Poverty Spreads in Deposit Markets

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April 30, 2025

Abstract

We document significant deposit interest rate differentials along the income distribution - moving from the bottom to the top income decile increases deposit rates by 55% of the sample median rate. These spreads persist independent of banking competition, and instead appear to arise from banks internalizing households' participation in nondeposit markets. Consistent with this hypothesis, only income components related to participation can explain our baseline findings, and quasi-exogenous reductions in participation incentives through increases in capital gains taxes are associated with lower spreads along the participation distribution. Our findings highlight lack of participation as a source of deposit market power.

JEL Codes: G12, G21, G51.

Keywords: Consumer deposit pricing, deposit market power, inequality, participation.

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1 Introduction

Do banks offer lower deposit rates to low-income households than to high-income households? Recent work shows that firms in the consumer goods sector target households with different income differently, leading to inflation inequality along the income distribution (Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019). Bank deposits such as savings accounts and time deposits are the simplest and most widespread financial products with which households store, transfer, and save their resources.[1] While a long literature documents banks' deposit market power (e.g., Hannan and Berger, 1991; Neumark and Sharpe, 1992; Drechsler et al., 2017), we lack systematic evidence on whether banks price discriminate depositors based on their income, and an understanding of the potential sources and implications of such differences.

In this paper, we study deposit rate setting along the income distribution and its economic sources. We start our analysis by documenting several facts about banks' deposit product offerings across areas with different income levels. We first show that banks offer significantly lower rates in low-income zipcodes than in high-income zipcodes: moving from the bottom to the top decile of the income distribution is associated with a 0.22 percentage point (pp) increase in average deposit interest rates (24% of the average deposit rate and 55% of the median deposit rate in our sample). These income-based deposit spreads hold within county-year, suggesting substantial rate variation even within geographic areas commonly used to define deposit markets (Heitfield, 1999, Biehl, 2002). They also hold within bank-product-year, suggesting that the same bank may offer different deposit rates across different areas where it operates based on local income. Banks also appear to offer higher product variety in high-income zipcodes than in low-income zipcodes: the number of products offered by individual branches, as well as the average product maturity and minimum subscription size, all increase in local income. However,

[1] In 2021, 96% of the households in the United States had an active deposit account. See https://www.fdic.gov/household-survey.

product variety and account size cannot fully explain our baseline results. We find large spreads even for the same deposit product, minimum subscription size, and maturity.

A natural hypothesis is that our baseline findings may be a byproduct of competition within the banking sector if competition is higher in high-income areas than in low-income areas. They may also do so to cross-subsidize other products targeting wealthy customers, such as insurance or other investment products generating fee-based income. However, our point estimates are quantitatively similar between zipcodes and counties with different levels of bank competition and between banks with different dependence on fee-based income, suggesting that competition within the banking sector is unlikely to be a first-order determinant of our findings. Indeed, our estimates are largest for banks below the top of the size distribution and operating in nonmetropolitan areas, which derive most of their profits from traditional lending and deposit-taking activities.

We test the hypothesis that the observed deposit spreads may result from banks internalizing differential participation in nondeposit assets (henceforth "participation") along the income distribution. This hypothesis is two-fold. First, high-income households have a higher participation propensity than low-income households. Second, if banks internalize high-income households' higher participation propensity, they may offer higher rates to avoid losing their deposit base to nondeposit assets. In sum, the observed spreads may be a byproduct of differential participation in nondeposit assets along the income distribution.

We provide four sets of results consistent with our hypothesis. First, using disaggregated data on zipcode-level income, we confirm that participation is increasing in income, and that only the components of income related to participation (such as net capital gains and interest income, see Fagereng et al., 2019; Chodorow-Reich et al., 2021; and Smith et al., 2023) exhibit a positive relationship with local deposit rates. In contrast, we do not observe any relationship between deposit rates and income components unrelated to participation, such as salaries. These findings hold *even conditioning on income levels* (e.g., by performing our tests within income brackets), suggesting that explanations purely related to local income (e.g., banks' facing high marginal costs to open an account in low-income areas) are unlikely to explain our findings.

Second, we show that deposit quantities' volatility and their responsiveness to the performance of nondeposit assets are both increasing in local participation. For example, a one standard deviation increase in excess market returns (13.94 pp) is associated with a contemporaneous 1.1 pp decrease in branch-level deposit growth, around 23.3% of the average branch-level deposit growth in our sample. This baseline negative sensitivity increases (i.e., becomes more negative) by around 57% in high-participation zipcodes, consistent with the hypothesis that households in high-participation zipcodes have a higher propensity to invest in other assets when their returns are high (as previously shown in Lin, 2020). We also document similar findings when we consider outflows in response to both past performance and analysts' recommendations about local stocks, which may more closely approximate the past and expected returns of local households' equity investment opportunities than the aggregate stock market (Lin and Pursiainen, 2023); as well as outflows in response to the performance of local municipal bonds, suggesting that our findings may extend to a broad range of nondeposit asset classes other than stocks.

Third, we find that the relationship between rates and local participation is most pronounced for money market accounts (MMAs) and certificates of deposit (CDs), suggesting that these products are relatively close substitutes to nondeposit investment opportunities (e.g., Xiao, 2020), and weaker for callable deposits such as checking and savings accounts, suggesting that the demand for these products is inelastic regardless of local participation (e.g., Driscoll and Judson, 2013; Egan et al., 2022a).

In the cross-section of time deposits, we also find larger spreads for long-maturity CDs than for short-term CDs—not only the level of deposit rates but also the slope of their term structure is increasing in participation. This finding has two implications. First, it suggests that long-maturity time deposits may be closer substitutes to (and thus more sensitive to the performance of) nondeposit assets than short-maturity deposits. We find empirical support for this hypothesis. Second, by focusing on rate differences between long- and short-maturity time deposits with the same subscription size offered by the same branch at the same point in time, we remove any confounding variation potentially correlated with interest rate levels. As a result, any alternative explanation to our proposed participation channel would need to rationalize both i) differences in average interest rate levels and ii) differences in term structure slopes conditional on levels along the participation distribution.

Fourth, we provide a causal interpretation for our proposed mechanism by exploiting quasi-experimental variation in participation incentives for households with different participation levels. In the time series, we study how changes in state-level capital gains taxes for top earners affect local participation and deposit rates. Our hypothesis in these tests is that capital gains taxes on top earners change participation incentives at the top of the participation distribution (and thus banks' rate-setting incentives in high-participation areas), while they do not affect participation incentives at the bottom of the participation distribution.

Consistent with our hypothesis, in a two-stage least squares regression framework we find that increases in state level capital gains taxes are associated with large decreases in our measures of local participation. For example, a one standard deviation increase in state-level taxes is associated with a 0.4 pp reduction in the average ratio of net capital gains to total income at the zipcode-level, around 8.6% of the sample mean and 6.5% of the sample standard deviation. When we instrument net capital gains to total income using state-level capital gains taxes, we confirm a strong positive relationship between participation and deposit interest rates. While these results hold when we use alternative measures of participation such as interest income to total income, they again lose all economic and statistical significance when we investigate a possible relationship between capital gains taxes and other income sources such as salaries. In turn, this suggests that variation in local economic conditions potentially correlated with capital gains taxes and deposit rates is unlikely to be a systematic source of omitted variable bias (OVB) in our two-stage least square regressions.[2]

In the cross-section, we also study the effects of broker misconduct during the financial crisis (Egan et al., 2019, 2022b) on subsequent participation and deposit rates. Consistent with our time series findings, we find that broker misconduct during the crisis is negatively associated with subsequent participation, and that the component of participation correlated with changes in broker misconduct is positively associated with local deposit rates. In sum, given the available evidence, deposit rate spreads along the income distribution seem to be primarily consistent with local banks internalizing households' participation incentives.

Our results have two implications. First, they suggest that lack of participation is a source of bank deposit market power. Consistent with this hypothesis, we find that variation in participation can explain as much variation in local deposit betas (Drechsler et al., 2021) as variation in traditional measures of local banking competition such as deposit HHI and the number of bank branches. Second, they suggest that nominal spreads in deposit rates combined with inflation inequality (Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019) may result in even larger differences in the *real* rates that households with different income levels earn on their deposits. Consistent with this hypothesis, we find that spreads in nominal rates contribute to around one third of the total variation in real deposit spreads along the income distribution.

Our paper contributes to three areas of the literature. A first literature in financial intermediation studies the determinants of banks' rate setting behavior in local deposit markets (e.g., Hannan and Berger, 1991; Neumark and Sharpe, 1992; Ben-David et al.,

[2]We also formally test for and find little evidence of OVB in our estimates. In a stacked difference-in-differences (DiD) regression framework, we also find that capital gains tax cuts at the state level increase the sensitivity of deposit rates to local income. These dynamic tests show no evidence of differential trends in the sensitivity of income to rates across treated states that implement tax rate cuts and control states that do not implement such cuts, further supporting a causal interpretation of our findings.

2017; d'Avernas et al., 2023; Bisetti and Karolyi, 2024; Oberfield et al., 2024; Yankov, 2024), and establishes competition within the banking sector as well as households' preferences (e.g., for branch location and liquidity services) as primary determinants of deposit rate setting. Our paper complements this literature by showing that banks also internalize differential participation in nondeposit assets when pricing retail deposits. The closest papers to ours in this literature are Drechsler et al. (2017) and Xiao (2020), which study how deposit rates and quantities respond to changes in the Fed funds rate as functions of banks' competition within the banking sector and with money market funds. We complement these studies by focusing on average differences in interest rates levels along the income and participation distributions as opposed to monetary policy pass through, by showing that banks internalize households' differential participation in a broad range of assets (e.g., stocks and municipal bonds), and by providing formal evidence of a meaningful source of deposit market power—lack of participation in nondeposit markets.

Second, our paper contributes to the household finance literature on financial sophistication and participation (e.g., Campbell, 2006; Calvet et al., 2007; Guiso et al., 2008; Agarwal et al., 2017; Brown et al., 2019), and in particular to the branch of this literature that studies how financial firms internalize differential sophistication across consumers (e.g. Gurun et al., 2016; Egan, 2019). To the best of our knowledge, our paper is the first in this literature to show that banks internalize differential participation and propensity to switch across asset classes when pricing their retail deposits, which are arguably the simplest and most widespread financial products available to retail investors.[3]

Third, the economics literature has long debated the presence of a poverty penalty (Kunreuther, 1973; Attanasio and Frayne, 2006) and, more recently, found evidence of inflation inequality in consumer goods (Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019;

[3]Recent strands of the macroeconomics and macro-finance literature also study the relationship between portfolio choice, returns on wealth, and wealth inequality (e.g., Fagereng et al., 2016, Fagereng et al., 2019, Hubmer et al., 2021, Catherine et al., 2023), typically taking the return of the assets available to different households as given (Gomez, 2024 being an exception). We complement this literature by showing that the rates of returns on some financial products may endogenously respond to investors' income and participation.

Argente and Lee, 2021). We contribute to this literature by documenting income-related spreads in consumer deposit rates, and by showing that spreads in nominal financial returns can amplify the effects of inflation inequality along the income distribution.

2 Data

We obtain our data from two primary sources. Data on deposit rates and other product characteristics at the branch-product-week level comes from RateWatch. We collapse the RateWatch data at the branch-product category-year level according to six broad product categories, namely, CDs, regular and premium MMAs, interest-bearing checking accounts, savings accounts, and special products. The resulting branch-product category-year level panel contains 629,452 observations on the average rates offered by each branch in a given year for each product category. For convenience, in what follows we refer to product categories as "products."

We use information on the branch location provided by RateWatch to merge the annual deposit product panel with zipcode-year level information on local income from the U.S. Internal Revenue Service (IRS) Statistics of Income (SOI).[4] This data includes information on total income and number of tax returns for different income brackets, as well as on income sources such as salaries, capital gains, and taxable interest. We use the IRS-SOI data to compute average *Per Capita Income* at the zipcode-year level as adjusted gross income (SOI item a00100) divided by the number of returns at the zipcode level (SOI item n1).

Similar to Chodorow-Reich et al. (2021), we also use the IRS-SOI data to obtain our two main measures of local participation in nondeposit markets: *Net Capital Gains to Total Income* (SOI item a01000 divided by SOI item a00100) and *Interest to Total Income* (SOI item a00300 divided by SOI item a00100). *Net Capital Gains to Total Income* and *Interest to Total*

[4] The data is continuously publicly available since 2004 at https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi.

Income offer complementary advantages as measures of local participation. On the one hand, the main benefit of *Net Capital Gains to Total Income* is that it is unaffected by participation in deposit markets, since deposit products typically do not entail capital gains. However, this measure of participation may also vary due to households' incentives to *realize* capital gains and losses, which may act as a confound to the identification of our main mechanism. On the other hand, *Interest to Total Income* is relatively unaffected by these realization incentives. Additionally, it contains information about fixed income investments, which are arguably closer substitutes to deposits than, for example, stocks or real estate. However, this measure of participation also includes interest income from investment in deposits products, which can act as a confounding factor. We trade off these costs and benefits by using both measures of participation.

We also use data from six sets of secondary sources. First, we use the Federal Deposit Insurance Corporation's (FDIC) Summary of Deposits (SOD) and Call Reports to study how our baseline findings vary with local deposit market structure and bank characteristics. Second, we use merged data from SOD, Compustat, the Center for Research on Security Prices (CRSP), the Institutional Brokers' Estimate System (I/B/E/S), and the Municipal Securities Rulemaking Board (MSRB) to study how the sensitivity of deposit flows to the performance of outside assets varies along the participation distribution.

Third, we use data on state taxes for top income earners to study how changes in participation incentives along the income distribution affect banks' deposit rate-setting. This data is publicly available on the NBER-TAXSIM website.[5] Fourth, we use city-level adviser misconduct data during the financial crisis from Egan et al. (2019, 2022b) to study how broker misconduct affects local participation and deposit rates in the post-crisis period. Fifth, we use NielsenIQ homescan data to construct measures of zipcode-level inflation (similar to Kaplan and Schulhofer-Wohl, 2017, and Jaravel, 2019) and study how variation in nominal deposit rates and inflation jointly determine spreads in *real* deposit

[5]See https://taxsim.nber.org/ and Feenberg and Coutts (1993) for an introduction to the data.

rates along the income distribution. Sixth, we use Rural-Urban Commuting Area Codes (RUCA) data from the Economic Research Service of the U.S. Department of Agriculture to study how our results vary with local geographic characteristics.[6] We describe these secondary datasets in the relevant sections of the paper and in the Appendix. Table 1 reports summary statistics for the main variables used in the paper.

3 Deposit Rates Along the Income Distribution

We start our analysis by documenting income spreads in deposit markets: banks offer lower deposit rates in low-income zipcodes than in high-income zipcodes. To do so, we estimate the baseline regression

$$d_{ipb(z)t} \quad = \quad \alpha + \beta log(Per\ Capita\ Income)_{zt} + \gamma_{FE} + \varepsilon_{ib(z)pt}, \tag{1}$$

where $d_{ib(z)pt}$ is the average deposit rate that bank i offers on product category p in branch b (located in zipcode z) during year t, $log(Per\ Capita\ Income)$, measured at the zipcode-year level, is the main dependent variable, γ_{FE} is a vector containing different combinations of fixed effects, and $\varepsilon_{ib(z)pt}$ is an error term.

In our baseline estimations, we include several combinations of fixed effects including bank $\times$ product fixed effects to control for average differences in the rates offered on the same product by different banks, zipcode $\times$ product fixed effects to control for average income and rate differences across different geographic areas, and year fixed effects to control for average differences in income and rates across years. While we are interested in average differences in rates across zipcodes, we show that our baseline also hold when we include bank $\times$ product $\times$ year fixed effects, thus absorbing variation related to bank deposit supply common across all branches (Drechsler et al., 2017) and comparing the

[6]The data is publicly available at https://www.ers.usda.gov/data-products/rural-urban-commuting-area-codes.aspx.

rates offered by the same bank on the same product in the same year across zipcodes with different levels of income. In robustness tests, we also document similar results when we include county $\times$ year fixed effects, which allows us to focus on cross-sectional variation in income within the same county and the same year. The coefficient of interest in equation (1) is β, which pins down marginal changes in interest rates at the branch level for a marginal change in local income. We follow the design-based approach of Abadie et al. (2020) and cluster standard errors at the zipcode level.

3.1 Deposit Rates

In Panel A of Figure 1, we start by asking whether the data provides evidence a correlation between local deposit rates and income. To build this panel, we only residualize rates and local income with respect to year fixed effects, so that we remove aggregate variation due to the business cycle and monetary policy, while focusing on rate and income differences across zipcodes in the same year. Panel A provides evidence of a positive, nearly monotonically increasing cross-sectional relationship between local income and deposit rates.

In Table 2, we formally estimate our baseline specification (1) under different and increasingly restrictive combinations of fixed effects. In column (1), we control for year and zipcode fixed effects, thus removing aggregate variation and average variation in income and rates stemming from local economic conditions that are likely time invariant in the 2004-2020 sample, such as, for example, slow-moving local demographics. Column (1) shows that a 1% increase in local income is associated with 0.12 bps increase in average local deposit rates, around 0.13% of the sample mean and 0.3% of the sample median. In other words, if the relationship between income and rates was linear, moving from the bottom to the top decile of the income distribution (a 182% increase in income) would imply a 0.218 pp increase in average deposit interest rate (24% of the sample mean and 55% of the sample median).

How large are the average deposit rate spreads we document between low- and high-income areas? At the household-level, average (median) zipcode-level deposits per tax return in the bottom decile of the income distribution amount to around USD 79,375 (USD 27,624). Keeping deposit-per-tax return quantities constant, a 22 bp difference in average deposit rates translates into around USD 175 (USD 60) lower deposit interest income per year, or 0.5% (0.2%) of average income per capita in the bottom decile of the distribution relative to the top decile.

In the aggregate (i.e., when we aggregate deposits across all zipcodes in the SOD data), average annual total deposits in zipcodes in the top decile of the income distribution amount to USD 2.21 trillion. Keeping this aggregate quantity constant, a 22 bp difference in average deposit rates translates into a USD 4.86 billion extra deposit interest expense that banks pay in high-income areas relative to low-income areas, around 6.3% of the average annual total deposit interest expense over our sample period.[7]

In columns (2) to (4), we also confirm that the point estimate of column (1) is quantitatively stable when adding incrementally restrictive combinations of fixed effects such as bank $\times$ product and zipcode $\times$ product fixed effects that respectively allow us to control for time-invariant bank characteristics (e.g., propensity to offer a given product), and time-invariant demand of specific products in some zipcodes.

The results of columns (1) to (4) of Table 2 document a positive and quantitatively large relationship between income and average deposit rates offered by local branches. In column (5), we also document similar results when we exploit purely cross-sectional variation within the same bank, product, and year across different geographic areas: the results of column (5) imply that the same bank offers on average higher rates on the same product in high-income zipcodes than in low-income zipcodes at the same point in time. Panel B of Figure 1 confirms this evidence graphically. While the results of Table 2, column (5), and Figure 1, Panel B, provide powerful variation identified within the same bank

[7] The average annual interest expense on US banks' deposits held in domestic offices is USD 76.7 billion over the 2004-2020 period. See, e.g., https://fred.stlouisfed.org/series/QBPQYTIEXDOFFDP.

and product, the sample size in this test shrinks by almost two thirds, suggesting that only a subset of banks in our sample offers differentiated rates across branches (Begenau and Stafford, 2022; Granja and Paixao, 2023). Since in this paper we are interested in quantifying average rate differences along the income distribution, in what follows we use the specification used in column (4) rather than that in column (5) as our preferred specification.

3.2 Intensive and Extensive Margins

Do the results presented in Table 2 reflect higher variety in the product mix that banks offer in high-income areas, or do banks offer higher rates on the very same product along the income distribution? In Table 3, we study how our baseline results vary on the extensive and on the intensive margins. In Panel A, column (1), we show that deposit product variety (i.e., the number of deposit subproducts offered within a broad product category) increases in local income: a 182% increase in income (i.e., moving from the bottom to the top decile of the income distribution in our sample) is associated with a 11.7% increase in the number of subproducts for each product category. In a similar spirit, columns (2) and (3) respectively show that the same 182% increase in income is associated with a 14.5% increase in minimum subscription size for the average product available in the branch, and with a 1.85% increase in the average CD maturity.

In Panel B of Table 3, we document the existence of income spreads even within narrowly defined deposit products (e.g., MMAs with minimum subscription size of USD 2.5k and 3-month CDs with a minimum subscription size of USD 10k). While the intensive margin magnitudes documented in Panel B are smaller (around one half) of the baseline effects documented in Table 2, these estimate still imply large differences between the rates that banks offer to high- and low-income depositors even on the same products. In sum, Table 3 shows that banks do not only seem to offer a more diversified choice within the same product category, but also higher rates on the very same product on the intensive margin.[8]

3.3 Additional Results and Robustness

We provide five sets of additional tests on the baseline results presented in Table 2. First, in Appendix Table A.1 we document economically similar magnitudes when we estimate the baseline specification (1) using Poisson regressions rather than ordinary least squares regressions, thus reducing potential concerns that our baseline estimates may be biased by skewness in the deposit rate distribution or by near-zero rate observations. Second, in Appendix Table A.2 we show that our results are also quantitatively similar when we include zipcode $\times$ branch fixed effects, thus reducing potential bias arising from branches switching zipcodes over time during our sample period.

Third, in Appendix Table A.3 we document similar effects when we focus on *within-county-year* variation in income and rates, suggesting that our baseline results are unlikely to be explained by variation in economic conditions across geographic areas. These results document substantial income-related spreads even within narrowly defined geographic areas, and thus call for a potential reevaluation of widely used measures of deposit market structure such as county-level deposit HHI (e.g., Heitfield, 1999, Biehl, 2002).

Fourth, in Appendix Table A.4 we ask whether our baseline results vary in the cross-section based on individual branches' ability to set their own deposit rates. While we find an effect even when branches' interest rates are set by other branches, our estimates are economically larger when individual branches set their own rates. This finding suggests that individual branches' information about local depositors, as well as their ability to autonomously act upon such information, may be a source of deposit market power. Fifth,

[8] A potential concern with our interpretation of the intensive margin results is that there still may exist quantity variation within subscription/balance brackets. Although this concern cannot be fully ruled out, Argyle et al. (2024) show that depositors have a high propensity to bunch at minimum subscription and balance thresholds, resulting in low within-bracket variation. We also find similar results for relatively narrow subscription/balance brackets, which further reduces this concern.

in Appendix Figure A.1 we document an increasing relationship between banks' deposit interest expense ratios (i.e., deposit interest expense divided by total deposits) and average income across all zipcodes where banks operate, suggesting that not only quoted but also paid deposit interest rates are increasing in depositors' income.

4 Competition in the Banking Sector

The results presented so far are consistent with two main channels. First, a long literature in banking shows that lack of competition and entry barriers allow bank to extract rents from depositors. If local income is correlated with competition in the banking sector (i.e., if banks compete more aggressively with each other in high-income areas), then our results may just be a byproduct of competition rather than driven by income per se. Second, banks may internalize differential participation along the income distribution, and thus implicitly compete with other asset classes to attract funds. While we view these two channels as complements, in this section we show that our baseline results are statistically and economically similar in counties and zipcodes where banks compete aggressively for customers, strongest for relatively small banks that do not offer a wide range of consumer investment products, and insensitive to bank dependence on noninterest income. In sum, our baseline findings cannot be fully attributed to competition within the banking sector.

In Table 4 we start by asking whether our baseline results vary in the cross-section of local deposit market structure. In column (1) of Table 4, we start by showing that our baseline estimates from Table 2 are statistically and economically unchanged when we include zipcode-level deposit HHI as an independent variable in our regression specifications, suggesting that the deposit rates spreads we document between low- and high-income areas are not purely due to contemporaneous correlation with local deposit market structure. In column (2), we also show that our estimates do not appear to vary systematically based on local deposit market concentration: While the sensitivity of deposit rates to income slightly increases in relatively concentrated markets, the estimated interaction term between local income and concentration is economically small and not statistically significant at conventional levels. In the remaining columns of Table 4, we also show that our baseline findings hold in areas with relatively high levels of banking competition, either in terms of deposit concentration (column (3)) or branch presence (column (4)), further providing support to our claim that the documented spreads are not purely driven by banking competition. In Appendix Table A.5 we also document nearly identical results when we rely on more commonly used measures of banking competition at the county-level, suggesting that the findings of Table 4 are not systematically driven by how we define local banking markets.

The results presented in Tables 4 and A.5 provide initial evidence that deposit competition within the banking sector cannot fully rationalize our baseline results. While these tables are informative about deposit competition, banks may also compete more aggressively on deposits to attract wealthy customers and cross-subsidize their fee-generating arms (e.g., private wealth management and brokerage). If this is the case, we should expect our baseline effects to increase in bank size and dependence on fee-generating income. To examine this possibility, we proceed in two steps. In Appendix Table A.6, we start by showing that the documented spreads are decreasing in bank assets, and disappear at the very top of the bank size distribution. For example, columns (4) and (5) show that the deposit rates offered by banks in the bottom nineteen vigintiles of the bank size distribution are around 6.9 times more sensitive to local income than the rates offered by banks in the top 5% of the distribution. Additionally, the correlation between income and the rates offered by very large banks is not statistically significant at conventional levels, consistent with recent evidence on uniform rate setting by major banks (Begenau and Stafford, 2022; Granja and Paixao, 2023) especially in urban areas (d'Avernas et al., 2023).[9]

[9]In Appendix Table A.7, we specifically ask how income spreads vary across geographies for banks of different size. Consistent with d'Avernas et al. (2023) and Oberfield et al. (2024), Table A.7 provides

Second, in Appendix Table A.8 we study whether our baseline results vary in the cross-section based of banks' dependence on noninterest income. In column (1), we test this hypothesis by interacting local income with bank-level noninterest income to interest income. In columns (2) to (4), we break down noninterest income into components most likely to originate from retail customers, namely, fiduciary, product servicing, and brokerage income.[10] We find no evidence of statistically significant interaction effects between local income and these variables, suggesting that cross-subsidization is unlikely to be a main driver of our baseline findings. In sum, our baseline do not appear to be systematically correlated with bank size and dependence on fee-based income. If anything, large banks that rely more on fee-generating activities seem to engage less in income-related price discrimination.

5 Participation in Nondeposit Assets

In this section, we test the hypothesis that banks internalize differential participation in nondeposit assets along the income distribution when pricing their deposit products.

5.1 Participation, Income, and Deposit Rates

We first show that income components related to nondeposit market participation such as net capital gains and interest income are the primary empirical drivers of the observed correlation between total income per capita and local deposit rates. In the first two columns of Table 5, we start by documenting a positive relationship between our two participation measures (*Net Capital Gains to Total Income* and *Interest to Total Income*) and

evidence of larger spreads in relatively less dense areas such as small cities, towns, and rural areas. The table also shows that this geographic variation is driven primarily by banks below the top of the size distribution: We do not observe quantitatively meaningful rate variation across geographies for very large banks.

[10]Fiduciary activities include those rendered by the bank's trust department acting in any fiduciary capacity. Product servicing fees are derived from servicing real estate mortgages, credit cards, and other financial assets. Brokerage fees include fees and commissions from securities brokerage, fees and commissions from annuity sales, underwriting income from insurance and reinsurance activities, and income from other insurance activities.

local deposit rates. The estimated coefficients imply economically large differences in deposit rates along the participation distribution. For example, the point estimate reported in column (1) implies that moving from the bottom to the top decile of the *Net Capital Gains to Total Income* distribution (a 8.8 p.p increase in this participation measure) is associated with a 3.6 bps increase in average deposit rates in our sample, around 9% of the sample median. Similarly, the point estimate reported in column (2) implies that moving from the bottom to the top decile of the *Interest to Total Income* distribution is associated with a 5.5 bps increase in average deposit rates in our sample, around 13.7% of the sample median.

In columns (3) to (5), we also show that the findings reported in columns (1) and (2) are not driven by spurious correlation with other income sources. Columns (3) and (4) document *negative* relationships between *Salaries to Total Income* and local rates and *Other Income to Total Income* and local rates, respectively, confirming that these income components are not responsible for the overall positive correlation between total income and deposit rates documented in our baseline tests. Additionally, columns (4) and (5) show that the economic and statistical significance of the relationships between salaries, other income, and rates completely disappear when we remove common variation by including our participation variables variables in the regression.[11] In contrast, the economic magnitude and statistical significance of our main participation measures remain virtually unchanged relative to the first two columns, confirming that these measures carry orthogonal information about participation in different asset classes.

A potential concern with the results of Table 5 is that as we decompose income into its relative components, we may not control for differential income levels between zipcodes. A related concern is that, if the relationship between income and rates is nonlinear, our participation measures (which are nonlinearly increasing in income) may just pick

[11]Since *Net Capital Gains to Total Income*, *Interest Income to Total Income*, *Salaries to Total Income* and *Other Income to Total Income* sum up to one, we cannot simultaneously include all of these variables in our regression.

up such nonlinearity rather than independent economic content on participation. In Appendix Table A.9, however, we show that our point estimates are economically and statistically similar even after controlling for various continuous proxies of local income, as well as after including income decile fixed effects in our specification and thus estimating our participation regressions *within income brackets*.

The estimates presented in Tables 5 and A.9 carry two implications. First, these estimates provide initial support for our main hypothesis that the income-related spreads documented in the previous tables are due to participation and not to generic correlation between income, rates, and local economic conditions. In other words, for our baseline results to be explained by other correlated variables, these variables would have to be simultaneously correlated with our participation measures and at the same time *orthogonal* to local salaries, wages, and other income components.

Second, the tables provide initial evidence that high-income households receive more favorable deposit offerings because of their participation in nondeposit markets, and not purely because of their income. Put differently, the estimated income spreads do not appear to be inherently related to income, but rather a byproduct of lower participation in nondeposit markets by households with relatively low income. Figure 2 provides additional evidence consistent with this observation: both of our participation measures are increasing in total income, and this positive relationship is particularly stark in high-income buckets (consistent with Fagereng et al., 2019).

5.2 Deposit Flows

We use data on local deposit flows to provide two additional pieces of evidence suggesting that banks internalize differential participation in nondeposit markets along the income distribution. In Figure 3, Panel A, we start by showing that the time series volatility of branch-level deposit quantities is increasing in local participation: it increases from USD 10.50 million to USD 48.89 (126.5) million when moving from the bottom decile to ninth (top) decile of the participation distribution, a 4.7-fold (12-fold) increase. Panel B of Figure 3 shows that this pattern also holds when we compute deposit volatility at the zipcode-level, suggesting that the results of Panel A are unlikely due to more aggressive competition within the banking sector in high-participation areas. Instead, Panel B shows that even at the aggregate zipcode level (and thus conditional on depositor switching banks within the same zipcode), the volatility of the deposit base is increasing in local participation. Appendix Figure A.2 shows similar patterns when we use *Interest to Total Income* as an alternative measure of local participation, thus confirming that the result of Figure 3 is valid regardless of how we measure local participation.

Second, we hypothesize that local deposit growth should be negatively correlated with the performance of nondeposit asset classes, and that this negative correlation should be particularly strong in high-participation areas. For example, an increase in stock market performance may lead to deposit outflows, especially when local households have a higher propensity to participate in the stock market (as in Lin, 2020). In Table 6, we start by using the aggregate U.S. stock market as a reference nondeposit asset class. In columns (1) and (2) of Table 6, we report the estimated coefficient of a regression of year-on-year deposit growth at the branch-level on the cumulative excess return of the market factor (*Ex. Market Return*, from Kenneth French's website), an indicator (*High Participation*) equal to one for zipcodes with above-median levels of *Net Capital Gains to Total Income* and equal to zero otherwise, and on the interaction between these two variables. We find that branch-level deposit growth is strongly negatively correlated with the excess performance of the market: a one standard deviation (13.94 pp) increase in the market factor leads to a 1.1 pp decrease in branch-level deposit growth, around 23.3% of the unconditional branch-level deposit growth in our sample. Consistent with our hypothesis, this negative sensitivity is around 57% higher in high-participation zipcodes than in low-participation zipcodes, suggesting that the deposit base is not only more volatile but also more sensitive to the performance of nondeposit assets in high-participation areas than in low-participation areas.

In columns (3) and (4) of Table 6, we document even larger differences in deposit base sensitivity when we measure deposit growth at the zipcode-level rather than the branch-level. For example, column (3) shows that the same one standard deviation increase in the market factor leads to a 0.5 pp decrease in branch-level deposit growth (around 11% of the sample mean), and that this sensitivity approximately doubles in high participation zipcodes. Consistent with our previous findings, the results of columns (3) and (4) also suggest that the findings in columns (1) and (2) are unlikely to be driven by reallocation within the banking sector, but rather by reallocation between deposits and nondeposit assets.

In Table A.10, we complement the results of Table 6 by studying the sensitivity of deposit growth to the performance of assets that may closely represent local households' investment opportunities. In columns (1) and (2) of Table A.10, we replace the excess return of the market portfolio with the excess return of a value-weighted portfolio of local stocks (i.e., stocks of companies headquartered in the state where bank branches are located, as in Lin and Pursiainen, 2023) as the main independent variable. In columns (3) and (4), we replace the excess return of the market portfolio with the average fraction of local stocks rated "Buy" or "Strong Buy" by analysts during the year as the independent variable, thus measuring the expected (as opposed to realized) performance of local equities during the previous year. In the last two columns of Table A.10 we study local deposits outflows in response to changes in local municipal bond yields. The estimates reported in Appendix Table A.10 largely line up with our main findings in Table 6: branch-level deposit growth is negatively correlated with the performance of local stocks, and this baseline effect is much larger in high-participation zipcodes than in low-participation zipcodes. Additionally, the results of columns (5) and (6) suggest that the main intuition of Table 6 may extend to a large set of nondeposit asset classes other than stocks.

5.3 Participation and the Cross-section of Deposit Products

In Table 7, we test our participation mechanism in the cross-section of deposit products' characteristics. In Panel A of Table 7, we start by asking how the deposit rates that banks offer on checking and savings accounts (column (1)), MMAs and premium MMAs (column (2)), and CDs (column (3)) vary with local participation. Panel A shows that only the rates offered on MMAs and CDs exhibit a positive relationship with local participation, suggesting that the demand for these products is relatively elastic, especially in high-participation areas. In contrast, we do not observe a relationship between participation and the rates offered on callable deposit products such as savings and checking accounts, which confirms previous evidence on deposit demand stickiness across products (see, e.g., Driscoll and Judson, 2013; Egan et al., 2022a; Bisetti and Karolyi, 2024).

Next, we test two sets of related hypotheses about deposit flows and rates along the term structure. Households typically buy and hold nondeposit assets for multiple years (e.g., Van Binsbergen, 2021, Greenwald et al., 2023), and this behavior is particularly pronounced for high-income households (Catherine et al., 2023). We first hypothesize that, as a result, long-maturity deposits should represent a closer substitute to nondeposit assets than short-maturity deposits, and thus that their pricing should be more sensitive to the performance of nondeposit assets. For example, everything else equal, banks' supply of long-term CDs should be more negatively affected by an increase in stock market performance than that of short-term CDs. In Appendix Table A.11, we provide evidence consistent with this first hypothesis: using FDIC Call Report data on banks' stocks of time deposits of different maturities, we confirm that the growth in long-maturity time deposits is more sensitive to the performance of the aggregate stock market than that of short-maturity time deposits.[12]

If banks internalize households' differential participation incentives along the term

[12] The SOD data does not contain disaggregated data at the product or maturity level, which prevents us from performing this test at the branch-level.

structure, we may also expect the term structure of deposit rates to be steeper in high-participation areas than in low-participation areas. Our second hypothesis is then that banks offer lower premia for short-maturity products (which are less sensitive to the performance of nondeposit assets), and larger premia for long-maturity products (which are more sensitive). In Panel B of Table 7, we provide evidence consistent with this second hypothesis: branch-level term spreads between 3 month CDs and 12, 24, and 36 month CDs are steeper in high-participation than in low-participation zipcodes.

By focusing on rate differences between long- and short-maturity products with the same minimum subscription size offered by the same branch at the same point in time, the estimates presented in Panel B of Table 7 also allow us to remove any variation in interest rate *levels* from our estimates. As a result, these estimates allow us to rule out potential competing explanations able to rationalize differences in average interest rate levels across the participation distribution, but not differences in term structure slopes conditional on levels.

5.4 Capital Gains Taxes and Participation Incentives

A possible concern is that our reduced-form estimates may be affected by OVB. Specifically, local households' and local banks' asset returns are likely to be positively correlated. If in response to an increase in their return on assets banks increase deposit rates to attract funding, then the relationships documented in Table 5 are not driven by our proposed participation mechanism but rather by a change in deposit supply. In sum, local economic conditions affecting banks' deposit supply are a source of OVB, and we would expect such OVB to inflate the estimated coefficients in our baseline regressions.

In this section, we exploit time series variation in top earners' capital gains taxes as a source of quasi-exogenous variation in participation incentives. Our hypothesis is that increases in high earners' state tax rates may decrease their marginal propensity to participate in nondeposit markets, and increase their marginal propensity to keep funds in deposit products (which are unaffected by these taxes). In contrast, low earners' incentives to participate in nondeposit markets should be relatively unaffected by these tax rate changes. If banks internalize changes in participation incentives by high earners, we may also observe that capital gains tax changes lead to changes in deposit spreads along the participation distribution. At the same time, banks' deposit supply should *not* be affected by top earners' capital gains taxes, which should reduce our main OVB concern.

We test our hypotheses by estimating the following two-stage OLS model:

$$\hat{NCG}_{z(s)t} = \tilde{\alpha} + \tilde{\beta} CG\ Tax_{st} + \tilde{\gamma}_{FE} + \epsilon_{z(s)t}, \tag{2}$$

$$d_{ipb(z)t} = \alpha + \beta \hat{NCG}_{z(s)t} + \gamma_{FE} + \varepsilon_{ipb(z)t}, \tag{3}$$

where (2) is the first stage and (3) is the second stage. In the first stage, *NCG* is *Net Capital Gains (NCG) to Total Income* in zipcode z and year t, and *CG Tax* is the state capital gains tax on top income earners in state s and year t. The second stage (3) is identical to our baseline regression model (1), with the exception that *NCG to Total Income* is instrumented by state-level capital gains taxes. In all of our estimates we include zipcode fixed effects, thus focusing on within-state variation in state taxes over time. As in our baseline specifications, we cluster standard errors at the zipcode-level.

In Table 8, column (1), we present the results of the first stage, where we regress the state-level capital gains tax rate on *NCG to Total Income*. This column documents a negative relationship between state-level capital gains taxes and net capital gains: a one standard deviation increase in state-level capital gains tax rates is associated with a 0.4 pp reduction in *NCG to Total Income*, around 8.6% of the sample mean and 6.5% of the sample standard deviation. Appendix Table A.12 also confirms that these estimates are disproportionately larger in high participation zipcodes, thus providing an additional piece of evidence to support the instrument's validity. In column (2), we present the results of the second stage, where we regress *NCG to Total Income* instrumented by the state tax rate on capital gains on branch-level deposit APYs. Consistent with our main hypothesis, column (2) reports a positive and statistically significant correlation between rates and our instrumented participation measure.

One possible concern is that capital gains taxes may affect not only local households' participation incentives, but also their incentives to *realize* capital gains. To mitigate this concern, in columns (3) and (4) we use *Interest to Total Income* as a second measure of participation. Our hypothesis is that, if the results of the first two columns of Table 8 are driven by household incentives to realize capital gains, we would expect *Interest to Total Income* and capital gains taxes to be uncorrelated (or even positively correlated if an increase in capital gains taxes induces households to keep their funds in interest-bearing products instead of realizing the gains). Conversely, if the results of the first two columns are due to lower participation incentives, and if accordingly an increase in capital gains taxes deters households from purchasing interest-bearing products that may entail capital gains, we would expect *Interest to Total Income* to be negatively correlated with capital gains taxes in the first stage.

Column (3) shows that increases in state-level capital gains taxes are negatively associated with *Interest to Total Income*, thus providing support for the participation hypothesis. As in column (1), the magnitude of the estimated relationship between capital gains taxes and *Interest to Total Income* is economically large: a one standard deviation increase in state capital gains taxes is associated with a 0.23 pp decrease in *Interest to Total Income*, around 14.6% of the sample mean and 17.6% of the sample standard deviation. Since deposit products are typically unaffected by capital gains taxes, column (3) also mitigates possible concerns that *Interest to Total Income* may be mainly comprised of deposit interest income. Instead, the results of column (3) suggest that variation in *Interest to Total Income* variable can be largely attributed to interest from nondeposit fixed-income products such as corporate bonds and treasuries, thus confirming recent evidence in Smith et al. (2023) on household portfolio composition. Consistent with column (2), the point estimate for the second stage in column (4) also shows that *Interest to Total Income* instrumented by capital gains taxes can explain substantial variation in local deposit rates.

The results presented in columns (1) to (4) of Table 8 are generally consistent with the interpretation that our baseline findings are driven by a participation channel. However, there are still potential OVB concerns if the observed changes in state-level taxes are correlated with the local economic conditions of some areas within the state, and if, at the same time, these different economic conditions are associated with different deposit-borrowing behavior by local banks. Continuing with our illustrative example, suppose that some zipcodes experience periods of high economic growth and loan demand, and that as a response local banks increase deposit interest rates in order to attract funding. If state taxes are positively correlated with economic growth in these zipcodes, then the results documented in columns (1) to (4) of Table 8 may be due to increased deposit supply by banks, not by participation.

In columns (5) and (6), we provide the results of tests aimed at further mitigating these concerns. First, the data provides no evidence of a relationship between *Salaries to Total Income* and state-level tax rates (column (5)): a one standard deviation increase in capital gains taxes is associated with a small 0.088 pp increase in *Salaries to Total Income*, around 0.13% of the sample mean and 0.8% of the sample standard deviation. Importantly, this point estimate is not statistically different from zero at conventional levels.

The second-stage estimates in column (6) also show no correlation between *Salaries to Total Income* instrumented by capital gains tax rates and local deposit rates, with an *F*-statistic for instrument underidentification well below the rule-of-thumb value of ten. In sum, the results presented in columns (5) and (6) suggest that for our illustrative endogeneity example (or any other similar example) to be the main driver of the findings in columns (1) to (4), local economic growth correlated with state-level taxes would have to be systematically correlated with local capital gains and interest income, but orthogonal to salaries. More generally, these results confirm that any alternative channel would have to jointly explain a strong correlation between local rates and our participation measures, and lack of correlation between local rates and other income sources.

In the Internet Appendix, we provide two sets of robustness tests on the results of Table 8. In section A.I.1, we estimate meta-regressions in the cross-section of deposit products to formally quantify OVB in our instrumental variable estimation (Pancost and Schaller, 2024). Our estimates show that OVB is likely to be negligible in our setting, and that the most likely reason why the estimated coefficients in Table 8 are relatively large (Jiang, 2017) is that *NCG to Total Income* and *Interest to Total Income* are imperfect proxies of participation.

In Internet Appendix A.I.2, we also complement the 2SLS analysis with dynamic (stacked) difference-in-differences tests (e.g., Cengiz et al., 2019), where we study how the sensitivity of rates to income changes once a state experiences a large decline in capital gains taxes for the first time in our sample. Consistent with our 2SLS evidence, we find that lower tax rates increase the sensitivity of local rates to income, supporting the interpretation that changes in state taxes have an impact on households trade-off between deposits and other financial products, and that this trade-off is internalized by banks when pricing their deposits. We provide additional details in Internet Appendix A.I.2.

In Appendix Section A.II, we also exploit cross-sectional variation in participation incentives arising from geographic variation in broker misconduct during the financial crisis (Egan et al., 2019, 2022b). We show that broker misconduct during the crisis is associated with lower subsequent participation by local households and that participation instrumented by broker misconduct is positively correlated with local deposit interest rates, thus providing additional supporting evidence for our main mechanism.[13]

[13]This analysis comes from a different sample and excludes many areas for which brokerage misconduct data is unavailable. As a result, we interpret the evidence from these cross-sectional tests as suggestive and complementary to the main analysis presented in this section.

6 Implications

6.1 Participation and Deposit Market Power

A first implication of our findings is that lack of participation should be a substantial source of deposit market power. To test this implication, we ask to what degree local participation is able to explain cross-sectional variation in local deposit betas—a comprehensive measure of deposit market power based on the degree to which banks pass through changes in statutory interest rates to deposit rates (Drechsler et al., 2021).

We proceed in two steps. First, we estimate deposit betas for each zipcode in our dataset as the slopes of time series regressions of year-on-year changes in zipcode-level average interest rates on year-on-year changes in the target Fed funds rate. In Figure 4, we then produce bin scatter plots of these estimated deposit betas as functions of average participation in each zipcode in our sample. Importantly, in these plots we residualize betas and participation with respect to zipcode-level deposit HHI and number of bank branches, so that we capture variation in deposit market power orthogonal to local banking market structure.[14]

Figure 4 presents our results. In Panel A, we document a positive relationship between participation (as measured by *NCG to Total Income*) and deposit betas, implying a negative relationship between participation and local deposit market power. Panel A shows that a shift from the bottom to the top bucket of the *NCG to Total Income* distribution in our sample is associated with a change in local deposit betas of nearly 0.04, around 16% of the average deposit beta in our sample and around 44% of a standard deviation. Panel B documents similar economic magnitudes when shifting from the bottom to the top bucket of the *Interest Income to Total Income* distribution.

The relationships reported in Panels A and B of Figure 4 are estimated conditional

[14]The potential concern is that participation may be systematically positively correlated with local banking market structure, and that the results may therefore be driven by deposit market structure rather than by participation. While Appendix Figure A.3 shows that this is unlikely to be the case„ we formally residualize betas and participation with respect to deposit market structure in our tests.

on local deposit HHI and number of bank branches, suggesting that variation in participation orthogonal to local banking market structure explains a large fraction of local deposit market power. For comparison, in Panels C and D of Figure 4 we produce bin scatter plots of local deposit betas on deposit HHI and on the number of local branches, respectively. These two panels show that moving from the top to the bottom decile of the HHI distribution (Panel C) and from the bottom to the top decile of the branch count distribution (Panel D) is associated with quantitatively similar changes in deposit betas to those documented in Panels A and B. In sum, Figure 4 shows that variation in participation orthogonal to banking market structure is able to explain a similar amount of variation in local deposit market power as banking market structure alone.

6.2 Inflation Inequality and Real Deposit Rates

Mounting evidence suggests that consumers face differential inflation along the income distribution (e.g., Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019; Argente and Lee, 2021). In this section, we combine our findings with those in this literature to study how *real* deposit rates vary in the cross section of income and how nominal deposit spreads and inflation differentials quantitatively contribute to this variation. To do so, we obtain NielsenIQ homescan data on quantities and prices paid by individual households on a wide range of consumer products. We aggregate this data at the zipcode level using total quantities and average prices across all participating households living in the zipcode. Following Jaravel (2019), we then construct zipcode-year Törnqvist inflation measures for the average household in the zipcode. Finally, we compute real deposit rates at the branch-product-year level as the nominal rates in our main panel minus the zipcode-year level Törnqvist index. Appendix A.III provides more details.

In Figure 5, we show that inflation inequality along the income distribution compounds with nominal deposit spreads to generate substantial variation in real spreads. This figure shows that, while the difference in nominal deposit rates between the top and bottom deciles of the income distribution is around 0.13 pp, this difference almost triples to 0.38 pp when we study differences in real deposit rates. In other words, differences in nominal deposit rates are quantitatively meaningful relative to inflation inequality, and are able to explain more than 30% of the total income-related variation in real rates observed in the data.

7 Conclusions

This paper shows that banks offer systematically lower deposit rates (even on the same product) in low-income areas than in high-income areas. These spreads are not driven by deposit competition within the banking sector, or by mechanisms related to income per se. Rather, they appear to stem from banks internalizing differential participation along the income distribution. Consistent with this hypothesis, we find that deposit quantities are more volatile and sensitive to the performance of nondeposit assets in high-income areas than in low-income areas. Quasi-exogenous variation in participation incentives from state-level capital gains taxes and from local broker misconduct support a causal interpretation of our findings. When combined with inflation inequality, our findings suggest that a disproportionately large portion of the negative real interest rates that we observe in the data are borne by households in the lower end of the income distribution.

The fact that we observe participation-related spreads in bank deposits, arguably the simplest financial product available to consumers, suggests potentially large price differences in more complex financial products such as pension plans, credit cards, and consumer loans. In our opinion, studying the extent to which the financial industry internalizes consumers' income and access to substitute products in these products represents a potentially fruitful avenue for future research.

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Figure 1

Residualized Rates by Income Deciles

This figure provides visual evidence of cross-sectional differences in the average deposit rates offered by banks as functions of local income. In Panel A, we residualize deposit rates with respect to year fixed effects and plot the average regression residuals in ten income deciles based on the annual distribution of *Per Capita Income* across zipcodes. In Panel B, we residualize deposit rates with respect to bank-product-year fixed effects and plot the average regression residuals in the same income deciles. All the variables are defined as in Table 1. The sample period is 2004-2020.

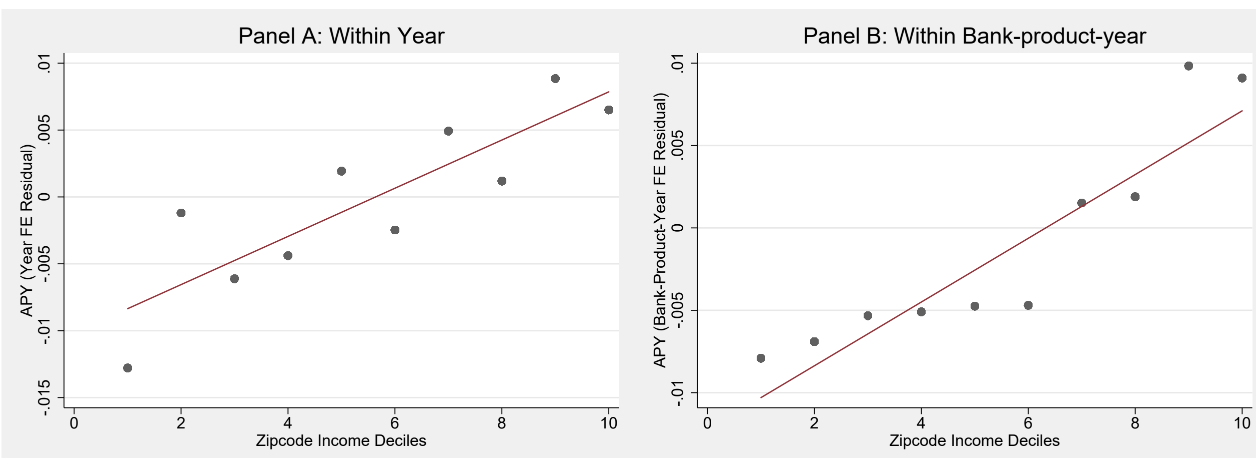

Figure 2

Participation by Income Deciles

In this figure, we study differential participation along the income distribution. In Panel A, we plot average *Net Capital Gains to Total Income* in ten income deciles based on the annual distribution of *Per Capita Income* across zipcodes. In Panel B, we plot average *Interest to Total Income* in the same income deciles. All the variables are defined as in Table 1. The sample period is 2004-2020.

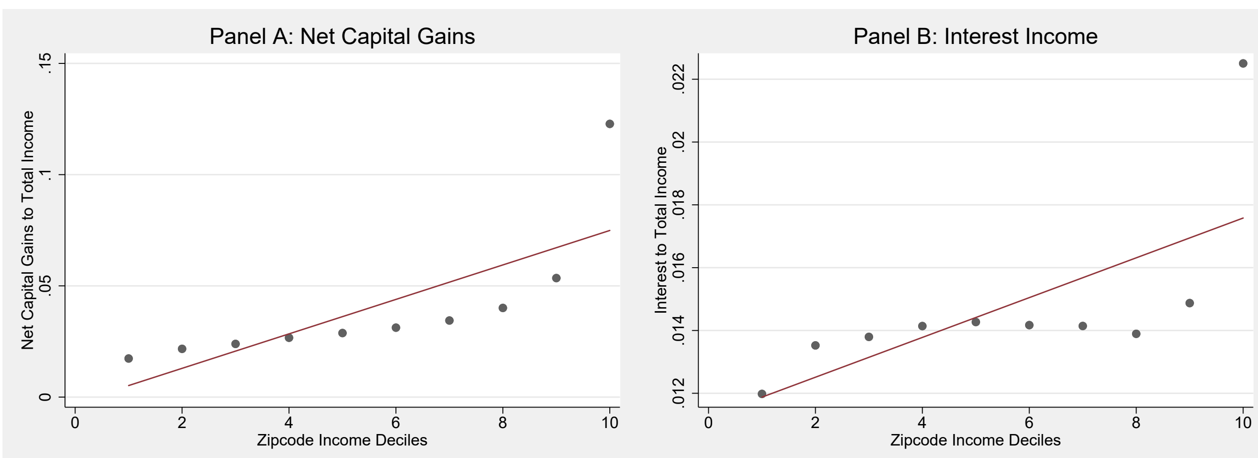

Figure 3

Participation and Deposit Base Volatility

In this figure, we study how the volatility of the deposit base varies as a function of local participation. In Panel A, we first compute the time series volatility of total deposits for each branch in the SOD data for the 1994-2023 period, and then plot branch-level deposit volatility (in USD millions) in ten deciles based on the unconditional distribution of average *Net Capital Gains to Total Income* across zipcodes and years for the 2004-2020 period (as in our main sample). In Panel B, we repeat the same exercise by first aggregating total deposits at the zipcode-year level, and then computing the time series volatility of total deposits for each zipcode. The data comes from the FDIC SOD for the 1994-2023 period, and the IRS-SOI for the 2004-2020 period.

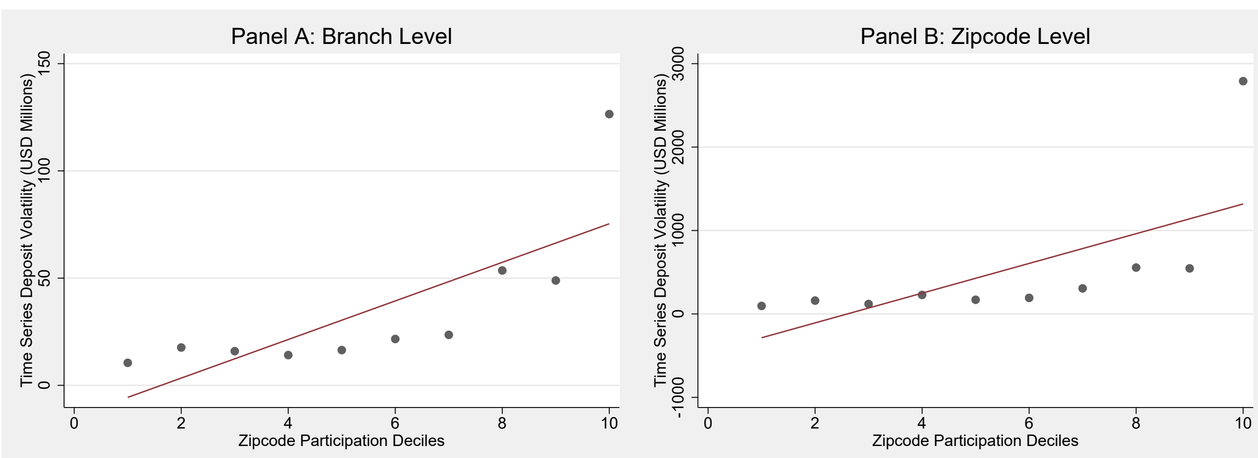

Figure 4

Participation and Deposit Market Power

In this figure, we ask whether cross-sectional variation in local participation helps explain bank deposit market power. We first follow Drechsler et al. (2021) and estimate local deposit market power using deposit betas—the slope of a regression of year-on-year changes in zipcode-level average interest rates on year-on-year changes in the target Fed funds rate. In Panels A and B, we produce bin scatter plots of the estimated deposit betas on average participation for each zipcode in our sample. We remove variation in deposit betas correlated with banking market structure by first orthogonalizing deposit betas and participation with respect to zipcode-level deposit HHI and number of bank branches. In Panel A, we use *Net Capital Gains to Total Income* as a proxy of participation. In Panel B, we use *Interest Income to Total Income*. In Panels C and D, we repeat the same exercise by plotting deposit betas on average deposit HHI and average number of branches in the zipcode throughout our sample period, respectively. Data on the target Fed funds rate comes from the Federal Reserve of St. Louis' website. The sample period is 2004-2020.

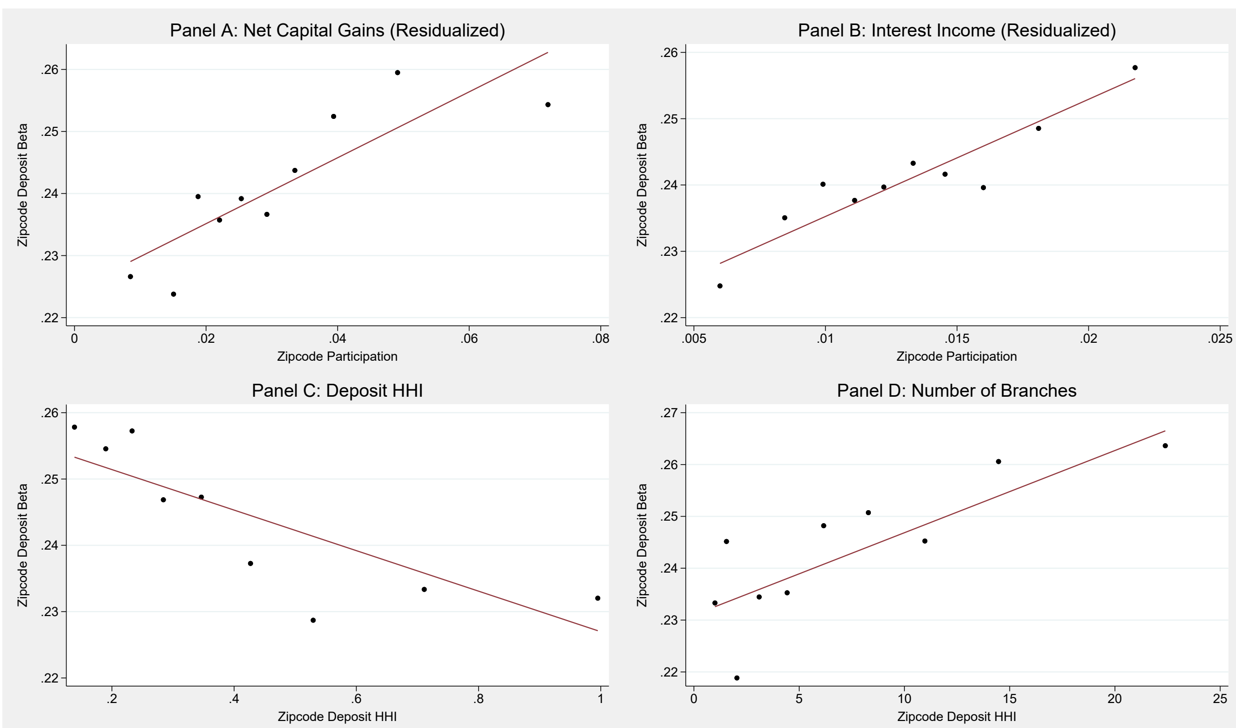

Figure 5

Inflation Inequality and Real Spreads

In this figure, we study how nominal spreads in deposit rates along the income distribution combined with inflation inequality jointly determine spreads in real deposit rates. We first compute average APYs at the zipcode-product-year level. We combine these APYs with zipcode-year level Törnqvist measures of inflation, whose construction we detail in Appendix Section A.III. We then study how nominal and real APYs vary with local income by assigning zipcodes to ten deciles of income within each year, and computing average nominal and real APYs across all branches and products in each decile. In the figure, we plot the time series average nominal and real APYs in each of these buckets. To compare spreads (i.e., rate differences between low- and high-income areas) across nominal and real APYs, we normalize both variables by subtracting their average across buckets. The sample period is 2004-2020.

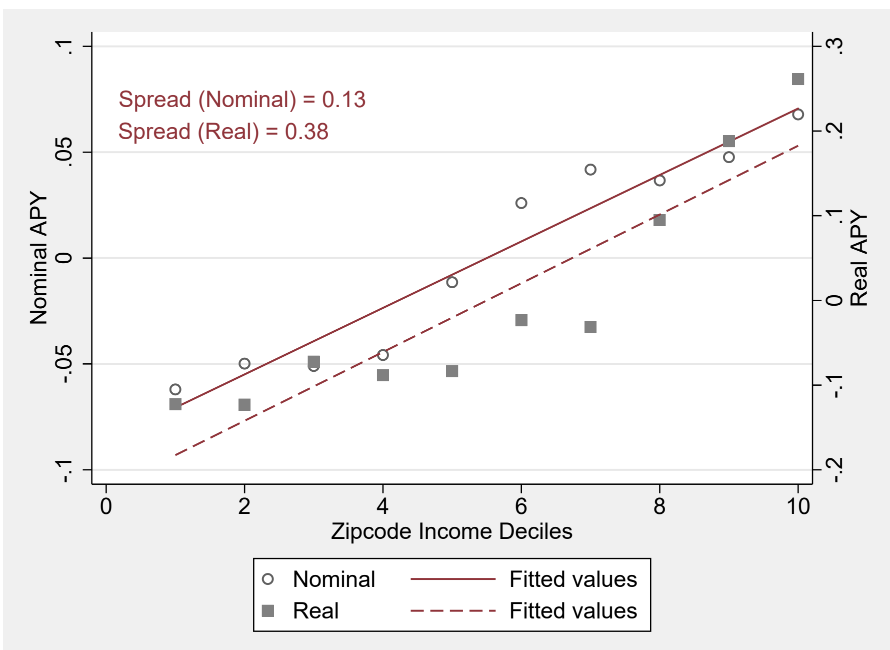

Table 1

Summary Statistics

This table provides summary statistics for the main variables in our paper. *Deposit Product APY* is the annualized percentage yield for six broad deposit categories available in RateWatch, namely, CDs, regular and premium MMAs, interest-bearing checking accounts, savings accounts, and special products. We compute these rates as branch-year-level averages of weekly rates for each product category. *Number of Products* is the average number of subproducts offered within each product category. *Minimum Subscription Size* is the average minimum subscription size for each subproduct. *CD Maturity* is the average maturity across all CDs offered by the branch. *CD Term Spreads* are term spreads between CDs with different maturities (e.g., 3 and 12 months) and a minimum subscription size of USD 10k. *Per Capita Income* is adjusted gross income from the IRS-SOI (item a00100) divided by the number of returns at the zipcode level (item n1). *Net Capital Gains (NCG) to Total Income*, *Interest to Total Income*, and *Salaries to Total Income* are SOI items a01000, a00300, and a00200, respectively, all normalized by SOI item a00100. *Other Income to Total Income* is total adjusted gross income minus net capital gains, interest income, and salaries, all normalized by total adjusted gross income. *Deposit HHI* is the deposit Herfindahl-Hirschman index, calculated at the zipcode-level and at the county-level using branch deposits from the FDIC SOD. *Deposit Growth* is the June-to-June log difference in deposits at the branch-level or at the zipcode-level. *State Rate, Long Gains* is the state-level tax rate on capital gains for top earners, publicly available on the NBER website. *Local Deposit Beta* is the estimated slope coefficient of a regression of year-on-year changes in average deposit rates at the zipcode-year level on year-on year changes in the target Fed funds rate. *Real APY* is equal to *Deposit Product APY* minus the Törnqvist inflation rate, calculated at the zipcode-level as described in Appendix Section A.III. The sample period is 2004-2020.

	Mean	SD	p10	p50	p90	Observations
Deposit Product APY	0.90	1.18	0.05	0.40	2.67	629,452
Number of Subproducts	22.93	41.03	1.02	8.62	62.00	629,452
Minimum Subscription Size	50.50	43.85	2.03	40.80	113.78	553,728
CD Maturity	23.13	4.01	17.65	23.83	26.45	131,722
12–3 Months CD Term Spread	0.50	0.44	0.10	0.40	1.06	120,559
24–3 Months CD Term Spread	0.75	0.48	0.21	0.66	1.37	116,283
36–3 Months CD Term Spread	0.96	0.54	0.30	0.88	1.70	111,860
Per Capita Income	64.88	65.99	34.87	50.11	98.15	629,452
NCG to Total Income	4.63	6.11	0.84	2.88	9.72	629,452
Interest to Total Income	1.58	1.31	0.46	1.23	3.06	629,452
Salaries to Total Income	68.26	10.96	54.84	70.32	79.30	629,452
Other Income to Total Income	25.53	7.71	17.07	24.66	34.90	629,452
Zipcode Deposit HHI	0.41	0.28	0.15	0.31	1.00	625,014
County Deposit HHI	0.20	0.13	0.09	0.16	0.36	628,068
Branch Deposit Growth	0.05	0.30	-0.12	0.03	0.22	221,181
Zipcode Deposit Growth	0.05	0.26	-0.09	0.03	0.19	126,630
State Rate, Long Gains	4.86	2.92	0.00	5.07	7.98	629,452
Local Deposit Beta	0.25	0.09	0.15	0.25	0.35	542,752
Real APY	-0.96	4.58	-5.67	-0.76	3.54	416,829

Table 2

Deposit Interest Rates and Local Income

This table presents the results of estimating the baseline specification (1) in our branch-product-year panel using increasingly stringent combinations of fixed effects. The dependent variable is the average APY offered by a branch on a given deposit product and year. The independent variable is the natural logarithm of *Per Capita Income* at the zipcode-year level. All variables are defined as in Table 1, and the sample period is 2004-2020.

	Dep. Variable: Deposit Product APY				
	(1)	(2)	(3)	(4)	(5)
log(Per Capita Income)	0.121***	0.129***	0.128***	0.136***	0.015***
	(0.016)	(0.015)	(0.013)	(0.014)	(0.002)
Year FE	Yes	Yes	Yes	Yes	No
Zipcode FE	Yes	Yes	Yes	No	No
Product FE	No	Yes	Yes	No	No
Bank FE	No	No	Yes	No	No
Bank × Product FE	No	No	No	Yes	No
Zipcode × Product FE	No	No	No	Yes	No
Bank × Product × Year FE	No	No	No	No	Yes
R-Squared	0.406	0.740	0.751	0.824	0.977
Observations	629,391	629,391	629,384	621,409	244,894

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 3

Product Characteristics and Local Income

In this table, we study how the baseline estimates documented in Table 2 vary on the extensive and intensive margins. In Panel A, we study the relationship between local income and deposit product characteristics: the dependent variables in columns (1) to (3) is the natural logarithm of *Number of Subproducts*, *Minimum Subscription Size* and *CD Maturity*, respectively. We estimate these regressions using our preferred combination of fixed effects from Table 2. In Panel B, we switch to a panel of granular subproducts, namely, savings deposits with minimum subscription size of USD 2k; checking accounts without minimum subscription size and with minimum subscription size of USD 2.5k; MMAs with minimum subscription size of USD 2.5k, 10k, 25k, and 100k; and CDs with maturities of 3, 6, 12, 24, and 36 months and minimum subscription size of USD 10k and 100k. The sample period is 2004-2020.

Panel A: Extensive Margin

	N. of Subproducts	Min. Subscription Size	CD Maturity
	(1)	(2)	(3)
log(Per Capita Income)	0.081***	0.103***	0.012*
	(0.019)	(0.030)	(0.007)
Year FE	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes
R-Squared	0.899	0.793	0.824
Observations	621,409	547,231	130,464

Panel B: Intensive Margin

	Dep. Variable: Deposit Subproduct APY				
	(1)	(2)	(3)	(4)	(5)
log(Per Capita Income)	0.063***	0.061***	0.072***	0.075***	0.007***
	(0.017)	(0.016)	(0.014)	(0.015)	(0.002)
Year FE	Yes	Yes	Yes	Yes	No
Zipcode FE	Yes	Yes	Yes	No	No
Subproduct FE	No	Yes	Yes	No	No
Bank FE	No	No	Yes	No	No
Bank × Subproduct FE	No	No	No	Yes	No
Zipcode × Subproduct FE	No	No	No	Yes	No
Bank × Subproduct × Year FE	No	No	No	No	Yes
R-Squared	0.525	0.744	0.761	0.839	0.975
Observations	1,505,878	1,505,878	1,505,877	1,490,925	558,527

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 4

Banking Market Structure

In this table, we study the interaction between our baseline findings and local banking market structure. In column (1), we control for zipcode-level deposit HHI (*Dep. HHI*) in our main specification. In column (2), we interact local income with *High Dep. HHI*, an indicator equal to one if zipcode-level deposit HHI is above the sample median in a given year, and equal to zero otherwise. In column (3), we report our findings when we estimate our baseline regression in the bottom quartile of the deposit HHI distribution in each year. In column (4), we report our findings when we estimate our baseline regression in the top quartile of the zipcode-level branch count distribution in each year. All the deposit and branch data come from the FDIC SOD. The sample period is 2004-2020.

	Full Sample		Competitive Zipcodes	
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.137***	0.135***	0.106***	0.101***
	(0.014)	(0.015)	(0.018)	(0.020)
Dep. HHI	0.035*			
	(0.019)			
High Dep. HHI		-0.013		
		(0.043)		
log(Per Capita Income) × High Dep. HHI		0.006		
		(0.011)		
Year FE	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes	Yes
R-Squared	0.824	0.824	0.835	0.824
Observations	617,056	619,039	281,695	330,259

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 5

Income, Participation, and Local Deposit Rates

In this table, we break down local income into its various components to study their individual contribution to our baseline findings. In Panel A, column (1), we use *Net Capital Gains (NGC) to Total Income* as the independent variable. In column (2), we perform a similar exercise but use *Interest to Total Income* as the independent variable. In columns (3) to (6), we perform two sets of placebo tests where we use *Salaries to Total Income* and *Other Income to Total Income* as the main independent variables, both in isolation and while controlling for *NCG to Total Income* and *Interest to Total Income*. The data used to construct the independent variables comes from the IRS-SOI, and all the variables are defined as in Table 1. The sample period is 2004-2020.

	Dep. Variable: Deposit Product APY					
	(1)	(2)	(3)	(4)	(5)	(6)
NCG to Total Income	0.00387***				0.00376***	0.00373***
	(0.0004)				(0.0005)	(0.0004)
Interest to Total Income		0.02146***			0.02053***	0.02053***
		(0.0029)			(0.0029)	(0.0028)
Salaries to Total Income			-0.00146***		0.00006	
			(0.0003)		(0.0003)	
Other Income to Total Income				-0.00095***		0.00004
				(0.0003)		(0.0003)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.824	0.824	0.824	0.824	0.824	0.824
Observations	621,409	621,409	621,409	621,409	621,409	621,409

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 6

Aggregate Stock Market Performance and Deposit Outflows

In this table, we study whether the sensitivity of local deposit outflows to the performance of the aggregate stock market varies across zipcodes with different levels of participation. In columns (1) and (2), we regress year-on-year deposit growth at the branch level on the year-on-year cumulative excess return of the market factor (*Ex. Market Return*), on an indicator equal to one for zipcodes with above-median levels of average *Net Capital Gains to Total Income* (*High Participation*), and on the interaction between these two variables. In columns (3) and (4), we repeat the same exercise using zipcode-level deposit growth as the dependent variable. Deposit growth is the log-difference in annual total deposits at the branch (or zipcode) level, measured at the end of each June in the SOD. The cumulative excess return of the market factor is calculated as the June-to-June cumulative return of the monthly market factor, minus the June-to-June cumulative return of the risk-free rate. The data comes from Kenneth French's website. The sample period is 2004-2020.

	Branch Dep. Growth		Zipcode Dep. Growth	
	(1)	(2)	(3)	(4)
Ex. Market Return	-0.079***		-0.037***	
	(0.005)		(0.005)	
High Participation	0.041***			
	(0.011)			
Ex. Market Return × High Participation	-0.046***	-0.045***	-0.035***	-0.035***
	(0.007)	(0.007)	(0.008)	(0.008)
Year FE	No	Yes	No	Yes
Branch FE	Yes	No	No	No
Zipcode FE	No	No	Yes	Yes
Branch × Zipcode FE	No	Yes	No	No
R-Squared	0.123	0.155	0.097	0.126
Observations	221,084	220,909	126,604	126,604

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 7

Participation and the Cross-section of Deposit Products

In this table, we provide evidence for our participation mechanism from the cross-section of deposit products. In Panel A, we study how the deposit rates offered on checking and savings accounts (column (1)), money market accounts and premium money market accounts (column (2)), and certificates of deposits (column (3)) vary with local participation. Column (3) includes only one product category, and bank × product and zipcode × product fixed effects are therefore equivalent to bank and zipcode fixed effects, respectively. In Panel B, we ask how branch-level CD term spreads vary with local participation. For each branch in our sample, we construct a branch-year panel with information on the term spreads between long- and short-maturity CDs offered at the branch. We take the yield on 3-month CDs with minimum subscription size of USD 10k as the baseline. In columns (1) to (3), we respectively subtract this yield from the yield on 12-month, 24-month, and 36-month CDs with minimum subscription size of USD 10k, and regress the resulting term spreads on *Net Capital Gains to Total Income*. The sample period is 2004-2020.

Panel A: Product Breakdown

	Checking and Savings	Money Market Acc.	CDs
	(1)	(2)	(3)
NCG to Total Income	-0.0004	0.0046***	0.0066***
	(0.0004)	(0.0008)	(0.0006)
Year FE	Yes	Yes	Yes
Bank (× Product) FE	Yes	Yes	Yes
Zipcode (× Product) FE	Yes	Yes	Yes
R-Squared	0.734	0.789	0.959
Observations	255,735	161,048	130,464

Panel B: CD Term Spreads

	12–3 Months	24–3 Months	36–3 Months
	(1)	(2)	(3)
NCG to Total Income	0.176***	0.131**	0.105*
	(0.054)	(0.057)	(0.063)
Year FE	Yes	Yes	Yes
Zipcode FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
R-Squared	0.696	0.697	0.719
Observations	119,221	114,926	110,454

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 8

State Capital Gains Taxes: 2-Stage Least Squares

In this table, we report estimates of the 2-stage least squares system of equations (2)-(3). In column (1), we report the results of the first stage, where we regress state-level tax rates on capital gains for top income earners (*State Rate, Long Gains*) on *NCG to Total Income*. In column (2), we present the results of the second stage, where we regress *NCG to Total Income* instrumented by *State Rate, Long Gains* on local deposit APYs (similar to our main regressions). In columns (3) and (4), we repeat the same exercise using *Interest to Total Income* as a measure of participation. In columns (5) and (6), we present the results of a placebo test where we instead use *Salaries to Total Income* as the independent variable in the first stage and dependent variable in the second stage. Columns (2), (4), and (6) report the Kleibergen-Paap Wald F-statistic for weak identification of the instrument. The data on state-level tax rates for top income earners comes from the NBER website. The sample period is 2004-2020.

	Net Capital Gains		Interest		Salaries	
	(1)	(2)	(3)	(4)	(5)	(6)
State Rate, Long Gains	-0.137***		-0.079***		0.030	
	(0.039)		(0.017)		(0.049)	
NCG to Total Income		0.636***				
		(0.196)				
Interest to Total Income				1.095***		
				(0.162)		
Salaries to Total Income						-2.888
						(4.797)
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic		12.438		21.676		0.380
Observations	629,384	629,384	629,384	629,384	629,384	629,384

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Internet Appendix for "Poverty Spreads in Deposit Markets"

Intended for online publication only

A.I State-level Taxes: Robustness

A.I.1 Instrumental Variable Meta-regressions

In this section, we follow Pancost and Schaller (2024) to estimate the degree to which our state-level tax rate instrumental variable (IV) estimation exercise is affected by omitted variable bias (OVB) and classical measurement error (ME). In this paper we are mainly concerned about OVB, which could affect the interpretation of our results. We are less concerned about ME, as our main goal is to document a participation mechanism rather than estimating its exact quantitative impact on deposit rates.[A.1]

To perform the meta-regression analysis of Pancost and Schaller (2024), we start by separately estimating six ordinary least squares (OLS) and IV models across the six deposit product categories in our data (indexed by p). We then estimate the meta-regression

$$\hat{\beta}_p^{OLS} = \hat{a} + \hat{b}\hat{\beta}_p^{IV} + \nu_p, \tag{A.1}$$

where $\hat{\beta}_p^{OLS}$ is the OLS slope coefficient from regressing average deposit rates offered on product category p at the branch-year level on zipcode-year-level participation (measured either by *Net Capital Gains to Total Income* or by *Interest Income to Total Income*); β_p^{IV} is the IV slope coefficient from estimating the same regressions while instrumenting participation with state-level capital gains taxes; and ν_p is a regression error term. As in Table 8 in the main paper, both the OLS and the IV regressions are estimated while including zipcode- and bank-level fixed effects.

The meta-regression coefficients $\hat{a}$ and $\hat{b}$ are informative about potential OVB in the IV regression, and about ME in participation. Specifically, it can be shown that

$$\hat{b} \equiv \frac{cov\left(\hat{\beta}_p^{OLS}, \hat{\beta}_p^{IV}\right)}{var\left(\hat{\beta}_p^{IV}\right)} = \tau^2\hat{\rho}^2, \tag{A.2}$$

$$\hat{a} \equiv \mathbb{E}\left[\hat{\beta}_p^{OLS}\right] - \hat{b}\mathbb{E}\left[\hat{\beta}_p^{IV}\right] = \mathbb{E}\left[\gamma_p\right] + \tau^2\left(1-\hat{\rho}^2\right)\mathbb{E}\left[\hat{\beta}_p^{IV}\right], \tag{A.3}$$

[A.1] We are also aware that, absent high-frequency measures at the household level, *Net Capital Gains to Total Income* and *Interest Income to Total Income* are likely to be noisy measures of participation.

where τ^2 captures ME in our participation measures; $\mathbb{E}\left[\gamma_p\right]$ captures average OVB across the p product-level regressions; and $\hat{\rho}^2$ is the coefficient of determination of a hypothetical regression of the estimated IV coefficients on the true parameters β_p, which can be estimated using the sample standard error of $\hat{\beta}_p^{IV}$ and its variance across the p regressions. Given estimates of $\hat{a}$, $\hat{b}$, and $\hat{\rho}^2$ from model (A.1), we can therefore use equations (A.2) and (A.3) to back out estimates of τ^2 and $\mathbb{E}\left[\gamma_p\right]$ (see section 2.1 of Pancost and Schaller, 2024 for more details).

In Appendix Figure A.4, we plot the estimated OLS coefficients $\hat{\beta}_p^{OLS}$ as functions of the IV coefficients $\hat{\beta}_p^{IV}$ across the six product categories in our sample using *Net Capital Gains to Total Income* (Panel A) and *Interest Income to Total Income* (Panel B) as measures of participation. In both panels, we also report the coefficients $\hat{a}$ and $\hat{b}$ from estimating the meta-regression model (A.1). Figure A.4 suggests that OVB appears to be negligible in our IV estimates: The estimated constant terms $\hat{a}$ are economically small and are not statistically different from zero in both Panels A and B. Since $\hat{\rho}^2$ as implied by the estimates in Panel A (Panel B) is equal to 0.61 (0.93), this in turn implies an estimate of $\mathbb{E}\left[\gamma_p\right]$ equal to -.026 (0.038), which is also economically small. In contrast, Figure A.4 suggests that our IV estimates are potentially affected by classical ME: the estimated slope terms $\hat{b}$ in Panels A and B are positive, statistically different from zero at conventional levels, and imply estimates of τ^2 equal to 0.11 and 0.44, respectively. The results of this exercise confirm that classical ME is the most likely reason behind the relatively large coefficient estimates reported in Table 8 in the main paper, and mitigate overall concerns about OVB.

A.I.2 Stacked Difference-in-differences

We provide evidence from stacked DiD tests (see, e.g., Cengiz et al., 2019) aimed at studying treatment timing and response dynamics. To perform these tests, we construct cohorts of treated and control states in an interval of $[t-3, t+3]$ years around each year t in our sample. Within each cohort, we assign a state to the treatment group if the capital gains tax rate in that state declines for the first time in our sample in year t. We focus on tax rate cuts rather than increases because during our sample period the average state implements capital gain tax cuts. We focus on the first time a state implements a tax cut in our sample because in many cases tax cuts are rolled out over more than one year. We assign a state to the control group if the state does not experience a tax rate change over the entire sample period or within the cohort (e.g., Baker et al., 2022), depending on the specification.

In the resulting stacked panel, we test the hypothesis that, by decreasing participation propensity among high earners, a decline in state taxes increases the sensitivity of deposit rates to local income. To test this hypothesis, we estimate the following specification:

$$\begin{aligned} d_{ipb(zs)ot} &= \beta_1 log(PerCapitaIncome)_{zt} \\ &\quad + \beta_2 Treated_{so} \times Post_{ot} \times log(PerCapitaIncome)_{zt} + X_{LO} + \gamma_{FE} + \varepsilon_{ipb(zs)ot}, \end{aligned} \tag{A.4}$$

where i, p, b, z, and t respectively denote banks, products, branches, zipcodes, and years as in our previous specifications, s denotes states, o denotes cohorts, *Treated* is an indicator equal to one if state s is part of the treatment group and equal to zero if state s is part of the control group in cohort o, *Post* is an indicator equal to one if year t is equal to or higher than the treatment year in the cohort, $log(PerCapitaIncome)$ follows the same definition used in the previous sections, X_{LO} is a vector of low-order terms (i.e., the standalone indicators $Treated_{so}$ and $Post_{ot}$, as well as their interactions and their interactions with $log(PerCapitaIncome)$), γ_{FE} is a vector of fixed effects, and ε is an error term. Following the sampling-based approach of Abadie et al. (2020), in these tests we cluster standard errors at the state-cohort level. The coefficient of interest in the stacked specification (A.4) is β_2, which pins down changes in the sensitivity of branch-level rates to income after state-level cuts on capital gain taxes.

In Table A.13, we report estimates of the coefficient β_2, as well as estimates of the baseline coefficient β_1 as a benchmark. Table A.13 documents two sets of findings. First, the table confirms an economically large and statistically significant relationship between local income and deposit rates. For example, the first row of column (1) shows that a 1% increase in local income per capita is associated with a 0.195 bps in average deposit rates, such that moving from the bottom to the top decile of the income distribution is associated a 0.355 pp increase in average deposit rates (around 39.4% of the sample mean). Consistent with our participation hypothesis and with our previous findings, the second row of column (1) shows that, following a decrease in state-level tax rates, the baseline sensitivity of deposit rates to income increases by around 15%. The remaining columns of

Table A.13 also show that these estimates are economically and statistically similar across various combinations of fixed effects, confirming the overall stability of this finding.

We conduct two sets of robustness on the results presented in Table A.13. First, we replace the $Post_{ct}$ indicator in equation (A.4) with year-of-event indicators to study the dynamics of deposit rate sensitivity to income around changes in state-level capital gains tax rates. We report these estimated dynamic coefficients in Figure A.5. Figure A.5 documents a large, statistically significant, and persistent jump in the sensitivity of deposit rates to income around the year of the tax rate cut, lasting for around two years. The figure also shows no evidence of preexisting trends in the sensitivity of rates to income before a tax rate change, thus supporting a causal interpretation of our estimates. Second, in Appendix Table A.14 we also show that our estimates are nearly identical when we extend the control group to states that do not experience tax rate changes within the cohort (as opposed to the entire sample), thus reducing concerns that our baseline estimates may be driven by never-treated states in the control group being intrinsically different from those in the treatment group (e.g., Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Baker et al., 2022). Overall, the results of this section provide additional supporting evidence that changes in state taxes have an impact on households' trade-off between deposits and other financial products, and that this trade-off is internalized by banks when pricing their deposits.

A.II Broker Misconduct During the Crisis

In this Appendix section, we focus on cross-sectional identifying variation across geographic areas differently exposed to broker misconduct during the financial crisis. The geographic unit of observation for the Egan et al. (2019) misconduct data is a city, which we denote by c in our reduced-form estimates. Since many cities contain more than one zipcode, and since zipcodes sometimes span more than one city, the panel is constructed at the zipcode-city pair level. In this panel, misconduct varies at the city level, while participation and deposit rates vary at the zipcode level. Our two-stage least squares specification for these tests take the form

$$N\hat{C}G_{z(c)t} = \tilde{\alpha} + \tilde{\beta} Crisis\ Misconduct_{(z)ct} + \tilde{\gamma}_{FE} + \epsilon_{z(c)t}, \tag{A.5}$$

$$d_{ipb(zc)t} = \alpha + \beta N\hat{C}G_{z(c)t} + \gamma_{FE} + \varepsilon_{ipb(zc)t}, \tag{A.6}$$

where (A.5) is the first stage and (A.6) is the second stage. In the first stage, we regress post-crisis *Net Capital Gains to Total Income* at the zipcode level on the average share of brokers that committed misconduct in the city during the 2007-2010 period, *Crisis Misconduct*. These first stage estimations are informative of whether, in the cross-section, higher misconduct decreases households' propensity to participate in nondeposit markets. The second stage is identical to equation (3), with the exception that *Net Capital Gains to Total Income* are instrumented by *Crisis Misconduct*. To avoid contemporaneous contaminating variation we focus only on the post-crisis sample starting in 2011. Consistent with our previous tests, we cluster standard errors at the zipcode-city level.

In Table A.15, we present the results of estimating the two stage specification (A.5)-(A.6). Table A.15 shows qualitatively similar results to those reported in Table 8 in the main paper even when we use a completely different source of variation in participation incentives, arising from the cross-section as opposed to the time series. First, column (1) documents a strong negative correlation between broker misconduct exposure during the crisis and post-crisis participation, and column (2) shows that the component of participation that is correlated to cross-sectional variation in broker misconduct is also correlated with income-related deposit spreads in the post-crisis period. Table A.16 shows that the baseline results documented in columns (1) and (2) hold when we consider shorter samples further away from the crisis, suggesting that our baseline results are not sensitive to our definition of the post-crisis period and further mitigating concerns that our results may be driven by confounding variation from other contemporaneous variables. Second, columns (3) and (4) document similar effects when we use *Interest to Total Income* as an alternative measure of participation, thus mitigating potential concerns about systematic biases in our main participation measure. Third, consistent with Table 8, columns (5) and (6) show that our estimates lose economic and statistical significance when we study the impact of misconduct on *Salaries to*

Total Income as opposed to our participation measures.

In Table A.17, we complement the results of Table A.15 with panel regressions aimed at studying how *contemporaneous* changes in broker misconduct in a city change the sensitivity of local deposit rates to income.[A.2] To do so, we augment our baseline tests from Table 2 with indicators equal to one if the city where the bank branch is located experiences any broker misconduct in a year, and equal to zero otherwise, as well as with continuous variables measuring the share of brokers operating in the city that ever committed misconduct. Different from our previous cross-sectional tests, in these tests we are able to include zipcode-city fixed effects and thus remove time-invariant economic differences across zipcodes. Importantly, this estimation strategy allows us to study the interaction between income and misconduct while controlling for *time-varying* correlated economic conditions by including the baseline level of broker misconduct in the city in our regression. The estimates presented in Table A.17 show that an increase in local misconduct reduces the sensitivity of local deposit rates to income. In turn, this suggests that an increase in local misconduct deters participation in nondeposit markets by relatively high-income households, which in turn allows banks to reduce their participation premia.

A.III Zipcode-level Inflation Measurement

To calculate inflation at the zipcode level, we proceed in three steps. First, we obtain data on individual households' purchases of goods from NielsenIQ homescan (Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019). Second, we compute total quantities and average prices paid for each 3-digit universal product code (UPC) across all households residing in a given zipcode and year. Third, we construct the Törnqvist inflation index at the zipcode-year level as

$$1 + Törnqvist_{zt} = \prod_u \left(\frac{Price_{uzt}}{Price_{uzt-1}}\right)^{\frac{Share_{uzt}+Share_{uzt-1}}{2}}, \tag{A.7}$$

where u, z and t respectively denote UPC codes, zipcodes, and years, *Törnqvist* is the Törnqvist inflation index at the zipcode-year level, *Price* is the average price paid for UPC product u across

[A.2] The Egan et al. (2019) data is available for 2007-2015, which limits our sample to this period in these tests.

all purchases by all households residing in zipcode z in year t, and *Share* is the spending share in UPC product u (i.e., total amount spent on product u divided by total amount spent across all products) in zipcode z during year t.

A.IV Additional Results

Figure A.1

Bank-level Deposit Interest Expense Ratios

This figure reports bank-level deposit interest expense ratios as functions of average income across the areas where banks operate. To construct the figure, we first compute average income per capita across all the zipcodes where the banks in our sample have at least one branch. We residualize bank-year-level deposit interest expense ratios (i.e., average quarterly deposit interest expense divided by average quarterly total deposits in a year) with respect to year fixed effects, and plot the average regression residuals in ten income deciles based on the annual distribution of *Per Capita Income* across banks. The data on bank-level total deposits and deposit interest expense comes from the FDIC Call Reports. The sample period is 2004-2020.

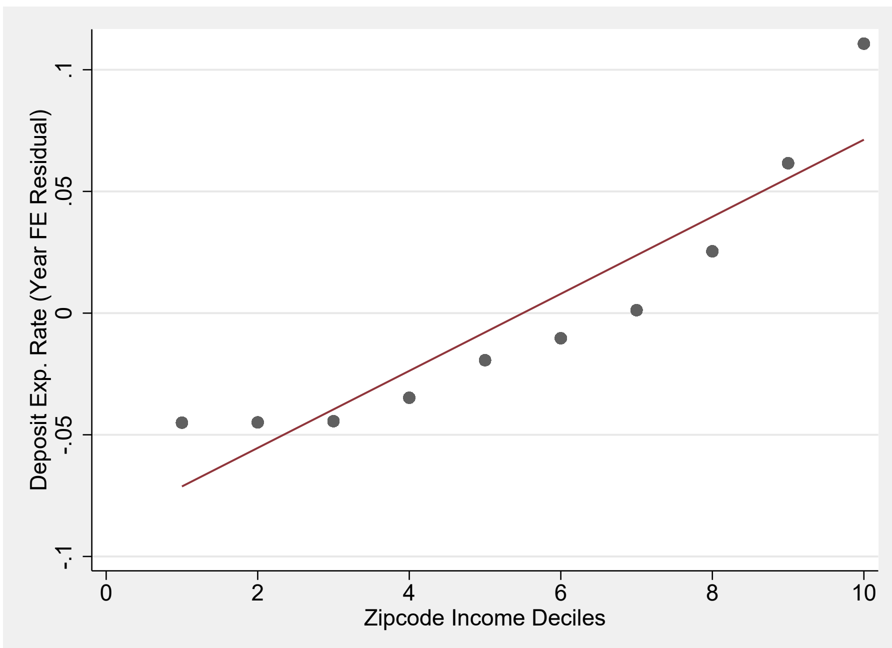

Figure A.2

Participation and Deposit Base Volatility: Robustness

This figure provides a robustness test on the results of Figure 3 in the main paper by displaying deposit volatility as a function of *Interest to Total Income* (rather than *Net Capital Gains to Total Income*) as a proxy of local participation. The procedure employed to produce this figure is otherwise identical to the procedure described in the header of Figure 3.

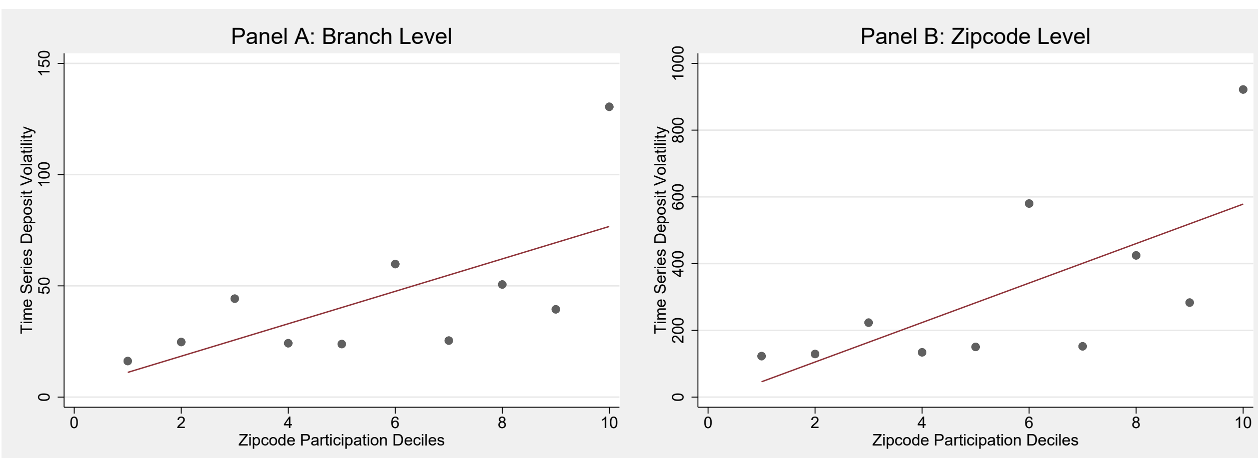

Figure A.3

Participation and Deposit Market Structure

In this figure, we ask whether participation is systematically correlated with local deposit concentration. We first build ten deciles based on annual deposit HHI at the county level. We then plot average *Net Capital Gains to Total Income* (Panel A) and *Interest to Total Income* (Panel B) in each of these deciles. The sample period is 2004-2020.

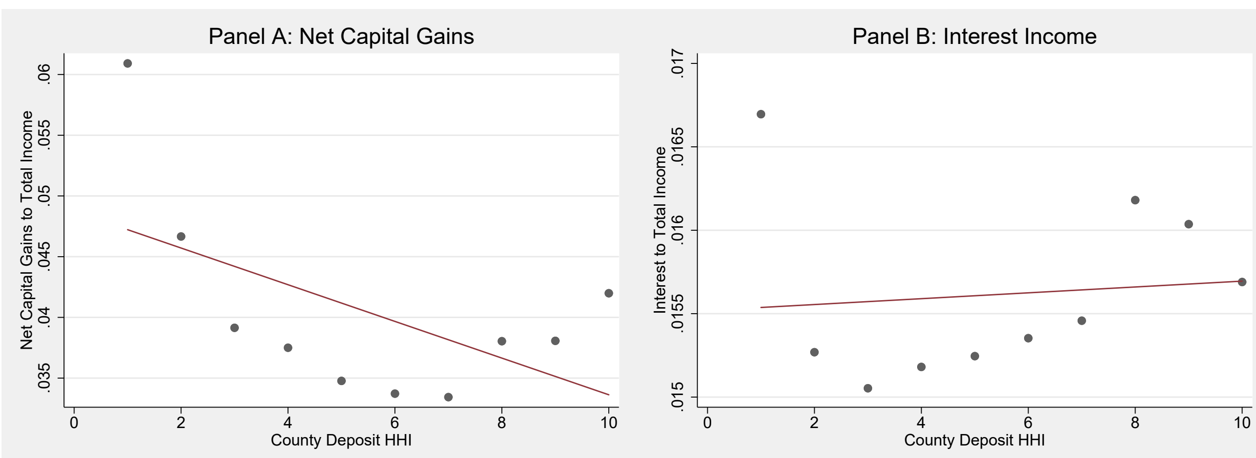

Figure A.4

2SLS: Meta-regression Results

In this figure, we present the results of a meta-regression analysis aimed at separating OVB and measurement error in our IV estimates. For each deposit product category p in our sample, we first estimate an OLS regression of branch-year-level deposit rates on zipcode-year-level *Net Capital Gains to Total Income* and bank and zipcode fixed effects to obtain six estimated OLS coefficients, $\hat{\beta}_p^{OLS}$. We then repeat the same estimation exercise in the same panel and using the same set of fixed effects, but instrumenting *Net Capital Gains to Total Income* with state-level capital gains taxes on top earners to obtain six estimated IV coefficients, $\hat{\beta}_p^{IV}$. In this figure, we plot the estimated $\hat{\beta}_p^{OLS}$ and $\hat{\beta}_p^{IV}$ for each product p, and we report estimates and associated standard errors of the parameters $\hat{a}$ and $\hat{b}$ from estimating regression (A.1) across products. The sample period is 2004-2020.

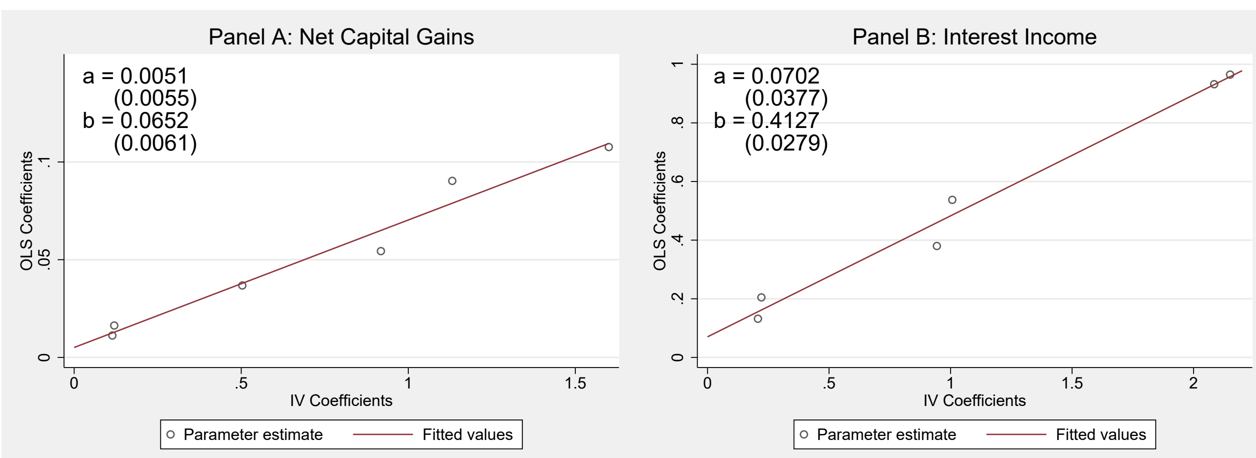

Figure A.5

Stacked DiD on State Tax Rate Changes: Dynamics

This figure plots estimates of the interaction coefficient β_2 of the stacked DiD specification (A.4) for each event-year in a cohort. The underlying regression specification is identical to that of Table A.13, column (1), with the exception that we replace the *Post* indicator with individual indicators for each event-year. We take year $t-1$ as the baseline. The sample period is 2004-2020.

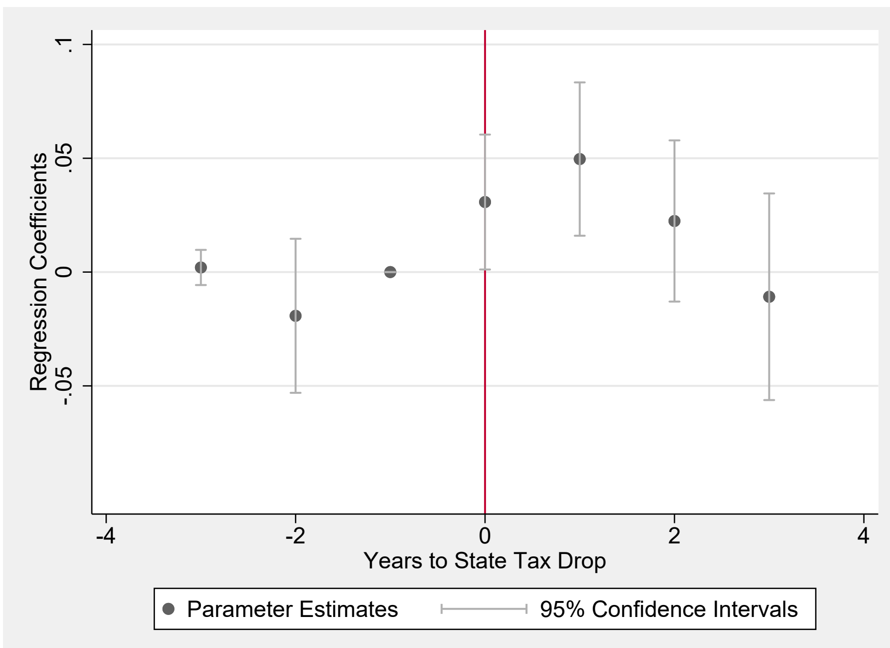

Table A.1

Deposit Rates and Local Income: Poisson Regressions

This table reports the results of estimating our baseline specification (1) using Poisson regressions rather than linear ordinary least squares. The table is otherwise identical to Table 2.

	Dep. Variable: Deposit Product APY				
	(1)	(2)	(3)	(4)	(5)
log(Per Capita Income)	0.156***	0.173***	0.177***	0.172***	0.021***
	(0.015)	(0.015)	(0.014)	(0.014)	(0.003)
Year FE	Yes	Yes	Yes	Yes	No
Zipcode FE	Yes	Yes	Yes	No	No
Product FE	No	Yes	Yes	No	No
Bank FE	No	No	Yes	No	No
Bank $\times$ Product FE	No	No	No	Yes	No
Zipcode $\times$ Product FE	No	No	No	Yes	No
Bank $\times$ Product $\times$ Year FE	No	No	No	No	Yes
Pseudo R-Squared	0.219	0.399	0.405	0.427	0.479
Observations	629,391	629,391	629,384	621,409	244,894

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.2

Deposit Rates and Local Income: Branch-zipcode Fixed Effects

This table reports the results of columns (1) to (4) of Table 2 with the addition of branch-zipcode fixed effects. The specifications are otherwise identical to those in Table 2, columns (1) to (4).

	Dep. Variable: Deposit Product APY			
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.117***	0.131***	0.131***	0.139***
	(0.015)	(0.014)	(0.014)	(0.014)
Year FE	Yes	Yes	Yes	Yes
Product FE	No	Yes	Yes	No
Bank FE	No	No	Yes	No
Bank × Product FE	No	No	No	Yes
Branch × Zip Code FE	Yes	Yes	Yes	No
Branch × Zip Code × Product FE	No	No	No	Yes
R-Squared	0.417	0.750	0.752	0.831
Observations	629,109	629,109	629,106	609,514

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.3

Deposit Rates and Local Income: Within County-year Estimation

This table reports the results of columns (1) to (4) of Table 2 with the addition of county-time fixed effects. The specifications are otherwise identical to those in Table 2.

	Dep. Variable: Deposit Product APY			
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.050***	0.045***	0.033**	0.033**
	(0.018)	(0.016)	(0.014)	(0.014)
Zipcode FE	Yes	Yes	Yes	Yes
Product FE	No	Yes	Yes	Yes
Bank FE	No	No	Yes	No
Bank $\times$ County FE	No	No	No	Yes
County $\times$ Year FE	Yes	Yes	Yes	Yes
R-Squared	0.418	0.751	0.760	0.762
Observations	629,224	629,224	629,217	629,022

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.4

Rate Setters

In this table, we test whether our baseline results differ across branches that set their own deposit rates and banks whose rates are set by other branches of the same bank. To do so, we interact our baseline specifications from Table 2 with a branch-product-year indicator, *Own Rate Setter*, equal to one if the focal branch sets its own rate for a given product in a year, and equal to zero otherwise. The data on branch rate setting comes from RateWatch. The sample period is 2004-2020.

	Dep. Variable: Deposit Product APY			
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.095***	0.094***	0.106***	0.122***
	(0.019)	(0.017)	(0.016)	(0.016)
Own Rate Setter	-0.132***	-0.103***	-0.078**	-0.060*
	(0.044)	(0.037)	(0.033)	(0.031)
log(PCI) × Own Rate Setter	0.027**	0.035***	0.022***	0.014*
	(0.011)	(0.009)	(0.008)	(0.007)
Year FE	Yes	Yes	Yes	Yes
Zipcode FE	Yes	Yes	Yes	No
Product FE	No	Yes	Yes	No
Bank FE	No	No	Yes	No
Bank × Product FE	No	No	No	Yes
Zipcode × Product FE	No	No	No	Yes
R-Squared	0.405	0.740	0.751	0.824
Observations	629,295	629,295	629,288	621,309

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.5

County Banking Market Structure

In this table, we conduct a robustness test on the results from Table 4 in the main paper, studying cross-sectional variation in county-level deposit market structure. All the market structure variables are identical to those in Table 4, with the exception that they are constructed the county-level rather than at the zipcode-level. All other variables are the same as those used in Table A.5. The sample period is 2004-2020.

	Full Sample		Competitive Counties	
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.136***	0.124***	0.118***	0.098***
	(0.014)	(0.014)	(0.016)	(0.020)
Dep. HHI	0.062**			
	(0.030)			
High Dep. HHI		-0.191***		
		(0.052)		
log(Per Capita Income) × High Dep. HHI		0.053***		
		(0.013)		
Year FE	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes	Yes
R-Squared	0.824	0.824	0.832	0.835
Observations	620,039	621,393	341,834	237,887

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.6

Bank Size

This table shows how the baseline estimates from Table 2 vary in the cross-section of bank size. In column (1), we control for the natural logarithm of the offering bank's total assets and for the interaction between this variable and *Per Capita Income*. In columns (2) and (3), we report our baseline estimates in the bottom nine deciles and in the top decile of the annual bank size distribution, respectively. In columns (4) and (5), we report our baseline estimates in the bottom 19 vigintiles and in the top 5% of the annual bank size distribution, respectively. Data on bank size comes from the FDIC Call Reports. The sample period is 2004-2020.

	All	Bottom 90th	Top 10th	Bottom 95th	Top 5th
	(1)	(2)	(3)	(4)	(5)
log(Per Capita Income)	0.300***	0.170***	0.037*	0.165***	0.024
	(0.036)	(0.019)	(0.020)	(0.018)	(0.021)
log(Assets)	0.045***				
	(0.011)				
log(PCI) $\times$ log(Assets)	-0.011***				
	(0.002)				
Year FE	Yes	Yes	Yes	Yes	Yes
Bank $\times$ Product FE	Yes	Yes	Yes	Yes	Yes
Zipcode $\times$ Product FE	Yes	Yes	Yes	Yes	Yes
R-Squared	0.833	0.830	0.855	0.832	0.855
Observations	573,136	418,788	149,071	462,128	106,241

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.7

Geographic Variation

In this table, we study how the baseline estimates presented in Table 2 vary in the cross-section of banks' geographic presence and size. In column (1), we interact the natural logarithm of *Per Capita Income* with the primary 2010 Rural-Urban Commuting Area (RUCA) code, available at the zipcode-level from the U.S. Census. Higher levels of the RUCA code denote more rural areas. In column (2), we interact the natural logarithm of *Per Capita Income* with four indicators based on RUCA scores. The baseline omitted interaction term captures metropolitan areas (primary RUCA codes 1, 2, and 3). The other interaction terms capture micropolitan areas (primary RUCA codes 4, 5, and 6), small towns (primary RUCA codes 7, 8, and 9), and rural areas (primary RUCA code 10). In columns (3) to (6), we study geographic variation across banks of different size. In columns (3) and (4), we report the estimates of column (2) in the bottom nine deciles and in the top decile of the annual bank size distribution, respectively. In columns (5) and (6), we report the same estimates in the bottom 19 vigintiles and in the top 5% of the annual bank size distribution, respectively. Data on bank size comes from the FDIC Call Reports. Data on 2010 RUCA codes comes from the U.S. Department of Agriculture's website. The sample period is 2004-2020.

	All		Bottom 90th	Top 10th	Bottom 95th	Top 5th
	(1)	(2)	(3)	(4)	(5)	(6)
log(PCI)	0.102***	0.110***	0.132***	0.031	0.131***	0.016
	(0.016)	(0.014)	(0.021)	(0.020)	(0.020)	(0.021)
RUCA × log(PCI)	0.009***					
	(0.002)					
RUCA Score=2 × log(PCI)		0.073***	0.071**	0.080*	0.074**	0.049
		(0.024)	(0.032)	(0.045)	(0.030)	(0.053)
RUCA Score=3 × log(PCI)		0.138***	0.143***	0.058	0.142***	0.097
		(0.025)	(0.030)	(0.050)	(0.029)	(0.059)
RUCA Score=4 × log(PCI)		0.042*	0.032	0.034	0.030	0.122*
		(0.023)	(0.027)	(0.069)	(0.026)	(0.065)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.824	0.824	0.830	0.855	0.832	0.855
Observations	621,374	621,374	418,788	149,071	462,128	106,241

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.8

Bank Noninterest Income

In this table, we study whether our baseline results from Table 2 vary in the cross-section based on the degree to which banks engage in activities generating noninterest income. In column (1), we interact the natural logarithm of *Per Capita Income* with bank-level noninterest income (Call item RIAD4079) to interest income (Call item RIAD4107). In the remaining columns of the table, we repeat the same exercise using different components of noninterest income, namely fiduciary income (Call item RIAD4070), product servicing income (Call item RIADB492), and brokerage income (the sum of Call items RIADC886 RIADC887 RIADC386 RIADC387, available from 2007 onward), all scaled by interest income. The data on bank-level noninterest income and its components comes from the FDIC Call Reports. The sample period in columns (1) to (3) is 2004-2020. The sample period in column (4) is 2007-2020.

	Dep. Variable: Deposit Product APY			
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.130***	0.135***	0.133***	0.133***
	(0.015)	(0.014)	(0.014)	(0.013)
log(PCI) × Noninterest Income	0.010			
	(0.013)			
log(PCI) × Fiduciary Income		-0.034		
		(0.095)		
log(PCI) × Procuct Servicing			-0.328	
			(0.276)	
log(PCI) × Brokerage Income				0.113
				(0.116)
Low Order Terms	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes	Yes
R-Squared	0.824	0.824	0.824	0.806
Observations	620,811	620,811	620,239	494,456

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.9

Participation Spreads: Controlling for Income

In this table, we provide a robustness test on the results presented in Table 5 by estimating our main participation regressions while controlling for local income levels. In column (1), we control for the natural logarithm of total income per capita. In column (2), we include the natural logarithm of net capital gains per capita. In column (3), we include the natural logarithm of net capital gains per capita, interest income per capita, and salaries per capita. In column (4), we include income decile fixed effects, calculated at the annual level. The specifications are otherwise identical to those in column (1) of Table 5. The sample period is 2004-2020.

	Dep. Variable: Deposit Product APY			
	(1)	(2)	(3)	(4)
NCG to Total Income	0.0024***	0.0042***	0.0043***	0.0033***
	(0.000)	(0.001)	(0.001)	(0.000)
Income Level Controls	Yes	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes
Bank $\times$ Product FE	Yes	Yes	Yes	Yes
Zipcode $\times$ Product FE	Yes	Yes	Yes	Yes
Income Decile FE	No	No	No	Yes
R-Squared	0.824	0.824	0.824	0.824
Observations	621,409	617,730	617,675	621,409

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.10

Local Stock Market Performance and Deposit Outflows

In this table, we study the sensitivity of deposit outflows to the performance of local assets. In columns (1) and (2), we repeat the same estimation exercise as in Table 6, but we replace the cumulative excess return of the market portfolio with the year-on-year cumulative excess return of a value-weighted portfolio of local stocks (i.e., stocks of companies headquartered in the state, as in Lin and Pursiainen, 2023). These two columns are otherwise identical to the first two columns of Table 6. In columns (3) and (4), we repeat the same exercise but replace the cumulative excess return of local stocks with the average fraction of local stocks (relative to the total number of local stocks) that are rated "Buy" or "Strong Buy" by analysts during the year. In columns (5) and (6), we replace the excess return on the market portfolio with the median municipal bond yield (in excess of the risk-free rate) of the county where the branch is located. Data on local stocks' performance comes from Compustat/CRSP. Data on analyst recommendations comes from I/B/E/S. Data on municipal bond yields comes from MSRB. The sample period is 2004-2020.

	Dep. Variable: Branch Deposit Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Local Portfolio Ex. Ret.	-0.058***					
	(0.004)					
High Participation (HP)	0.036***		0.067***		0.036***	
	(0.011)		(0.012)		(0.011)	
Local Portfolio Ex. Ret. × HP	-0.019***	-0.017***				
	(0.006)	(0.005)				
Buy Local Stocks (%)			-0.022***			
			(0.001)			
Buy Local Stocks (%) × HP			-0.012***	-0.011***		
			(0.002)	(0.002)		
Muni Bond Yield					-0.902***	-0.075
					(0.047)	(0.069)
Muni Bond Yield × HP					-0.204***	-0.178***
					(0.068)	(0.065)
Year FE	No	No	No	No	No	Yes
Branch FE	Yes	No	Yes	No	Yes	No
Branch × Zipcode FE	No	Yes	No	Yes	No	Yes
State × Year FE	No	Yes	No	Yes	No	No
R-Squared	0.122	0.170	0.124	0.170	0.124	0.157
Observations	210,462	210,283	210,462	210,283	209,901	209,725

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.11

Time Deposit Maturity and Outflows

In this table, we ask whether short- and long-maturity time deposits experience different flows depending on aggregate stock market performance. We first use FDIC Call Report data to build a bank-time deposit maturity-year panel containing the year-on-year log growth of short-maturity time deposits and long-maturity time deposits, defined as time deposits with remaining maturity of less than and more than one year, respectively. We then regress the resulting growth rates on the excess market return, defined as in Table 6. For consistency with Table 6, we compute the annual log growth in time deposits only using the June Call Reports. The sample period is 2001-2023.

	Full Sample		Short Maturity		Long Maturity	
	(1)	(2)	(3)	(4)	(5)	(6)
Ex. Market Return	-0.186**	-0.175**	-0.139	-0.138	-0.233*	-0.215*
	(0.075)	(0.072)	(0.095)	(0.093)	(0.115)	(0.113)
Bank FE	No	Yes	No	Yes	No	Yes
R-Squared	0.006	0.059	0.006	0.099	0.006	0.062
Observations	285,622	285,597	143,858	143,533	141,764	141,429

Note: Standard errors (in parentheses) are clustered at the year level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.12

State Tax Rates in Local Participation Buckets

In this table, we study how state-level capital gains taxes for top earners affect participation in different participation buckets. In column (1), we extend the first-stage regression of Table 8 by including an interaction term between *State Rate, Long Gains* and *High NCG*, and indicator equal to one if *Net Capital Gains to Total Income* is above the annual sample median, and equal to zero otherwise. In columns (2) to (5), we interact *State Rate, Long Gains* with indicators for inclusion in different quantiles of the annual distribution of *Net Capital Gains to Total Income* distribution. The low order terms include the baseline levels of the participation quantile indicators. The sample period is 2004-2020.

	Dep. Variable: NCG to Total Income			
	(1)	(2)	(3)	(4)
State Rate, Long Gains	-0.077**	-0.067*	-0.067**	-0.063*
	(0.035)	(0.034)	(0.033)	(0.033)
High NCG=1 × State Rate, Long Gains	-0.048***			
	(0.016)			
NCG Tercile=2 × State Rate, Long Gains		-0.004		
		(0.011)		
NCG Tercile=3 × State Rate, Long Gains		-0.062**		
		(0.028)		
NCG Quartile=2 × State Rate, Long Gains			0.007	
			(0.009)	
NCG Quartile=3 × State Rate, Long Gains			-0.013	
			(0.015)	
NCG Quartile=4 × State Rate, Long Gains			-0.079**	
			(0.036)	
NCG Quintile=2 × State Rate, Long Gains				0.005
				(0.009)
NCG Quintile=3 × State Rate, Long Gains				0.002
				(0.013)
NCG Quintile=4 × State Rate, Long Gains				-0.019
				(0.019)
NCG Quintile=5 × State Rate, Long Gains				-0.083*
				(0.044)
Zipcode FE	Yes	Yes	Yes	Yes
Product FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Low Order Terms	Yes	Yes	Yes	Yes
R-Squared	0.777	0.787	0.795	0.802
Observations	629,384	629,384	629,384	629,384

Note: Standard errors (in parentheses) are clustered at the zipcode level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.13

State Tax Rate Changes: Stacked DiD

In this table, we report the results of the stacked DiD model (A.4), which we use to study how the sensitivity of deposit rates to local income varies after a decrease in state-level tax rates on net capital gains. We first construct cohorts of treated and control states in an interval of $[t-3, t+3]$ years of each year t in our sample. In each cohort, we assign a state to the treatment group if the capital gain tax rates on top incomes in that state declines for the first time in our sample in year t, and to the control group if the state does not experience a tax rate change over the entire sample period. We then estimate the triple interaction model (A.4). *Treated* is an indicator equal to one if a state is treated in a given cohort, and equal to zero otherwise. *Post* is an indicator equal to one if a year is equal to or larger than the treatment year in a given cohort, and equal to zero otherwise. The vector of low-order terms includes the standalone *Treated* and *Post* indicators, as well as their interactions and their interactions with *log(Per Capita Income)*. All the other variables are defined as in Table 1. The sample period is 2004-2020.

	Dep. Variable: Deposit APY			
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.195***	0.191***	0.178***	0.177***
	(0.020)	(0.020)	(0.020)	(0.021)
Post × Treated × log(Per Capita Income)	0.031**	0.032**	0.032**	0.033**
	(0.013)	(0.014)	(0.014)	(0.015)
Low-Order Terms	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	No
Zipcode FE	Yes	No	No	No
Bank FE	Yes	No	No	No
Cohort × State FE	No	No	Yes	Yes
Cohort × Year FE	No	No	No	Yes
Bank × Product FE	No	Yes	Yes	Yes
Zipcode × Product FE	No	Yes	Yes	Yes
R-Squared	0.413	0.852	0.852	0.852
Observations	1,045,156	1,039,906	1,039,906	1,039,906

Note: Standard errors (in parentheses) are clustered at the state-cohort level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.14

Stacked DiD: Robustness

In this table, we report the results of a robustness test on our estimates from Table A.13. The treatment group is identical to that in Table A.13. The control group consists of states that do not experience capital gains tax rate changes within the cohort (as opposed to never-treated states). The table is otherwise identical to Table A.13.

	Dep. Variable: Deposit APY			
	(1)	(2)	(3)	(4)
log(Per Capita Income)	0.124***	0.134***	0.120***	0.121***
	(0.011)	(0.011)	(0.011)	(0.011)
Post × Treated × log(Per Capita Income)	0.036***	0.031**	0.031**	0.032**
	(0.014)	(0.015)	(0.015)	(0.015)
Low-Order Terms	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	No
Zipcode FE	Yes	No	No	No
Bank FE	Yes	No	No	No
Cohort × State FE	No	No	Yes	Yes
Cohort × Year FE	No	No	No	Yes
Bank × Product FE	No	Yes	Yes	Yes
Zipcode × Product FE	No	Yes	Yes	Yes
R-Squared	0.437	0.832	0.832	0.832
Observations	2,968,576	2,967,226	2,967,226	2,967,226

Note: Standard errors (in parentheses) are clustered at the state-cohort level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.15

Broker Misconduct During the Crisis: 2-Stage Least Squares

In this table, we present the results of estimating the 2-stage least squares specification (A.5)-(A.6). In column (1), we report the results of the first stage, where we regress *Crisis Misconduct* on post-crisis *NCG to Total Income*. In column (2), we report the results of the second stage, where the dependent variable is average deposit rates at the branch-product-year level during the same period. In columns (3) and (4), we repeat the same exercise but use *Interest to Total Income* as opposed to *NGC to Total Income* as a measure of participation. In columns (5) and (6), we report the results of a placebo test where we study the effects of broker misconduct on *Salaries to Total Income* rather than our participation measures. The estimation sample in this table is 2011-2020. The sample only includes zipcode-city pairs where at least one broker was registered during the crisis.

	Net Capital Gains		Interest		Salaries	
	(1)	(2)	(3)	(4)	(5)	(6)
Crisis Misconduct (%)	-0.223***		-0.012***		0.165*	
	(0.055)		(0.004)		(0.093)	
NCG to Total Income		0.005**				
		(0.002)				
Interet to Total Income				0.098**		
				(0.049)		
Salaries to Total Income						-0.007
						(0.005)
No Misconduct FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic		16.405		7.176		3.105
Observations	223,573	223,573	223,573	223,573	223,573	223,573

Note: Standard errors (in parentheses) are clustered at the zipcode-city level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.16

Broker Misconduct and Participation: Sample Bandwidth

This table reports a robustness test on the results reported in columns (1) and (2) of Table A.15, where we estimate the first stage (Panel A) and the second stage (Panel B) using increasingly shorter sample periods further away from the crisis. The baseline results are reported in column (1) of both panels for reference.

Panel A: First Stage

	Baseline	$\geq$ 2012	$\geq$ 2013	$\geq$ 2014	$\geq$ 2015	$\geq$ 2016	$\geq$ 2017	$\geq$ 2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crisis Misconduct (%)	-0.223***	-0.223***	-0.221***	-0.223***	-0.215***	-0.214***	-0.219***	-0.215***
	(0.055)	(0.055)	(0.056)	(0.059)	(0.060)	(0.064)	(0.074)	(0.071)
No Misconduct FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	223,573	223,573	194,473	167,282	141,689	116,244	91,200	67,010

Panel B: Second Stage

	Baseline	$\geq$ 2012	$\geq$ 2013	$\geq$ 2014	$\geq$ 2015	$\geq$ 2016	$\geq$ 2017	$\geq$ 2018
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NCG to Total Income	0.514**	0.514**	0.486**	0.472**	0.469*	0.494*	0.514*	0.615*
	(0.205)	(0.205)	(0.223)	(0.237)	(0.256)	(0.278)	(0.295)	(0.365)
No Misconduct FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic	16.405	16.405	15.695	14.100	13.099	11.041	8.876	9.082
Observations	223,573	223,573	194,473	167,282	141,689	116,244	91,200	67,010

Note: Standard errors (in parentheses) are clustered at the zipcode-city level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A.17

Broker Misconduct and Deposit Spreads: Panel Evidence

In this table, we report the results of a panel estimation aimed at studying how changes in local broker misconduct change the sensitivity of local rates to income. In column (1), we interact *log(Per Capita Income)* with *Misconduct*, an indicator equal to one if the city has experienced any broker misconduct over the past year and equal to zero otherwise. In column (2), we interact *log(Per Capita Income)* with *Ever Misconduct*, an indicator equal to one if any of the brokers operating in the city were ever found liable of misconduct and equal to zero otherwise. In column (3), we interact *log(Per Capita Income)* with *Ever Misconduct Share (%)*, the share of local brokers ever found guilty of misconduct in a city. All the other variables are identical to those in our baseline Table 2. The sample period is 2007-2015.

	Dep. Variable: Deposit Product APY		
	(1)	(2)	(3)
log(Per Capita Income)	0.191***	0.244***	0.194***
	(0.025)	(0.025)	(0.024)
Misconduct	0.082**		
	(0.032)		
log(PCI) × Misconduct	-0.021***		
	(0.008)		
Ever Misconduct		0.318***	
		(0.056)	
log(PCI) × Ever Misconduct		-0.082***	
		(0.014)	
Ever Misconduct Share (%)			0.544**
			(0.219)
log(PCI) × Ever Misconduct Share (%)			-0.144**
			(0.056)
Year FE	Yes	Yes	Yes
Zipcode-city × Product FE	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes
R-Squared	0.814	0.815	0.814
Observations	265,897	265,897	265,897

Note: Standard errors (in parentheses) are clustered at the zipcode-city level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.