Poverty Spreads in Deposit Markets

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Abstract

This paper documents a source of financial inequality: banks offer significantly lower deposit interest rates and product variety to the poor. These *poverty spreads* in deposit markets are substantial - moving from the bottom to the top income decile increases deposit rates by 55% of the median rate in our sample. The spreads are not explained by banking competition for deposits or other products, but appear to be driven by banks internalizing differential participation in nondeposit markets across the income distribution. Consistent with this hypothesis, deposit flows in high-income areas are more responsive to stock market performance than in low-income areas, and quasi-exogenous reductions in participation incentives through increases in capital gains taxes are associated with lower poverty spreads. Our findings highlight lack of participation as a substantial source of deposit market power, and suggest that increasing access may reduce poverty spreads and improve outcomes for low-income depositors.

JEL Codes: G12, G21, G51.

Keywords: poverty spreads, deposit products, inequality, participation.

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1 Introduction

Do the poor receive lower interest rates on their deposit savings than the wealthy? Recent work shows that firms in the consumer goods sector target poor and wealthy households 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), however, we lack systematic evidence of whether banks price these consumer products differently along the income distribution, and, if so, an understanding of the sources and implications of such disparities.

In this paper, we study deposit rate-setting along the income distribution and its underlying 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). This magnitude is quantitatively large: holding income and liquid asset holdings constant, it implies an interest income loss of around USD 400 a year (1.14% of average household income) for poor households. Following an early literature on consumer price inequality (see, e.g., Kunreuther, 1973 and the subsequent "poverty penalty" literature), we refer to these differential rate-setting patterns along the income distribution as "poverty spreads."

The observed poverty spreads hold within a county-year, suggestive of substantial rate variation even within geographic areas commonly used to define deposit markets

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

(Heitfield, 1999, Biehl, 2002), as well as within bank-year, suggesting that the same bank may exploit its market power to offer different deposit rates across different areas where it operates (Drechsler et al., 2017). Banks also appear to offer higher product variety in high-income 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, the presence of higher-yield products in high-income zipcodes cannot fully explain the observed poverty spreads. We find large poverty spreads even for the same product, the same subscription size, and the same maturity.

A natural hypothesis is that our baseline findings may simply be a byproduct of competition within the banking sector—banks may offer higher interest rates in rich areas simply due to higher deposit competition. They may also do so to cross-subsidize other products targeting relatively wealthy customers, such as insurance or other investment products generating fee-based income for the bank. However, our point estimates are quantitatively similar across zipcodes and counties with high and low levels of bank competition, and across banks with high and low dependence on fee-based income, suggesting that competition within the banking sector is unlikely to be a first-order determinant of our findings.

While we do not find evidence that the observed poverty spreads are primarily driven by banking competition, we test the hypothesis that banks internalize differential participation in nondeposit assets (henceforth "participation") along the income distribution. This hypothesis is twofold. First, high-income households have a higher participation propensity than low-income households. Second, if banks internalize high-income households' participation propensity, they may offer higher rates to avoid losing their deposit base to nondeposit assets. In sum, the observed poverty spreads may not inherently be due to differences in income, but rather a byproduct of lower participation in nondeposit assets by the poor.

We provide three 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 Chodorow-Reich et al., 2021 and Smith et al., 2023) exhibit a positive relationship with local deposit spreads, even conditioning for income levels (e.g., by performing our tests within income brackets). We do not observe any relationship between deposit rates and income components unrelated to participation such as salaries. Overall, this first set of findings implies that competing explanations to our proposed mechanism need to jointly rationalize why poverty spreads are increasing in income components related to participation, but orthogonal to others.

Second, we show that deposit quantities are more volatile and responsive to the performance of nondeposit assets such as stocks. 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. We also document similar findings when we consider outflows in response to both the past performance and analysts' recommendations about local stocks, which more closely approximate the past and expected returns of local households' equity investment opportunities (Lin and Pursiainen, 2023).

In the cross-section of time deposits, we also find larger spreads for long-maturity certificates of deposits (CDs) than for short-term CDs. In other words, 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, this finding suggests that long-maturity time deposits are closer substitutes to nondeposit assets held by the rich than short-maturity deposit products. As a result, long-maturity time deposits should be more sensitive to the performance of other assets than, for example, short-maturity time 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, these results allow us to remove any confounding variation potentially correlated with interest rate levels from our estimates. In other words, any competing explanation to our participation hypothesis would need to rationalize *both* differences in average interest rate levels and differences in term structure slopes conditional on levels along the participation distribution.

In our third set of tests, we provide a causal interpretation of our proposed mechanism by exploiting quasi-experimental variation in participation incentives along the income distribution. In the time series, we study how changes in state-level capital gains taxes for top income earners affect local participation and poverty spreads. Our hypothesis in these tests is that capital gains taxes on top income earners change the participation incentives at the top of the income distribution (and thus banks' rate-setting incentives for top income earners), while they do not affect participation incentives at the bottom of the income 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 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 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 disappear when we investigate a possible relationship between capital gains taxes and other income sources such as salaries.[2]

[2] 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 local deposit rates to local income, which we interpret as

In the cross-section, we also study whether differential exposure to broker misconduct during the financial crisis (Egan et al., 2019, 2022) is associated with subsequent participation and poverty spreads. 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 local broker misconduct is positively associated with local deposit rates. In sum, given the available evidence, poverty spreads along the income distribution seem to be primarily consistent with local banks internalizing households' participation incentives along the income distribution.

Our results carry three implications. First, our results suggest that lack of participation is a substantial 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 number of bank branches. Second, our paper shows that poverty spreads along the income distribution amplify inflation inequality (Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019): when we jointly consider differences in inflation and differences in nominal deposit rates between the poor and the wealth, we find that poverty spreads in nominal rates contribute to around one third of real rate spreads along the income distribution. Third, we show that broadband usage is positively associated with participation (consistent with Hvide et al., 2024) and negatively associated with poverty spreads in the cross-section of zipcodes. Based on these findings, our paper proposes increased access to financial markets as a potential area of policy intervention to reduce poverty spreads in deposit markets.

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.,

additional evidence that banks internalize differences in participation incentives along the income distribution. These dynamic tests also document 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 paper to ours in this literature is Drechsler et al. (2017), which studies how deposit rates and quantities respond to changes in the Fed funds rate, and uses cross-sectional variation in deposit market structure to study monetary policy pass through as a function of banks' local market power. Our findings complement those in Drechsler et al. (2017) by studying average differences in interest rates levels along the income distribution 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 by providing formal evidence of a quantitatively meaningful source of banks' market power—lack of participation in nondeposit markets.

Second, our paper also 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), 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 literature on consumer inequality has long discussed the presence of a poverty penalty (Kunreuther, 1973; Attanasio and Frayne, 2006) and, more recently, infla-

[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. We complement this literature by showing that the rates of returns on some financial products may endogenously respond to investors' income and participation.

tion inequality (Kaplan and Schulhofer-Wohl, 2017; Jaravel, 2019; Argente and Lee, 2021) in consumer goods. We contribute to this literature by documenting poverty spreads on the deposit rates that banks offer to consumers with different income levels, and by showing that poverty spreads in deposit markets and inflation inequality in nonfinancial markets jointly result in large spreads between the real rates that the poor and the wealthy can achieve on their savings.

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 branch-product-week level RateWatch panel at the product category-year level according to six broad product categories provided by RateWatch, namely, certificates of deposits (CDs), regular and premium money market accounts (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 each product category in a year. 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).

[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.

Similar to Chodorow-Reich et al. (2021), we also use the IRS-SOI data to obtain measures of local participation in nondeposit financial 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 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 this measure is unaffected by participation in deposit markets since investment in deposit products typically does not entail capital gains. However, this measure of participation may also contain variation from households' incentives to realize capital gains and losses, which may act as confounds 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 investment in fixed-income securities, thus allowing us to capture investment in financial products that are close 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 may act as a confound. Overall, we trade off these costs and benefits by using both measures of participation in our tests.

We also use data from seven secondary sources. First, we use branch-year information on deposit quantities from the Federal Deposit Insurance Corporation (FDIC) Summary of Deposits (SOD) and Call Report data 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), and the Institutional Brokers' Estimate System (I/B/E/S) to study how the sensitivity of deposit outflows to aggregate and local stock market performance varies across the income distribution.

Third, we use data on state taxes for top income earners to study how changes in participation incentives across the income distribution affect deposit poverty spreads. This data is publicly available on the NBER-TAXSIM website.[5] Fourth, we use city-level ad-

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

viser misconduct data during the crisis from Egan et al. (2019, 2022) to study how broker misconduct affects local participation and poverty spreads 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), which we use to study how variation in nominal deposit rates and inflation across the income distribution jointly determine poverty spreads in real deposit rates. Sixth, we use zipcode-level broadband usage data in 2020 from Microsoft to ask how enabling access to financial markets may affect participation and poverty spreads across the income distribution.[6] Seventh, we use Rural-Urban Commuting Area Codes (RUCA) data from Economic Research Service of the U.S. Department of Agriculture to study how our results vary with geographic characteristics.[7] 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 Poverty Spreads in Deposit Markets

We start our analysis by documenting poverty spreads in deposit markets: households located in poorer zipcodes are offered deposit interest rates that are on average lower than households in wealthier areas. 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, $log(Per\ Capita\ Income)$ 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.

[6] The data is publicly available at https://github.com/microsoft/USBroadbandUsagePercentages.

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

In our baseline estimation exercises, we include several combinations of fixed effects including bank $\times$ product fixed effects to control for average differences in the rates offered on the 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 thus comparing the 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, thus achieving identification from cross-sectional variation in income within the same county. 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 the dependent variable. We follow the design-based approach of Abadie et al. (2020), and cluster standard errors at the zipcode level.

3.1 Deposit Rates

In Table 2, we start by presenting the results of estimating the baseline specification (1). Column (1) of Table 2 documents an unconditional positive correlation between local income and the average rate offered by local bank branches on each of their products. This estimate suggests that, after removing aggregate variation at the year-level, a 1% increase in zipcode-level average income per capita is associated with a 0.13 bps rate increase for the average product in our sample, around 0.01% of the sample mean and around 0.033% of the sample median. Panel A of Figure 1 confirms this baseline result and provides additional evidence of an increasing relationship between local income and deposit rates.

In columns (2) to (5) of Table 2 we include incrementally restrictive sets of fixed effects to our baseline specification. In column (2), we control for zipcode fixed effects, thus removing 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 (2) shows that, once we control for these characteristics, we obtain one order of magnitude larger estimates than in the baseline column (1): 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). In columns (3) to (5), we confirm that the point estimate of column (2) 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 certain products in different zipcodes.

Recent evidence shows that the ratio of liquid assets to income for households at the bottom of the income distribution is around 5.[8] Multiplying this ratio by the average income in the bottom decile of the distribution (around USD 35,000 per year, see Table 1) this implies that poor households in our sample keep around USD 175,000 as liquid assets. If these liquid assets were only consisting of deposits, a 0.22 pp increase in average rates such as the one implied by our estimates would then increase their interest income by around USD 400 a year (1.14% of average household income). This number does not take into account potential endogenous responses by depositors, and would increase to up to USD 960 (2.7% of average household income) were the ratio of liquid assets to income increase to the level of the rich. Overall, the estimates presented in the first five columns of Table 2 imply large differences in the interest income received by poor depositors on

[8] See https://www.richmondfed.org/research/national_economy/macro_minute/2023/mm_06_27_23.

their deposit savings, both in dollar terms and relative to their total income.

Overall, the results of columns (1) to (5) of Table 2 document a positive and quantitatively large relationship between income and average deposit rates offered by local branches. In column (6), we also document similar results exploiting purely cross-sectional variation within the same bank, product, and year across different geographic areas (as in Drechsler et al., 2017): the results of column (6) 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 column (6) provide powerful variation identified within the same bank 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). Since in this paper we are interested in quantifying *average* rate differences across the income distribution, in what follows we use the specification reported in column (5) rather than that in column (6) as our preferred specification.

3.2 Robustness

We provide four sets of robustness 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 main 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 identical when we include zipcode $\times$ branch fixed effects, thus reducing potential bias arising from branch being assigned to different 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 of the U.S.

These results document substantial poverty spreads even within the same county and at the same point in time, thus suggesting substantial depositor switching costs (Yankov, 2024) even within narrowly-defined geographic areas, and calling 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 confirm the existence of poverty spreads even when we modify our main panel to include and control for granular deposit product characteristics such as maturity and minimum subscription size. In other words, banks do not only seem to offer a more diversified choice within the same product category, as we show below, but also higher rates on the exact same product.

3.3 Other Product Characteristics

While our main focus is on deposit interest rates, we also hypothesize that branch managers may adopt other tools to cater to and retain customers with different income levels. Table 3 we provides evidence consistent with this hypothesis. Column (1) shows 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) 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, respectively. Overall, the results of Table 3 suggest that banks offer more product variety in high-income than in low-income neighborhoods.

3.4 Competition within the Banking Sector

The results presented so far are consistent with two main channels. First, banks may compete with each other, and do so more intensively in rich areas. They may do so to attract deposits, or to cross-subsidize other products such as insurance or other investments that generate fee-based income. Second, banks may internalize differential investment opportunities by the poor and the wealthy, thus banks implicitly competing 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, are strongest for relatively small banks that do not offer a wide range of consumer investment products, and do not vary with the degree of bank dependence on noninterest income. In sum, our baseline findings cannot be fully attributed to competition within the banking sector.

In column (1) of Table 4, we start by showing that our baseline estimates from Table 2 are statistically and economically unaffected when we include zipcode-level deposit HHI as an independent variable in our regression specifications, suggesting that our baseline results are not purely due to contemporaneous correlation with banking market structure. In column (2), we also confirm poverty spreads of similar magnitude to those documented in Table 2 independent of local deposit market concentration. While the sensitivity of deposit rates to income increases in relatively concentrated markets, suggesting that concentration increases banks' ability to offer differential rates across the income distribution, our estimate of the interaction term between local income and concentration is 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 concentration (column (4)), further providing support to our claim that the poverty spreads we find in the data are not purely driven by banking competition. In Appendix Table A.6 we also document nearly identical results when we rely on more commonly used measures of banking competition at the county-level (see, e.g., Drechsler et al., 2017), suggesting that the findings of Table 2 are not systematically driven by how we define local banking markets.

The results of Tables 4 and A.6 provide initial evidence that deposit competition within the banking sector cannot fully rationalize our baseline results. While the results presented in 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). To examine this possibility, we next ask whether our baseline descriptive results vary in the cross-section based on bank size and dependence on fee-based income. In Appendix Table A.7, we start by showing that deposit poverty 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 vigintile of the distribution.[9] Additionally, the correlation between local income and deposit 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) especially in major urban areas (d'Avernas et al., 2023).

Next, in Appendix Table A.9 we study whether our baseline results from Table 2 vary in the cross-section based on the degree to which banks engage in noninterest income-generating activities. Our hypothesis is that cross-subsidization incentives should be stronger when banks are relatively more active in noninterest income-generating 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

[9] In Appendix Table A.8, we also ask how deposit poverty spreads vary across rural and urban areas for banks of different sizes (d'Avernas et al., 2023; Oberfield et al., 2024). Table A.8 provides evidence of larger spreads in relatively less dense areas such as small cities, towns, and rural areas, and shows that this geographic variation is mostly due to different rate-setting by banks below the top of the size distribution—we do not observe quantitatively meaningful rate variation across for very large banks across different geographies.

into components closely related to retail customers, namely, fiduciary income to interest income, product servicing income to interest income, and brokerage income to interest income.[10] Overall, 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.

4 Participation and Deposit Poverty Spreads

The previous section shows that competition within the banking sector (either for deposits or for other products) along the income distribution is not fully capable of explaining our baseline deposit poverty spreads. In this section, we test banks' internalizing differential participation in nondeposit assets across the income distribution as an alternative explanation.

4.1 Income, Participation, and Poverty Spreads

We first show that two income components related to nondeposit market participation—net capital gains and interest income—are mainly responsible for 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, namely *Net Capital Gains to Total Income* and *Interest to Total Income*, and local deposit rates. The estimated coefficients imply economically large differences in deposit rates across 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

[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.

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) and (4) of Table 5, we show that our findings on the first two columns are not driven by spurious correlation with other income sources. Column (3) documents a baseline negative relationship between *Salaries to Total Income* and local rates, confirming that the non-wage components of household income are responsible for the overall positive correlation between total income and deposit rates documented in our baseline tests. Additionally, column (4) shows that the economic and statistical significance of the relationship between salaries and rates completely disappear when we remove common variation between our participation measures and *Salaries to Total Income* by simultaneously including all three independent variables in the regression. 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 independent information about participation in different asset classes.

A potential concern with the results of Table 5 is that in decomposing income in its relative components we may not control for differential income levels across zipcodes. In Appendix Table A.5, 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. In sum, Table 5 shows that our proxies for participation explain large variation in local deposit rates even within the same income level.

The estimates presented in Tables 5 and 5 carry two implications. First, these estimates provide initial support for our main hypothesis that the deposit poverty spreads documented in the previous tables are due to participation and not to generic correlation between income 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 and wages.

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 poverty spreads do not appear to be inherently related to income, but rather a byproduct of lower participation in nondeposit markets by the poor. Figure 2 provides additional evidence consistent with this observation: both our measures of participation are increasing in total income, and this positive relationship is particularly strong for high-income buckets.[11]

4.2 Deposit Flows

We provide three additional pieces of evidence suggesting that banks internalize differential participation in nondeposit markets across 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: the annual volatility of branch-level total deposits 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 branches within the same zipcode), the volatility of the deposit base is increasing in local participation. Appendix Figure A.1 shows similar patterns when we use *Interest to Total Income* as an alternative measure of local participation, thus confirming

[11] A possible concern is that participation may be systematically positively correlated with local banking competition, and that, as a result the results of this section may be driven by deposit competition rather than by participation. Appendix Figure A.2 shows that this is unlikely to be the case.

that the result of Figure 3 hold independent of how we measure 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. In Table 6, we use 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 equal to one for zipcodes with above-median levels of *Net Capital Gains to Total Income* (*High Participation*), 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 increase in the market factor (13.94 pp) 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 between low- and high-participation areas 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 excess 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.11, we complement the results of Table 6 by studying the sensitivity of deposit growth to the performance of assets that may more closely represent local households' investment opportunities. In columns (1) and (2) of Table A.11 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, similar to 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 main independent variable, thus measuring the expected (as opposed to realized) performance of local equities during the year.

Our estimates in Appendix Table A.11 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. Consistent with Table 6, Appendix Table A.12 also documents an even larger dispersion across high- and low-participation zipcodes when deposit growth is measured at the zipcode-level rather than at the branch-level, confirming the overall stability of our finding across different measurement choices.

Finally, we test two sets of related hypotheses about deposit outflows and poverty spreads 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 rich households (Catherine et al., 2023). As a result, we first hypothesize that long-maturity deposits should represent a closer substitute to nondeposit assets than short-maturity products, and be more sensitive to the performance of nondeposit assets. For example, we hypothesize that, everything else equal, banks' issuance of long-term CDs may be more negatively affected by an increase in stock market performance than that of short-term CDs. In Appendix Table A.18, we provide evidence consistent with this first hypothesis: using FDIC Call Report data on banks' stocks of time deposits across 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 structure, we may also expect the term structure of deposit rates to be steeper in high-participation areas than in low-participation areas. In other words, our second hypothesis is that banks offer lower participation premia for short-maturity products (which are less sensitive to the performance of nondeposit assets), and larger for participation premia for long-maturity products (which are more sensitive). In 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 zipcodes with high participation relative to zipcodes with low participation.

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 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.

4.3 Capital Gains Taxes and Participation Incentives

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 par-

[12]The SOD data does not contain disaggregated data at the product or maturity level, which makes it difficult to perform this test at the branch-level.

ticipate in nondeposit markets and increase their marginal propensity to keep their funds in deposit products (which are unaffected by these taxes). On the other hand, low earners' incentives to participate in nondeposit markets should be unaffected by high earners' capital gains tax 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 poverty spreads across the income distribution.[13]

4.3.1 Two-stage Least Squares

We test our hypotheses using two complementary strategies. In our first set of tests, we estimate the following two-stage OLS model:

$$\begin{aligned} \hat{NCG}_{z(s)t} &= \tilde{\alpha} + \tilde{\beta} CG\ Tax_{st} + \tilde{\gamma}_{FE} + \epsilon_{z(s)t}, & (2) \\ d_{ipb(z)t} &= \alpha + \beta \hat{NCG}_{z(s)t} + \gamma_{FE} + \varepsilon_{ipb(z)t}, & (3) \end{aligned}$$

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 the first stage. 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 taxes and net capital gains: a one standard deviation

[13] Complementing the evidence in this section, in Appendix Section A.II we also show that broker misconduct during the financial crisis is associated with lower subsequent participation and poverty spreads. This analysis comes from a different sample and excludes many areas for which brokerage misconduct data is unavailable. As a result, we interpret this evidence as suggestive and complementary to our main analysis.

increase in state-level taxes 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.13 also confirms that these estimates are disproportionately larger in high participation zipcodes, thus providing a first piece of evidence for the instrument 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 main 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 savings in interest-bearing products instead of realizing their 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 this 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 on deposit poverty spreads are driven by a participation channel. However, there are still potential concerns that the observed changes in state-level taxes may be correlated with the local economic conditions of some areas within the state, and that, at the same time, these different economic conditions may be associated with different deposit borrowing behavior by local banks. For example, suppose that some zipcodes experience periods of high economic growth and high loan demand, and that local banks increase deposit interest rates 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 a first set 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 0.088 pp increase in *Salaries to Total Income*, around 0.13% of the sample mean and 0.8% of the sample standard deviation, and this point estimate is not statistically different from zero at conventional levels.

The second-stage estimates in column (6) also show no statistically significant 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 10. In sum, the results presented in columns (5) and (6) suggest that for our illustrative endogeneity example (or any other similar example) to be verified, 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.

4.3.2 Stacked Difference-in-Differences

We also provide evidence from stacked DiD tests (see, e.g., Cengiz et al., 2019) aimed at further reducing potential concerns about endogenous timing of state-level taxes. 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.[14] 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:

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

where i, p, b, z, and t respectively denote banks, products, branches, zipcodes, and years

[14]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.

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 larger 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 (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 9, we report estimates of the coefficient β_2, as well as estimates of the baseline coefficient β_1 as a benchmark. Table 9 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 main 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 9 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 9. First, we replace the $Post_{ct}$ indicator in equation (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 4. Figure 4 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 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 deposit products.

5 Implications

5.1 Participation and Deposit Market Power

A first implication of our findings is that participation should be a substantial source of banks' local 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 offered to their customers (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 5, we then produce bin scatter plots of these estimated deposit betas on average participation for 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 to capture variation in deposit market power orthogonal to local banking market structure.

Figure 5 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 5 are estimated conditional on local deposit HHI and number of bank branches, suggesting that variation in participation orthogonal to local banking market structure explain a large fraction of local deposit market power. For comparison, in Panel C and D of Figure 5 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 in our sample and from the bottom to the top decile of the branch count distribution in our sample is associated with quantitatively similar changes in deposit betas to those documented in the first two panel. In sum, Figure 5 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.

5.2 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 and study how *real* deposit rates vary in the cross section of income, and how nominal spreads and inflation 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 a 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 available from RateWatch minus the zipcode-year level Törnqvist index. Appendix A.I provides more details.

In Figure 6, we show that inflation inequality along the income distribution compounds with nominal poverty spreads to generate substantial variation in real poverty spreads: while the difference in nominal deposit rate between the top and the bottom deciles of the income distribution is around 0.13 pp (17% of the sample mean), this difference almost triples to 0.38 pp when we study differences in real deposit spreads. In other words, inequality in nominal deposit rates is quantitatively meaningful relative to inflation inequality, and is able to explain more than 30% of the total variation in real rates that we observe in the data.

5.3 Increasing Access: Evidence from Broadband Usage

In this section, we argue that policy interventions aimed at increasing participation in nondeposit markets by the poor may be a potential tool to reduce deposit poverty spreads. Following recent work by Hvide et al. (2024), we focus on broadband as a tool to foster access and participation.

We proceed in two steps. In Figure 7, we start by confirming a necessary condition for broadband usage to reduce deposit poverty spreads: this figure documents a strong and almost linearly increasing relationship between our participation measures and broadband usage. For example, Panel A shows that *Net Capital Gains to Total Income* are around four times as large in the top decile of the broadband usage distribution than in the bottom decile, and Panel B shows that *Interest to Total Income* is more than twice as large in the top decile of the broadband usage distribution than in the bottom decile. While the evidence in Figure 7 comes from a single cross-section, and while broadband usage at the zipcode-level is an imperfect measure of nondeposit market access (which may also partly capture participation in online deposit markets, see Sakong and Zentefis, 2023), the patterns in Figure 7 line up with those in Hvide et al. (2024), and thus suggest that policies aimed at increasing broadband access may facilitate financial markets participation in contexts other than Norway.

Next, we ask whether higher access (as proxied by broadband usage) can reduce poverty spreads in deposit markets. Our maintained hypothesis is that, if banks internalize high participation in high-broadband-usage zipcodes, we should observe lower poverty spreads in these zipcodes relative to low-broadband-usage zipcodes.[15] Table 10 provides estimates consistent with this hypothesis. For example, the first row column (1) shows that moving from the bottom decile to the top decile of the income distribution increases the average local deposit rate by around 0.393 pp in a hypothetical zipcode with zero broadband access. However, moving from the bottom decile to the top decile of the income distribution increases the average local deposit rate only by around 0.162 pp in a hypothetical zipcode with 100% broadband access, a nearly 60% reduction in the estimated poverty spread.

The remaining two columns of Table 10 confirm how deposit spreads vary across the broadband usage distribution. Our estimates show that the estimated deposit poverty spreads decline by around 25% in zipcodes with above-median broadband usage relative to zipcodes with below-median broadband usage (column (2)), and they decline by around 45% in zipcodes in the top quartile of the broadband usage distribution relative to zipcodes in the bottom three quartiles (column (3)). Together with the estimates of col-

[15] The U.S. broadband usage data is limited to 2020, and a two-stage least squares exercise as the one in Table A.15 is thus counterintuitive. As a result, we limit our analysis of this section to the interaction between broadband access, income, and deposit rates similar to the one of Table 9.

umn (1), these estimates suggest that policy interventions aimed at increasing access may foster financial market participation and reduce the local poverty spreads.

6 Conclusions

This paper documents poverty spreads in deposit markets—households in poorer areas are offered substantially lower deposit rates on the same products than households living in wealthy areas. These spreads do not appear to be driven by deposit competition within the banking sector, but rather by banks internalizing differential participation along the income distribution. Consistent with this hypothesis, deposit quantities are more volatile and sensitive to the performance of nondeposit assets such as stocks 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 the poor.

Our paper suggests that fostering participation (e.g., by increasing access and financial education) may reduce banks' incentives to price-discriminate consumers based on participation, and thus reduce the poverty spreads that we observe in the data. At the same time, an increase in deposit funding costs may push some banks (especially local banks) to cut lending, and thus generate unintended spillovers. In this sense, policy interventions aimed at increasing participation may need to consider the potential costs and benefits associated with higher participation by poor customers.

The fact that we observe poverty spreads in bank deposits, arguably the simplest financial product available to consumers, also suggests potentially larger price discrimination 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 may discriminate consumers based on their income and access to substitute 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 average deposit rates offered by banks as a function 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.

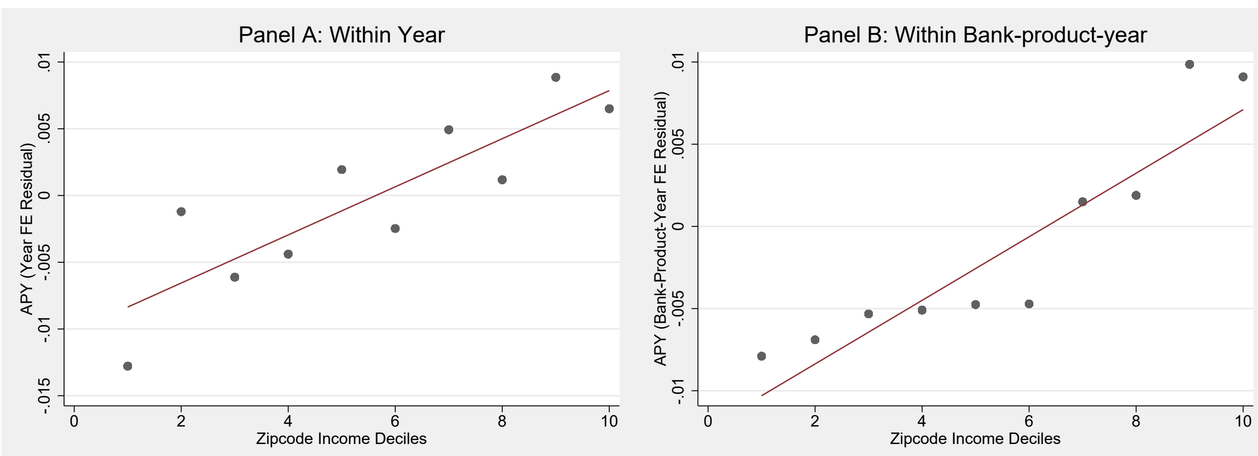

Figure 2

Participation by Income Deciles

In this figure, we study differential participation across 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 *Taxable Interest to Total Income* in the same income deciles. All the variables are defined as in Table 1. The sample period is 2004-2020.

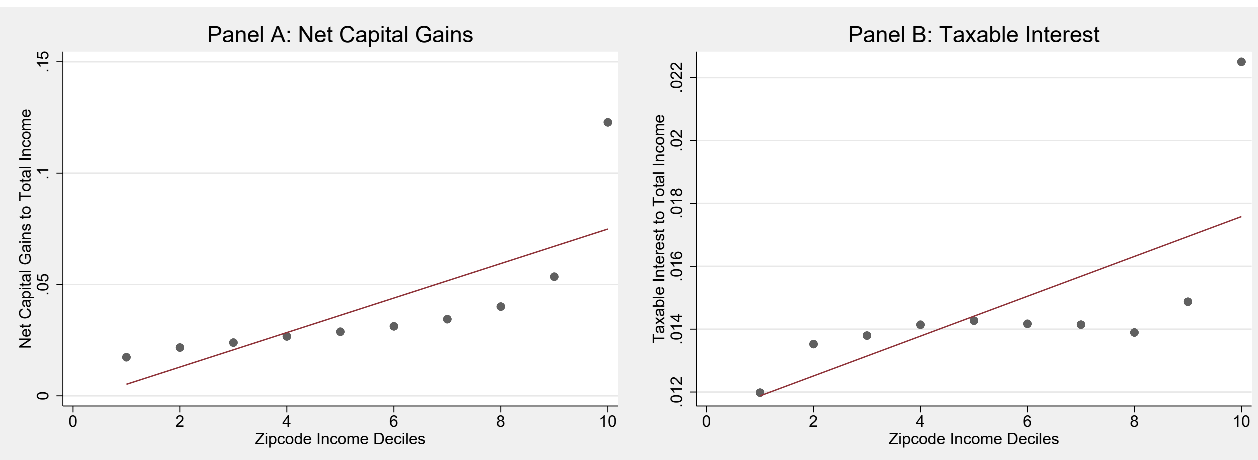

Figure 3

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, 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. 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 and the IRS-SOI.

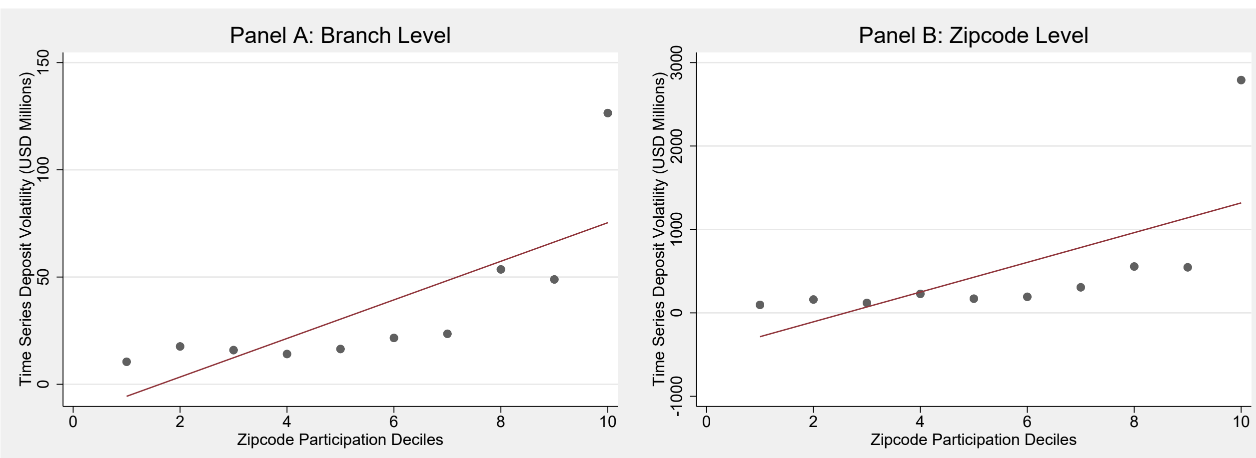

Figure 4

Stacked DiD on State Tax Rate Changes: Dynamics

This figure plots estimates of the interaction coefficient β_2 of the stacked DiD specification (4) for each event-year in a cohort. The underlying regression specification is identical to that of Table 9, column (1), with the exception that we replace the *Post* indicator with individual indicators for each event-year taking year $t-2$ as the baseline. The sample period is 2004-2020.

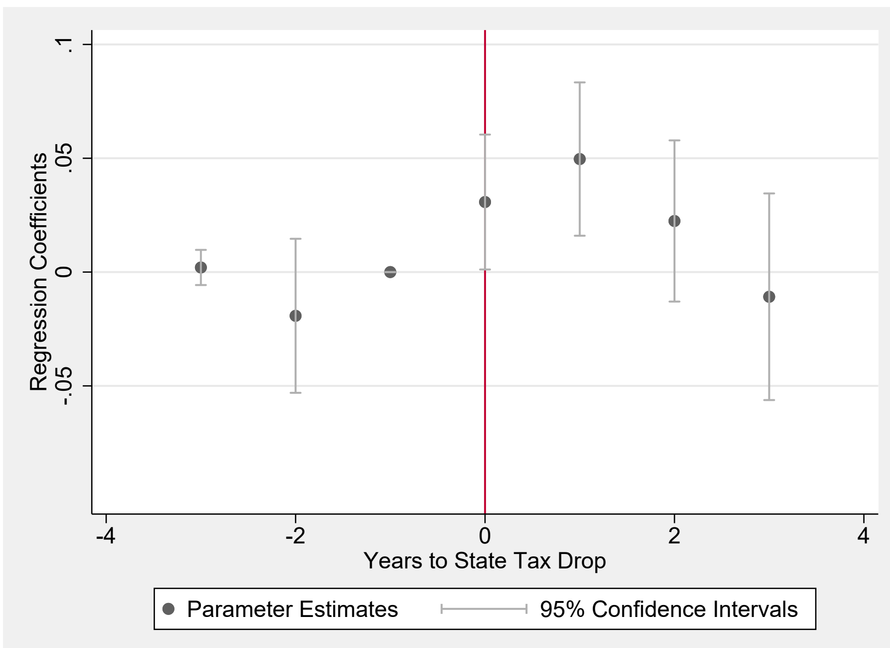

Figure 5

Participation and Deposit Market Power

In this figure, we ask whether cross-sectional variation in local participation helps explain market power. To construct this figure, 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 then produce a bin scatter plot of the estimated deposit betas on average participation for each zipcode in our sample. We remove variation in deposit betas due to 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 as a proxy of participation. In Panel B, we use interest income. In Panels C and D we repeat the same exercise by plotting the estimated deposit betas on average deposit HHI and average number of branches in the zipcode throughout our sample period. Data on the target Fed funds rate comes from the Federal Reserve of St. Louis' website. The sample period is 2004-2020.

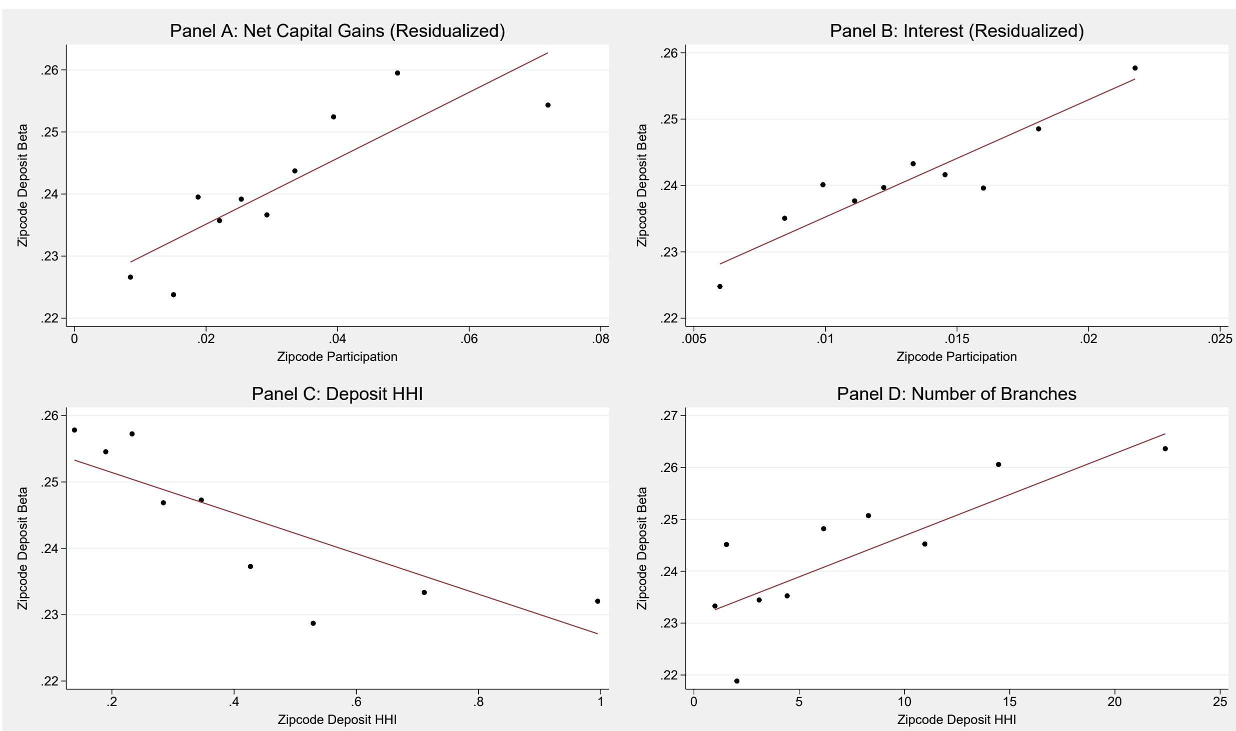

Figure 6

Inflation Inequality and Real Poverty Spreads

In this figure, we study how nominal poverty spreads and inflation inequality jointly determine real poverty spreads. We first compute average APYs at the zipcode-product-year level. We then combine these APYs with zipcode-year level Törnqvist measures of inflation, whose construction we detail in Appendix Section A.I. 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 across nominal and real APYs, we normalize both variables by subtracting their average across all buckets.

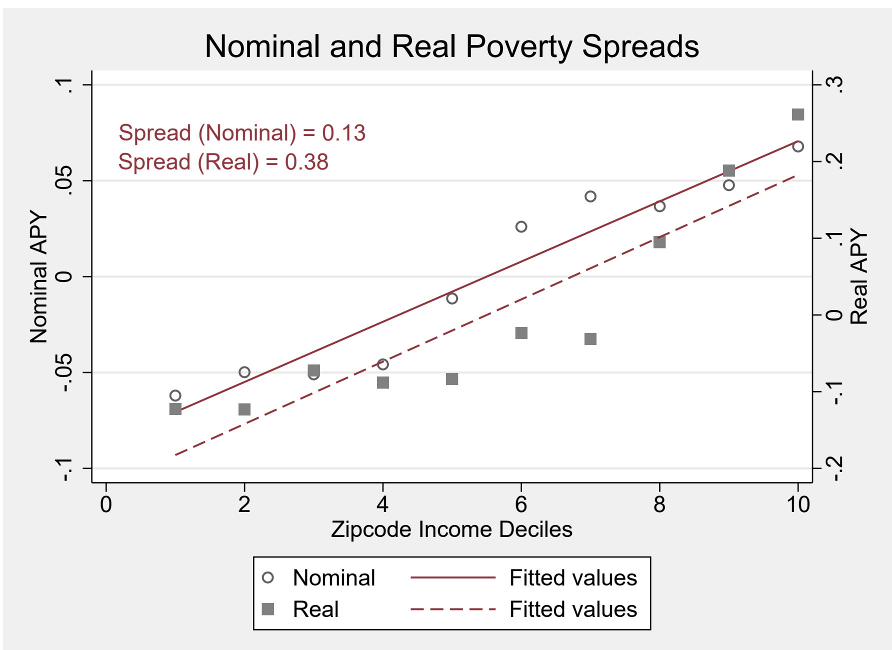

Figure 7

Broadband Usage and Participation

In this figure, we study the relationship between broadband usage and participation in financial markets. In Panel A, we plot average *Net Capital Gains to Total Income* in each decile of the broadband usage distribution. In Panel B, we plot average *Interest to Total Income* in the same deciles. The figure comes from a single cross-section of zipcode-level data in 2020, as described in section 5.3.

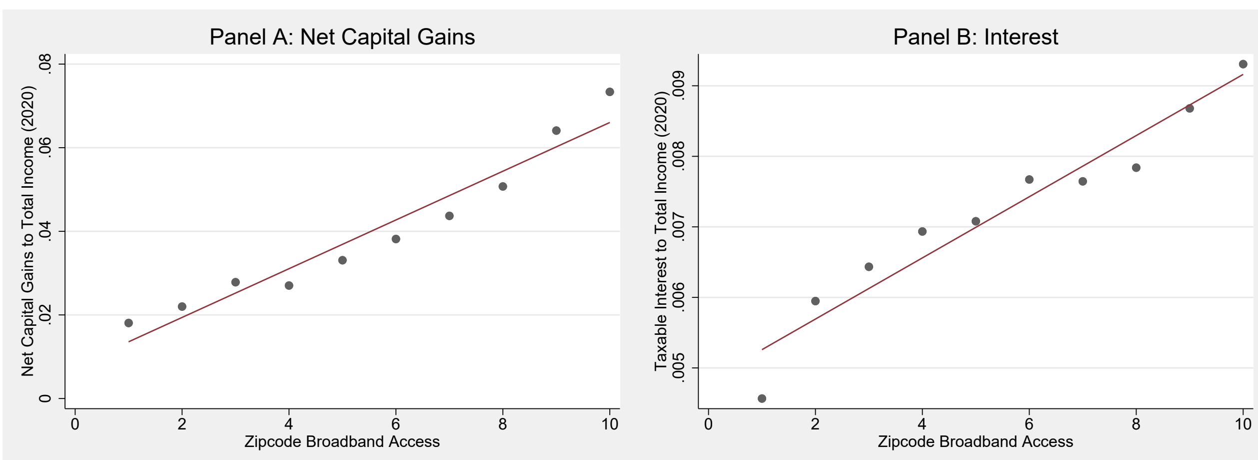

Table 1

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 sub-product. *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. *Deposit HHI* is the deposit Herfindahl-Hirschman index, calculated at the zipcode- or county-level using bank-year level deposit shares from the FDIC SOD. *Deposit Growth* is the June-to-June log difference in deposits at the branch-level or the zipcode-level. *State Rate, Long Gains* is the state tax on capital gains for top earners, publicly available on the NBER website. *Local Deposit Beta* is the 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.I. *Broadband Usage* is the fraction of computers with broadband access in 2020, publicly available at the zipcode-level from Microsoft. The sample period for *Crisis Misconduct* is 2011-2020. The sample period for all other variables 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
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
Broadband Usage (%)	54.43	31.36	12.70	52.60	100.00	650,495

Table 2

Deposit 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)	(6)
log(Per Capita Income)	0.013**	0.121***	0.129***	0.128***	0.136***	0.015***
	(0.005)	(0.016)	(0.015)	(0.013)	(0.014)	(0.002)
Year FE	Yes	Yes	Yes	Yes	Yes	No
Zipcode FE	No	Yes	Yes	Yes	No	No
Product FE	No	No	Yes	Yes	No	No
Bank FE	No	No	No	Yes	No	No
Bank × Product FE	No	No	No	No	Yes	No
Zipcode × Product FE	No	No	No	No	Yes	No
Bank × Product × Year FE	No	No	No	No	No	Yes
R-Squared	0.386	0.406	0.740	0.751	0.824	0.977
Observations	629,452	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

Catering to Depositors: Product Characteristics and Local Income

In this table, we study the relationship between local income and several deposit product characteristics. The dependent variable in columns (1) to (3) are the natural logarithms of *Number of Subproducts*, *Minimum Subscription Size* and *CD Maturity*, respectively. We estimate the baseline model (1) using our preferred combination of fixed effects from column (5) of Table 2. All the variables are defined as in Table 1. The sample period is 2004-2020.

	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 $\times$ Product FE	Yes	Yes	Yes
Zipcode $\times$ Product FE	Yes	Yes	Yes
R-Squared	0.899	0.793	0.824
Observations	621,409	547,231	130,464

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 and *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 in the bottom quartile of the deposit HHI distribution in each year. In column (4), we report our findings 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 various components to study their individual impact on poverty spreads. In 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) and (4), we perform two placebo tests respectively using *Salaries to Total Income* as the main independent variable and *Salaries to Total Income* as an independent variable while controlling for *NCG to Total Income* and *Interest to Total Income*. The data used in the construction of 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)
NCG to Total Income	0.0039***			0.0038***
	(0.000)			(0.000)
Interest to Total Income		0.0215***		0.0205***
		(0.003)		(0.003)
Salaries to Total Income			-0.0015***	0.0001
			(0.000)	(0.000)
Year FE	Yes	Yes	Yes	Yes
Bank $\times$ Product FE	Yes	Yes	Yes	Yes
Zipcode $\times$ Product FE	Yes	Yes	Yes	Yes
R-Squared	0.824	0.824	0.824	0.824
Observations	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 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 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 CD Term Spreads

In this table, we show 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.

	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 the results of estimating 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 on *NCG to Total Income* as a measure of participation. In column (2), we present the results of the second stage, where we regress *NCG to Total Income* instrumented by the state-level tax rate on local deposit APYs as in 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 eaners 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.

Table 9

State Tax Rate Changes: Stacked DiD

In this table, we report the results of the stacked DiD model (4) 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 (4) within each cohort. *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 10

Broadband Usage and Poverty Spreads

In this table, we ask whether poverty spreads vary based on local broadband usage. In column (1), we interact our baseline independent variable from 2, namely, the natural logarithm of *Per Capita Income*, with *Broadband Usage*. In column (2), we interact the same baseline variable with an indicator equal to one for zipcodes with above-median broadband usage, and equal to zero otherwise. In column (3), we interact the baseline variable with an indicator for zipcodes in the top quartile of the broadband usage distribution, and equal to zero otherwise. The data on 2020 broadband usage comes Microsoft. The sample period is 2004-2020.

	Dep. Variable: Deposit Product APY		
	(1)	(2)	(3)
log(Per Capita Income)	0.216***	0.172***	0.183***
	(0.021)	(0.021)	(0.016)
log(Per Capita Income) × Broadband Usage	-0.127***		
	(0.025)		
log(Per Capita Income) × Above Median Br. Usage		-0.044**	
		(0.019)	
log(Per Capita Income) × Top Quartile Br. Usage			-0.083***
			(0.016)
Year FE	Yes	Yes	Yes
Bank × Product FE	Yes	Yes	Yes
Zipcode × Product FE	Yes	Yes	Yes
R-Squared	0.823	0.823	0.823
Observations	618,599	618,599	618,599

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 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

$$1 + Törnqvist_{zt} \quad = \quad \prod_{u} \left(\frac{Price_{uzt}}{Price_{ut-1}} \right)^{\frac{Share_{ut} + Share_{ut-1}}{2}}, \qquad \text{(A.I.1)}$$

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 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.II Broker Misconduct During the Crisis

In this Appendix section, we focus on cross-sectional identifying variation across geographic areas differently exposed to financial market misconduct during the financial crisis.[A.II.1] Our two-stage least squares specification for these tests take the form

$$N\hat{C}G_{z(c)t} \quad = \quad \tilde{\alpha} + \tilde{\beta} Crisis\ Misconduct_{(z)ct} + \tilde{\gamma}_{FE} + \epsilon_{z(c)t}, \qquad \text{(A.II.1)}$$

$$d_{ipb(zc)t} \quad = \quad \alpha + \beta N\hat{C}G_{z(c)t} + \gamma_{FE} + \varepsilon_{ipb(zc)t}, \qquad \text{(A.II.2)}$$

where (A.II.1) is the first stage and (A.II.2) is the second stage. In the first stage, we regress post-crisis *Net Capital Gains to Income* at the zipcode level on the average share of brokers that committed

[A.II.1] 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.

misconduct during the 2007-2010 period. 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 Income* are instrumented by the the share of brokers in misconduct during the crisis. 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.II.1)-(A.II.2). Table A.15 shows qualitatively identical results to Table 8 even when we use a completely different source of variation in participation incentives 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 poverty spreads in the years after the crisis.[A.II.2] 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 the results disappear 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.II.3] 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

[A.II.2]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.

[A.II.3]The Egan et al. (2019) data is available for 2007-2015, which limits our sample to this period in these tests.

us to study the interaction between income and misconduct while controlling for *time-varying* correlated economic conditions with the baseline level of broker misconduct in the city. 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 wealthier households, which in turn allows banks to reduce their deposit poverty spreads.

A.III Additional Results

Figure A.1

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 taxable interest income (rather than net capital gains) to total income as a proxy of local non-deposit participation. The procedure employed to produce this figure is otherwise identical to the procedure described for Figure 3.

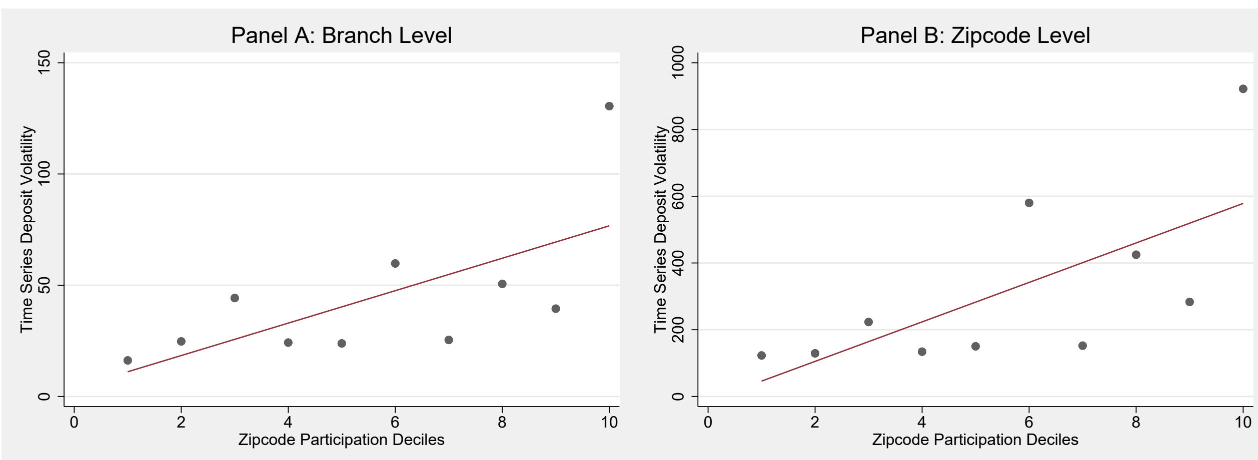

Figure A.2

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.

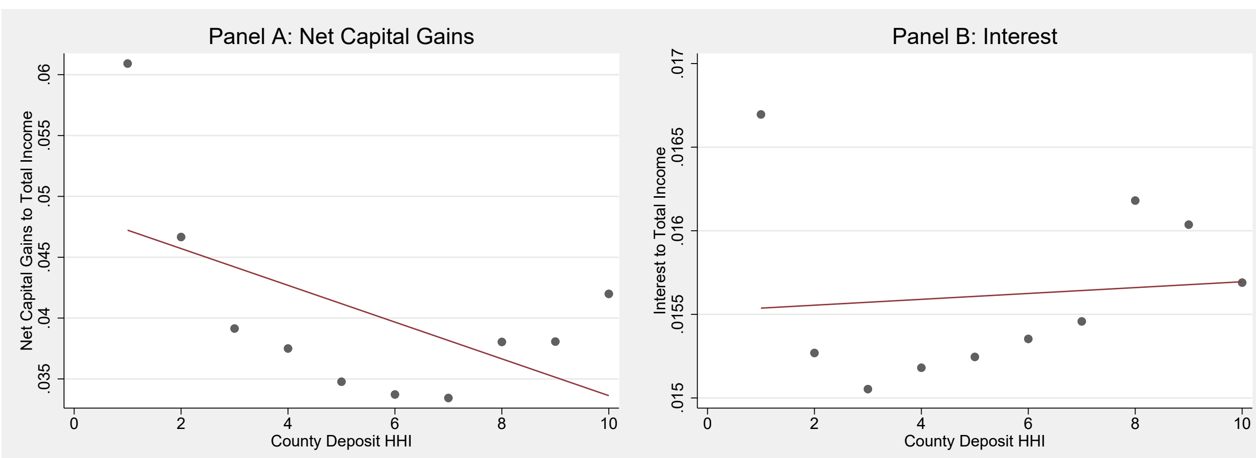

Table A.1

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)	(6)
log(Per Capita Income)	0.015***	0.156***	0.173***	0.177***	0.172***	0.021***
	(0.006)	(0.015)	(0.015)	(0.014)	(0.014)	(0.003)
Year FE	Yes	Yes	Yes	Yes	Yes	No
Zip Code FE	No	Yes	Yes	Yes	No	No
Product FE	No	No	Yes	Yes	No	No
Bank FE	No	No	No	Yes	No	No
Bank $\times$ Product FE	No	No	No	No	Yes	No
Zip Code $\times$ Product FE	No	No	No	No	Yes	No
Bank $\times$ Product $\times$ Year FE	No	No	No	No	No	Yes
Pseudo R-Squared	0.207	0.219	0.399	0.405	0.427	0.479
Observations	629,452	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 (2) to (5) of Table 2 with the addition of branch-zipcode 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)	0.117***	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 (2) to (5) 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)
Zip Code 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

Deposit Rates and Local Income: Granular Product Definitions

This table reports the results of a robustness test on Table 2 in the main paper, using a panel of granular subproducts defined by their maturity and minimum subscription size rather than averaging rates at the broad category level. The granular subproducts reported in this table are savings deposits with minimum subscription size of USD 2k, checking accounts with no minimum subscription size of 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.

	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 A.5

Participation and Poverty Spreads: Controlling for Income

In this table, we provide a robustness test for the results of Table 5 by conditioning on local income levels in our main participation regressions. In column (1), we include the natural logarithm of total income per capita in the regression. 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.6

County Deposit Market Structure

In this table, we conduct a robustness test on the results from Table A.6 in the main paper, using cross-sectional variation in county-level deposit market structure as opposed to zipcode-level deposit market structure. All the market structure variables are identical to those in Table A.6, with the exception that they are constructed the county-level rather than the zipcode-level. All the other variables are identical to those in Table A.6. 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) $\times$ High Dep. HHI		0.053***		
		(0.013)		
Year FE	Yes	Yes	Yes	Yes
Bank $\times$ Product FE	Yes	Yes	Yes	Yes
Zipcode $\times$ 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.7

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 our baseline estimate with 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 vigintile 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.8

Geographic Variation

In this table, we study how the baseline estimates presented in Table 2 vary in the cross-section of bank geography 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 RUCA levels indicate zipcodes located in more rural areas. In column (2), we interact the natural logarithm of *Per Capita Income* with four indicators based on the 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 vigintile 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.9

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) to interest income, product servicing income (Call item RIADB492) to interest income, and brokerage income (the sum of Call items RIADC886 RIADC887 RIADC386 RIADC387, available from 2007 onward) to 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.10

Participation and Poverty Spreads: Controlling for Income

In this table, we provide a robustness test the results from Table 5 by conditioning on local income levels in our main participation regressions. In column (1), we include the natural logarithm of total income per capita in the regression. 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.11

Local Stock Market Performance and Deposit Outflows

In this table, we study the sensitivity of local deposit outflows to the performance of local assets across the distribution of participation in non-deposit markets. In columns (1) and (2), we repeat the same exercise as in Table 6 but replace the cumulative excess return of the market portfolio with the 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 that are rated "Buy" or "Strong Buy" by analysts during the year. Data on local stocks' performance comes from Compustat/CRSP. Data on analyst recommendations comes from I/B/E/S. The sample period is 2004-2020.

	Dep. Variable: Branch Deposit Growth			
	(1)	(2)	(3)	(4)
Local Portfolio Ex. Ret.	-0.058***			
	(0.004)			
High Participation	0.038***		0.066***	
	(0.011)		(0.012)	
Local Portfolio Ex. Ret. × High Participation	-0.016***	-0.017***		
	(0.005)	(0.005)		
Buy Local Stocks (%)			-0.021***	
			(0.001)	
Buy Local Stocks (%) × High Participation			-0.010***	-0.011***
			(0.002)	(0.002)
Branch FE	Yes	No	Yes	No
Branch × Zipcode FE	No	Yes	No	Yes
State × Year FE	No	Yes	No	Yes
R-Squared	0.120	0.168	0.123	0.168
Observations	221,084	220,909	221,084	220,909

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.12

Local Stock Market Performance and Deposit Outflows: Robustness

In this table, we perform a robustness test on the results of Table A.11 in the main paper. Instead of computing deposit growth at the branch-level, we compute first aggregate deposits across all branches operating in a zipcode, and then compute growth at the zipcode level. The table is otherwise identical to Table A.11.

	Dep. Variable: Zipcode Deposit Growth			
	(1)	(2)	(3)	(4)
Local Portfolio Ex. Ret.	-0.026***			
	(0.004)			
Local Portfolio Ex. Ret. × High Participation	-0.016***	-0.015**		
	(0.006)	(0.006)		
Buy Local Stocks (%)			-0.016***	
			(0.001)	
Buy Local Stocks (%) × High Participation			-0.007***	-0.006***
			(0.002)	(0.002)
Zipcode FE	Yes	Yes	Yes	Yes
State × Year FE	No	Yes	No	Yes
R-Squared	0.096	0.146	0.099	0.146
Observations	126,604	126,604	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 A.13

State Tax Rates in Local Participation Buckets

In this table, we study how state-level capital gains taxes for top earners affect participation along the participation distribution. 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.14

Stacked DiD Robustness

In this table, we report the results of a robustness test on the results of Table 9. The treatment group is identical to that in Table 9. The control group consists of states that do not experience state rate changes within the cohort. The table is otherwise identical to Table 9.

	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.II.1)-(A.II.2). 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 estimating sample for 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 $\times$ 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 the results of 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 columns (1) of both panels for reference.

Panel A: First Stage

	Baseline	≥ 2012	≥ 2013	≥ 2014	≥ 2015	≥ 2016	≥ 2017	≥ 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	≥ 2012	≥ 2013	≥ 2014	≥ 2015	≥ 2016	≥ 2017	≥ 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 Poverty 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 the same area. 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 active 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.

Table A.18

Time Deposit Maturity and Outflows

In this table, we ask whether short- and long-maturity time deposits experience quantitatively different outflows depending on the 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.