTURES	Regular Levy	0.02796182191
	CTYEVT	EVERETT	Regular Levy	1.90529928265
	CTYEVT	EVERETT EMS PERMANENT 2001-ON	Regular Levy	0.46801912362
	PRTEVT	PORT OF EVERETT MAINTENANCE	Regular Levy	0.23664019462
	RTACPS	CENTRAL PUGET SOUND REGIONAL TRANSIT AUTHORITY	Regular Levy	0.19937000000
	SCH002EVT	SCHOOL 002 BONDS	Excess Levy	2.41352285021
	SCH002EVT	SCHOOL 002 CAPITAL PROJECTS	Excess Levy	0.54775496112
	SCH002EVT	SCHOOL 002 ENRICHMENT	Excess Levy	1.92148220699
	STASCH	STATE SCHOOL 1	Regular Levy	1.86415073934
	STASCH	STATE SCHOOL 2	Regular Levy	1.00352181207
			Sum of Regular Levy Rate	6.34245673148
TCA Value:	$188,410,518		*Sum of Excess Levy Rate*	4.88276001832
			Sum of TCA 00020	***11.22521674980***
00021	CNT	COUNTY REGULAR	Regular Levy	0.63749375727
	CNT	COUNTY CONSERVATION FUTURES	Regular Levy	0.02796182191
	CTYEVT	EVERETT	Regular Levy	1.90529928265
	CTYEVT	EVERETT EMS PERMANENT 2001-ON	Regular Levy	0.46801912362
	HSP001VAL	HOSPITAL DIST 1 MAINTENANCE	Regular Levy	0.23340678097
	PRTEVT	PORT OF EVERETT MAINTENANCE	Regular Levy	0.23664019462
	RTACPS	CENTRAL PUGET SOUND REGIONAL TRANSIT AUTHORITY	Regular Levy	0.19937000000
	SCH002EVT	SCHOOL 002 BONDS	Excess Levy	2.41352285021
	SCH002EVT	SCHOOL 002 CAPITAL PROJECTS	Excess Levy	0.54775496112
	SCH002EVT	SCHOOL 002 ENRICHMENT	Excess Levy	1.92148220699
	STASCH	STATE SCHOOL 1	Regular Levy	1.86415073934
	STASCH	STATE SCHOOL 2	Regular Levy	1.00352181207
			Sum of Regular Levy Rate	6.57586351245
TCA Value:	$148,078		*Sum of Excess Levy Rate*	4.88276001832
			Sum of TCA 00021	***11.45862353077***

Figure 3: Tax Code Area Map from Snohomish County, WA

Map of tax code areas (TCA) in Snohomish County, WA. Tax code areas (TCA) are small geographical areas where every house has access to the same set of property-tax funded government services and pay the same statutory tax rate. TCA numbers are printed in red. TCA boundaries are drawn with red lines. There are six TCAs in this map: 03992, 03953, 04132, 04134, 04110, and 03399. Blocks numbered and drawn with thin black lines are parcels. The land area covered by this map is approximately 3.2 by 1.4 miles.

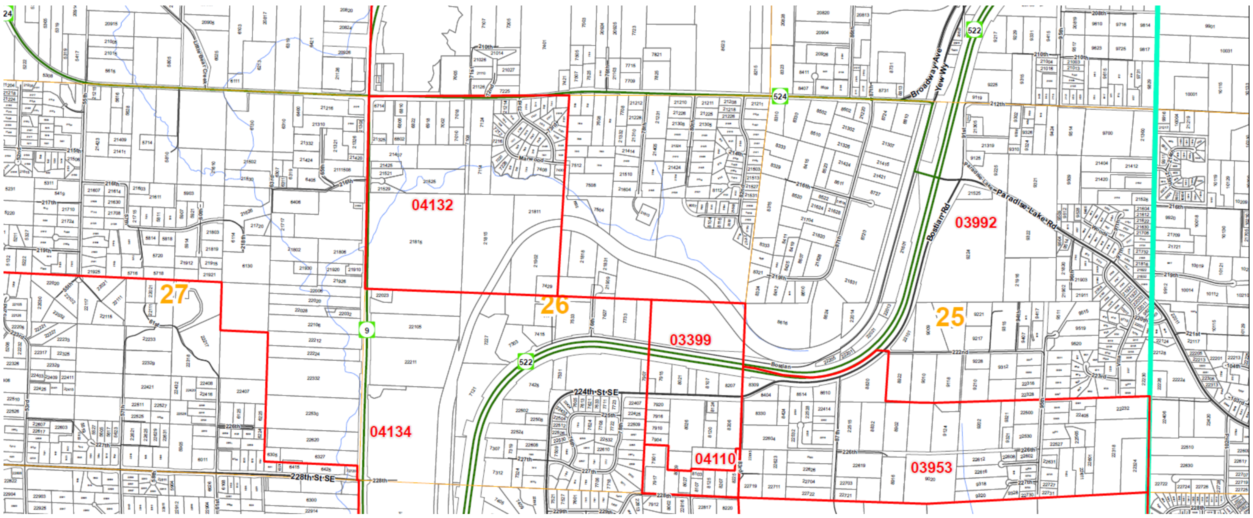


Figure 4: Benchmark Areas in Snohomish County, WA

Map of benchmark areas used in Snohomish County's appraisal model. Benchmark areas are drawn with blue boundaries. Individual parcels are drawn with pink lines. This image was taken from Snohomish County's 2019 Region 2 Mass Appraisal Report.

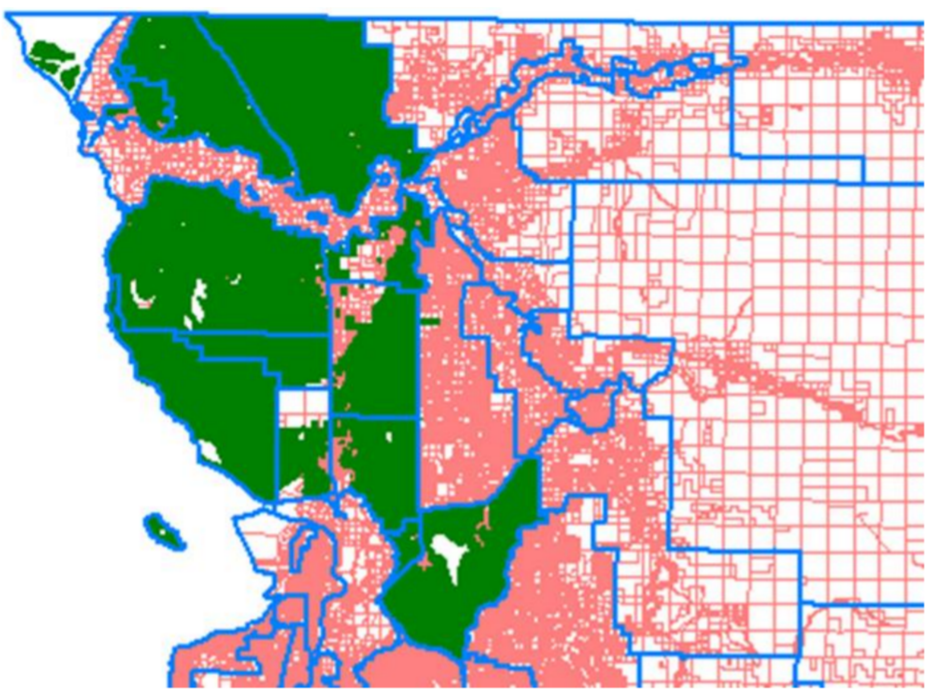


Figure 5: Binnned Scatter Plot of β_{kt} on $R^2_{\hat{m}(\mathbf{h}^*),kt}$

Each observation is a TCA-year, indexed by kt. β_{kt} is estimated for each TCA-year by regressing log valuation ratio $logA_{it} - logM_{it}$ onto log of sale price. $R^2_{\hat{m}(\mathbf{h}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house characteristics. Both variables are residualized by county-year indicator variables. The sample contains TCA-years where there are at least 50 transactions.

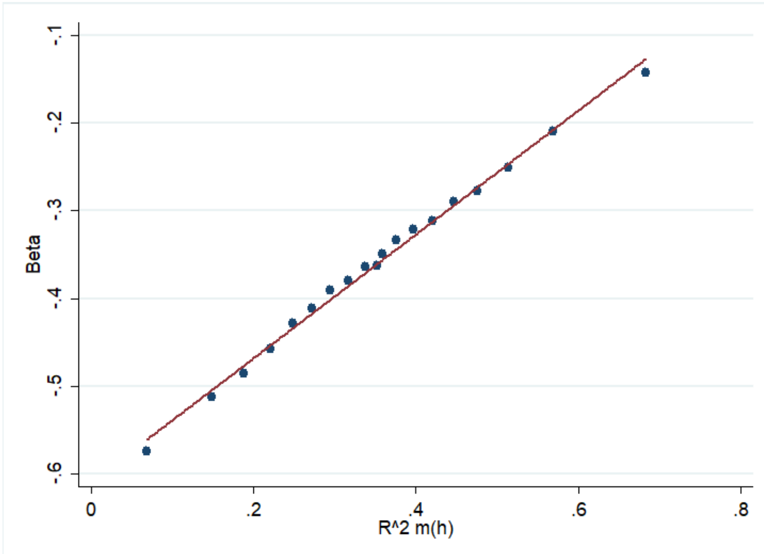


Figure 6: Binnned Scatter Plot of β_{kt} on ΔR^2_{kt}

Each observation is a TCA-year, indexed by kt. β_{kt} is estimated for each TCA-year by regressing log valuation ratio $logA_{it} - logM_{it}$ onto log of sale price. $R^2_{\hat{m}(\mathbf{h}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house characteristics. $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house and neighborhood characteristics. $\Delta R^2_{kt} = R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt} - R^2_{\hat{m}(\mathbf{h}^*),kt}$. Both variables are residualized by county-year indicator variables. The sample contains TCA-years where there are at least 50 transactions.

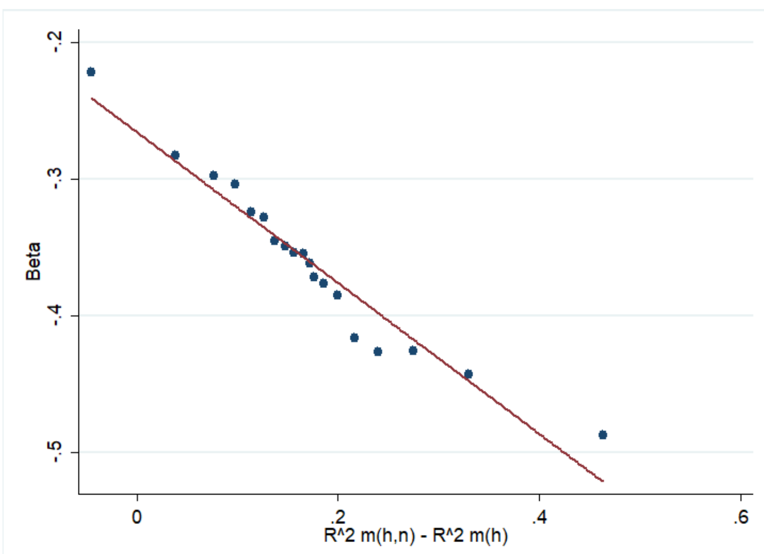


Figure 7: Appeal Probability by Within-TCA-Year House Price Bin

Binnned scatter plot of appeal probability against wihtin-TCA-year house price bins for transacted houses in Cook County, IL. The sample contains transactions from 2007 to 2017. Appeal probability is calculated from an appeal indicator variable which equals 1 if the homeowner filed an appeal in the year that it was sold and zero otherwise. Houses in each TCA-year are evenly divided into twenty price bins. The cheapest houses are in the first bin and the most expensive houses are in the twentieth bin.

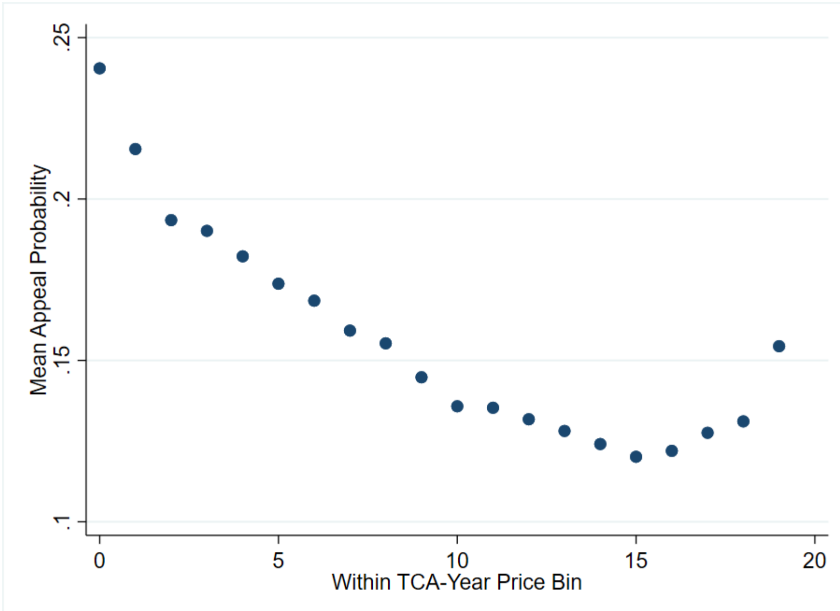


Figure 8: Win Probability by Within-TCA-Year House Price Bin

Binnned scatter plot of win probability against wihtin-TCA-year house price bins for transacted houses in Cook County, IL. The sample contains transactions from 2007 to 2017. The sample only includes houses where the homeowner filed an appeal. Win probability is calculated from a win indicator variable which equals 1 if the homeowner appealed and won in the year that the house was sold and zero otherwise. Houses in each TCA-year are evenly divided into twenty price bins. The cheapest houses are in the first bin and the most expensive houses are in the twentieth bin.

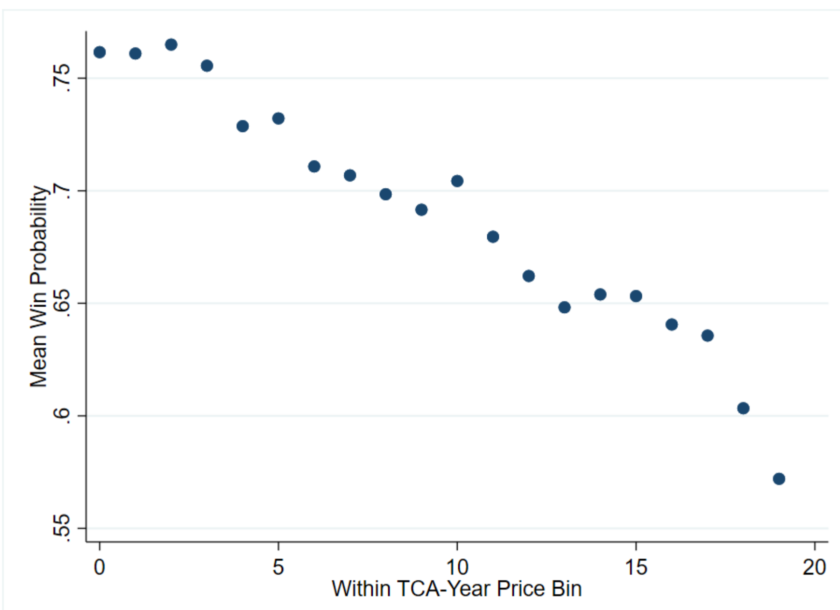


Figure 9: Average Percentage Appraised Value Reduction by Within-TCA-Year House Price Bin

Binnned scatter plot of average percentage appraised value reduction against wihtin-TCA-year house price bins for transacted houses in Cook County, IL. The sample contains transactions from 2007 to 2017. The sample includes only houses where the homeowner won the appeal that he or she filed in the same year that the house was sold. Appraised value reduction percentage is calculated as the amount of appraisal reduction that the homeowner received divided by the proposed appraised value.

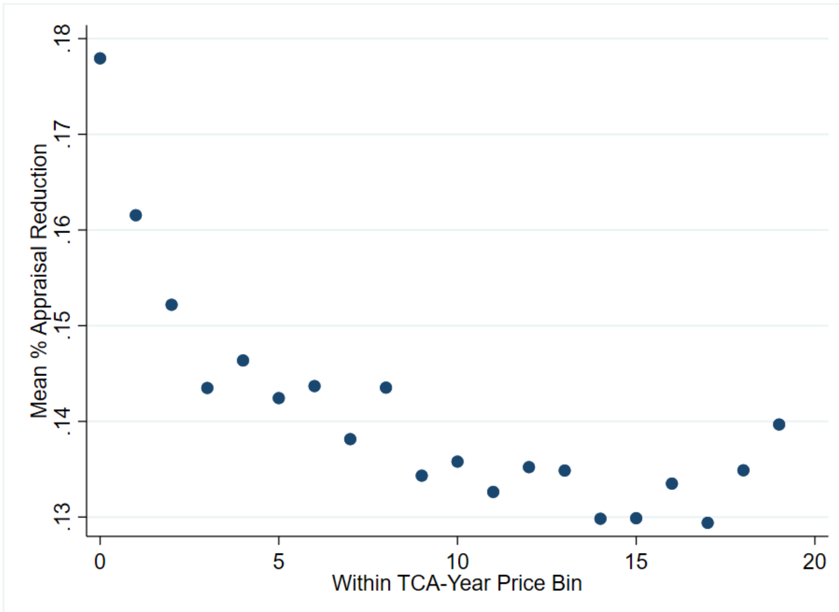


Figure 10: Black Assessment Gap by Price Decile

This plot compares average log valuation ratio for non-mixed black mortgage holders to average log valuation ratio for non-mixed non-Hispanic white mortgage holders, conditional on TCA-year price decile. Blue dots are average log valuation ratio for non-mixed non-Hispanic white mortgage holders in each TCA-year price decile. Red dots are average log valuation ratio for non-mixed black mortgage holders in each TCA-year price decile. Blue dots are the sum of the average log valuation ratio for non-mixed non-Hispanic white mortgage holders in each TCA-year price decile and the respective coefficients from column 1 of table 10. The bars are 95% confidence interval drawn from the same t-tests.

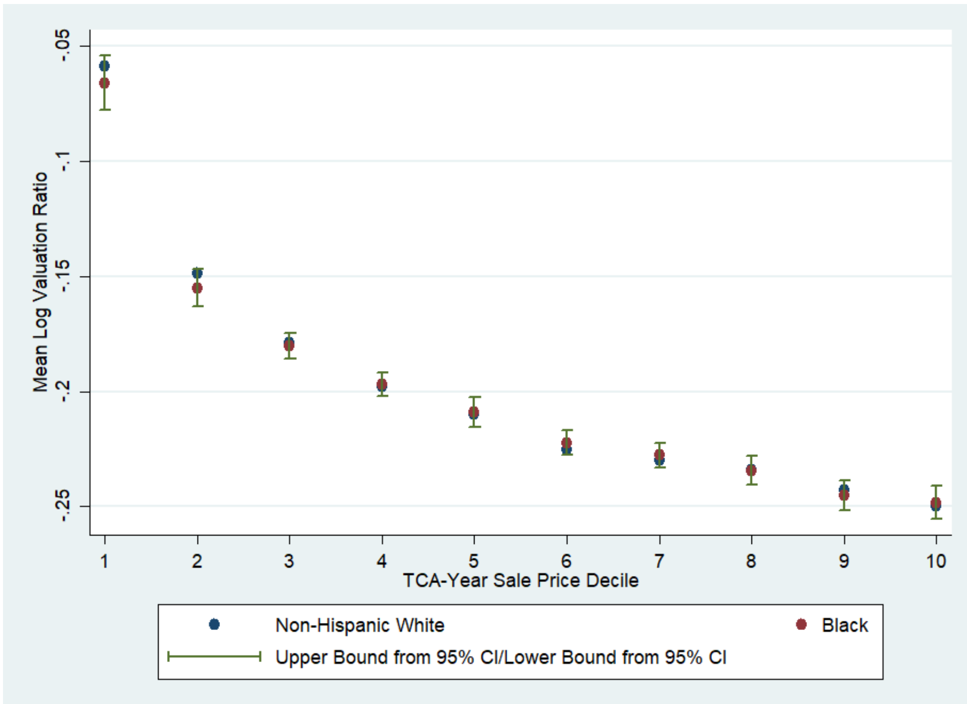


Figure 11: Hispanic Assessment Gap by Price Decile

This plot compares average log valuation ratio for Hispanic mortgage holders to average log valuation ratio for non-Hispanic white mortgage holders, conditional on TCA-year price decile. Blue dots are average log valuation ratio for non-Hispanic white mortgage holders in each TCA-year price decile. Red dots are average log valuation ratio for Hispanic mortgage holders in each TCA-year price decile. Blue dots are the sum of the average log valuation ratio for non-Hispanic white mortgage holders in each TCA-year price decile and the respective coefficients from column 2 of table 10. The bars are 95% confidence interval drawn from the same t-tests.

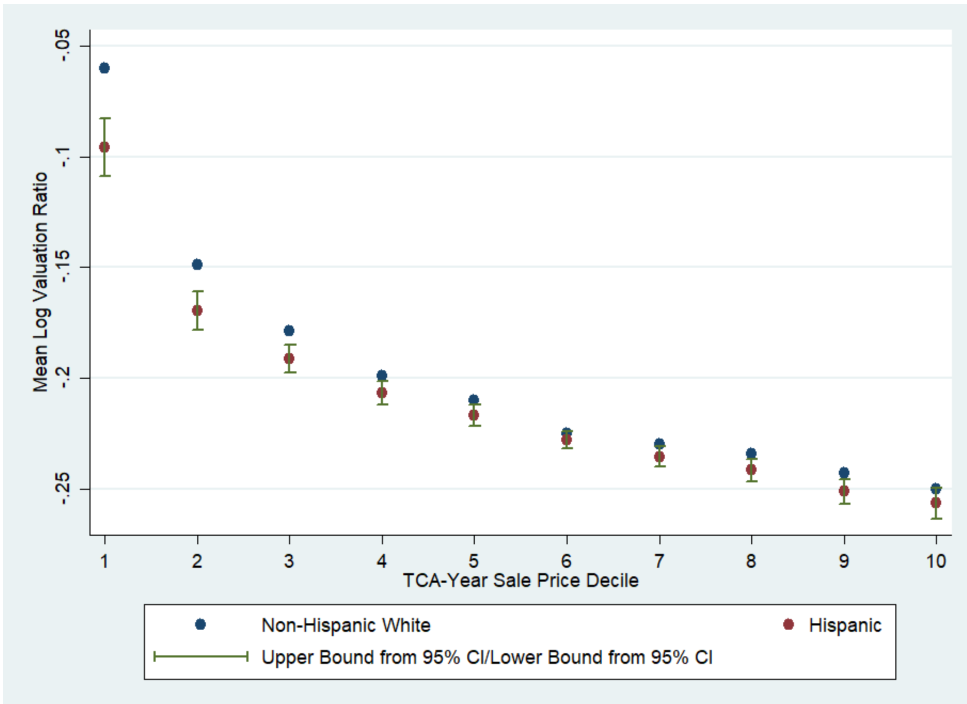


Figure 12: Low-Income Assessment Gap by Price Decile

This plot compares average log valuation ratio for low-income mortgage holders to average log valuation ratio for middle and high-income mortgage holders, conditional on TCA-year price decile. Blue dots are average log valuation ratio for middle and high-income mortgage holders in each TCA-year price decile. Red dots are average log valuation ratio for low-income mortgage holders in each TCA-year price decile. Blue dots are the sum of the average log valuation ratio for middle and high-income mortgage holders in each TCA-year price decile and the respective coefficients from column 3 of table 10. The bars are 95% confidence interval drawn from the same t-tests. Low-income mortgage holders are those with reported incomes lower than 80% of their mortgage application year's national median household income.

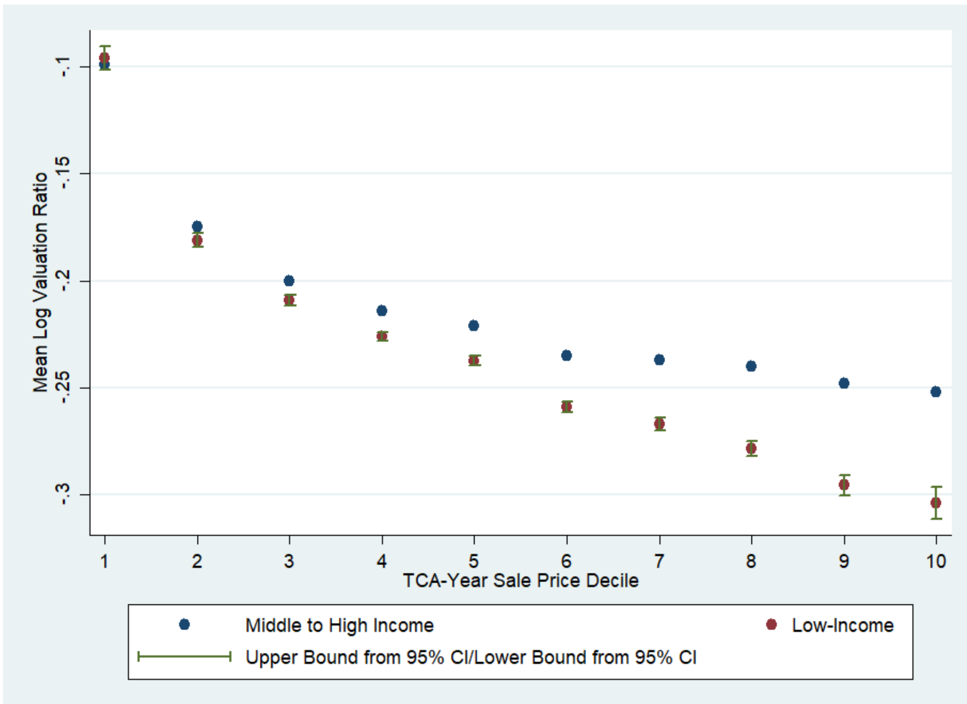


Table 1: Summary Statistics of Estimated Parameters

Each observation is a TCA-year, indexed by kt. β_{kt} is estimated for each TCA-year by regressing log valuation ratio $logA_{it} - logM_{it}$ onto log of sale price. $R^2_{\hat{m}(\mathbf{h}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house characteristics. $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house and neighborhood characteristics. $\Delta R^2_{kt} = R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt} - R^2_{\hat{m}(\mathbf{h}^*),kt}$. $R^2_{\hat{m}(\mathbf{n}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto neighborhood characteristics. The sample contains TCA-years where there are at least 50 transactions.

Variable	n	Mean	S.D.	Min	25th	Median	75th	Max
β_{kt}	14,478	-0.36	0.20	-0.90	-0.48	-0.35	-0.23	0.15
$R^2_{\hat{m}(\mathbf{h}^*),kt}$	14,478	0.35	0.18	0.03	0.21	0.34	0.48	0.77
$R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$	14,478	0.52	0.16	0.16	0.41	0.52	0.64	0.88
ΔR^2_{kt}	14,478	0.17	0.15	0.00	0.07	0.12	0.22	0.86

Table 2: TCA-Year Panel Regression Results

OLS regression results where β_{kt} is regressed onto $R^2_{\hat{m}(\mathbf{h}^*),kt}$, ΔR^2_{kt}, and $R^2_{\hat{m}(\mathbf{n}^*),kt}$, separately, with county by year fixed effects. Each observation is a TCA-year, indexed by kt. β_{kt} is estimated for each TCA-year by regressing log valuation ratio $logA_{it} - logM_{it}$ onto log of sale price. $R^2_{\hat{m}(\mathbf{h}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house characteristics. $R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto house and neighborhood characteristics. $\Delta R^2_{kt} = R^2_{\hat{m}(\mathbf{h}^*,\mathbf{n}^*),kt} - R^2_{\hat{m}(\mathbf{h}^*),kt}$. $R^2_{\hat{m}(\mathbf{n}^*),kt}$ is the coefficient of determination from TCA-year regressions where log of sale price is regressed onto neighborhood characteristics. The sample contains TCA-years where there are at least 50 transactions. Standard errors are calculated from a bootstrapping procedure outlined in the text. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

		β_{kt}	
$R^2_{\hat{m}(\mathbf{h}^*),kt}$	0.712***		
	[0.011]		
ΔR^2_{kt}		-0.554***	
		[0.018]	
$R^2_{\hat{m}(\mathbf{n}^*),kt}$			-0.055***
			[0.016]
County-Year FE	Y	Y	Y
Observations	12,254	12,254	12,254
R-squared	0.595	0.415	0.324

Table 3: Synthetic Valuation Ratio Regression Results

OLS regression results where log of observed valuation ratio and log of synthetic valuation ratio are regressed onto log of sale prices along with TCA fixed effects. The sample is composed of single family homes that were sold in 2018, have at least one previous sale price, and located in a zip code where Zillow publishes a single family home price index. Synthetic valuation ratios are calculated using synthetic appraised values described in section 5.6. Columns 1 and 2 compare observed valuation ratios to synthetic valuation ratios for all houses. Columns 3 and 4 compares the two valuation ratios for houses that were reappraised in 2018. Standard errors are clustered by TCA and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	Log(Appraised Value) - Log(Sale Price)			
	Observed	Synthetic	Observed	Synthetic
Log(Sale Price)	-0.140***	-0.088***	-0.126***	-0.088***
	[0.004]	[0.002]	[0.004]	[0.003]
Years Since Reappraisal	Any	Any	Zero	Zero
TCA FE	Y	Y	Y	Y
Observations	1,653,001	1,653,001	1,093,236	1,093,236
R-squared	0.569	0.118	0.408	0.125

Table 4: Infrequent Reappraisal Regression Results

OLS regression results where log of valuation ratio is regressed onto log of sale prices. Column 1 shows result for all houses sold in 2018. Column 2 shows result for houses that were reappraised and sold in 2018. All specifications include TCA fixed effects. Standard errors are clustered by TCA and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	Log(Appraised Value) - Log(Sale Price)	
	(1)	(2)
Log(Sale Price)	-0.160***	-0.151***
	[0.004]	[0.004]
TCA FE	Y	Y
Years Since Reappraisal	Any	Zero
Observations	3,080,725	2,145,742
R-squared	0.555	0.580

Table 5: House Price, Appeal Behavior, and Outcomes Regression Results - Cook County, IL

OLS regression results where appraised value appeal-related variables are regressed onto log of sale price. Appeal indicator equals 1 if the homeowner filed an appeal in the year that the house was sold. The sample in column 1 is composed of houses in Cook County Illinois that were sold between 2007 and 2017. Win indicator equals 1 if the homeowner won the appeal that he or she filed in the same year that the house was sold. The sample in column 2 includes all houses where the owner filed an appeal in the same year that the house was sold. Percentage appraisal reduction is the reduction in appraised value that the house received from its appeal that was filed in the year that the house was sold as a percentage of the proposed appraised value. The sample in column 3 includes all houses where the owner won an appeal that was filed in the year that the house was sold. All regressions include TCA by year fixed effects. Standard errors are clustered by TCA and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	Appeal Indicator	Win Indicator	% Appraisal Reduction
Log(Sale Price)	-0.045***	-0.039***	-0.020***
	[0.009]	[0.009]	[0.005]
Sample	All	Appealed	Won
TCA-Year FE	Y	Y	Y
Observations	501,881	76,088	52,539
R-squared	0.295	0.369	0.356

Table 6: Assessment Regressivity's Impact on Wealth Distribution in 2016

Distribution of households' home values and average net worth are collected from the 2016 Survey of Consumer Finance. Percentile group upper and lower bounds are rounded to the nearest thousand. Numbers not shown as percentages are in 2019 USD. Excess tax payment for each house is calculated as the difference between the observed 2017 tax bill, which is calculated from the house's 2016 appraised value, and a counterfactual tax bill where the house is taxed according to its 2016 sale price. Change in home equity for each house is calculated as its excess tax payment treated as a perpetuity and discounted at 4%. Mean percentage change in net worth is calculated as mean change in home equity divided by mean net worth.

Home Value Percentile Group	Minimum Home Value	Maximum Home Value	Mean Net Worth	Mean Excess Tax Payment	Mean Change Home Equity	% Change Net Worth
$<$ 10th	1	64,000	101,052	684	17,100	16.9%
10th - 20th	64,000	96,000	166,526	406	10,144	6.1%
20th - 30th	96,000	132,000	214,855	240	5,994	2.8%
30th - 40th	132,000	160,000	316,823	156	3,908	1.2%
40th - 50th	160,000	197,000	319,683	129	3,229	1.0%
50th - 60th	197,000	245,000	409,073	108	2,693	0.7%
60th - 70th	245,000	319,000	587,328	69	1,718	0.3%
70th - 80th	319,000	425,000	938,840	30	752	0.1%
80th - 90th	425,000	638,000	1,668,463	49	1,218	0.1%
90th - 99th	638,000	2,127,000	4,065,906	-225	-5,616	-0.1%
$\geq$ 99th	2,127,000	196,136,000	22,419,290	-29,056	-726,402	-3.2%

Table 7: Baseline Assessment Gap Results

OLS regression results where log of valuation ratio is regressed onto demographic indicator variables. Black mortgage holder equals 1 if the mortgage holder is a non-mixed black individual and zero for non-Hispanic non-mixed whites. Hispanic mortage holder equals 1 for a Hispanic individual and zero for non-Hispanic whites. Low-income mortgage holder equals 1 for mortgage holders with annual income lower than 80% of the median household income in the application year and zero otherwise. The number of observations in each column is different because each column compares two different groups of mortgage holders. Each model includes TCA by year fixed effects. Standard errors are clustered by TCA and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	Log(Appraised Value) - Log(Sale Price)		
Black Mortgage Holder	0.015***		
	[0.002]		
Hispanic Mortgage Holder		0.009***	
		[0.002]	
Low-Income Mortgage Holder			0.044***
			[0.001]
TCA-Year FE	Y	Y	Y
Observations	3,228,405	3,424,808	3,956,964
R-squared	0.792	0.787	0.787

Table 8: Average 2017 Mortgage Holder Characteristics by Within-TCA House Price Decile

All numbers are averages, except for within-TCA price decile. The unit for numbers not presented as percentages is 2017 USD. Home equity is calculated as the difference between the house's sale price and the total mortgage amount. Household income, percent black, and percent Hispanic are calculated from HMDA mortgage holder data merged into CoreLogic.

Within-TCA Price Decile	Home Equity	Household Income	% Black	% Hispanic
1	20,944	60,023	8.8%	17.3%
2	27,988	70,268	8.3%	15.7%
3	34,582	77,478	7.7%	14.0%
4	41,329	85,287	7.0%	12.9%
5	46,990	91,597	6.7%	11.5%
6	58,398	101,883	6.0%	10.5%
7	66,880	110,507	5.7%	9.3%
8	79,465	124,377	5.5%	8.5%
9	101,238	144,499	4.8%	7.6%
10	165,919	224,629	4.3%	6.1%

Table 9: Assessment Gaps Conditional on Price Decile Regression Results

OLS regression results where log valuation ratio is regressed onto demographic indicator variable, TCA-year price decile indicator, and their interaction terms. "Demographic Indicator" is a placeholder for the demographic characteristic listed at the head of each column. Black mortgage holder equals 1 if the mortgage holder is a non-mixed black individual and zero for non-Hispanic non-mixed whites. Hispanic mortgage holder equals 1 for a Hispanic individual and zero for non-Hispanic whites. Low-income mortgage holder equals 1 for mortgage holders with annual income lower than 80% of the median household income in the application year and zero otherwise. The number of observations in each column is different because each column compares two different groups of mortgage holders. Each regression includes TCA by year fixed effects. Standard errors are clustered by TCA and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

Dependent Variable	Log(Appraised Value) - Log(Sale Price)		
Demographic Variable	Black	Hispanic	Low Income
Demographic Indicator	-0.007	-0.036***	0.003
	[0.006]	[0.007]	[0.003]
Price Decile 2	-0.085***	-0.085***	-0.078***
	[0.001]	[0.001]	[0.001]
Price Decile 3	-0.114***	-0.114***	-0.104***
	[0.002]	[0.002]	[0.002]
Price Decile 4	-0.131***	-0.131***	-0.120***
	[0.002]	[0.002]	[0.002]
Price Decile 5	-0.143***	-0.143***	-0.132***
	[0.002]	[0.002]	[0.002]
Price Decile 6	-0.153***	-0.152***	-0.140***
	[0.002]	[0.002]	[0.002]
Price Decile 7	-0.160***	-0.160***	-0.148***
	[0.002]	[0.002]	[0.002]
Price Decile 8	-0.167***	-0.166***	-0.155***
	[0.002]	[0.002]	[0.002]
Price Decile 9	-0.174***	-0.174***	-0.163***
	[0.002]	[0.002]	[0.002]
Price Decile 10	-0.188***	-0.188***	-0.177***
	[0.003]	[0.003]	[0.003]
Price Decile 2 × Demographic Indicator	0.001	0.015***	-0.009***
	[0.004]	[0.004]	[0.002]
Price Decile 3 × Demographic Indicator	0.006	0.023***	-0.012***
	[0.005]	[0.005]	[0.002]
Price Decile 4 × Demographic Indicator	0.008	0.028***	-0.015***
	[0.006]	[0.005]	[0.003]
Price Decile 5 × Demographic Indicator	0.008	0.029***	-0.019***
	[0.006]	[0.005]	[0.003]
Price Decile 6 × Demographic Indicator	0.010	0.033***	-0.027***
	[0.007]	[0.006]	[0.003]
Price Decile 7 × Demographic Indicator	0.009	0.030***	-0.033***
	[0.007]	[0.006]	[0.003]
Price Decile 8 × Demographic Indicator	0.007	0.028***	-0.041***
	[0.007]	[0.007]	[0.003]
Price Decile 9 × Demographic Indicator	0.005	0.027***	-0.050***
	[0.007]	[0.007]	[0.004]
Price Decile 10 × Demographic Indicator	0.009	0.029***	-0.055***
	[0.007]	[0.008]	[0.004]
TCA-Year FE	Y	Y	Y
Observations	3,228,405	3,424,808	3,956,964
R-squared	0.803	0.798	0.797

Table 10: Significance Test Results for Assessment Gaps Conditional on Price Decile

Significance test results on regression coefficients presented in table 9. For each t-test, asterisks denote statistical significance at the 10% level or above. Column 1 compares non-mixed black mortgage holders to non-mixed non-Hispanic white mortgage holders in each TCA-year price decile. Column 2 compares Hispanic mortgage holders to non-Hispanic white mortgage holders in each TCA-year price decile. Column 3 compares low-income mortgage holders to middle and high-income mortgage holders in each TCA-year price decile. The last row reports p-values from a joint test against the hypothesis that coefficients in each column are jointly equal to zero.

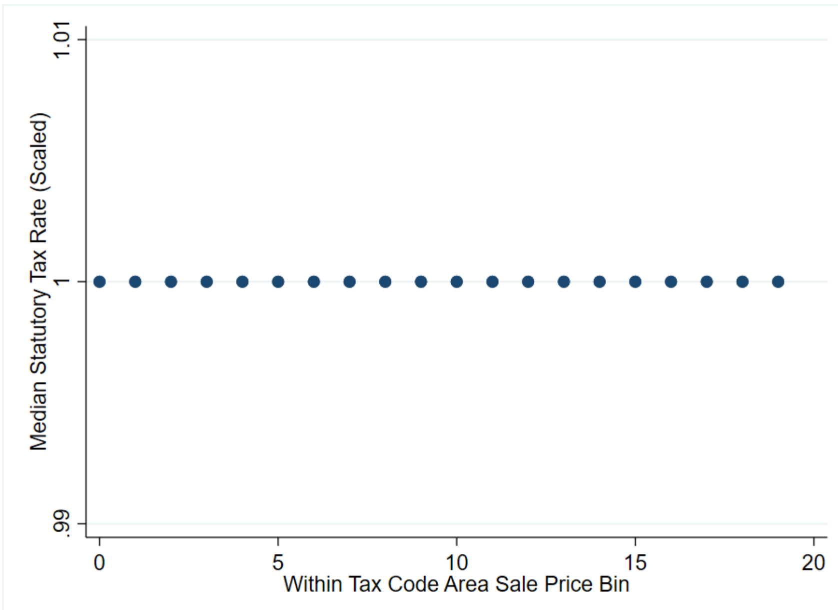
	Demographic Indicator		
	Black	Hispanic	Low Income
Demographic Indicator	-0.007	-0.036*	0.003
Dem Ind + Dem Ind × Price Decile 2	-0.006	-0.021*	-0.006*
Dem Ind + Dem Ind × Price Decile 3	-0.001	-0.012*	-0.009*
Dem Ind + Dem Ind × Price Decile 4	0.001	-0.008*	-0.012*
Dem Ind + Dem Ind × Price Decile 5	0.001	-0.007*	-0.016*
Dem Ind + Dem Ind × Price Decile 6	0.003	-0.003	-0.024*
Dem Ind + Dem Ind × Price Decile 7	0.002	-0.006*	-0.03*
Dem Ind + Dem Ind × Price Decile 8	0.000	-0.008*	-0.038*
Dem Ind + Dem Ind × Price Decile 9	-0.002	-0.008*	-0.047*
Dem Ind + Dem Ind × Price Decile 10	0.002	-0.007	-0.052*
Joint Test P-value	0.66	0.00	0.00

A Appendix

A.1 Within-TCA Statutory Tax Rate Plot

Figure A1: Median Scaled Statutory Tax Rate by 2016 Within-TCA House Price Bin

Binnned scatter plot of mean scaled statutory tax rate for houses in each within-TCA price bin. Tax code areas (TCA) are small geographical areas where every house has access to the same set of property-tax funded government services and pay the same statutory tax rate. Each house's statutory tax rate is scaled by the median effective tax rate in its TCA. Houses in each TCA are evenly divided into twenty price bins. The cheapest houses are in the first bin and the most expensive houses are in the twentieth bin. The sample contains houses in 49 states and the District of Columbia that were sold in 2016.



A.2 Variance of Sample Means

Let X be a random variable with variance σ_X^2. With n independent draws, $X_1, X_2, ..., X_n$, the variance of the sample mean $\overline{X}$ is

$$
\begin{aligned}
Var(\overline{X}) &= Var\left(\frac{X_1 + X_2 + ... + X_n}{n}\right) \\
&= \frac{1}{n^2} Var(X_1 + X_2 + ... + X_n) \\
&= \frac{1}{n^2} n\sigma_X^2 \\
&= \frac{\sigma_X^2}{n} \\
&< \sigma_X^2
\end{aligned}
$$

If draws are not independent, then $\sigma_{\overline{X}}^2 \leq \sigma_X^2$. The two quantities are equal to each other in the extreme case where draws are perfectly correlated.

A.3 Covariance of Sample Means

Let X and Y be random variables with positive covariance. With n independent paired samples (X_i, Y_i), the covariance of the sample means is

$$
\begin{aligned}
Cov(\overline{X}, \overline{Y}) &= Cov\left(\frac{1}{n}\sum_{i=1}^{n} X_i, \frac{1}{n}\sum_{j=1}^{n} Y_j\right) \\
&= \frac{1}{n^2}\sum_{i=1}^{n}\sum_{j=1}^{n} Cov(X_i, Y_j) \\
&= \frac{1}{n^2}\sum_{i=1}^{n} Cov(X_i, Y_i) \\
&= \frac{1}{n} Cov(X, Y) \\
&< Cov(X, Y)
\end{aligned}
$$

Similarly for the covariance of X and $\overline{Y}$

$$
\begin{aligned}
Cov(X,\overline{Y}) &= Cov\left(X_i, \frac{1}{n}\sum_{j=1}^{n} Y_j\right) \\
&= \frac{1}{n}\sum_{j=1}^{n} Cov(X_i, Y_j) \\
&= \frac{1}{n} Cov(X,Y) \\
&< Cov(X,Y)
\end{aligned}
$$

If draws are not independent, then $Cov(X,\overline{Y}) \leq Cov(X,Y)$ and $Cov(\overline{X},\overline{Y}) \leq Cov(X,Y)$. The quantities are equal to each other in the extreme case where draws are perfectly correlated.

A.4 Low $Cov(a,m)$ Under the Cost Approach

The cost approach operates on the premise that, when a buyer purchases a home, he is paying for the cost of the structure less depreciation plus the land price (IAAO, 2014). The cost approach is often implemented in the following steps. First, the appraiser needs to assign a cost to the structure that sits on the land parcel. The most common approach is to use the average construction cost of similar structures in the same area, e.g. state or county (Pickens County Assessor's Office, 2018). To adjust this construction cost for the location of the property, e.g. city or zip code, the appraiser applies a local multiplier to the construction cost. The local multiplier is calculated either by the appraisal office or provided by the Computer Assisted Mass Appraisal Software (CAMA) that the office uses. The multiplier is the average sale price to cost ratio of a group of similar properties in a comparable neighborhood. The idea is that, if neighborhoods are defined correctly, then these multipliers should capture the neighborhood's quality that is impounded into the cost of the structure. Finally, the appraiser uses the comparable sales approach or the land residual method to assign a market value to the land parcel that the structure sits on (Snohomish County Assessor's Office, 2010).[19] The sum of the cost of the structure and the land price gives the property's total

[19] The residual method finds transacted houses in the same neighborhood as the house that is being appraised, subtracts their estimated construction costs from their sale prices, and calculates the land price for the house that is being appraised by averaging these residuals (Town of Lenox, 2018).

appraised value (Snohomish County Assessor's Office, 2019a; Thurston County Assessor's Office, 2015).

Similarly to CSA, the flaw of the cost approach lies in how appraisers define neighborhoods and choose comparable houses. Neighborhoods are defined too broadly, i.e., covering to large of an area. Comparable houses are chosen based on observable characteristics. The procedure ignores latent house characteristics that may differ across houses. Formally, appraised values under the cost approach can be expressed as follows.

$$A_i^{Cost} = S_i^{Cost} + P_i^{CSA}$$

S^{Cost} denotes the construction cost of the structure and P^{CSA} denotes the price of the land parcel estimated using CSA. Suppose that the true market value of house i can be expressed in a similar fashion.

$$M_i = S_i + P_i$$

S is now the true market value of the structure and P is the true market value of the land parcel. Since S^{Cost} and P^{CSA} are sample means, the same arguments made for the CSA apply and it follows that $Cov(A, M) < Cov(M, M) = Var(M)$. Assuming that $\mathbb{E}(A)\mathbb{E}(M)$ is sufficiently large and using the following approximation, it follows that $Cov(a, m) < Cov(m, m) = Var(m)$..

$$Cov(A, M) \approx \mathbb{E}(A)\mathbb{E}(M) \times (e^{Cov(a,m)} - 1)$$

A.5 Low $Cov(a, m)$ Under the Income Approach

Under the income approach, the appraiser collects gross rents and sales data. To price an arbitrary house i, the appraiser multiplies the house's gross annual rental income with a sales multipler, which is the average price-to-gross rent ratio from a sample of recently sold houses located in the same area as house i (IAAO, 2014). Formally, the log of appraised values can be expressed in the following way.

$$a_i^{Income} = \overline{q}_i + r_i$$

$\overline{q}_i$ is the average price-to-rent ratio that appraisers apply to house i's gross rent, r_i. Under the Gordon Growth Model, the log of market values can be expressed in a similar way (Gordon, 1962).

$$m_i = q_i + r_i$$

q_i is the inverse of house i's discount rate under the Gordon Growth Model. Since $\overline{q}_i$ is a sample mean and assuming that its correlation with r is weakly positive, the same arguments made for the CSA apply and it follows that $Cov(a, m) < Cov(m, m) = Var(m)$.

A.6 Implicit Assumptions for Wealth Inequality Calcuation

Calculations in section 7 make several simplifying assumptions. The first assumption is that redistributing tax burdens among houses that were sold is close enough to the tax burden distribution that would have realized if, instead, all houses were sold and the calculations were repeated on this larger sample. Secondly, these calculations make the assumption that every government entity that collects property taxes from a TCA shares the same property tax base, which is made up of all single family homes in the TCA. In practice, each government entity has its own service and taxing boundaries, which differs from each other, and unique overlapping areas of these service boundaries form TCAs. Therefore, the correct calculation requires a data set that contains the complete set of property-tax-collecting government entities, each government's tax base, and each government's statutory tax rate. Results from similar back-of-the-envelope calculations that use this more comprehensive data set may be different from the results presented above.

Chapter II

Bond Insurance and Public Sector Employment

1 Introduction

Leading up to the 2008-9 financial crisis, the bond insurance business grew from an obscure feature of the municipal bond market to become the main source of cheap debt that state and local governments heavily relied upon. However, the financial crisis caused this industry to fail and it never recovered. Figure 1 shows the dramatic rise and fall of the municipal bond insurance industry. This image raises the question: does the health of bond insurance companies matter for insurees' economic outcomes?

This article explores how financing frictions that arise from loss of bond insurance affect public sector employment outcomes during the 2008-9 financial crisis. To do so, I construct a new data set that combines local governments' characteristics, employment, reliance on bond insurance, and bond issuance. This data set allows me to answer two related questions: (1) which type of governments used more bond insurance and (2) how did the demise of the bond insurance industry affect these governments' ability to issue new debt and employ workers?

To answer the first question, I begin by studying the correlation between issuers' characteristics and bond insurance use on the intensive margin. Bond insurance use on the intensive margin is defined as the percentage of municipal bonds that the issuer issued with insurance between 1980 and 2007. I find that smaller and more opaque issuers used more bond insurance in the pre-crisis period. Specifically, special district governments and government entities with no credit ratings tend to issue more debt with insurance. This pattern in the data suggests that issuers with higher degrees of information asymmetry used bond insurance more intensively.

To answer the second question, I compare employment outcomes at governments that bought bond insurance from relatively healthy insurance companies in the pre-crisis period to those that bought bond insurance from insurance companies that were more adversely affected by the crisis.

This methodology relies on two facts. First, bond insurance relationships are sticky. In other words, governments tend to buy bond insurance from the same insurance companies as they issue more debt. Persistence in bond insurance relationships indicate that switching cost is nontrivial. Second, the 2008-9 financial crisis began outside of the municipal bond market. This fact implies that insurance companies' willingness to write new insurance policies during the crisis was plausibly orthogonal to insurees' characteristics.

The following example further clarifies the thought experiment. Leading up to the crisis, there were nine companies in the bond insurance business. Assured Guranty Corp. (AG) and Financial Security Assurance Inc. (FSA) underwrote their RMBS bond insurance business conservatively, while the other seven took more risk. When the housing bubble popped, these two companies suffered relatively less losses in their RMBS insurance portfolio and were able to continue writing new municipal bond insurance policies, while the rest stopped. The fact that only some insurance companies failed allows me to compare bond issuance and employment outcomes of governments linked to healthy insurers to those linked to less healthy ones.

I establish support for my empirical methodology by documenting the stickiness of bond insurance relationships. Similarly to bank lending relationships, insurance relationships are sticky. The empirical persistence in issuer-insurer relationships exceeds by a factor of three relative to what one would predict based only on insurers' market shares. This finding indicates that switching to a new insurer is costly. Furthermore, insurance relationships are stickier for issuers with higher degrees of information asymmetry.

I then construct a measure for a government's insurers' health using the insurers' growth in municipal bond insurance volume during the crisis. Specifically, for each government, the measure captures how much municipal bond insurance its insurers underwrote for *other* governments. Calculating this measure from the insurer's business dealings with *other* governments ensures that the correlation between insurer's health and bond issuance is not mechanical. This insurer's health measure proxies for the insurer's shadow cost of writing new insurance policies, which should vary with the insurer's exposure to the financial crisis and, hence, capital position. I then use this measure to explore how insurers' health affected a government unit's ability to issue new debt and hire workers.

To get a causal interpretation, I instrument my measure of insurers' health with two instruments that capture each insurer's exposure to risky asset-backed securities. These instruments exploit the fact that the financial crisis originated from asset-backed securities, which were unrelated to the public sector, except through the bond insurance industry.

With this identification strategy, I study the impact that the sharp contraction in the availability of bond insurance had on government units' ability to issue debt and hire workers. Financing frictions that arose from unhealthy insurers were most evident on the extensive margin. Pre-crisis clients of bond insurers in worse financial conditions were able to issue 9% less debt during the crisis than clients of healthier insurers. These financing frictions translated into real effects on issuers' ability to hire workers. A one standard deviation decrease in insurers' health lowered an issuer's full-time employment growth and part-time employment growth between 2007Q2 to 2009Q2 by approximately 1% and 3%, respectively. Futhermore, the effect on full-time employment grew in the long run, while the effect on part-time employment shrank. Specifically, a one standard deviation decrease in insurers' health lowered an issuer's full-time employment growth and part-time employment growth between 2007Q2 to 2011Q2 by 1.4% and 2.1%, respectively. This result suggests that affected governments may have converted full-time employees into part-time employees.

The financing friction effect was concentrated among special-purpose government units, which were smaller and more opaque. The heterogeneous impact highlights the value of bond insurance to more opaque issuers and the mechanism at play. When an issuer chooses to issue an uninsured bond, investors produce information about the issuer to decide how much they would like to pay for the bond. When an issuer chooses to issue an insured bond, the insurance company produces information about the issuer to decide how much premium to charge for the new issue. With insurance, the bond is issued with a AAA rating and investors have very little incentive to study the issuer because the bond is very safe and the insurance company becomes the relevant entity to study. When bond insurers failed during the crisis, there may not have been enough informed investors to buy new risky municipal bonds. The shortage of informed investors could increase financing costs for municipal bond issuers with the highest degree of information asymmetry. Since government entities use debt to consistently fund a substantial part of total spending, this disruption could affect their ability to hire workers. A partial equilibrium calibration exercise based on these heterogeneous effects shows that special-purpose governments in the sample could have employed 38,000 more full-time employees and 58,500 more part-time employees. These levels translate to approximately 4% higher full-time employment growth and 15% higher part-time employment growth.

This article mainly relates to two strands of literature: municipal bond insurance and economic effects of credity supply shocks. The bond insurance literature largely ignores the effect that bond insurance has on government entity's access to financing and real outcomes such as employment. Theoretical works in this literature focus on why bond insurance exists (Nanda and Singh, 2004; Thakor, 1982). Recent articles explore the benefits of bond insurance with regards to bond yields and liquidity (Chun et al., 2018; Cornaggia et al., 2018b; Lai and Zhang, 2013). Other articles study the performance of insured bonds during the financial crisis (Bergstresser and Shenai, 2010; Bergstresser et al., 2015; Cornaggia et al., 2018a). To my knowledge, the current article is the first to explore the effects that the demise of bond insurers had on government entities and their workers during the financial crisis.

The literature on negative effects of credity supply shocks has mainly focused on the impact that credit supply shocks have on the private sector, mostly through the bank lending channel. Chodorow-Reich (2013) explores the impact that deterioration in lender's health had on related firms during the financial crisis. The study finds that firms that had prior relationships with less healthy banks were less likely to get loans and experienced lower employment growth. Kim (2018) uses the same identification strategy to study the impact of credit supply shocks on output prices. Almeida et al. (2009) and Gan (2007) find that negative credit supply shocks lower firm-level investments. Ashcraft (2005) and Peek and Rosengren (2000) find that contraction in bank lending led to worse local economic conditions. Two recent papers study the real effects of credity supply shocks in the public sector. Adelino et al. (2017) find that credit rating upgrades allow local governments to increase spending and stimulate their local economies. Dagostino (2017) find that, when Congress raised the limit of bank qualified bonds from $10 to $30 million, local governments were able to issue more debt and increased government spending. Unlike prior works, the current article explores the importance of bond insurance to government finance and focuses on how negative credit supply shocks from this channel affected state and local governments during the financial crisis.

2 Institutional Details on Municipal Bond Insurance

Bond insurance is an insurance policy that bond issuers buy from specialized insurance companies, often called monolines. For most policies, the issuer pays an upfront fee to the insurance company. On average, premium payments amount to approximately one percent of the total face value of insured bonds (Joffe, 2017). The insurer then provides insurance for the bond in the event of default. If the issuer defaults on its obligation, the insurance company continues to pay interest and principal as scheduled and the bond continues to trade as usual. The bond assumes the insurance company's credit rating instead of the issuer's. The insurance policy stays with the bond until the bond matures or is called.

Municipal bond insurance began with the founding of American Municipal Bond Assurance Corp. (AMBAC) in 1971 and grew in popularity after the Washington Public Power Supply System (WPPSS) defaulted on $2.25 billion worth of revenue bonds in 1983. Figure 1 shows the pre-crisis rise of bond insurance. In 1980, only about 2% of newly issued municipal bonds were insured. By 2007, approximately half of newly issued municipal bonds were insured. Between 1980 and 2007, 32.2% of all municipal bonds, measured by inflation-adjusted face value, were issued with insurance.

3 Data

Municipal bond issuance data is from SDC Platinum. Bond-level data is from Thomson Reuters EIKON.

I hand match issuers in SDC to government units that appear in the Annual Survey of State and Local Government Finances, which provides government-entity-level financial data such as revenue, expense, and debt. The Census Bureau assigns each government unit a unique identifier that is consistent across census data sets. After the first round of matching, I use the matched identifiers to merge in employment information from the Annual Survey of Public Employment and Payroll.

Each insurer's RMBS bond and CDO insurance portfolio risk is hand collected from S&P's credit risk reports.

4 Issuers' Characteristics and Bond Insurance Use

In this section, I explore how different issuer characteristics correlate with how much the issuer relied on bond insurance between 1980 and 2007. I first define a variable to capture each issuer's use of bond insurance on the intensive margin. Insurance ratio is the percentage of municipal bond that a government entity issued with insurance between 1980 and 2007. A high insurance ratio means that the government entity relied heaviliy on bond insurance before the financial crisis. Out of 45,944 unique issuers that appear in the data set between 1980 and 2007, 21,155 have insurance ratios greater than zero. In other words, less than half of all issuers had used some bond insurance in the pre-crisis period. However, this group of 21,155 issuers issued 92% of all new municipal bonds between 1980 and 2007. Therefore, on value-weighted terms, bond insurance was a significant feature of the municipal bond market. The following analyses focus on this group of issuers.

I sort each issuer into four groups according to each issuer's insurance ratio and examine how their characteristics differ across groups. Table 1 presents summary statistics on this sorting exercise. The first group of issuers has insurance ratios that are greater than zero but not more than 0.25, the second group has insurance ratios greater than 0.25 but not more than 0.5, and so on. The first characteristic that I examine is size, as measured by each issuer's total revenue in 1982. I choose to use total revenue in 1982 because 1982 is a census year, which has better coverage of government entities than in non-census years. Furthermore, I want to use size to "predict" how much bond insurance each government entity will use in the years leading up to the crisis. The first observation is that size decreases monotonically as insurance ratio increases across the four groups. The average government entity in the first group is almost 12 times larger than the average government entity in the fourth group. Therefore, smaller governments tend to use more bond insurance.

The next characteristic that I examine is government type. In the municipal bond literature, state, county, and city governments are considered to be general governments because they serve many functions and draw revenue from many sources. Other governments such as school districts and water authorities are considered to be special district governments because each serves one very specific function and draws revenue from only a few sources. This dichotomy is important for my analysis because general governments are usually subject to more stringent financial reporting requirements, while special district governments are not. Therefore, due to function and disclosure requirements, special district governments are more opaque than general governments. In order to study them, investors need to spend more resources to acquire the necessary information. All else equal, the degree of information asymmetry between the issuer and the investor is higher when the issuer is a special district government. This feature predicts that special governments will use more bond insurance. Table 1 shows that this is the case. The percentage of general governments decreases monotonically as I move up the insurance ratio scale. Almost half of the issuers in the first group are general governments, while less than 20% of issuers in the fourth group are general governments.

The last characteristic that I examine is rating status. *Not rated* equals 1 if the issuer had no rating from S&P or Moody's when it first issued a municipal bond in the data set. This variable is another proxy for information asymmetry. Although there are many criticisms related to credit ratings' reliability and timeliness, credit rating agencies contribute to the information environment of debt securities. Investors can learn about issuers by reading credit reports, which lowers the total cost of information production that investors face. All else equal, issuers that has no credit rating agency covering it should be more expensive to study than those that do. The final row of Table 1 show that issuers with no rating use bond insurance more than those that do. The percentage of unrated issuers increases from 52% to 78% as I move up the insurance ratio scale.

5 Bond Insurance Relationships

For shocks to bond insurers' health to create significant financing friction for bond issuers, insurance relationships must be sticky, i.e., switching to a new insurer is costly. There are several reasons for this to be the case. The municipal bond market is more opaque when compared to the corporate bond market. Financial disclosure by municipal bond issuers are largely voluntary (Baber and Gore, 2008). Therefore, it is costlier for investors to asses the credit risk of municipalities that issue debt. With insurance, bond insurers study municipal debt issuers on behalf of investors. In turn, investors need to only study the bond insurer to understand the bond's credit risk. Bond insurers are regulated similarly to banks so information regarding their financial health is more readily available. Once an insurance company forms a relationship with the issuer, it is costly for the issuer to switch to another insurance company because the new company needs to do its own research on the issuer before it can insure any new bond. Hence, when an insurance company's ability to write new insurance policies declines, associated issuers face significant financing frictions, especially if the issuer is opaque and risky.

In this section, I explore whether insurance relationships are persistent. Specifically, I ask the question - conditional on buying bond insurance, how likely is it for the issuer to buy bond insurance from the insurance company that insured its previous bond issue? I estimate variants of the following choice model.

$$\begin{aligned} Current\,insurer_{ijkt} = \alpha_k &+ \beta_1(Previous\,insurer_{jk}) \\ &+ \beta_2(Previous\,insurer_{jk} \times General\,government_j) \\ &+ \beta_3(Previous\,insurer_{jk} \times General\,obligation_i) \\ &+ \beta_4(Previous\,insurer_{jk} \times Rated\,issuer_{jt}) \\ &+ \boldsymbol{\gamma' x} + \kappa_k + \theta_t + \epsilon_{ijkt} \end{aligned} \tag{1}$$

$Current\,Insurer_{ijk}$ equals 1 if insurance company k serves as the insurer for bond package i issued by issuer j and zero otherwise. $Previous\,Insurer_{jk}$ equals 1 if insurance company k served as the bond insurer for issuer j's previous insured bond package and zero otherwise. Each bond package is matched with each bond insurance company that was active in the year that the package was issued. For example, if there were nine active bond insurers in year t, then every insured bond package that was issued in that year appears in the data set nine times. The average value of $Current\,Insurer_{ijk}$ is 0.14. In column 1, the estimated value of β_1 is 0.319. The coefficient means that, after controlling for each insurer's average market share (α_k), a previous insurer is 31.9% more likely to serve as the insurer for issuer j's current bond package.

The stickiness of insurance relationships also depends on credit risk and degree of information asymmetry associated with the package and the issuer. This is shown by the negative sign on β_2, β_3, and β_4. $General\,government$ equals 1 if the issuer is a state, county, or city government and zero otherwise. These government entities have lower credit risk compared to special purpose government because they have more diversified revenue sources and larger budgets. *General obligation* equals 1 if bonds in the package are general obligation bonds, which are backed by all of the government unit's revenue, instead of by revenue from a particular source. The negative coefficients on β_2 and β_3 show that insurance relationships are less sticky when credit risk is lower. *Rated issuer* equals 1 if the issuer has a rating from S&P or Moody's. In the case that the insurer goes out of business, an investor can judge the credit of the issuer that has a rating more easily than the credit risk of another issuer that has no rating. The negative coefficient on β_4 shows that insurance relationships are less sticky when information asymmetry is lower. This result suggests that insurance relationships are similar to banking relationships, which are stickier when information asymmetry is higher (Chodorow-Reich, 2013). Column 2 includes a set of fixed effects that aims to capture each insurer's specialization. For example, insurer by state fixed effects capture insurers' specialization by geography. The results remain largely the same as those in column 1.

6 State and Local Government Debt Financing

How much do local governments rely on municipal bonds for financing needs? Figure 2 uses data from the Annual Survey of State and Local Government Finances to plot the percentage of government entities that issued debt in each year. Government entities that are included in this sample are those that reported financial information to the Census Bureau in every single year from 1980 to 2007. This requirement ensures that the sample remains consistent throughout. The first fact is a substantial proportion of government entities issued some debt every year. Approximately 38% of government entities in the sample issued some debt in 1980. This proportion increased to approximately 55% in 2007. The steady increase indicates that local governments increasingly relied on debt financing in the period leading up to the crisis. Furthermore, the average government in this sample issued some debt in 13 out of the 28 years between 1980 to 2007. This means that the average government issued some debt approximately once every 2 years.

To get a sense of the importance of debt issuance relative to total expenditure, figure 3 uses the same sample of governments and plots the total amount of debt as a percentage of total expenditure for each year. For most of the sample, this percentage stayed between 9% and 16%, with an average of 12.2%. This finding shows that the average government consistently uses new debt to finance approximately 12% of its annual expenditure. Putting everything together, this section finds that local governments frequently issue debt and use the proceeds to finance a substantial part of their expenditures. Therefore, without additional revenue, a significant disruption to these governments' ability to issue new debt could lead to a substantial drop in their ability to spend.[1]

7 Empirical Methodology

The previous sections establish that municipal bond issuers rely on bond insurance to issue debt more cheaply. Furthermore, insurance relationships are sticky, especially for issuers with higher credit risk and opacity. Hence, shocks to insurance companies' capital during the financial crisis could cause significant financing frictions for these municipal bond issuers. Since local governments use debt to consistently fund a substantial part of total spending, this disruption could affect their ability to hire workers. This section outlines the identification strategy that I use to show the causal effect that changes in bond insurance companies' health has on municipal bond issuers' ability to issue debt and employment growth.

7.1 Municipal Bond Insurance During the Financial Crisis

Leading up to the financial crisis, bond insurers began to insure asset-backed securities (ABS). Some also wrote credit default swaps (CDS) on these securities. Prior to the finance crisis, there were nine bond insurers – ACA Financial Guaranty Corp. (ACA), Assured Guranty Corp. (AGC), Ambac Assurance Corp. (AMBAC), CIFG Assurance North America Inc. (CIFG), Financial Guaranty Insurance Co. (FGIC), Financial Security Assurance Inc. (FSA), MBIA Insurance Corp. (MBIA), Radian Asset Assurance Inc.(RADIAN) and XL Capital Assurance Inc. (XLCA). When the housing market bubble burst in 2006 and 2007, these insurance companies began to experience losses from policies written on ABS. The amount of loss varied with how much risk each company took in

[1]For example, a 10% drop in annual debt issuance should lead to a 1.2% drop in total expenditure, all else equal.

writing policies on ABS from the 2006 and 2007 vintages. Out of the nine, only AGC and FSA maintained their AAA financial enhancement rating from S&P and continued to write new bond insurance policies throughout the crisis. Table 3 summarizes each insurance company's municipal bond insurance volume and financial enhancement ratings dynamics throughout the crisis.[2]

This setup allows me to identify the effect that insurance companies' health had on municipalities' financing friction and employment outcomes. First, problems in the bond insurance industry originated in the ABS market and not from the municipal bond market. This feature gives plausibly exogenous variation in the supply of bond insurance available to municipal bond issuers. As ABS bonds began to default, insurance companies experienced losses and their capital deteriorated. With less capital, their ability to write new insurance policies on new municipal bonds decreased. Second, insurance relationships are sticky. Shocks to insurance companies can be traced to related municipal bond issuers through these relationships. Any financing and employment effects that I find can be interpreted as the result of shocks from specific insurance companies that get transmitted to specific bond issuers through existing business relationships. Lastly, not all insurance companies stopped writing new policies during the crisis, which offers cross sectional variation in insurers' health that I can exploit.

7.2 Insurers' Health Measure

I am interested in studying the impact that variation in the availability of bond insurance has on local governments' ability to issue new debt and hire employees. The availability of bond insurance that each government entity faces depends on the financial health of insurers that it has prior business relationships with. For each issuer, I construct a measure of change in insurers' health from the pre-crisis period to the crisis period. The first component of the measure is $PreCrisis\,Business_{ij}$. For each issuer-insurer pair, I calculate the amount of municipal debt that insurer j insured in the pre-crisis period (2006Q1 to 2007Q2) minus the amount of municipal debt issued by issuer i that insurer j insured. The second component is $Crisis\,Business_{ij}$. For each issuer-insurer pair, I calculate the amount of municipal debt that insurer j insured in the crisis

[2] The financial enhancement rating is the rating that gets assigned to bonds that the insurance company insures. This rating is separate from but is highly correlated with the insurance company's credit rating.

period (2008Q1 to 2009Q2) minus the amount of municipal debt issued by issuer i that insurer j insured. The final component is α_{ij}. This quantity is the share of insured municipal bonds that insurer j insured for issuer i between 1980Q1 to 2005Q4, adjusted for inflation. This quantity aims to capture the complete set of insurance relationships that each issuer had prior to the crisis and the relative importance of each insurer to issuer i. I choose to start the calculation in 1980 because I want to capture the complete set of insurance relationships. During the crisis, an issuer would most likely try to issue insured debt with its most recent insurer. If it could not do so, it would choose an insurer that it had a prior relationship with over other insurers. Starting the calculation in later years would exclude some of this information. I choose to end the calculation in 2005Q4 to make sure that the results are not driven by issuers switching to healthier insurers in anticipation of the crisis.[3] With all three components, I define, for each local government, related insurers' health as follows.

$$\Delta I_i = \sum_{j=1}^{n} \alpha_{ij} \times [log(1 + Crisis\,Business_{ij}) - log(1 + PreCrisis\,Business_{ij})] \tag{2}$$

This measure is the weighted average change in insurers' health, measured by the log difference in municipal bond insurance that each insurer was able to underwrite from the pre-crisis period to the crisis period. A higher value of ΔI_i means that the group of insurance companies associated with issuer i was healthier because it was able to write relatively more insurance policies during the crisis. With this measure, I estimate variants of the following cross-sectional regression.

$$Y_i = \beta(\Delta I_i) + \boldsymbol{\gamma'x} + \kappa_s + \epsilon_i \tag{3}$$

In the following sections, Y_i are various measures of financing quanitites and employment outcomes.[4] This cross-sectional regression relies on a strong identification assumption, which is that the cross-

[3] Bond issues that had multiple insurers were divided evenly among all participating insurers. All results are qualitatively and quantitatively similar if these issues were divided according to insurers' pre-crisis market share.

[4] In the following sections, I cluster standard errors on unique groups of insurance companies or insurance syndicates. For example, if the issuer is related to insurance company A, this is one group. A group of insurance companies A and B is another group. I cannot cluster by the insurance company that had the largest relationship share because the resulting number of clusters would be too small (Cameron and Miller, 2015).

sectional variation in insurance volume reflects only supply factors and observed characteristics of the municipalities. In other words, that unobserved characteristics of municipalities that affect insurance demand are not correlated at the insurer level. This assumption may not hold but the direction of the omitted variable bias is unclear because insurance demand can reflect either a healthy municipality wanting to expand or an unhealthy municipality wanting to cushion a fall in revenue.

7.3 Instrumental Variables

To relax this identification assumption, I propose the following instruments for ΔI_i. The first instrument is the proportion of policies in each insurer's RMBS insurance portfolio that was written on AAA bonds, observed at the end of 2007Q3. The second instrument is the proportion of policies in each insurer's CDO insurance portfolio that was written on high quality CDOs, observed at the end of 2007Q3.[5] These two instruments, $AAA\,RMBS\,exposure$ and $HQ\,CDO\,exposure$, should have a strong positive correlation with ΔI_i if excessive ABS risk caused insurance companies to fail.

Table 4 presents first stage regression results where I regress ΔI_i on $AAA\,RMBS\,exposure$ and $HQ\,CDO\,exposure$.

$$\Delta I_i = \beta(Instrument) + \boldsymbol{\gamma'x} + \kappa_s + \epsilon_i \tag{4}$$

Both instruments are positively correlated with ΔI_i at the 1% level. In column 1, a one percentage point increase in $AAA\,RMBS\,exposure$ is correlated with a 11.7% increase in ΔI_i. In column 2, a one percentage point increase in high quality CDO exposure is correlated with a 3.7% increase in ΔI_i. With very high R^2 values, these findings confirm that an insurer's ABS risk is a major determinant of its survival during the crisis. The strong correlation between ΔI_i and these instruments is also confirmed by the large first-stage F-statistics shown in subsequent regression tables. These

[5]S&P separates insured CDOs into five risk levels with 1 being the safest and 5 being the riskiest. This variable is constructed from the proportion of each insurer's CDO insurance portfolio that falls into levels 1 and 2.

F-statistics pass the Stock and Yogo (2005) criteria for sufficiently strong instruments.

With the instruments above, the identifying assumption becomes that less healthy insurance companies, as measured by each instrument, did not also insure bonds of governments drawn from a different distribution of issuer health. In other words, there is random assignment along the spectrum of AAA RMBS exposure and high quality CDO exposure. This identification assumption plausibly holds for the ABS-related instruments for the following reasons. First, the RMBS and CDO markets are separate from the municipal bond market except through these insurance relationships. Second, the real estate downturn that caused the 2008-9 financial crisis was largely unexpected so local governments should not have considered ABS insurance risk to be a salient risk factor when choosing insurers (Cheng et al., 2014) .

It is still possible that variation in these instrumental variables reflects the insurer's unobservable risk appetite, i.e., insurers with riskier ABS portfolios are more risk-loving and choose to insure riskier governments. Table B1 provides supporting evidence for the random assignment assumption by presenting average values of government-level covariates sorted by levels of each instrument. In panel A, there is substantial variation in AAA RMBS exposure across the four bins. The average exposure in the first bin is 16.6%, while the average exposure in the fourth bin is 43%. Despite this large variation in AAA RMBS exposure, I find that other covariates are well balanced, i.e., average values of each covariate across the four columns are similar when compared to the sample's standard deviation. In particular, variation in the insurer's municipal bond insurance portfolio risk is small and not monotonic. Municipal bond insurance portfolio risk is the weighted average capital charge of the insurer's municipal bond insurance portfolio. The average municipal bond insurance portfolio risk in the first bin is 11.2%, while the same quantity for the fourth bin is 9.1%. This means that the average bond in the insurance portfolio of the worst insurance companies are 2.1% more likely to default than that of the safest insurance companies. Compared to the variation in RMBS insurance portfolio risk, this difference is economically insignficant, especially considering the fact that municipal bonds rarely default. Next, there is essentially no variation in S&P credit ratings and the proportion of insured governments that have no credit rating. Lastly, these governments were located in counties that experienced similar employment growth during the crisis. The same patterns hold for Panel B. The overall balance of these observables shows that the identifying assumption plausibly holds because it does not seem to be the case that inferior governments were paired with insurers that were worse off during the crisis.

8 Government Summary Statistics

To explore the relationship between changes in insurers' health and financing friction, I begin with government entities that issued at least one insured bond package during the pre-crisis period (2006Q1 to 2007Q2). This filter aims to alleviate the concern that results presented in the next sections are driven by the difference in each government entity's financing position. I end up with 4,775 government entities and table 5 presents summary statistics of their characteristics. The summary statistics on ΔI_i show that most government entities saw the health of their group of insurance companies deteriorate during the crisis. Special-purpose governments make up 64% of the sample, which is close to the proportion of special-purpose governments that appear in the 2007 census survey. The average government entity has more than 2,700 full-time employees and more than 700 part-time employees.

Table 5 divides government entities into four bins, according to each entity's value of ΔI_i. Despite substantial variation in ΔI_i, values of other observables are well balanced across bins. Panel B explores the geographical distribution of government entities in each bin. I find that there is no obvious geographical bias across the range of ΔI_i. This balance in geographical distribution alleviates the concern that results in the following sections are driven by geographical coincidence, i.e., insurers that performed worse during the 2008-9 crisis coincidentally insured government entities that were located in regions that suffered more from the real estate downturn.

9 Bond Insurance and Financing Frictions

If the deterioration of insurers' health created significant financing frictions, it should be the case that issuers that had prior relationships with insurers that failed during the crisis were less able to issue insured and uninsured bonds. This prediction should hold because (1) less healthy insurers have less capital to insure new bonds and, (2) since insurance relationships are sticky, it is costly for governments to switch to issue insured bonds with surviving or new insurers.

Table C1 presents summary statistics on government entities' ability to issue new debt between 2008 and 2012. I separate the 4,775 governments into two groups: those that had no prior relationships with surviving insurers (AGC and FSA) and those that did not. The top portion of the table presents the cummulative probability that a government entity in each group had issued at least one insured bond with a surviving or new insurer (BHAC) by the end of each year. By 2008Q4, 9% of governments that did not have prior relationships with surviving insurers were able to issue at least one insured bond with AGC, FSA, or BHAC. This shows that only 9% of governments that had no prior relationships with surviving insurers were able to switch insurers. On the other hand, 21% of governments that had prior relationships with AGC and FSA were able to issue insured bonds. The wedge of 12% between the two group means that it was more than twice as hard for a new client to switch to one of the surviving insurers. Recall that every government entity in this sample had issued at least one insured bond between 2006Q1 and 2007Q2, hence, it is unlikely that this wedge was caused by differences in funding needs.

Fast forward to the end of 2012, I find that the wedge between the two groups is 15%. The wedge's persistence suggests that the rate at which additional governments in each group were able to issue new insured bonds in each year is comparable. The relatively parallel trend suggests that the group with no prior relationship did not manage to catch up to the other group, but also did not fall behind significantly. Therefore, any negative credit supply shock that occured in the crisis period could have had a lasting effect. The bottom portion repeats the same exercise for all bond issuances. I find a similar trend where members of the disadvantaged group were less likely to issue debt by 2008Q4 and the wedge between the two groups remains constant to the end of 2012. Overall, summary statistics in table C1 suggest that governments with no prior relationships with surviving insurers experienced some difficulty in issuing new bonds during the crisis.

9.1 Bond Issuance

To confirm that changes insurers' health matter for governments' ability to issue new debt, I first estimate variants of the following equation and results are presented in table 6.

$$Issued\,insured\,bond_i = \beta(\Delta I_i) + \boldsymbol{\gamma' x} + \kappa_s + \epsilon_i \tag{5}$$

$Issued\,insured\,bond_i$ equals 1 if issuer i issues at least one insured bond package during the crisis period (2008Q1 to 2009Q2) and zero otherwise. Column 1 shows that a one standard deviaion increase in ΔI_i raised the probability of insured bond issuance by 2.6%. With the baseline probability of insured bond issuance of 23.5%, the point estimate translates to more than a 10% increase in probability of issuance, which is economically large. In the next two columns, I instrument for ΔI_i with $AAA\,ABS\,exposure_i$ and $HQ\,CDO\,exposure_i$ respectively. I find point estimates ranging from 2.6% to 3.9%.

Next, I explore whether the financing frictions that arose from variation in insurers' health could have affected total debt issuance. Intuitively, issuers that relied on bond insurance to issue cheap debt could face substantial financing friction when bond insurance disppeared because the issuer would be forced to issue more expensive uninsured debt or would not be able to issue any debt at all because interest rates became prohibitively high.

Table 7 presents results for total bond issuance amount during the crisis. The dependent variable is the natural log of one plus the amount of bond that issuer i issued during the crisis. In column 1, a one standard deviation increase in insurers' health increased the amount of bonds issued by 7%. The instrumented coefficients in columns 2 and 3 range from 8.9% to 9.9%. Comparing these point estimates to the fact that state and local governments finance approximately 12% of their spending with new debt shows that financing frictions from ailing insurers could have caused a substantial reduction in government entities' ability to spend.

9.2 Yield Spreads

This section explores how changes in insurers' health affect government units' ability to finance themselves on the intensive margin. I compare individual bonds that these government units issued in the pre-crisis period with those that they issued in the crisis period. Specifically, I match each pre-crisis-period bond to a crisis-period bond that shares the same characteristics – amount issued, maturity, source of funds (general obligation or revenue), tax status and coupon type (fixed-rate or zero coupon). For issuers that issued more than one bond in the pre-crisis period, I keep the bond that was issued nearest to the end of the pre-crisis period. After this matching procedure, each issuer is assigned one pre-crisis period bond and one-crisis period bond. Next, I use coupon equivalent treasury yield data from Gürkaynak et al. (2007) to calculate yield spreads for each of these bonds. Lastly, I calculate the difference in the yield spreads of these two bonds and estimate the following equation.

$$\Delta Yield\, spread_i = \beta(\Delta I_i) + \boldsymbol{\gamma' x} + \kappa_s + \epsilon_i \tag{6}$$

The dependent variable is the difference between the yield spread of the bond issued during the crisis and the yield spread of a similar bond that was issued during the pre-crisis period, measured in percentage points. A positive number means that the yield spread for issuer i increased between the per-crisis period and the crisis period. Table 8 presents the regression results. First, the average government saw its yield spread increased by 2.57% during the crisis. Column 1 shows that, conditional on issuing a similar type of bond, a one standard deviation increase in insurers' health decreased the change in yield spread that a local government paid by 12 bps. The next two columns split the sample into insured bonds and uninsured bonds. Column 2 shows that a one standard deviation increase in insurers' health decreased yield spreads for insured bonds by 16 bps. Column 3 shows that the impact on uninsured bonds is smaller and not statistically different from zero. Columns 4 to 6 show the 2SLS results, which are qualitatively similar. These results show that governments associated with ailing insurers also faced significant financing frictions on the intensive margin and these effects were concentrated among insured bonds.[6]

[6] Results are qualitatively similar when I instrument ΔI_i with $HQ\,CDO\,exposure$.

10 Bond Insurance and Government Employment Growth

Section 9 shows that variation in insurers' health created significant financing frictions for related governments. In this section, I explore whether these financing frictions affected real variables such as employment growth. To do this, I match issuers from the previous section to government units that report employment numbers in the Annual Survey of Public Employment and Payroll. Out of the 4,775 issuers, I was able to match 1,272 issuers. There are 468 special-purpose governments in this matched sample.[7] When compared to the original 4,775 sample, the matched sample has a larger proportion of general government units because census surveys tend to collect data from larger government units.

Figure 4 plots median normalized full-time employment levels for governments in the highest and lowest quartiles of ΔI_i. Each observation of full-time employment level is normalized by the government's 2007Q2 full-time employment level. As expected, governments in the highest quartile of ΔI_i experienced faster full-time employment growth between 2007Q2 and 2009Q2. Furthermore, the gap between the two groups widen by the 2011Q2. However, it is clear from the picture that the two groups experienced similar macro trends that pushed the aggregate full-time employment level downward. This picture suggests that the financing friction that arose from the demise of bond insurers did not cause the general downturn in public sector employment, but made the downturn worse for some governments than others. Figure 5 repeats the exercise for part-time employment. For 2009Q2, the part-time employment gap between the two groups appears to be wider than the full-time employment gap. However, the gap shrinks as time passes. The two lines follow the same macro trend, which suggests a similar story to the full-time employment plot.

Regression analyses in this section use the growth rate of various measures of employment as dependent variables. The growth rate rate is defined as follows.

$$g_i = \frac{e_{c,i} - e_{pc,i}}{0.5 \times (e_{c,i} + e_{pc,i})} \tag{7}$$

[7] Regression sample sizes are smaller because of data limitations for calculations of lagged employment growth and inclusion of fixed effects.

This measure is a second-order approximation of the log difference in growth rates around zero. It is bounded in the range [-2, 2]. $e_{pc,i}$ is the employment quantity of interest during the pre-crisis period, e.g., the number of full-time employees that a government unit had. The pre-crisis quantity is observed at the end of 2007Q2. $e_{c,i}$ is the employment quantity of interest during the crisis period. The crisis quantity is observed at the end of 2009Q2. The Annual Survey of Public Employment and Payroll reports full-time and part-time employment numbers for each government unit that it surveys. This feature allows me to investigate the heterogeneous effect of changes in insurers' health on each type of worker.

10.1 Full-time Employment Growth

Table 9 presents results on changes insurers' health and full-time employment growth. It is interesting to note that the average government unit in my sample experienced a small and positive employment growth between 2007Q2 and 2009Q2. Tables 9 and 10 show that the average government unit experienced full-time employment growth of 0.62% and part-time employment growth of 0.79%.[8] This trend is starkly different from the average 9.2% decrease in employment in the group of non-financial firms that Chodorow-Reich (2013) studies.

Column 1 of table 9 shows that a one standard deviation increase in insurers' health increased full-time employment growth by 0.93%. The instrumented results in columns 2 and 3 yield point estimates between 0.79% to 0.84%.

10.2 Part-time Employment Growth

The public sector labor force has a significant proportion of part-time employees. The 2007 Annual Survey of Public Employment and Payroll shows that approximately 24% of state and local government employees are part-time workers. This proportion is similar to the ratio of part-time employees in the main sample. Since part-time workers are relatively easier to hire and fire than full-time workers, this group of workers may have been more vulnerable to effects of financing

[8] The difference between private and public sector employment growth may be the result of the American Recovery and Reinvestment Act of 2009. However, it is beyond the scope of this paper to test this hypothesis.

shortfalls.

Table 10 presents results on changes in insurers' health and part-time employment growth. The impact that variation in insurers' health had on part-time employment growth was large. Column 1 shows that, for the average government unit, a one standard deviation increase in insurers' health increased part-time employment growth by 3.69%. The instrumented point estimates in the remaining columns range from 3.29% to 4.45%. The difference between the impact that changes insurers' health had on full-time and part-time employment growth shows that the financial crisis may have exacerbated income inequality between these two groups. Part-time workers, on average, earned less than full-time workers and they suffered more from this particular financing friction (Hirsch, 2005).

10.3 Long-run Effects

Table 11 presents OLS regression results for the effect that changes in insurers' health have on employment growth at different time horizons. Columns 1 to 3 present results for full-time employment growth. First, note that full-time employment growth in the public sector increased slightly from 2007Q2 to 2009Q2, but started to fall in 2010Q2 and 2011Q2. The same pattern holds for part-time employment growth. Column 1 repeats the first regression from table 9 as a benchmark. The coefficient on ΔI_i increases monotonically as the time horizon increases from 2009Q2 to 2011Q2. These results show that local governments that received a negative shock from ailing insurers during the crisis never caught up to the other group and even fell behind slightly. Columns 3 to 6 present results for part-time employment growth. The estimates in these columns show the opposite trend. The effect attenuates as the time horizon increases from 2009Q2 to 2011Q2. These results suggest that what the disadvantaged group lost in full-time employees, they made up with part-time employees. In other words, there may have been a substitution between full-time and part-time workers in the years that follow the 2008-9 financial crisis.[9]

[9] 2SLS results are qualitatively and quantitatively similar.

10.4 Heterogeneous Effects

To explore heterogeneous effects of financing friction on employment growth, I split the sample into two groups: general and special-purpose governments. General governments are state, county, and city governments, while special-purpose governments are public entities such as school districts and utility authorities. General governments tend to be larger and have a diversified set of revenue sources, while special-purpose governments are smaller and often depend on a single specialized source of revenue. Special-purpose governments also report their financial data less often than general governments and so information production on special-purpose government debt may be more expensive. Overall, information asymmetry should be higher for special-purpose governments because of higher credit risk and opacity. These features predict that financing frictions from ailing insurers may have larger effects on special-purpose governments because it is more costly for them to switch insurers and issue uninsured debt. Recall that results from section 5 show that insurance relationships are stickier for special-purpose governments.[10]

Table 12 presents OLS regression results for the effect that changes in insurers' health have on employment growth at general and special-purpose governments. Columns 1 presents results on full-time employment growth at general governments and finds that financing frictions from ailing insurers essentially have no impact on this group of governments. On the other hand, column 2 shows that a one standard deviation increase in ΔI_i increased full-time employment growth at special-purpose governments by 1.63%. Columns 3 and 4 repeat the same exercise for part-time employment growth and find the same pattern. Column 3 shows that financing frictions from ailing insurers have no significant impact on part-time employment growth at general governments. Column 4 finds that a one standard deviation increase in ΔI_i increases part-time employment growth at special-purpose governments by 5.44%.[11]

[10] Special-purpose governments account for 21% of total expenditure and 28% of total debt issuance in 2007.
[11] 2SLS results are qualitatively and quantitatively similar.

10.5 Placebo Employment Growth Regressions

Tables D1 and D2 show regression results for the effect of changes in insurers' health on employment growth in the pre-crisis period. In particular, I regress full-time and part-time employment growth between 2006Q2 and 2007Q2 on ΔI_i and the same set of covariates from previous employment growth regressions.[12] A positive coefficient on ΔI_i could mean that governments associated with healthier insurers were on a higher secular employment growth path prior to the crisis. A negative coefficient on ΔI_i could mean that this particular group of governments experienced abnormal cyclical employment growth patterns. Results in tables D1 and D2 show that ΔI_i has no predictive power for neither full-time nor part-time employment growth in the pre-crisis period.[13] Therefore, governments attached to worse insurers and had worse employment outcomes appear to be similar to other governments in the pre-crisis period.

10.6 Aggregate Effects in the Sample

This section uses some additional assumptions to quantify aggregate employment effects that bond insurance-related financing friction has on local governments in the sample. The following partial equilibrium calibration exercise uses two assumptions.

1. The total employment effect equals the sum of the direct employment effects measured at each government entity.

2. The growth rate for each insurer's municipal bond insurance business between the pre-crisis period and the crisis period is zero. In other words, if the crisis did not occur, each insurer would have written the same amount of municipal bond insurance as it did in the pre-crisis period. Quantitatively, this means that ΔI_i equals zero instead of a negative number.

For this exercise, I use estimates from table E1. This table presents 2SLS regression results for columns 2 and 4 from table 12 with the unnormalized version of ΔI_i. Results from table 12

[12] Lagged employment growth rate is adjusted such that the end period is 2006Q2.

[13] The number of observations in these tables are smaller because fewer governments appeared in the census employment survey in years prior to 2007. This data limitation reduced the number of governments that I could calculate lagged employment growth for.

shows that the financing friction effects only had significant impact on speical-purpose governments. Therefore, I assume that the marginal effect of ΔI_i on general governments is zero and drop these governments from the analysis.

To begin, I define the counterfactual employment growth rate if government i's syndicate of bond insurers had experienced no contraction in its municipal bond insurance business as the following.

$$g_i^* = \hat{g}_i + \beta(-\Delta I_i) \tag{8}$$

$\hat{g}_i$ is the predicted employment growth rate from table E1. β is the instrumented point estimate from table E1. The appropriate β values are 1.17% for full-time employment growth and 3.75% for part-time employment growth. The equation adds $\hat{g}_i$ to the product of β and negative ΔI_i because every government in the sample has a negative ΔI_i value.

Next, I define $Q(x)$ as the mapping from symmetric employment growth rates to the end-period level, holding the initial employment level fixed.

$$Q(x) = \frac{1 + 0.5x}{1 - 0.5x} e_{pc,i} \tag{9}$$

The counterfactual crisis period employment level becomes

$$y_{c,i}^* = Q(g_i^*) \tag{10}$$

And the fitted value employment level is

$$\hat{y}_{c,i} = Q(\hat{g}_i) \tag{11}$$

Then, the total workers lost from insurance financing friction is

$$\sum y^*_{c,i} - \hat{y}_{c,i} \tag{12}$$

The partial equilibrium calibration exercise shows that, between 2007Q2 and 2009Q2, special-purpose governments in the sample lost approximately 38,000 full-time employees and 58,500 part-time employees to bond insurance financing frictions. In 2007Q2, these special-purpose governments employed approximately 1 million full-time employees and 400,000 part-time employees, which implies that the estimated aggregate effect is economically large, especially for part-time employees. Aggregate employment growth between 2007Q2 and 2009Q2 would have been close to 4% higher for full-time employees and 15% higher for part-time employees.

11 Conclusion

This article shows that financing frictions that stemmed from the demise of bond insurance companies during the financial crisis had significant real effects on local special-purpose governments. Before the financial crisis, bond insurance was an important resource for municipal issuers becuase it allowed them to issue debt more cheaply. When bond insurers faltered during the financial crisis, governments faced significant financing frictions, which harmed their ability to hire employees. In addition, these effects lasted for at least two more years after the crisis had ended. This article outlines the mechanism in which problems in the RMBS markets affected local governments' operations through a financing friction channel. Futhermore, results from this article offer a potential explanation for why local governments were not able to issue more debt and increase public spending during the financial crisis.

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Figure 1: Annual Municipal Bond Issuance

This chart shows annual U.S. municipal bond issuance in billions of U.S. dollars. The blue bars capture total issuance volume. The red bars capture insured issue volume.

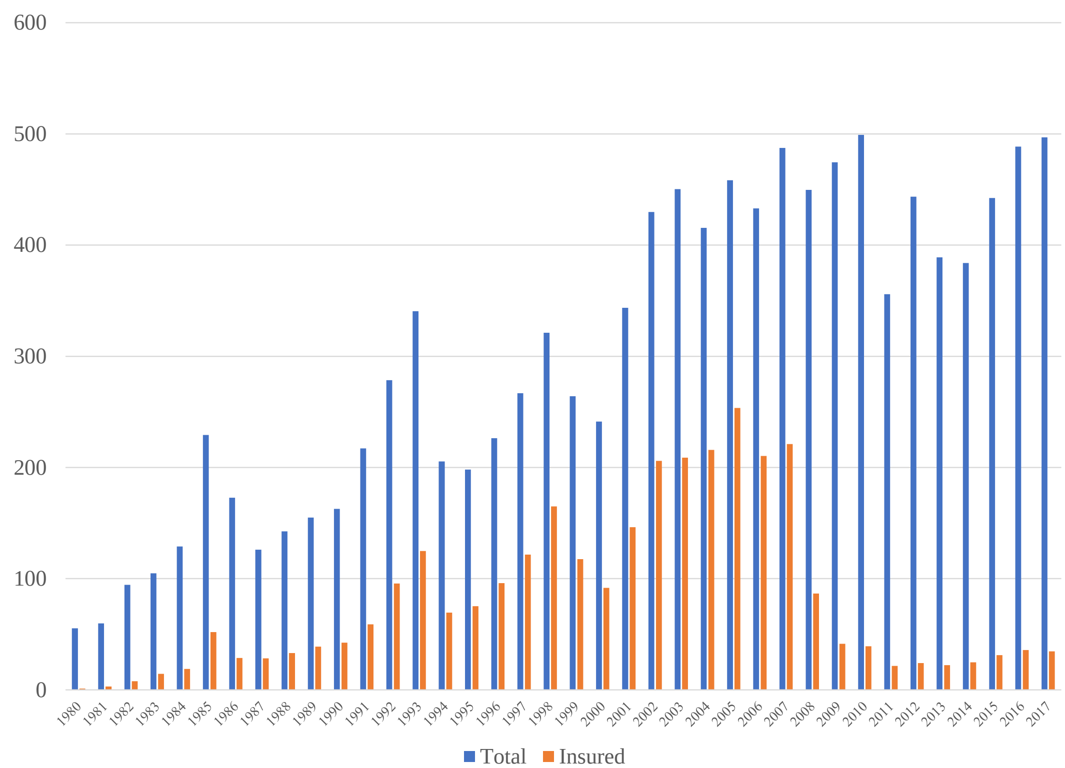

Figure 2: What Percentage of Governments Issued Debt Each Year?

This chart shows the percentage of government entities that issued a positive amount of debt in each year form 1980 to 2007. The sample includes all government entities that responded to the Annual Survey of State and Local Government Finances in each year from 1980 to 2007.

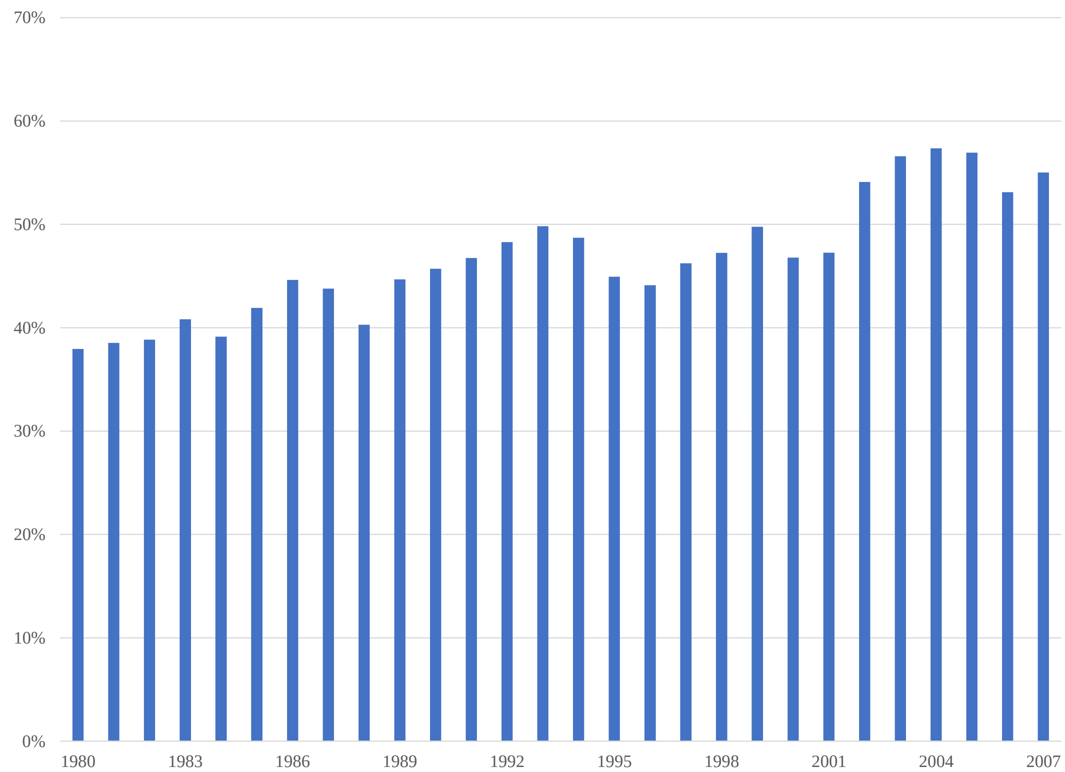

Figure 3: Pre-Crisis Annual Debt Issuance as Percentage of Total Expenditure

This chart shows total amount of debt issued as percentage of total expenditure. The sample includes all government entities that responded to the Annual Survey of State and Local Government Finances in each year from 1980 to 2007.

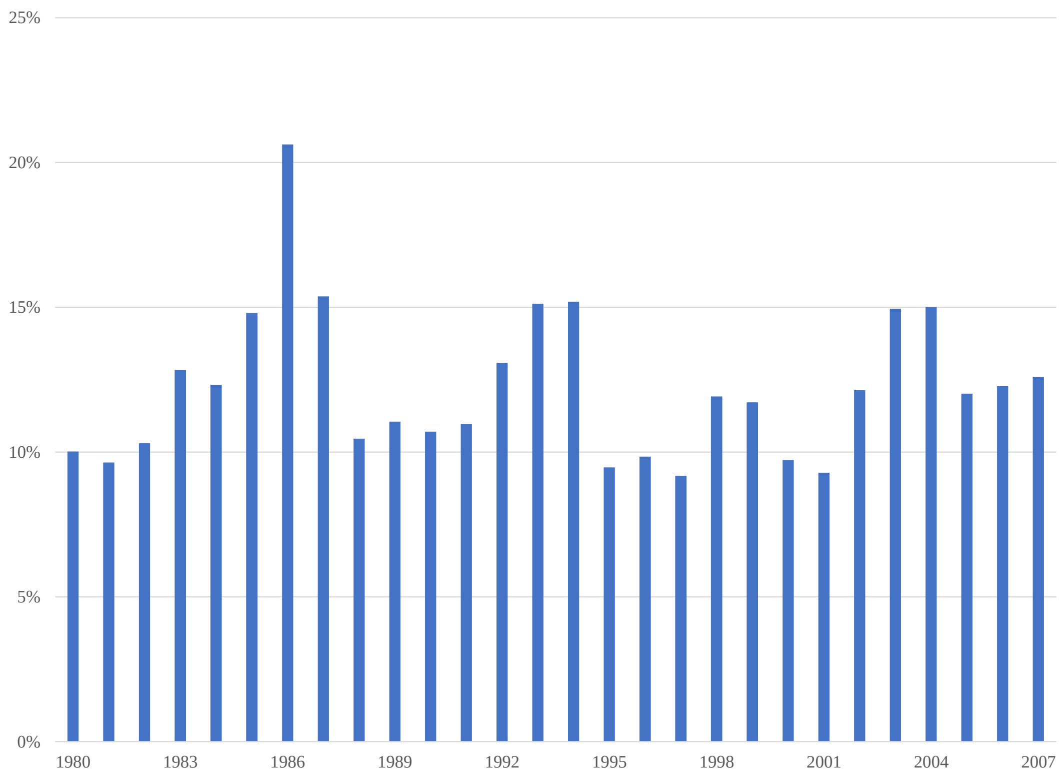

Figure 4: Full-Time Employment Level by Changes in Insurers' Health

This chart shows median full-time employment levels from 2007Q2 to 2011Q2 for government entities in the highest quartile of changes in insurers' health and those in the lowest quartile. Each data point is normalized by the entity's 2007Q2 employment level.

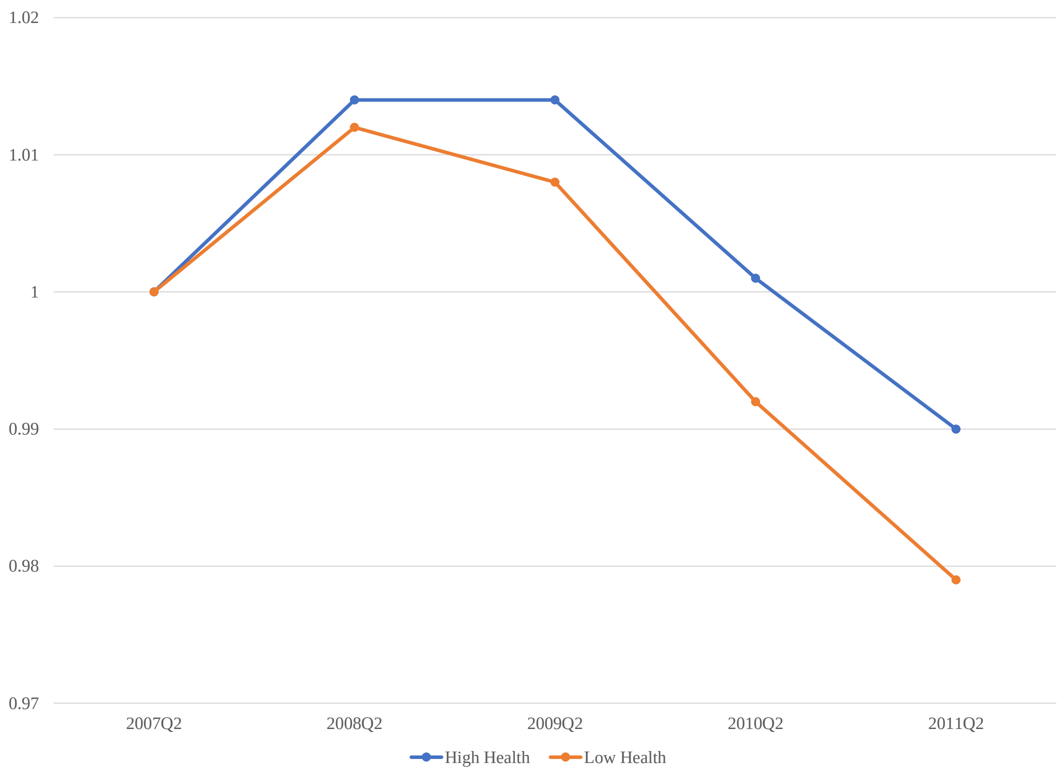

Figure 5: Part-Time Employment Level by Changes in Insurers' Health

This chart shows median part-time employment levels from 2007Q2 to 2011Q2 for government entities in the highest quartile of changes in insurers' health and those in the lowest quartile. Each data point is normalized by the entity's 2007Q2 employment level.

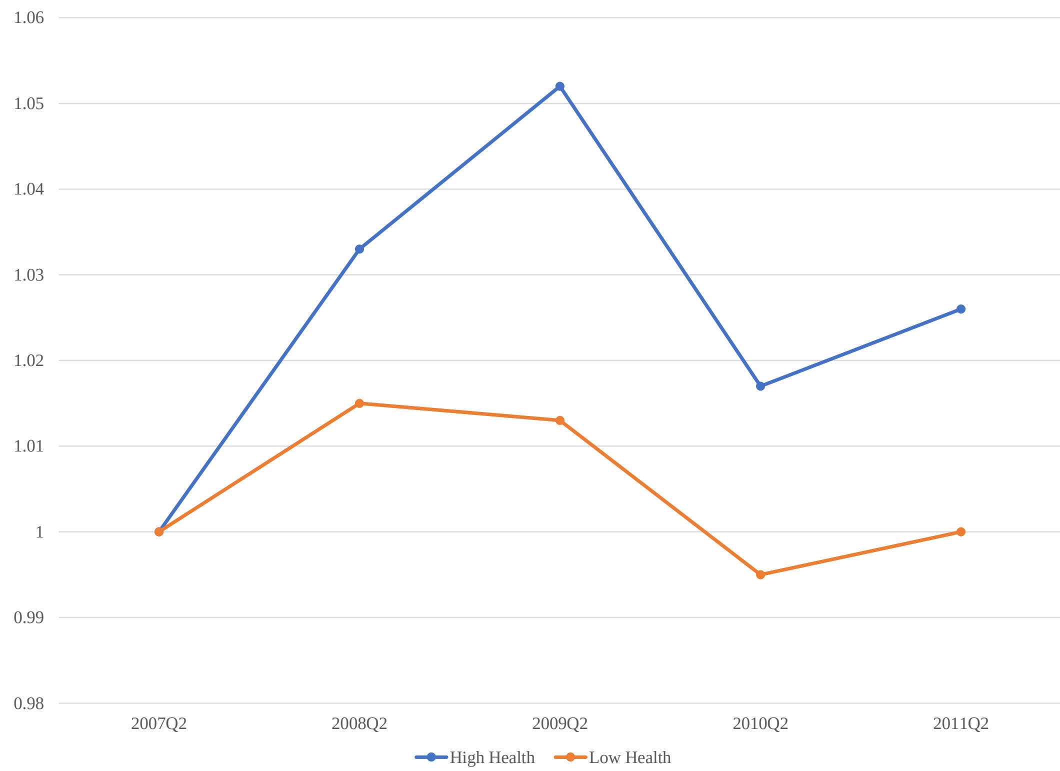

Table 1: Information Asymmetry and Bond Insurance Use

This table presents summary statistics on issuers' characteristics and insurance use on the intensive margin. Each issuer is sorted into four buckets according to their insurance ratio. Insurance ratio is the percentage of insured municipal bonds that each issuer issued between 1980 and 2007. An insurance ratio of 1 means that, between 1980 and 2007, the issuer had issued only insured bonds. Total revenue is the issuer's total revenue in 1982 in USD millions. State government equals 1 if the issuer is a state government. County government equals 1 if the issuer is a county government. City government equals 1 if the government is a city government. Special district government equals 1 if the government is none of the above. Not rated equals 1 if the government has no credit rating from S&P or Moody's when it first issued a bond between 1980 and 2007.

Insurance ratio	$\mathbf{0 < x \leq 0.25}$	$\mathbf{0.25 < x \leq 0.5}$	$\mathbf{0.5 < x \leq 0.75}$	$\mathbf{0.75 < x \leq 1}$
Sample size	3,566	4,953	4,475	8,161
Size				
Total revenue ($ mil)	129.3	27.5	22.1	11.3
Type				
State government	0.8%	0.1%	0.1%	0.0%
County government	10.2%	8.1%	7.4%	4.6%
City government	36.8%	29.4%	22.6%	14.6%
Special district government	52.1%	62.3%	69.9%	80.8%
Credit rating				
Not rated	52.3%	59.8%	63.2%	77.9%

Table 2: Pre-Crisis Insurance Relationships

This table presents OLS regression results for variants of equation 1. The unit of observation is an insured bond package. For each insured bond package that was issued, the data set contains one observation for each potential insurer, where a potential insurer is an insurer that was active in the municipal insurance business in that year. The dependent variable is $Current\,Insurer$, which equals 1 if insurance company j serves as the insurer for the current bond package and zero otherwise. $Previous\,Insurer$ equals 1 if insurance company j insured issuer i's previous bond package and zero otherwise. The sample contains insured bond packages issued between 1980Q1 and 2007Q4. Standard errors are clustered at the issuer-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)
Previous insurer	0.319***	0.286***
	[0.007]	[0.007]
Previous insurer x General government	-0.083***	-0.081***
	[0.006]	[0.007]
Previous insurer x General obligation	-0.039***	-0.054***
	[0.007]	[0.007]
Previous insurer x Rated issuer	-0.071***	-0.063***
	[0.006]	[0.006]
General government	0.006***	
	[0.001]	
General obligation	0.007***	
	[0.001]	
Rated Issuer	0.008***	
	[0.001]	
Insurer FE	Y	-
Year FE	Y	-
Insurer x Year FE	-	Y
Insurer x State FE	-	Y
Insurer x Government type FE	-	Y
Insurer x Bond type FE	-	Y
Insurer x Rated Issuer FE	-	Y
Observations	667,923	667,878
R-squared	0.154	0.197

Table 3: Municipal Bond Insurers During the Financial Crisis

Panel A summarizes municipal bond insurance volume by insurer-half-year. Panel B summarizes S&P financial enhancement (FE) rating by insurer-half-year. This table excludes insurers that entered the market during the crisis. A rating of 'R' means that the insurance company is being reviewed by regulators.

Panel A: Volume ($ billions)	**06H2**	**07H1**	**07H2**	**08H1**	**08H2**	**09H1**
ACA	0.27	0.49	0.16	0.00	0.00	0.00
AGC	1.55	1.45	3.62	21.34	9.94	20.40
AMBAC	24.13	30.00	21.50	0.74	0.00	0.00
CIFG	5.30	6.19	1.11	0.04	0.00	0.00
FGIC	18.46	22.96	10.00	0.24	0.00	0.00
FSA	28.46	28.69	26.07	38.59	5.49	3.00
MBIA	27.28	27.41	24.06	2.65	0.00	0.00
RADIAN	1.63	1.44	0.98	0.32	0.00	0.00
XLCA	6.68	7.14	7.78	0.03	0.00	0.00

Panel B: S&P FE Rating	**06H2**	**07H1**	**07H2**	**08H1**	**08H2**	**09H1**
ACA	A	A	CCC	CCC	NR	NR
AGC	AAA	AAA	AAA	AAA	AAA	AAA
AMBAC	AAA	AAA	AAA	AA	A	BBB
CIFG	AAA	AAA	AAA	A-	B	CC
FGIC	AAA	AAA	AAA	BB	CCC	NR
FSA	AAA	AAA	AAA	AAA	AAA	AAA
MBIA	AAA	AAA	AAA	AA	AA	BBB
RADIAN	AA	AA	AA	A	BBB+	BBB-
XLCA	AAA	AAA	AAA	BBB-	B	R

Table 4: First Stage - Instruments and Changes in Insurers' Health

This table presents first stage OLS regression results for the correlation between change in insurers' health, ΔI_i, and various instruments. The unit of observation is a government unit. The dependent variable is ΔI_i. $AAA\,RMBS\,exposure_i$ is the issuer's weighted average exposure to AAA RMBS through its insurers' relationships. $HQ\,CDO\,exposure_i$ is the issuer's weighted average exposure to high quality CDO through its insurers' relationships. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)
AAA RMBS exposure	11.673***	
	[0.297]	
HQ CDO exposure		3.728***
		[0.343]
Not rated	0.042	-0.067*
	[0.026]	[0.038]
Multiple insurers	-0.068	0.021
	[0.122]	[0.311]
Insurance ratio	0.022	0.094
	[0.073]	[0.092]
Debt due in crisis	-0.022	0.018
	[0.017]	[0.025]
Ln(total debt issued)	0.010	0.022
	[0.015]	[0.019]
Special government	0.053*	-0.068*
	[0.030]	[0.036]
Observations	4,773	4,773
R-squared	0.858	0.826

Table 5: Government Unit Summary Statistics

This table presents summary statistics on government-level variables. All variables are winsorized at the 1st and 99th percentile. The table first sorts government entities into quartiles according to their ΔI_i values. The last two columns present the sample mean and standard deviation for each variable. Panel A presents summary statistics on governments' observatble characteristics and panel B presents their geographical distribution.

Panel A	**ΔI_i Quartile**				**Sample**	
Observables	**1st**	**2nd**	**3rd**	**4th**	**Mean**	**S.D.**
Change in insurers' health (ΔI_i)	-5.027	-4.053	-3.270	-1.609	-3.492	1.355
S&P rating	17.566	17.991	17.821	17.539	17.674	1.883
Special government	0.630	0.550	0.613	0.767	0.640	0.480
Not rated	0.385	0.308	0.325	0.367	0.346	0.476
Debt due in crisis	0.334	0.507	0.438	0.348	0.407	0.491
Insurance ratio	0.645	0.640	0.620	0.629	0.634	0.271
Deals per year	0.862	1.470	1.214	0.865	1.104	1.381
Total debt issued ($ billions)	0.422	1.388	0.968	0.384	0.792	2.432
Total revenue ($ billions)	0.191	0.523	0.372	0.200	0.321	0.955
Full-time employees ('000s)	1.529	3.822	2.855	2.401	2.779	7.049
Part-time employees ('000s)	0.428	0.955	0.835	0.783	0.775	2.329
County employment growth (07:09)	-0.041	-0.040	-0.037	-0.040	-0.039	0.034

Panel B	**ΔI_i Quartile**				**Sample**	
Census Division	**1st**	**2nd**	**3rd**	**4th**	**Mean**	**S.D.**
New England (1)	4.1%	6.3%	5.7%	3.9%	5.0%	21.8%
Middle Atlantic (2)	18.3%	21.5%	19.8%	16.5%	19.0%	39.2%
East North Central (3)	25.1%	17.9%	20.1%	25.0%	22.0%	41.5%
West North Central (4)	8.1%	6.1%	9.0%	11.7%	8.7%	28.2%
South Atlantic (5)	6.7%	9.0%	8.4%	5.1%	7.3%	26.1%
East South Central (6)	5.7%	5.5%	5.3%	3.4%	5.0%	21.8%
West South Central (7)	10.7%	11.4%	9.7%	12.4%	11.1%	31.4%
Mountain (8)	5.6%	6.0%	5.8%	5.1%	5.6%	23.1%
Pacific (9)	15.4%	16.3%	15.8%	16.9%	16.1%	36.8%

Table 6: Insurers' Health and Insured Bond Issuance

This table presents OLS regression and 2SLS results for variants of equation 5. The unit of observation is a government unit. The dependent variable is $Issued\,insured\,bond_i$, which equals 1 if issuer i issued at least one insured bond package during the crisis period and zero otherwise. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$ and $HQ\,CDO\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

Model	(1) OLS	(2) 2SLS	(3) 2SLS
ΔI_i	0.026***	0.026***	0.039***
	[0.006]	[0.007]	[0.010]
Not rated	0.015	0.015	0.016
	[0.011]	[0.011]	[0.011]
Multiple insurers	0.076***	0.076***	0.077***
	[0.022]	[0.022]	[0.022]
Insurance ratio	0.214***	0.214***	0.217***
	[0.034]	[0.034]	[0.035]
Debt due in crisis	0.061***	0.061***	0.061***
	[0.011]	[0.011]	[0.011]
Ln(total debt issued)	0.046***	0.046***	0.047***
	[0.007]	[0.007]	[0.007]
Special government	-0.055***	-0.055***	-0.059***
	[0.017]	[0.018]	[0.017]
Average outcome	0.235	0.235	0.235
Instrument	-	RMBS	CDO
First stage F-stat	-	1541.981	118.412
Observations	4,773	4,773	4,773
R-squared	0.103	0.103	0.102

Table 7: Insurers' Health and Total Bond Issuance

This table presents OLS and 2SLS regression results for variants of equation 5. The unit of observation is a government unit. The dependent variable is $Ln(1 + issued\,amount)_i$, which is the log of one plus the amount of bonds that issuer i issued during the crisis. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$ and $HQ\,CDO\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)	(3)
Model	OLS	2SLS	2SLS
ΔI_i	0.070***	0.089***	0.099***
	[0.024]	[0.025]	[0.027]
Not rated	0.026	0.026	0.027
	[0.053]	[0.053]	[0.053]
Multiple insurers	-0.181**	-0.179**	-0.178**
	[0.089]	[0.088]	[0.089]
Insurance ratio	0.203*	0.208*	0.211*
	[0.111]	[0.111]	[0.110]
Debt due in crisis	0.346***	0.346***	0.345***
	[0.045]	[0.045]	[0.045]
Ln(total debt issued)	0.821***	0.822***	0.823***
	[0.052]	[0.052]	[0.051]
Special government	-0.080	-0.086	-0.090*
	[0.054]	[0.055]	[0.052]
Instrument	-	RMBS	CDO
First stage F-stat	-	1541.981	118.412
Observations	4,773	4,773	4,773
R-squared	0.461	0.461	0.461

Table 8: Insurers' Health and Changes in Yield Spreads

This table presents OLS and 2SLS regression results for variants of equation 6. The unit of observation is a bond. The dependent variable is $\Delta Yield\,spread_i$, which is the difference in yield spreads of the bond issued during the crisis and a similar bond that was issued in the pre-crisis period, measured in percentage points. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)	(3)	(4)	(5)	(6)
Model	OLS	OLS	OLS	2SLS	2SLS	2SLS
ΔI_i	-0.12*	-0.16*	-0.06	-0.15**	-0.16*	-0.11
	[0.06]	[0.08]	[0.10]	[0.07]	[0.08]	[0.10]
Not rated	0.06	-0.11	0.19	0.06	-0.11	0.19
	[0.14]	[0.18]	[0.22]	[0.14]	[0.18]	[0.22]
Multiple insurers	-0.15	-0.56***	0.16	-0.17	-0.56***	0.13
	[0.17]	[0.14]	[0.15]	[0.18]	[0.15]	[0.16]
Insurance ratio	-0.05	-0.22	0.55	-0.05	-0.22	0.52
	[0.25]	[0.38]	[0.73]	[0.25]	[0.38]	[0.73]
Debt due in crisis	0.06	0.12	-0.21	0.06	0.12	-0.24
	[0.12]	[0.15]	[0.34]	[0.12]	[0.15]	[0.34]
Ln(total debt issued)	-0.04	-0.10*	0.00	-0.04	-0.10*	0.00
	[0.04]	[0.06]	[0.06]	[0.04]	[0.06]	[0.06]
Special government	0.19**	0.12	0.31	0.20**	0.12	0.32
	[0.10]	[0.17]	[0.21]	[0.09]	[0.17]	[0.21]
Pre-crisis spread	-0.23***	-0.20*	-0.26***	-0.23***	-0.20*	-0.26***
	[0.03]	[0.12]	[0.05]	[0.03]	[0.12]	[0.05]
Insured	0.24			0.24		
	[0.15]			[0.15]		
Sample	All	Insured	Uninsured	All	Insured	Uninsured
Instrument	-	-	-	RMBS	RMBS	RMBS
First stage F-stat	-	-	-	1461.55	927.88	621.61
Observations	296	182	102	296	182	102
R-squared	0.33	0.23	0.59	0.33	0.23	0.59

Table 9: Insurers' Health and Full-time Employment Growth

This table presents OLS and 2SLS regression results for variants of equation 5. The unit of observation is a government unit. The dependent variable is g_i^f, which is issuer i's full-time employment growth rate between 2007Q2 and 2009Q2 multiplied by 100. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$ and $HQ\,CDO\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

Model	(1) OLS	(2) 2SLS	(3) 2SLS
ΔI_i	0.93***	0.84**	0.79*
	[0.34]	[0.35]	[0.41]
Not rated	-0.54	-0.54	-0.54
	[1.05]	[1.05]	[1.05]
Multiple insurers	0.62	0.62	0.62
	[1.06]	[1.08]	[1.09]
Insurance ratio	-0.48	-0.49	-0.50
	[2.52]	[2.51]	[2.50]
Debt due in crisis	0.21	0.21	0.21
	[0.61]	[0.61]	[0.61]
Ln(total debt issued)	0.12	0.10	0.10
	[0.33]	[0.33]	[0.33]
Special government	0.91	0.93	0.94
	[0.68]	[0.68]	[0.70]
Ln(FT employment)	0.02	0.03	0.04
	[0.39]	[0.38]	[0.38]
County emp. growth	0.04	0.04	0.04
	[0.23]	[0.23]	[0.22]
Lagged FT emp. growth	-0.09***	-0.09***	-0.09***
	[0.03]	[0.03]	[0.03]
Average outcome	0.62	0.62	0.62
Instrument	-	RMBS	CDO
First stage F-stat	-	4657.50	384.25
Observations	968	968	968
R-squared	0.14	0.14	0.14

Table 10: Insurers' Health and Part-time Employment Growth

This table presents OLS and 2SLS regression results for variants of equation 5. The unit of observation is a government entity. The dependent variable is g_i^p, which is issuer i's part-time employment growth rate between 2007Q2 and 2009Q2 multiplied by 100. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$ and $HQ\,CDO\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

Model	(1) OLS	(2) 2SLS	(3) 2SLS
ΔI_i	3.69***	3.29***	4.45***
	[1.11]	[1.15]	[1.29]
Not rated	-3.67	-3.69	-3.65
	[2.62]	[2.61]	[2.63]
Multiple insurers	-6.50**	-6.53**	-6.44**
	[3.04]	[3.18]	[2.80]
Insurance ratio	6.73	6.68	6.83
	[7.44]	[7.45]	[7.45]
Debt due in crisis	2.81	2.82	2.80
	[2.79]	[2.80]	[2.79]
Ln(total debt issued)	1.45	1.42	1.51
	[1.21]	[1.21]	[1.21]
Special government	-5.15*	-5.06*	-5.32*
	[2.78]	[2.77]	[2.77]
Ln(PT employment)	-1.24	-1.21	-1.30
	[1.51]	[1.51]	[1.50]
County emp. growth	0.37	0.36	0.40
	[0.51]	[0.51]	[0.51]
Lagged PT emp. growth	-0.23***	-0.23***	-0.22***
	[0.03]	[0.03]	[0.03]
Average outcome	0.47	0.47	0.47
Instrument	-	RMBS	CDO
First stage F-stat	-	4775.07	386.08
Observations	965	965	965
R-squared	0.17	0.17	0.17

Table 11: Long-run Effects of Insurers' Health on Employment Growth

This table presents OLS regression results for variants of equation 5. The unit of observation is a government entity. The dependent variable for columns 1 to 3 is issuer i's full-time employment growth rate between 2007Q2 and 2009Q2, 2010Q2, and 2011Q2, respectively, multiplied by 100. The dependent variable for columns 3 to 6 is issuer i's part-time employment growth rate between 2007Q2 and 2009Q2, 2010Q2, and 2011Q2, respectively, multiplied by 100. ΔI_i is normalized to have its standard deviation equal to 1. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Full-time employment growth			Part-time employment growth		
End period:	2009Q2	2010Q2	2011Q2	2009Q2	2010Q2	2011Q2
ΔI_i	0.93***	1.19**	1.37**	3.69***	3.13**	2.10
	[0.34]	[0.45]	[0.56]	[1.11]	[1.37]	[1.94]
Not rated	-0.54	-0.72	-0.18	-3.67	-0.11	1.53
	[1.05]	[1.26]	[1.36]	[2.62]	[3.72]	[4.30]
Multiple insurers	0.62	-0.73	0.42	-6.50**	-4.37	-7.39
	[1.06]	[1.41]	[1.27]	[3.04]	[5.07]	[6.71]
Insurance ratio	-0.48	1.66	-0.72	6.73	1.40	-0.95
	[2.52]	[2.92]	[3.35]	[7.44]	[8.18]	[7.88]
Debt due in crisis	0.21	-0.07	-0.63	2.81	-0.94	0.52
	[0.61]	[0.66]	[0.87]	[2.79]	[3.85]	[4.24]
Ln(total debt issued)	0.12	0.45	0.55	1.45	0.16	-0.20
	[0.33]	[0.36]	[0.40]	[1.21]	[1.63]	[2.03]
Special government	0.91	1.49	2.51**	-5.15*	-8.05**	-6.99*
	[0.68]	[0.90]	[1.09]	[2.78]	[3.31]	[3.81]
County emp. growth	0.02	-0.57	-0.98*			
	[0.39]	[0.44]	[0.54]			
Ln(FT employment)	0.04	0.16	0.21	0.37	0.39	-0.33
	[0.23]	[0.27]	[0.29]	[0.51]	[0.47]	[0.72]
Lagged FT emp. growth	-0.09***	-0.10***	-0.12***			
	[0.03]	[0.03]	[0.04]			
Ln(PT employment)				-1.24	-1.30	-1.30
				[1.51]	[1.65]	[1.63]
Lagged PT emp. growth				-0.23***	-0.29***	-0.27***
				[0.03]	[0.05]	[0.05]
Average outcome	0.62	-1.07	-2.75	0.47	-2.64	-3.94
Observations	968	968	967	965	965	964
R-squared	0.14	0.14	0.16	0.17	0.20	0.19

Table 12: Heterogeneous Effects of Insurers' Health on Employment Growth

This table presents OLS regression results for variants of equation 5. The unit of observation is a government entity. The sample for columns 1 and 3 is composed of state, county and city governments. The sample for columns 2 and 4 is composed of special-purpose governments. The dependent variable for columns 1 and 2 is g_i^f, which is issuer i's full-time employment growth rate between 2007Q2 and 2009Q2 multiplied by 100. The dependent variable for columns 3 and 4 is g_i^p, which is issuer i's part-time employment growth rate between 2007Q2 and 2009Q2 multiplied by 100. ΔI_i is normalized to have its standard deviation equal to 1. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)	(3)	(4)
Dependent variable:	Full-time emp. growth		Part-time emp. growth	
ΔI_i	0.06	1.63**	0.81	5.44***
	[0.36]	[0.62]	[1.50]	[1.71]
Not rated	0.63	-2.70	-3.32	-4.26
	[0.60]	[2.07]	[2.83]	[8.34]
Multiple insurers	-1.14	0.16	-9.68***	-12.34**
	[0.96]	[1.72]	[3.18]	[5.42]
Insurance ratio	1.57	-1.39	9.73	3.46
	[1.76]	[4.15]	[7.34]	[14.11]
Debt due in crisis	-0.55	1.32	3.88	2.28
	[0.46]	[1.53]	[3.35]	[5.51]
Ln(total debt issued)	0.72*	-0.11	2.77*	2.80
	[0.41]	[0.64]	[1.52]	[2.51]
County emp. growth	0.37***	-0.34	0.74	1.11
	[0.13]	[0.29]	[0.45]	[1.14]
Ln(FT employment)	-0.66	0.56		
	[0.43]	[0.83]		
Lagged FT emp. growth	-0.05	-0.16***		
	[0.03]	[0.04]		
Ln(PT employment)			-7.37***	3.03
			[1.69]	[2.68]
Lagged PT emp. growth			-0.17***	-0.27***
			[0.05]	[0.08]
Sample	General	Special	General	Special
Observations	590	373	588	372
R-squared	0.21	0.28	0.22	0.25

A Variable Definition

Previous insurer - Equals 1 if the insurance company insured the issuer's previous bond package and zero otherwise.

ΔI_i - Proxy for change in insurers' health. Please refer to section 7.2 for detailed definition.

Special purpose government - Equals 1 if the issuer is not a state, county, or city government and zero otherwise.

Insurance ratio - Percentage of insured debt issued by the government unit before the crisis.

Not rated - Equals 1 if the issuer does not have a rating from S&P or Moody's and zero otherwise.

Multiple insurers - Equals 1 if the issuer is related to multiple insurers and zero otherwise.

Debt due in crisis - Equals 1 if the issuer had at least 1 bond that was due during the crisis and zero otherwise.

Deals per year - Number of bond packages issued between the government's first year of issuance and 2007.

Total debt issued - The total dollar amount of debt that the government issued before the crisis in 2007 USD billions.

Total revenue - The government's total revenue for 2007 fiscal year in 2007 USD billions.

Full-time workers - Number of full-time workers that the government unit employed at the end of 2007Q2, in thousands.

Part-time workers - Number of part-time workers that the government unit employed at the end of 2007Q2, in thousands.

Ln(total debt issued) - The natural log of the total dollar amount of debt that the government issued before the crisis.

Pre-crisis spread - Bond yield spread in the pre-crisis period.

Insured - Equals 1 if the bond is insured and zero otherwise.

County emp. growth - Employment growth rate between 2007 and 2009 of the county where the government unit was located,

Ln(FT employment) - The natural log of the number of full-time employees that the issuer employed at the end of 2007Q2.

Lagged FT emp. growth - Government's full-time employment growth rate between 2002Q2 and 2007Q2.

Ln(PT employment) - The natural log of the number of part-time employees that the issuer employed at the end of 2007Q2.

Lagged PT emp. growth - Government's part-time employment growth rate between 2002Q2 and 2007Q2.

Muni insurance portfolio risk - Insurer's municipal bond insurance portfolio's weighted average capital charge.

B Government Characteristics by Instrument Values

Table B1: Government Characteristics by Instrument Values

This table presents summary statistics on observable characteristics of government entities in the sample sorted by each instrument. Each of the first four columns presents the mean of each variable within the quartile group. The last column presents the standard deviation of each variable within the whole sample. Muni insurance portfolio risk is the insurer's weighted average capital charge of its municipal bond insurance portfolio. All variables are winsorized at the 1st and 99th percentile.

Panel A	**AAA RMBS Exposure Quartile**				**Sample**
Observables	**1st**	**2nd**	**3rd**	**4th**	**S.D.**
AAA RMBS exposure	0.166	0.230	0.282	0.430	0.106
Muni insurance portfolio risk	0.112	0.116	0.108	0.091	0.034
S&P credit rating	17.685	17.771	17.794	17.721	1.883
Special government	0.629	0.580	0.600	0.750	0.480
Not rated	0.386	0.324	0.332	0.343	0.476
County employment growth (07:09)	-0.040	-0.041	-0.036	-0.041	0.034
Panel B	**HQ CDO Exposure Quartile**				**Sample**
Observables	**1st**	**2nd**	**3rd**	**4th**	**S.D.**
HQ CDO exposure	0.000	0.068	0.254	0.764	0.221
Muni insurance portfolio risk	0.115	0.108	0.103	0.098	0.034
S&P credit rating	17.719	18.234	17.830	17.399	1.883
Special government	0.632	0.429	0.602	0.779	0.480
Not rated	0.375	0.274	0.315	0.363	0.476
County employment growth (07:09)	-0.041	-0.041	-0.035	-0.042	0.034

C Cumulative Probability of Bond Issuance

Table C1: Cumulative Probability of Bond Issuance by Prior Insurance Relationships

This table presents cummulative probability of bond issuance by year for governments that had no prior relationship with surviving insurers (AGC and FSA) and those that did. The top portion presents cummulative probability of issuing at least one insured bond with surviving insurers or new insurers (BHAC). The bottom portion presents cummulative probability of issuing at least one bond.

	08Q4	**09Q4**	**10Q4**	**11Q4**	**12Q4**
Insured Bonds with Surviving Insurers					
No Prior Relationship	0.09	0.15	0.20	0.22	0.25
Had Prior Relationship	0.21	0.30	0.35	0.38	0.40
All Bonds					
No Prior Relationship	0.36	0.52	0.63	0.68	0.75
Had Prior Relationship	0.48	0.66	0.76	0.82	0.87

D Placebo Employment Growth Regressions

Table D1: Insurers' Health and Full-time Employment Growth - Placebo

This table presents OLS and 2SLS regression results for variants of equation 5. The unit of observation is a government. The dependent variable is g_i^f, which is issuer i's full-time employment growth rate between 2006Q2 and 2007Q2 multiplied by 100. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$ and $HQ\,CDO\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)	(3)
Model	OLS	2SLS	2SLS
ΔI_i	0.34	-0.00	0.04
	[0.33]	[0.31]	[0.29]
Not rated	0.06	0.05	0.05
	[0.44]	[0.44]	[0.44]
Multiple insurers	-0.12	-0.15	-0.15
	[0.60]	[0.63]	[0.63]
Insurance ratio	-0.04	-0.08	-0.07
	[0.87]	[0.88]	[0.88]
Debt due in crisis	0.30	0.30	0.30
	[0.58]	[0.58]	[0.58]
Ln(total debt issued)	-0.02	-0.06	-0.06
	[0.21]	[0.23]	[0.22]
Special government	-0.11	-0.05	-0.06
	[0.71]	[0.73]	[0.73]
Ln(FT employment)	0.32	0.37	0.36
	[0.25]	[0.26]	[0.25]
County emp. growth	-0.03	-0.04	-0.04
	[0.07]	[0.07]	[0.07]
Lagged FT emp. growth	0.43***	0.43***	0.43***
	[0.05]	[0.05]	[0.05]
Instrument	-	RMBS	CDO
First stage F-stat	-	2635.81	340.11
Observations	798	798	798
R-squared	0.33	0.33	0.33

Table D2: Insurers' Health and Part-time Employment Growth - Placebo

This table presents OLS and 2SLS regression results for variants of equation 5. The unit of observation is a government. The dependent variable is g_i^p, which is issuer i's part-time employment growth rate between 2006Q2 and 2007Q2 multiplied by 100. ΔI_i is normalized to have its standard deviation equal to 1. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$ and $HQ\,CDO\,exposure_i$. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

Model	(1) OLS	(2) 2SLS	(3) 2SLS
ΔI_i	1.23	0.69	0.49
	[1.15]	[1.19]	[1.10]
Not rated	-4.82*	-4.85*	-4.86*
	[2.58]	[2.59]	[2.59]
Multiple insurers	7.21***	7.13***	7.11***
	[1.98]	[1.86]	[1.79]
Insurance ratio	1.69	1.66	1.65
	[6.31]	[6.30]	[6.30]
Debt due in crisis	1.60	1.59	1.58
	[1.81]	[1.82]	[1.83]
Ln(total debt issued)	-1.32	-1.34	-1.35
	[1.47]	[1.46]	[1.46]
Speical government	6.81*	6.89*	6.93*
	[3.99]	[4.03]	[4.02]
Ln(PT employment)	-0.29	-0.24	-0.23
	[1.35]	[1.34]	[1.35]
County emp. growth	0.92***	0.90***	0.90***
	[0.31]	[0.31]	[0.31]
Lagged PT emp. growth	0.05	0.05	0.05
	[0.09]	[0.09]	[0.09]
Instrument	-	RMBS	CDO
First stage F-stat	-	2718.27	338.92
Observations	796	796	796
R-squared	0.10	0.10	0.10

E Partial Equilibrium Calibration Estimates

Table E1: Employment Growth Estimates - Special Governments

This table presents OLS and 2SLS regression results for variants of equation 5. The unit of observation is a government entity. The sample for all columns is composed of special-purpose governments. The dependent variable for columns 1 and 2 is g_i^f, which is issuer i's full-time employment growth rate between 2007Q2 and 2009Q2 multiplied by 100. The dependent variable for columns 3 and 4 is g_i^p, which is issuer i's part-time employment growth rate between 2007Q2 and 2009Q2 multiplied by 100. ΔI_i is instrumented with $AAA\,RMBS\,exposure_i$. ΔI_i is *not* normalized to have its standard deviation equal to 1. All specifications include state fixed effects. Standard errors are clustered at the insurance-syndicate-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), and 10% (*) level.

	(1)	(2)	(3)	(4)
Dependent variable	Full-time emp. growth		Part-time emp. growth	
Model	OLS	2SLS	OLS	2SLS
ΔI_i	1.22**	1.17**	4.09***	3.75***
	[0.47]	[0.47]	[1.29]	[1.33]
Not rated	-2.70	-2.71	-4.26	-4.34
	[2.07]	[2.06]	[8.34]	[8.32]
Multiple insurers	0.16	0.15	-12.34**	-12.40**
	[1.72]	[1.73]	[5.42]	[5.55]
Insurance ratio	-1.39	-1.40	3.46	3.42
	[4.15]	[4.14]	[14.11]	[14.12]
Debt due in crisis	1.32	1.30	2.28	2.17
	[1.53]	[1.53]	[5.51]	[5.49]
Ln(total debt issued)	-0.11	-0.12	2.80	2.80
	[0.64]	[0.63]	[2.51]	[2.51]
County emp. growth	-0.34	-0.35	1.11	1.07
	[0.29]	[0.28]	[1.14]	[1.14]
Ln(FT employment)	0.56	0.57		
	[0.83]	[0.82]		
Lagged FT emp. growth	-0.16***	-0.16***		
	[0.04]	[0.04]		
Ln(PT employment)			3.03	3.07
			[2.68]	[2.69]
Lagged PT emp. growth			-0.27***	-0.27***
			[0.08]	[0.08]
Instrument	-	RMBS	-	RMBS
First stage F-stat	-	3676.58	-	4235.16
Observations	373	373	372	372
R-squared	0.28	0.28	0.25	0.25

Chapter III

Different Types of Social Ties Lead to Different Outcomes: Evidence from Venture Capital Investments

1 Introduction

Hegde and Tumlinson (2014) show the ethnic composition of US venture capital investors (VC) and startup executives are very similar. More importantly, the study shows VC-executive ethnic ties drive investment decisions VC investors make, and increase the probability of investment success through post-investment influences. Another prevalent feature of the VC industry is schooling similarity between VC investors and startup founders. Table 1 shows the top 20 post-secondary schools VC investors and founders attended. First, 12 of the 20 schools on the VC investors' list also appear on the founders' list. Second, the top 20 schools account for over 40% of all post-secondary-education degrees VC investors hold. Similarly, the top 20 schools account for almost 30% of all post-secondary-education degrees startup founders hold. These statistics suggest a high degree of concentration, in terms of post-secondary-education institutions, among VC investors and founders. Thus, simple summary statistics suggest VC-founder school ties are an important feature of the VC industry.

I extend the work of Hegde and Tumlinson (2014) by conducting a comparative study between VC-founder school ties and VC-founder ethnic ties. In particular, this paper studies the effects of VC-founder school ties on VC investors' investment decisions and investment outcomes, while using the effects of VC-founder ethnic ties on the same variables as a benchmark for economic magnitude. To construct the data set for this study, I assemble a list of global VC investors and founders from Dow Jones VentureSource. Next, I fill in each individual's post-secondary schooling information by using data from LinkedIn, Bloomberg Businessweek, and company websites. This information allows me to establish school ties between VC investors and founders. A VC investor and a founder are coded as sharing a school tie if they received at least one degree from the same school. For example, a VC investor who attended Harvard College is coded as having a school tie with a startup founder who attended Harvard Business School for his MBA. To assign ethnicity to individuals, I use the name-based algorithm from Kerr and Lincoln (2010) and supplement any missing values with web searches. VC investors and founders are coded as sharing an ethnic tie if they belong to the same ethnic minority group. Lastly, I map these social ties to VentureSource's data on VC investments.

The first part of this paper studies the effects of VC-founder school ties and ethnic ties on investment decisions. I first show VC-founder school ties have a similar effect on investment decisions as VC-founder ethnic ties. In particular, the probability that a VC investor will invest in a founder increases by 33.3%, if the pair shares a school tie. Second, I find both VC-founder school ties and ethnic ties increase the probability of homophilic follow-on investments. A founder who receives funding from a VC investor with whom he shares a school tie is four times more likely to receive subsequent funding from another VC investor with whom he also shares a school tie. Ethnic ties have a similar effect. Lastly, I find that school ties between the founder and the follow-on investor are more important to follow-on investment decisions than school ties between the initial investor and the follow-on investor. Both types of ethnic ties are equally important. Again, the economic magnitudes for both types of social tie are similar. The conclusion from this portion of the paper is that both school ties and ethnic ties are equally important for investment decisions in the VC industry.

The results above not only speak to the effects of VC-founder homophily on investment decisions, but also to its effects on professtional network formation in the VC industry. The second result shows VC-founder homophily has a clustering effect; that is a VC-founder pair that shares a social tie attracts additional investors to the mix. Therefore, the initial VC investor's professional network grows in a homophilic way. In particular, when a VC investor invests in a startup that was founded by a homophilic founder, he increases the probability that he will get connected to a homophilic VC investor in the future. Furthermore, the third result shows that founders play an important role in how VC investors' professional networks grow. Specifically, the homophilic relationship between the founder and the follow-on investor is a key determinant in the follow-on investor's investment decision, and hence a key determinant of whom the initial VC investor gets connected to in the future.

The second part of the paper studies the effects of VC-founder school ties and ethnic ties on investment success. OLS regressions show VC-founder ethnic ties significantly increase the probability of investment success, measured by the probability of an IPO or a high-valued acquisition, whereas VC-founder school ties do not. I use the instrumental variable approach to address the endogeneity concern that omitted variable bias may make OLS estimates inconsistent. The instrumental variable approach yields more negative estimates for VC-founder school ties and more positive estimates for VC-founder ethnic ties. Following the logic from Hegde and Tumlinson (2014), the difference between the effects of VC-founder school ties and ethnic ties on investment success can be mostly be attributed to the difference between each type of social tie's post-investment influences. VC-founder school ties seem to increase the risk of groupthink and poor decision-making, which leads to inferior investment outcomes. On the other hand, VC-founder ethnic ties enhance the pair's post-investment communication and coordination. Considering both parts of the paper together, I find VC-founder school ties and ethnic ties have similar effects on investment decisions and network-formation dynamics, but vastly different effects on investment outcomes.

This paper contributes to two strands of literature. Firstly, it contributes to the literature on school ties and performance in financial markets. Gompers et al. (2016) and Rider (2012) study the effects of VC co-investor school ties on investment decisions and investment outcomes. This paper studies a different partnership structure, namely, the one between institutional VC investors and startup founders. Second, this paper adds to the literature on VC networks. Hochberg et al. (2007) find the size of a VC investor's professional network has a positive impact on his investment performance. Hence, studying how VC professional networks are formed is important. My work shows VC-founder school ties and ethnic are important for network formation. In particular, we not only learn that homophily determines which VC investors get connected to each other, but also that startup founders play a key role in this important activity.

2 Data

2.1 Data Sources

The data I use in this paper are generated from the Dow Jones VentureSource data sets. The VentureSource data set contains information on venture capital deals from the 1970s to 2012. From this data set, I am able to see which VC firm invested in which portfolio company and identify which deals gave the VC firm a board seat at the portfolio company. The data set also shows which VC partner at the VC firm sits on the board of the portfolio company. I assume that this individual is the VC partner who is responsible for the investment. Next, the VentureSource data contains information on the founding team of each portfolio company. From these two sets of information, I am able to create VC-founder pairs. The VentureSource data set also has office addresses of VC firms and portfolio companies. In this particular study, I use VC firms' headquarter addresses and portfolio companies' office addresses to pin down geographical locations.

To create social-tie variables, I use the following additional data sources. For schooling information, I scraped data from LinkedIn, Bloomberg Businessweek, and company websites. For ethnicity information, I use the name-based algorithm from Kerr and Lincoln (2010) and web searches.

2.2 Summary Statistics

The VentureSource data set contains 41,529 unique founders and 17,063 unique VC investors. Of the 41,529 founders, I was able to find schooling information for 21,591 and assign ethnicity to 41,467. Of the 17,063 VC investors, I am able to find schooling information for 13,012 and assign ethnicity to 17,017. Table 2 summarizes this information.

The final sample consists of 14,673 founders, 7,894 VC partners, and 10,023 portfolio companies. To construct the final sample, I take all relevant first VC investments and identify all the founders and VC investors involved. Then I pair all VC investors to founders by their identified deals. For example, a portfolio company with two founders that receives funding from three VC investors will appear in the final data set six times because six VC-founder pairs are possible. I then drop all

VC investors and founders with missing schooling and ethnicity data. The final data set consists of 36,035 VC-founder-company triads. Table 3 summarizes these data.

3 Investment Decisions Results

3.1 Ethnicity and Schooling in the VC Industry

This section discusses univariate analyses on social similarities between VC investors and founders. Table 1 lists the top 20 post-secondary institutions VC investors and founders attended. As mentioned before, the constituents are very similar between the two lists. Notably, four schools (Harvard, Stanford, MIT, University of Pennsylvania) top both lists. More importantly, the top 20 schools on the VC investors' list account for over 40% of all post-secondary degrees held by VC investors. The top 20 schools on the founders' list account for almost 30% of all post-secondary degrees held by founders. These two statistics suggest a high degree of concentration, in terms of post-secondary-education institutions, between both groups.

Table 2 summarizes individual-level characteristics for all individuals in the data set where data on schooling and ethnicity are available. VC investors and founders are less similar regarding the degrees they hold. VC investors tend to hold more professional degrees (e.g., MBA and JD). Founders tend to hold more technical degrees (e.g., non-MBA Master and PhD). VC investors also hold more top-school degrees than founders. A top school is defined according to the definition from Gompers et al. (2016).

The VC and founder samples have very similar ethnic compositions. On the VC-investor side, 71% are Caucasian, 16% are Jewish, 6% are East Asian, 4% are Indian/South Asian, and 3% are Hispanic. On the founder side, 68% are Caucasian, 16% are Jewish, 6% are East Asian, 6% are Indian/South Asian, and 4% are Hispanic. Both groups have very small proportions of Middle Eastern and African individuals. This exercise confirms the finding by Hegde and Tumlinson (2014).[1]

[1] Table 1 from Hegde and Tumlinson (2014).

Table 3 summarizes deal-related data. The most notable statistics from this table are the proportions of deals in which VC-founder social ties. The proportion of deals in which the VC-founder pair shares a school tie is similar to the proportion of deals where the VC-founder pair shares an ethnic tie. In summary, the statistics on schooling, ethnicity, and deal characteristics suggest both school ties and ethnic ties are prominent features of the VC industry.

3.2 Initial Investment Decisions

To formally investigate whether VC-founder school ties and ethnic ties increase the probability of investment, I follow the approach from previous works (Gompers et al., 2016; Sorenson and Stuart, 2001, 2008). The approach is to create counterfactual deals between VC investors and founders for each realized VC-founder collaboration. The listed works only use the investment date to generate counterfactuals. To make the pool of counterfactuals more comparable to the realized investments, I choose to create counterfactual collaborations along four dimensions: investment date, portfolio-company industry, portfolio-company investment stage, and portfolio-company location. Consider a VC investment made in a healthcare startup in the Northeast census region of the United States on January 1, 2000. The counterfactuals I include for this particular deal are all healthcare startups in the Northeast region of the United States that received VC investment from other VC investors between December 1, 1999, and January 31, 2000. With this approach, I get fewer but more realistic counterfactuals.[2] By matching on these characteristics, I do not have to control for the industry distance between the VC investor and the portfolio companies to which he is matched. I can control for geographical distance between the two parties because I measure distance at the city level. I estimate the following equation:

$$Invest_{ijk} = \beta_1(x_{ij}) + \beta_2(Geographical\,distance_{vk}) + \eta_t + \kappa_l + \epsilon_{ijk}. \quad (1)$$

Invest is an indicator variable that equals 1 for realized VC-founder collaborations and zero for counterfactual collaborations. x_{ij} is the social-tie variable of interest. *Same school* equals 1 if

[2]For US portfolio companies, I create counterfactual companies from the same census region. For non-US companies, I choose counterfactual companies from the same country.

the VC investor and the founder hold at least one degree from the same school. *Same ethnic minority* equals 1 if the VC investor and the founder belong to the same ethnic minority group. Each observation is a VC-founder-startup triad, that is, a deal. VC investors are indexed by i, founders are indexed by j, startups are indexed by k, and VC firms are indexed by v. η_t and κ_l are investment-year and porftolio-company-industry fixed effects.

Table 4 presents probit regression results for school ties, ethnic ties, and investment decisions. The first column presents results for school ties and the probability of investment. The coefficient on *Same school* is positive and statistically significant at the 1% level. The economic magnitude is also significant. The estimate says that a VC investor is 1.4% more likely to invest in a portfolio company with a founder who attended the same school. With a base investment rate of 4.2%, this estimate translates to a 33.3% increase in relative investment probability. Column 2 shows the results for ethnic tie and the probability of investment. The coefficient on *Same ethnic minority* is positive and statistically significant at the 1% level. The economic magnitude is similar to that of *Same school*. Column 3 puts the two variables together and shows these two types of social ties have a similar effect on the probability of investment. These results support the intuition gained from the descriptive statistics, which is that VC-founder collaboration choices are greatly influenced by the pairs' schooling and ethnic similarities. The economic magnitude of ethnic ties is similar to that of Hegde and Tumlinson (2014).

3.3 Probability of Homophilic Follow-on Investments

Do VC-founder school ties and ethnic ties attract additional funding from other VC investors who also share the same social traits as the founder? Because school ties and ethnic ties increase the probability of initial investment, the finding that these social ties have a clustering effect would not be surprising, that is, that when two people with a social tie work together, a third person with the same social trait is more likely to join in. For the VC industry, this dynamic would have important implications for professional network formation and future career outcomes (Hochberg et al., 2007).

For this analysis, I limit my sample size to first-round investments so that all observations are more comparable to each other in terms of the likelihood that they will receive additional funding. Each observation is a deal. I only keep first investments, so a follow-on investment by the same VC investor is dropped. I estimate the following equation:

$$y_{ijk} = \beta_1(x_{ij}) + \boldsymbol{z'_{ijk}\delta} + \eta_t + \kappa_l + \epsilon_{ijk}. \tag{2}$$

y_{ijk} is the dependent variable for follow-on investment. I study three outcome variables: *Same school F.O.*, *Same ethnic minority F.O.*, and *Social tie F.O. Same school F.O.* equals 1 if the portfolio company gets funding from another VC investor who attended the same school as the founder. *Same ethnic minority F.O.* follows the same logic, and *Social tie F.O.* is the maximum of the two. As before, x_{ij} is the social-tie variable of interest. Because follow-on investment is a sign of venture success, I control for deal quality with a vector of deal-level characteristics $\boldsymbol{z'_{ijk}\delta}$. Lastly, I include investment-year and portfolio-company-industry fixed effects.

Table 5 presents rare-event logit regression results for this analysis. Column 1 presents the result for the effect of VC-founder school ties on the probability of a follow-on investment by another VC investor who also has a school tie with the founder. The coefficient on *Same school* is positive and statistically significant at the 1% level. The economic magnitude is also large. A founder who gets first-round funding from a VC with whom he shares a school tie is 4.1 times more likely to get subsequent funding from another VC investor with whom he also shares a school tie. Column 2 presents the result for ethnic tie and follow-on investment by another ethnically similar VC investor. The results are similar in statisical significance and economic magnitude. Column 3 regresses *Social tie F.O.* on the two social-tie variables and finds their economic magnitudes are strikingly similar. These results suggest school ties and ethnic ties are equally important for follow-on investments and network formation in the VC industry.

For a founder, getting first-round funding from a socially similar VC investor greatly increases the likelihood that he will get additional funding in the future. Thus, his venture will survive longer than that of another founder who did not get a homophilic investment in the first round. For a VC investor, investing in a founder with whom he shares a social tie greatly increases the probability that he will be connected with another socially similar VC investor, which mechanically increases the size of both VC investors' professional networks. Thus, the finding that the distribution of school and ethnic groups are very similar between VC investors and founders is not surprising.

3.4 Homophily and Follow-on Investors

A question that arises from the results in the previous section is, who brought on the follow-on investor? More specifically, did the social similarity between the founder and the follow-on investor or the social similarity between the initial investor and the follow-on investor attract additional funding. Answering this question sheds more light on how professional networks in the VC industry grow and the role founders play in this activity.

To answer this question, I take a similar approach to that of Sorenson and Stuart (2008). For each first-round investment that receives a follow-on investment, I fix the VC-founder pair from the initial investment, and create counterfactual follow-on investors based on characteristics of the real follow-on investment. The criteria I use to construct the counterfactual set are as follows: (1) the counterfactual investor must have invested in another portfolio company within plus or minus 30 days of the real investment under consideration, (2) the counterfactual investment must have been in the same industry as the real portfolio company under consideration, (3) the counterfactual investment must have been made in a portfolio company that was in the same investment stage as the real portfolio company under consideration, and (4) the counterfactual investment must have been made in a portfolio company that was located in the same geographical region as the real portfolio company under consideration. Lastly, I drop all real investments that do not have at least one counterfactual investment. This procedure produced 69,917 observations with a base investment rate of 26.7%. I estimate the following equation:

$$\begin{aligned} F.O.\,Invest_{ijkm} = {} & \beta_1(Lead - VC\,same\,school_{im}) + \\ & \beta_2(FDR - VC\,same\,school_{jm}) + \\ & \beta_3(FDR - VC\,geographical\,distance_{jm}) + \\ & \eta_t + \kappa_l + \epsilon_{ijkm}. \end{aligned} \tag{3}$$

The equation above uses school tie as an example. Initial VC investors are indexed by i, founders are indexed by j, portfolio companies are indexed by k, and follow-on VC investors are indexed by m. The unit of observation is an initial VC investor, founder, portfolio company, and follow-on VC investor quartet. *F.O. Invest* is an indicator variable that equals 1 for realized follow-on investments and zero for counterfactual investments. *Lead-VC same school* and *FDR-VC same school* are the social-tie variables of interest. *Lead-VC same school* equals 1 if the initial VC investor and the follow-on VC investor hold at least one degree from the same school. *FDR-VC same school* equals 1 if the founder and the follow-on VC investor hold at least one degree from the same school. *FDR-VC geographical distance* measures the distance between the city in which the startup is located and the city in which the follow-on VC investor's headquarter office is located. η_t and κ_l are investment-year and porftolio-company-industry fixed effects. The same setup applies to ethnic ties.

Table 6 presents probit regression results for the equation discussed above. In column 1, I investigate whether the school tie between the initial investor and the follow-on investor or the school tie between the founder and the follow-on investor is more important in attracting follow-on investments. I find the former is small and insignificant, whereas the latter is economically important and statistically significant. A follow-on investor is 3% more likely to invest in a portfolio company where he shares a school tie with the founder. With a base investment rate of 26.7%, this effect translates to a 11.2% increase.

Column 2 performs the same analysis on ethnic ties. I find both *Lead-VC same ethnic minority* and *FDR-VC same ethnic minority* are positive and statistically significant. The results translate to a 15.4% and 19.8% increase in the probability of investment, respectively. Column 3 includes both school ties and ethnic ties. The results remain the same as before. Taken together, these results suggest ethnic ties between the triad and school ties between the founder and the follow-on investor are key drivers in VC professional network-formation. More generally, startup founders play a key role in VC investors' network formation process by using their startups as focal points of connection. Founders' social characteristics draw socially similar VC investors to the same place and allow them to connect and grow their networks.

4 Investment Outcome Results

4.1 Instrumental Variable Approach

The previous section shows school ties and ethnic ties between VC investors and founders play equally important roles in investment decisions. The logical next question is, how do these social ties affect investment outcomes? To answer this question, I cannot simply regress a measure of investment success on these two variables, because the naive OLS regression will suffer from omitted variable bias, specifically, unobserved quality of portfolio companies. The fact that homophily plays such an important role in investment decisions clearly shows sorting between VC investors and founders is not random, and omitted portfolio-company-quality variables are likely to be correlated with both VC-founder social ties and investment outcomes. However, the direction of this bias is unclear, because the literature documents mixed results for the impact of homophily on performance.

I use the instrumental variable approach to address this concern. I have two endogenous variables–*Same school* and *Same ethnic minority*–so I need two instruments. The two instruments are similar in spirit, so I will use school tie as my main example. The instrument I use for *Same school* is *Local school tie*. This variable is the average school tie between the VC investor and relevant founders in his local area. For each VC-founder-company triad, I look at the industry the deal belongs to and the location of the VC firm's headquarter. Next, I identify all existing startups that are based in the same location as the VC firm and that belong to the same industry as the startup under consideration.[3] Then I calculate the average school tie between the VC investor and the founders of this set of startups. The calculation excludes the portfolio company under consideration. This final feature takes care of the exclusion requirement, because individuals included in the calculation are not involved in the deal under consideration in any way. Constructed this way, the sources of variation for this variable are time, location, and VC investor. *Local ethnic tie* is calculated in a similar way. Many prominent works in corporate finance have used this type of local-variation

[3] A startup is counted as being alive between one year before its founding date and five years after its last observed round of funding. For US companies, I look at startups in the same state. For non-US companies, I look at startups in the same country.

instrument (Berger et al., 2005; Bottazzi et al., 2008; Gompers et al., 2016; Hegde and Tumlinson, 2014).

The instrument should yield strong first-stage regression results for the following reasons. First, an increase in *Local school tie* should lead to a higher probability of same-school collaborations, because the supply of available founders now have higher schooling similarity to the VC investor under consideration. This claim is true even in the world without a preference for homophilic collaboration. The second channel is based on a well-documented fact from the social psychology literature, which is the fact that individuals prefer familiar goods and people (Saegert et al., 1973; Zajonc, 1968). Applied to the context of VC-founder school ties, a VC investor who works in a locale where there are many founders who belong to the same social group should be conditioned to prefer to collaborate with socially similar founders than another VC investor who works in a more socially diverse locale. Through this channel, an increase in *Local school tie* should also lead to a higher probability of same-school collaborations. The same reasoning and prediction apply to *Local ethnic tie* and *Same ethnic minority.*

4.2 First-Stage Results

I estimate the following equation to confirm the intuition from the previous section:

$$Same\,school_{ij} = \beta_1(Local\,school\,tie_{ijk}) + \boldsymbol{z'_{ijk}\delta} + \eta_t + \kappa_l + \epsilon_{ijk}. \tag{4}$$

$Same\,school_{ij}$ is the endogenous VC-founder school-tie variable. $\boldsymbol{z'_{ijk}\delta}$ is the vector of deal-characteristic controls. η_t and κ_l are investment-year and porftolio-company-industry fixed effects. The same setup applies to *Same ethnic minority.*

Table 7 presents first-stage regression results for using *Local school tie* and *Local ethnic tie* to instrument for *Same school* and *Same ethnic minority*, respectively. Column 1 presents the first-stage regression result for using *Local school tie* as the instrument for *Same school.* First, a unit increase in *Local school tie* leads to a 0.65 increase in *Same school.* This result confirms the prediction above. Furthermore, this effect is both economically and statistically significant. The Kleibergen-Paap rank LM statistic for this equation is 261.92, which rejects the null hypothesis that the equation is underidentified.

Stock and Yogo (2002) discuss two flavors of weak identification: maximal relative bias (compared to OLS) and maximal size. For the first type, the null hypothesis is that the instrument suffers from the specified bias. The second type of weak-identification test comes from the result that weak-identification leads to the Wald test on the relevant regression coefficient rejecting too often. With one endogenous variable and one excluded instrument, Stock and Yogo (2002) only provide critical values for the second type of weak-identification test. As suggested by Baum et al. (2007), I compare the equation's Kleibergen-Paap Wald rank F statistic to critical values provided by Stock and Yogo (2002) to test for weak identification. The Kleibergen-Paap Wald rank F statistic for column 1 is 413.45, which rejects the null hypothesis that the equation suffers from weak identification of the second type. The partial R-squared for *Same school* is 5.65%, which is large compared to the reported specification R-squared. This comparison confirms the potency of the instrument (Jiang, 2017).

Column 2 shows the first-stage regression result for using *Local ethnic tie* as the instrument for *Same ethnic minority.* The coefficient on *Local ethnic tie* is large and statistically significant at the 1% level. The Kleibergen-Paap rank LM statistic is 415.53, which rejects the null hypothesis that the equation suffers from underidentification. Kleibergen-Paap Wald rank F statistic is 1363.77, which passes the Stock and Yogo (2002) weak-identification test of the second type. The partial R-squared for *Same ethnic minority* is 23.56%, which is large compared to the reported specification R-squared.

Columns 3 and 4 present the first-stage regression results for including both *Same school* and *Same ethnic tie* in the same equation and using both *Local school tie* and *Local ethnic tie* to instrument for them. The coefficient on *Local school tie* with respect to *Same school* and the coefficient on *Local ethnic tie* with respect to *Same ethnic minority* are similar to results from columns 1 and 2. The Kleibergen-Paap rank LM statistic is 286.13 and the Kleibergen-Paap Wald rank F statistic is 194.04. Thus, this equation passes the underidentification test and weak-identification test of the second type.[4]

4.3 Investment Success Results

I estimate the following success equation:

$$Success_{ijk} = \beta_1(x_{ij}) + \boldsymbol{z'_{ijk}\delta} + \eta_t + \kappa_l + \epsilon_{ijk}. \tag{5}$$

$Success_{ijk}$ equals 1 if the portfolio company conducted an IPO or was acquired for $100 million USD or more by 2012. x_{ij} is the endogenous VC-founder social-tie variable of interest. In the 2SLS estimates, this variable is instrumented with the appropriate local social-tie variable. $\boldsymbol{z'_{ijk}\delta}$ is the vector of deal characteristic controls. η_t and κ_l are investment-year and porftolio-company-industry fixed effects.

Table 8 presents OLS and 2SLS results for the effects of VC-founder school ties and ethnic ties on the probability of investment success. Column 1 presents the OLS result for VC-founder school ties and the probability of investment success. The coefficient on *Same school* is effectively zero. Column 2 presents results for the effect of VC-founder ethnic ties on the probability of investment success. The coefficient on *Same ethnic minority* is positive and significant at the 5% level. The result says an investment in which the VC investor and the founder shares an ethnic tie is 2.4% more likely to succeed. With a base success rate of 15.2%, this effect translates to a 15.8% increase in the relative probability of success. Column 3 puts school tie and ethnic tie in the same equation and finds similar results.

As discussed above, OLS estimates are inconsistent due to endogeneity issues. Columns 4 to 6 present the 2SLS counterparts of columns 1 to 3. Column 4 presents the 2SLS result for column 1 and finds VC-founder school tie lowers the probability of investment success. However, this point estimate is not statistically significant. Column 5 presents the 2SLS for column 2. The coefficient on *Same ethnic minority* is 3 times larger than its OLS counterpart and is statistically significant

[4] For the case of two endogenous variables and two excluded instruments, Stock and Yogo (2002) also do not provide critical values for the first type of weak identification test.

at the 1% level. This effect translates to a 50.7% increase in the relative probability of success. Column 6 presents the 2SLS counterpart of column 3. The coefficient on *Same ethnic minority* remains positive and statisically significant, and the coefficient on *Same school* is now negative and statistically significant at the 5% level. VC-founder ethnic-ties increase the probability of investment success by 10%, whereas VC-founder school ties decrease the probability of investment success by roughly the same amount.

Overall, results from Table 8 suggest VC-founder ethnic ties have a positive impact on the probability of investment success, whereas VC-founder school ties do not. As aptly explained by Hegde and Tumlinson (2014), VC investors invest in founders with whom they share ethnic ties, because they are able to screen these deals more effectively and provide valuable post-investment influences through superior communication and coordination. The authors reason that the fact that the 2SLS coefficient is much larger than the OLS coefficient supports this claim. I find similar changes in coefficient size, and so I conclude VC-founder pairs that share ethnic ties are able to communicate and coordinate better, which translates to superior investment outcomes (Bhowmik and Rogers, 1970). Thus, coethnic collaborations between VC investors and founders are driven by sound economic rationale, because both parties stand to reap economic benefits from the partnership.

For VC-founder school ties, the positive side of homophily appears to fails to overcome the negative side of homophily. Following the reasoning from Hegde and Tumlinson (2014), the fact that the coefficient on *Same school* becomes more negative and statistically significant under 2SLS suggests similar schooling leads to negative post-investment influences. The likely culprit is groupthink. Groupthink is the idea that individuals in homophilic groups are more likely to desire unanimity, less likely to see the downside of a favored decision, and less likely to seek second opinions (Janis, 1972). All of these components lead to poor decision-making and inferior dyadic performance (Callaway and Esser, 1984). This explanation seems intuitive, especially for schooling, because individuals who attend the same school learn to think in similar ways, which naturally increases the likelihood of groupthink and social conformity. Furthermore, the literature on homophily and dyadic performance in financial markets supports this reasoning (Gompers et al., 2016; Ishii and Xuan, 2014). On the other hand, individuals who share an ethnic tie are less likely to suffer from groupthink, because these individuals can still have an ethnic tie even though they have drastically different backgrounds in other dimensions (e.g., birthplace, schooling, and work experience). Combining this result with previous results on school ties and the likelihood of investment, I find that schooling-based investment decisions seem to be driven by homophilic preferences rather than sound economic rationale.

5 Conclusion and Future Research

This paper finds that VC-founder school ties and ethnic ties both increase the probability of collaboration between VC investors and founders. However, VC-founder ethnic ties increase the probability of investment success, whereas VC-founder school ties do not. Furthermore, this paper finds VC-founder school ties and ethnic ties play equally important roles in professtional network formation in the VC industry.

With respect to research on VC networks, results from this paper suggest different types of networks may effect performance differently. Because VC-founder ethnic ties lead to better investment outcomes, whereas VC-founder school ties do not, VC networks formed via ethnically connected deals might have a positive impact on investment outcomes, whereas VC networks formed via school networks might not. This conjecture speaks to the difference in quality between different network types, and suggests future works on VC investors' networks should consider how networks are formed. A possible extension of Hochberg et al. (2007) is to explore whether the VC investor's schooling-based networks and ethnicity-based networks have different effects on investment performance.

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Table 1: Top Degree-granting Institutions

This table lists the top 20 degree-granting institutions for VC investors and founders. Schools are ranked by the number of degrees individuals in the data set received from the school.

VC Investors		Founders	
Institution	**Percent**	**Institution**	**Percent**
Harvard University	8.68%	Stanford University	4.22%
Stanford University	5.75%	Harvard University	3.64%
University of Pennsylvania	3.80%	Massachusetts Institute of Technology	2.64%
Massachusetts Institute of Technology	2.17%	University of California (Berkeley)	2.17%
Columbia University	2.07%	University of Pennsylvania	1.82%
University of California (Berkeley)	1.82%	Tel Aviv University	1.60%
Northwestern University	1.49%	University of Michigan	1.07%
Yale University	1.49%	Columbia University	1.06%
Dartmouth College	1.47%	University of California (Los Angeles)	1.05%
University of Chicago	1.40%	Cornell University	1.03%
Cornell University	1.22%	Technion Israel Institute of Technology	1.02%
University of Michigan	1.19%	Cambridge University	0.84%
Princeton University	1.19%	Northwestern University	0.83%
University of Virginia	1.14%	University of Illinois (Urbana Champaign)	0.83%
Tel Aviv University	1.13%	University of Southern California	0.81%
New York University	1.12%	Yale University	0.79%
Duke University	1.05%	New York University	0.79%
INSEAD	1.03%	University of Texas (Austin)	0.76%
Oxford University	1.01%	Carnegie Mellon University	0.74%
University of California (Los Angeles)	0.96%	Hebrew University of Jerusalem	0.72%

Table 2: School Degrees and Ethnicity Summary Statistics

This table presents descriptive statistics for post-secondary-education degrees and ethnic composition of VC investors and founders.

Venture Investors	**Count**	**Percent**	**Founders**	**Count**	**Percent**
Education degree			**Education degree**		
Has graduate education	10,526	80.89%	Has graduate education	14,516	67.23%
MBA	6,169	47.41%	MBA	4,391	20.34%
Non-MBA Master	2,204	16.94%	Non-MBA Master	6,393	29.61%
JD	566	4.35%	JD	426	1.97%
MD	237	1.82%	MD	318	1.47%
PhD	848	6.52%	PhD	3,154	14.61%
Unknown graduate degree	2,827	21.73%	Unknown graduate degree	2,367	10.96%
Attended a top school	6,223	47.83%	Attended a top school	6,218	28.80%
Total	13,012	100.00%	Total	21,591	100.00%

Ethnicity	**Count**	**Percent**	**Ethnicity**	**Count**	**Percent**
Caucasian	12,083	71.01%	Caucasian	28,012	67.55%
Jewish	2,650	15.57%	Jewish	6,477	15.62%
East Asian	1,095	6.43%	East Asian	2,632	6.35%
Indian	641	3.77%	Indian	2,627	6.34%
Hispanic	530	3.11%	Hispanic	1,606	3.87%
Middle Eastern	15	0.09%	Middle Eastern	98	0.24%
African	3	0.02%	African	15	0.04%
Total	17,017	100.00%	Total	41,467	100.00%

Table 3: Investment Deals Descriptive Statistics

This table presents descriptive statistics for investment deals in the final sample. The total number of deals in the final sample is 36,035.

Deals and VC-founder social ties	**Count**	**Percent**
Deals with VC-founder school ties	2,911	8.08%
Deals with VC-founder ethnic ties	2,245	6.23%
Deal investment stage		
Startup	1,926	8.44%
Product development	8,592	37.64%
Expansion	11,293	49.47%
Profitable	1,015	4.45%
Deal industry		
Business and financial services	3,504	15.35%
Consumer goods	112	0.49%
Consumer services	2,029	8.89%
Energy and utilities	272	1.19%
Healthcare	4,528	19.84%
Industrial goods and materials	308	1.35%
Information technology	12,073	52.89%
Deal location		
Domestic	18,306	80.20%
International	4,520	19.80%
Deal year		
1981 - 1990	79	0.35%
1991 - 2000	8,662	37.95%
2001 - 2010	13,882	60.82%
2011 - 2012	203	0.89%

Table 4: Homophily and Probability of Investment

$$Invest_{ijk} = \beta_1(x_{ij}) + \beta_2(Geographical\, distance_{vk}) + \eta_t + \kappa_l + \epsilon_{ijk}$$

This table presents probit regression results for variants of equation 1. The unit of observation is a VC-founder-company triad. The dependent variable is *Invest*, which equals 1 for realized investments and zero for counterfactual investments. x_{ij} is the social-tie variable of interest. *Same school* equals 1 if the VC investor and the founder hold at least one degree from the same school. *Same ethnic minority* equals 1 if the VC investor and the founder belong to the same ethnic minority group. All specifications contain industry and investment-year fixed effects. Robust standard errors are clustered at the portfolio-company-level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), or 10% (*) level.

	(1)	(2)	(3)
Same school	0.168***		0.163***
	[0.014]		[0.014]
Same ethnic minority		0.199***	0.194***
		[0.016]	[0.016]
Geographical distance	-0.002***	-0.002***	-0.002***
	[0.000]	[0.000]	[0.000]
Observations	774,964	774,964	774,964

Table 5: Probability of Homophilic Follow-on Investments

$$y_{ijk} = \beta_1(x_{ij}) + \boldsymbol{z'_{ijk}\delta} + \eta_t + \kappa_l + \epsilon_{ijk}$$

This table presents rare-event logistic regression results for variants of equation 2. The unit of observation is a VC-founder-company triad. The dependent variable, y_{ijk}, for specification 1 is *Same school F.O.*, which equals 1 if, in a later round, the founder and portfolio-company pair receives funding from another VC investor who holds at least one degree from the same school as the founder, and zero otherwise. The dependent variable for specification 2 is *Same ethnic minority F.O.*, which equals 1 if, in a later round, the founder and portfolio-company pair receives funding from another VC investor who belongs to the same ethnic minority group as the founder, and zero otherwise. The dependent variable for specifications 3 is *Social tie F.O.*, which is the maximum of *Same school F.O.* and *Same ethnic minority F.O.* x_{ij} is the social-tie variable of interest. *Same school* equals 1 if the VC investor and the founder hold at least one degree from the same school. *Same ethnic minority* equals 1 if the VC investor and the founder belong to the same ethnic minority group. All specifications contain industry and investment-year fixed effects. Robust standard errors are clustered at the portfolio-company level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), or 10% (*) level.

	(1)	(2)	(3)
Dependent variable	Same school F.O.	Same ethnic minority F.O.	Social tie F.O.
Same school	1.430***		1.019***
	[0.089]		[0.083]
Same ethnic minority		1.824***	1.000***
		[0.108]	[0.100]
VC performance	0.177	0.013	0.182
	[0.156]	[0.192]	[0.130]
VC top school	-0.055	-0.052	-0.004
	[0.086]	[0.094]	[0.070]
Industry distance	-0.062	-0.119	-0.053
	[0.103]	[0.114]	[0.083]
Serial founder	0.175	0.737***	0.430***
	[0.206]	[0.198]	[0.159]
Successful serial founder	0.454	-0.944**	0.019
	[0.327]	[0.384]	[0.263]
Investment stage	-0.460***	-0.375***	-0.426***
	[0.075]	[0.077]	[0.059]
Observations	24,827	24,827	24,827

Table 6: Homophily and Follow-on Investors.

$$
\begin{aligned}
F.O.\,Invest_{ijkm} = \; & \beta_1(Lead - VC\,same\,school_{im}) + \\
& \beta_2(FDR - VC\,same\,school_{jm}) + \\
& \beta_3(FDR - VC\,geographical\,distance_{jm}) + \\
& \eta_t + \kappa_l + \epsilon_{ijkm}
\end{aligned}
$$

This table presents probit regression results for variants of equation 3. The unit of observation is an initial VC investor, founder, portfolio company, and follow-on VC investor quartet. The dependent variable is *F.O. Invest*, which equals 1 for realized follow-on investments and zero for counterfactual investments. *Lead-VC same school* equals 1 if the initial VC investor and the follow-on VC investor hold at least one degree from the same school. *FDR-VC same school* equals 1 if the founder and the follow-on VC investor hold at least one degree from the same school. *FDR-VC geographical distance* measures the distance between the city in which the startup is located and the city in which the follow-on VC investor's headquarter office is located. The same setup applies to ethnic ties. All specifications contain industry and investment-year fixed effects. Robust standard errors are clustered at the portfolio-company level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), or 10% (*) level.

	(1)	(2)	(3)
Lead-VC same school	0.004		-0.002
	[0.028]		[0.028]
FDR-VC same school	0.101**		0.093**
	[0.041]		[0.041]
Lead-VC same ethnic minority		0.135***	0.132***
		[0.044]	[0.043]
FDR-VC same ethnic minority		0.174***	0.171***
		[0.042]	[0.042]
FDR-VC geographical distance	-0.001	-0.001	-0.001
	[0.001]	[0.001]	[0.001]
Observations	69,917	69,917	69,917

Table 7: Homophily and Investment Success - First Stage

$$Same\,school_{ij} = \beta_1(Local\,school\,tie_{ijk}) + \boldsymbol{z'_{ijk}}\boldsymbol{\delta} + \eta_t + \kappa_l + \epsilon_{ijk}$$

$$Same\,ethnic\,minority_{ij} = \beta_1(Local\,ethnic\,tie_{ijk}) + \boldsymbol{z'_{ijk}}\boldsymbol{\delta} + \eta_t + \kappa_l + \epsilon_{ijk}$$

This table presents the first-stage regressions for 2SLS results in Table 8. It presents estimates for variants of equation 4. The unit of observation is a VC-founder-company triad. The dependent variable for columns 1 and 3 is *Same school*, which equals 1 if the VC investor and the founder hold at least one degree from the same school. The dependent variable for columns 2 and 4 is *Same ethnic minority*, which equals 1 if the VC investor and the founder belong to the same ethnic minority group. The instrument for *Same school* is *Local school tie* and the instrument for *Same ethnic minority* is *Local ethnic tie*. Column 1 corresponds to column 4 in Table 8. Column 2 corresponds to column 5 in Table 8. Columns 3 and 4 correspond to column 6 in Table 8. All specifications contain industry and investment-year fixed effects. Robust standard errors are clustered at the portfolio-company level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), or 10% (*) level.

Dependent variable	(1) Same school	(2) Same ethnic minority	(3) Same school	(4) Same ethnic minority
Local school tie	0.651***		0.625***	-0.009
	[0.032]		[0.032]	[0.015]
Local ethnic tie		0.909***	0.087***	0.910***
		[0.025]	[0.017]	[0.025]
VC performance	-0.013*	-0.006	-0.012*	-0.006
	[0.007]	[0.005]	[0.007]	[0.005]
VC top school	0.012***	-0.004	0.014***	-0.003
	[0.004]	[0.003]	[0.004]	[0.003]
Industry distance	0.011***	0.001	0.011***	0.001
	[0.004]	[0.004]	[0.004]	[0.004]
Serial founder	0.011	0.032***	0.010	0.032***
	[0.009]	[0.009]	[0.009]	[0.009]
Successful serial founder	0.007	-0.015	0.008	-0.015
	[0.014]	[0.012]	[0.014]	[0.012]
Investment stage	-0.010***	0.000	-0.010***	0.000
	[0.003]	[0.002]	[0.003]	[0.002]
Observations	36,035	36,035	36,035	36,035
R-squared	0.065	0.241	0.067	0.241

Table 8: Homophily and Investment Success

$$Success_{ijk} = \beta_1(x_{ij}) + \boldsymbol{z'_{ijk}\delta} + \eta_t + \kappa_l + \epsilon_{ijk}.$$

This table presents OLS and 2SLS results on the effect of VC-founder school ties and ethnic ties on the probability of investment success. It presents estimates for variants of equation 5. The unit of observation is a VC-founder-company triad. The dependent variable is *Success*, which equals 1 if the portfolio offered an IPO or was acquired for $100 million USD or more by 2012. x_{ij} is the social-tie variable of interest. *Same school* equals 1 if the VC investor and the founder hold at least one degree from the same school. *Same ethnic minority* equals 1 if the VC investor and the founder belong to the same ethnic minority group. In column 4, *Same school* is instrumented with *Local school tie*. In column 5, *Same ethnic minority* is instrumented with *Local ethnic tie*. In column 6, *Same school* and *Same ethnic minority* are instrumented with *Local school tie* and *Local ethnic tie*. All specifications contain industry and investment-year fixed effects. Robust standard errors are clustered at the portfolio-company level and reported in brackets. Asterisks denote statistical significance at the 1% (***), 5% (**), or 10% (*) level.

	(1)	(2)	(3)	(4)	(5)	(6)
Same school	0.000		-0.001	-0.068		-0.108**
	[0.009]		[0.009]	[0.044]		[0.046]
Same ethnic minority		0.024**	0.024**		0.077***	0.100***
		[0.012]	[0.011]		[0.029]	[0.030]
VC performance	0.114***	0.115***	0.115***	0.113***	0.115***	0.114***
	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]	[0.013]
VC top school	0.029***	0.029***	0.029***	0.031***	0.029***	0.034***
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Industry distance	-0.031***	-0.031***	-0.031***	-0.030***	-0.031***	-0.030***
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
Serial founder	-0.033**	-0.034**	-0.034**	-0.032**	-0.036**	-0.036**
	[0.015]	[0.015]	[0.015]	[0.015]	[0.015]	[0.015]
Successful serial founder	0.049	0.049*	0.049*	0.049	0.051*	0.052*
	[0.030]	[0.030]	[0.030]	[0.030]	[0.030]	[0.030]
Investment stage	0.016***	0.016***	0.016***	0.015**	0.016***	0.015**
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]
Model	OLS	OLS	OLS	2SLS	2SLS	2SLS
Observations	36,035	36,035	36,035	36,035	36,035	36,035
R-squared	0.102	0.103	0.103	0.100	0.102	0.094

A Appendices

A.1 Variable Definition

F.O. Invest - Equals 1 for realized follow-on investments and zero for counterfactual investments.

FDR-VC geographical distance - Distance between the portfolio company's city and the follow-on VC investor's headquarter office city, measured in hundreds of miles.

FDR-VC same ethnic minority - Equals 1 if the founder and the follow-on VC investor belong to the same ethnic minority group.

FDR-VC same school - Equals 1 if the founder and the follow-on VC investor hold at least one degree from the same school.

Geographical distance - Distance between the VC firm's headquarter city and the portfolio company's office city, measured in hundreds of miles.

Industry distance - The proportion of prior deals the VC investor completed that does not belong to the portfolio company's industry.

Invest - Equals 1 for realized investments and zero for counterfactual investments.

Investment stage - A discrete variable that ranges from 1 to 4, corresponding to startup, product development, expansion, and profitable.

Lead-VC same ethnic minority - Equals 1 if the initial VC investor and the follow-on VC investor belong to the same ethnic minority group.

Lead-VC same school - Equals 1 if the initial VC investor and the follow-on VC investor hold at least one degree from the same school.

Local ethnic tie - Average ethnic tie between the VC investor and the pool of local founders in the same industry as the portfolio company under consideration, excluding the portfolio company under consideration. Details on how this variable was constructed are outlined in the text.

Local school tie - Average school tie between the VC investor and the pool of local founders in the same industry as the portfolio company under consideration, excluding the portfolio company under consideration. Details on how this variable was constructed are outlined in the text.

Same ethnic minority - Equals 1 if the VC investor and the founder belong to the same ethnic minority group.

Same ethnic minority F.O. - Equals 1 if, in a later round, the founder and portfolio-company pair receives funding from another VC investor who belongs to the same ethnic minority group as the founder.

Same school F.O. - Equals 1 if, in a later round, the founder and portfolio-company pair receives funding from another VC investor who holds at least one degree from the same school as the founder.

Same school - Equals 1 if the VC investor and the founder hold at least one degree from the same school.

Serial founder - Equals 1 if the founder founded at least one other company in the past.

Social tie F.O. - The maximum of *Same ethnic minority F.O.* and *Same school F.O.*.

Success - Equals 1 if the portfolio offered an IPO or was acquired for $100 million USD or more by 2012.

Successful serial founder - Equals 1 if the founder founded at least one successful company in the past.

VC performance - Percentage of the VC investor's past deals that succeeded by 2012.

VC top school - Equals 1 if the VC investor attended a top school. The definition of a top school is the same as the definition from Gompers et al. (2016).