

Shadows on Main Street: Local Financial Architecture and Asset Reallocation*

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May 6, 2026

Abstract

We construct a novel panel of local financial architecture from Yellow Pages in Indiana, Michigan, and Ohio, covering 171 counties and over 17,000 branches from 1920 to 2000. Finance companies (shadow banks) were widespread, persistent, and co-moved positively with banks. Exploiting global corn-price changes as an exogenous shock, we show that corn-producing counties with more shadow banks experience greater farm consolidation, higher physical capital per farm, and larger land-value gains when prices rise. Broadly, we show that measuring financial development through banks alone can significantly distort empirical estimates across a wide range of banking studies.

Keywords: Banking, Shadow Banks, Local Finance, Asset Reallocation, Measurement Error

JEL Classification: G21, G23, L1

*We are grateful to Paolo Pasquariello, Rodney Ramcharan, Uday Rajan, James Weston, and seminar and conference participants at the Brigham Young University, University of Michigan, Miami, McGill, UT Arlington, Michigan State University Federal Credit Union Conference, Rochester, Southern Methodist University, the OCC-Treasury, Pontificia Universidad Catolica de Chile, the Fixed Income and Financial Institutions Conference, the Federal Reserve Monetary and Financial History Workshop, and the Lonestar Finance Conference. An earlier version of the paper was circulated under the title “Shadows on Main Street: Mapping America’s Financial Architecture from the Yellow Pages of the 20th Century.”

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1 Introduction

How does finance affect the allocation of productive assets across different agents in an economy? This is a foundational question in economics and finance, dating at least to Schumpeter (1911) and developed further in a rich theoretical and empirical literature.¹ Yet much of the empirical evidence on the effect of local finance on real outcomes focuses exclusively on banks rather than the full spectrum of financial services providers present in the area. That focus is often dictated by data availability, not by theory. Local credit markets have long included finance companies and other nonbank intermediaries that maintained branch networks and supplied credit for assets such as automobiles, farm equipment, and homes. We therefore know very little about how local non-bank intermediaries, i.e., shadow banks shape financial architecture and the reallocation of productive assets.

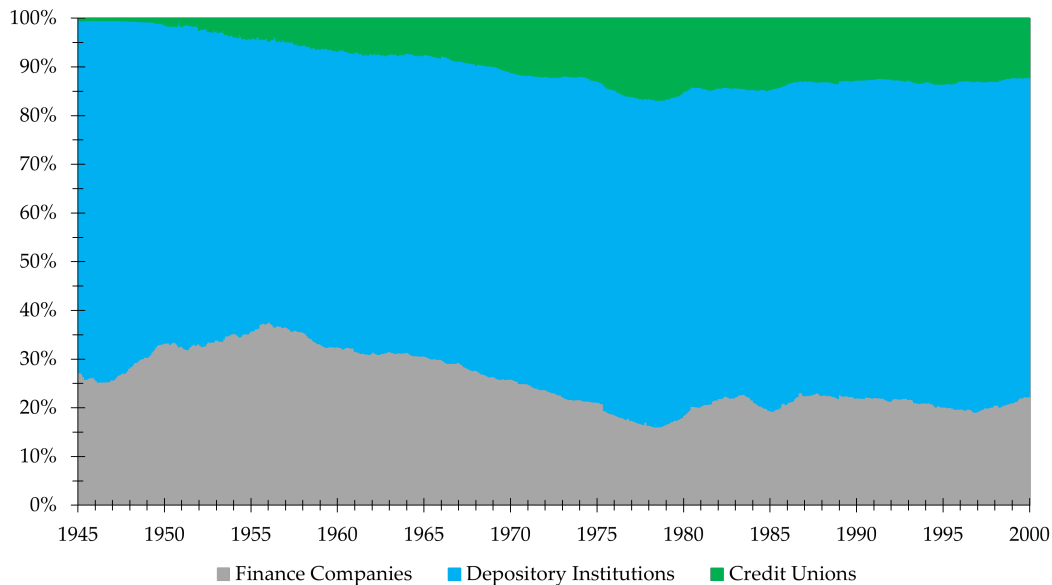
In light of their prominent role in the global financial crisis, shadow banks are often viewed as a modern phenomenon: recent innovations operating at the periphery of the traditional regulated banking system and Wall Street capital markets. This view overlooks a century of financial history. For much of the 20th century, a large sector of non-depository finance companies operated on America's Main Streets, providing credit for everything from automobiles to farm equipment. What we miss by leaving out these shadow banks is economically large: Figure 1 shows that nonbank finance companies consistently provided between 16% and 38% of all consumer credit in the post-war era. Because researchers typically lack systematic local data on nonbank financial institutions, this sector has remained largely invisible in empirical work on local finance.

Our first contribution is the data. The reason this sector has remained largely absent from empirical work is not that it was unimportant; it is that researchers have lacked reliable, granular, long-run data on local shadow-bank presence. We address this gap by constructing

¹See (e.g., Leland and Pyle, 1977; Ramakrishnan and Thakor, 1984; King and Levine, 1993; Boot and Thakor, 1997; Jayaratne and Strahan, 1996; Rajan and Zingales, 1998; Subrahmanyam and Titman, 1999; Allen and Gale, 2000; Levine, 2002; Allen and Gale, 2004; Levine, 2005; Chen, Goldstein, and Jiang, 2007; Diamond and Rajan, 2000; Butler and Cornaggia, 2011; Rajan and Ramcharan, 2015, and several others).

Figure 1: The Supply of Consumer Credit in the U.S.

This figure presents the provisioning of consumer credit in the U.S. from 1945–2000 broken down across depository institutions, finance companies, and credit unions. The data are provided by the Federal Reserve Board’s *G-19: Consumer Credit* files, which can be found at https://www.federalreserve.gov/releases/g19/HIST/cc_hist_mh_levels.html. The figure excludes consumer credit outstanding held by the federal government, nonprofit and educational institutions, and non-financial businesses.



a new hand-collected county-level panel of local financial architecture from Yellow Pages directories. These directories were a comprehensive source of local business listings before the internet era.² The panel covers 171 counties in Indiana, Michigan, and Ohio from 1920 to 2000 and identifies more than 17,000 branches from 5,700 unique financial institutions. The data distinguish banks, regulated nonbanks, and non-depository finance companies, which we treat as historical shadow banks because they supplied credit outside the traditional banking system while maintaining local branch networks. This data collection effort yields a long-run map of the level, mix, persistence, and evolution of local financial architecture over eight decades.

The data allow us to document three new facts about shadow banks and local financial

²A 1972 congressional study of consumer credit points to telephone-book Yellow Pages as a way to observe the local set of competing credit grantors, including banks, finance companies, savings and loans, industrial loan companies, credit unions, savings banks, and retailers (National Commission on Consumer Finance, 1972, p.11).

architecture that were not visible in standard regulatory sources. First, finance companies were a large component of local finance, accounting for roughly one-third of local financial-institution branches and nearly two-fifths of distinct local financial firms, on average. Second, local financial architecture was remarkably persistent: a county's mix of banks and finance companies predicts its structure even 50 years later. Third, banks and finance companies positively co-moved at the county level rather than simply substituting for one another. These facts matter for empirical work: bank-only data do not merely omit a fringe sector; they miss a large, persistent, and systematic part of local finance, with potential consequences for both the measurement of financial development and its effect on real outcomes. They also motivate our key exposure measure, the *Shadow Ratio*: finance-company branches as a share of total financial-institution branches in a county.

With this measurement in hand, we turn to the paper's main economic question: How does the local mix of banks and shadow banks shape asset reallocation following fundamental shocks? To answer this question, we need a locally financed asset and a shock to the value of reallocation that is plausibly unrelated to pre-existing local financial architecture. Farmland is a natural setting because it is a key productive asset whose financing, ownership, and value are inherently local, especially over much of our sample (Rajan and Ramcharan, 2015). The setting also connects directly to work on agricultural finance and real outcomes. Butler and Cornaggia (2011) study bank finance and agricultural productivity, while Rajan and Ramcharan (2015) show how credit conditions shaped farmland-price dynamics. Our distinction is that we observe the broader local financial architecture, including finance companies, and ask whether that architecture changes the reallocation of farmlands in the area.

As a shock to the need for reallocation we use plausibly exogenous variation in global corn prices which changes the profitability of farming and consequently the need for reallocation. Higher corn prices increase the incentive for capital investment such as replacing old tractors with newer and more efficient ones or installing a new irrigation system, as in the real option

model of Dixit and Pindyck (1994). This need for large scale capital investment is likely to increase the return to scale and hence the need for consolidation of farmlands. Shadow banks can facilitate such reallocation by writing flexible lending contracts that regulated banks are unable or unwilling to write, or by responding quickly to market conditions by bringing in funds from non-deposit sources of liability. For example, shadow banks were the first to offer installment loans for moveable assets like tractors that banks were reluctant to do (Nugent, 1939).³ Therefore, shadow banks are likely to expand the set of contracts that can be written between the borrowers and the savers, changing the nature of resource allocation in the system (Allen and Gale, 2004; Skrastins, 2023).

Using global corn prices as a source of variation rests on the idea that these prices are determined by forces such as weather, wars, and political instability in different regions around the globe, and that counties in Indiana, Michigan, and Ohio cannot meaningfully affect them. We use a triple-difference research design that compares corn-producing and non-corn-producing counties before and after global corn-price changes, allowing the response to vary with lagged *Shadow Ratio*. The design asks whether the same corn-price shock produces different reallocation responses in corn versus non-corn producing counties with different levels of pre-existing *Shadow Ratio*. The comparison with non-corn counties helps account for shocks affecting agriculture in these states more broadly.

The main estimates show a strong consolidation response. When corn prices rise, corn-producing counties with higher lagged *Shadow Ratio* experience larger declines in farm counts and larger increases in average farm size. Moving from the 25th to the 75th percentile of *Shadow Ratio* is associated with about a 9 percent larger reduction in the number of farms after a one-standard-deviation increase in corn-price returns. The response is not simply a decrease in the number of farms, average farm size rises as well suggesting that shadow banks

³ “Banks were extremely wary of loans secured by a commodity whose location could be shifted so readily and they looked askance at long-term credits for the purchase of such luxuries. The instalment finance company stepped into the breach between the consumer who wished to buy an automobile on partial payments and the dealer who wanted to make the sale but lacked the capital to finance it” (Nugent, 1939, p.80)

facilitate consolidation of farm-lands in corn-producing areas when corn prices rise.

We next examine investment in physical capital by these farms. The farming industry underwent heavy mechanization during much of our sample period, and tractors were a central capital input (Olmstead and Rhode, 2000). Tractors are especially informative in our setting because they are collateralizable movable assets, closely aligned with the installment-lending technologies in which finance companies specialized (Nugent, 1939). In high-*Shadow Ratio* corn counties, we document an increase in tractors per farm. The total number of tractors do not increase, however. Thus, the consolidation response is accompanied by a shift toward fewer farms that operate at larger scale and use larger and likely more efficient tractors more intensively.

Do markets value this reallocation? We cannot compute a farm-level production function, but we do observe land values, which allows us to shed light on this question. Farmland values should rise when buyers capitalize expected scale, cost, timing, or financing advantages associated with reallocating land and capital toward operators who can use them more intensively. This is what we find: moving from the 25th to the 75th percentile of *Shadow Ratio* is associated with roughly a 6 percent larger increase in land value per acre after a one-standard-deviation increase in corn-price returns. The land-value response is not accompanied by a detectable increase in corn yields. This absence of a yield response is unsurprising given that corn yields depend heavily on agronomic factors such as weather, soil fertility, seed variety, planting decisions, tillage, and input regulation (Wonders, 2004). Collectively, these results point toward a cost advantage that comes from effective scale of production and mechanization of physical capital such as tractors that shadow banks facilitate.

While these results point to potential benefits from the reallocation channel, the same channel may also introduce costs. Specifically, greater reallocation may be accompanied by higher asset-price volatility, as in several models of financial development (Allen and Gale,

2000, 2004). We find some support for this efficiency-volatility trade-off: counties with higher *Shadow Ratio* exhibit more volatile land-value growth over the long run.

In the final part of the paper, we provide two implications of our work and the usefulness of our novel data: one related to policy decisions and the other related to academic literature that uses banking data. On the policy front, we show that relying on measures of competition based simply on the presence of banks can result in a fundamental mischaracterization of local competition in the market for financial services. For example, the average “banks-only” Herfindahl-Hirschman Index (HHI) in our data is 0.27, suggesting a moderately concentrated market. However, the “all financial intermediary” HHI including all institutions is only 0.09, suggesting a relatively competitive market. A rank ordering of competitiveness based on bank-only HHI can lead to substantial misclassification of relative competitiveness.

In terms of empirical work in the literature, we show that omitting shadow banks from regressions that truly want to capture the entire range of financial services creates a non-classical measurement error. The measurement error depends on the empirical specification used by the researchers. For example, studies using banks-per-capita in levels *overestimate* the true effect of financial development on outcomes such as credit or GDP growth by 28% to 72%. In contrast, studies using the log of banks-per-capita *underestimate* the effect by 13% to 35%. This bias varies systematically across urban and rural counties, carrying significant implications for policy analyses that rely on these potentially flawed estimates. We also show that the presence of shadow banks can, in many circumstances, cause significant overestimates of the effect of banks on real outcomes because of omitted variable bias. We provide direct guidance for future research by quantifying the bias from mis-measuring financial development.

The paper makes three contributions. First, we provide a new long-run map of local financial architecture that measures finance companies and other nonbank intermediaries alongside banks. This data contribution is foundational: finance companies were large,

persistent, and positively related to banks, so banks alone are not sufficient statistics for local finance. Second, we contribute to the literature on asset allocation and the real effects of finance by showing that counties with higher *Shadow Ratio* respond differently to real shocks: farms consolidate more, tractor capital becomes more concentrated on surviving farms, and land values rise more (e.g., Leland and Pyle, 1977; Ramakrishnan and Thakor, 1984; Boot and Thakor, 1997; Subrahmanyam and Titman, 1999; Chen, Goldstein, and Jiang, 2007). Third, we identify and quantify a major measurement problem in empirical finance and provide calibration tools for settings where only bank data are available. Bank-only measures can distort financial development, local financial competition, and regression estimates when nonbank intermediaries are an important part of local credit markets.

2 Data Collection & Sample Construction

Most empirical studies measure local financial development using regulatory data on banks. These sources are valuable, but they were designed around regulated depository institutions and provide limited coverage of nonbank financial providers. Even for regulated banks, long-run branch-level data at a consistent local geography are limited, especially before the modern FDIC branch-data era. Comptroller reports, for example, cover national banks but omit state banks, credit unions, S&Ls, and finance companies. Further, even for national banks, the Comptroller reports do not include the location or number of bank branches, which are important for understanding local financial-provider presence.

We take a novel approach to our data collection by turning to historical city and county directories. Instead of relying only on regulatory databases and archives, we obtain information on financial institutions from directories that listed local businesses in the pre-internet era. These directories were printed for major cities by telephone and telegraph companies starting in the mid-1800s and became regularly available for many cities around 1920. By the 1920s, major publishers such as Polk's and Robinson's made phone-book formats broadly comparable

across many cities, although headings and coverage still vary across place and time. A typical phone book contains multiple sections: the white pages, which include listings for phone numbers and addresses of individuals; the Yellow Pages, which include listings for businesses by category; reverse-address sections; and occasionally other local reference material. Our data come from the Yellow Pages section of these books.

In the pre-internet era, these directories were printed and widely distributed to local citizens and businesses. They were a primary mechanism through which consumers found contact information for local merchants and service providers. Each book covers a specific geographic region, usually a city, county, metropolitan area, or set of surrounding communities. Many phone books also contain information about “rural routes,” which include smaller municipalities outside the main municipal coverage area. Other phone books cover an entire county or multiple cities and suburbs within a county. Together, the coverage of major cities and the frequent inclusion of rural routes and adjacent suburbs give us a very broad view of local financial-service listings in the areas and years we cover. Because directory availability and geographic scope still vary, we track the covered municipality, county, rural-route coverage, adjacent areas, source archive, and year for each directory, and use this inventory to define the county-period panel.

These directories have long been of interest to historians and genealogists, who use them to recover addresses, business names, and local activity that is otherwise hard to observe. As a result, city directories have been collected by genealogy libraries, local libraries, and state historical libraries, and many are now digitized. This preservation creates a clear archival trail for the source material.

We obtain these data for Indiana, Michigan, and Ohio from several sources. We begin with available city directories and phone books from the Library of Michigan, the Indiana State Library, and the Ohio History Connection Library. We augment those collections with digitized directories from Ancestry.com and supplement the Detroit-area data with material

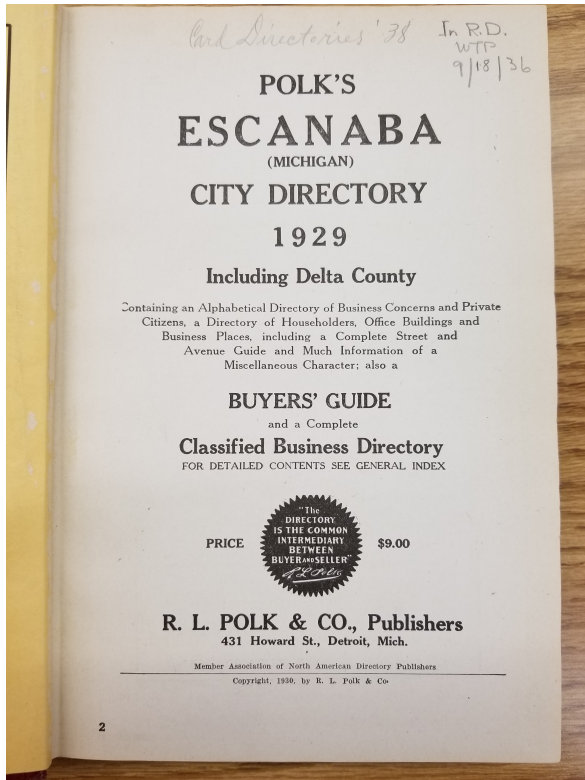
from the Royal Oak Public Library, which contains a more extensive collection of Detroit-area directories than the state Library of Michigan.

The phone book section of interest to us is the Yellow Pages, which list businesses by the service they provide. Some directories also include special advertising sections available for purchase by these businesses, but our data collection focuses on the categorical business listings. The vast majority of directories contain sections that list businesses in rough categories such as “plumbing,” “churches,” or “banks.” We record entries that appear under financial-service headings: bank, collections, credit/credit unions/credit bureaus, finance companies, loans (and any subcategories listed after loans), savings/savings and loan, building and loan, and trust/trust companies. Because listing requirements were not uniform, we photograph the same set of financial headings across books to maintain continuity, and we preserve the original heading in the data. These headings are fairly common across books, although earlier directories use fewer categories and later directories often include more detailed loan categories. For instance, in many cities, there are multiple categories of loans listed. It is not uncommon for a phone book to have “loans - auto,” “loans - home/mortgage,” and “loans - farm.” We preserve this information because it allows us to distinguish broader finance-company listings from product-specific loan categories when needed. From these raw data, we identify the branch networks of banks, credit unions, savings and loans/building and loans, trusts, and finance companies. For the main analysis, we group banks separately, classify credit unions and S&Ls/building and loans as regulated nonbanks, and classify non-depository finance companies as the historical shadow-bank category. We use the term shadow bank to denote lenders outside the traditional bank prudential regime, not institutions outside regulation altogether. The result is a long-run panel of listed local financial providers from 1920 to 2000, covering banks, regulated nonbanks, and finance companies in the counties and years for which directories are available.

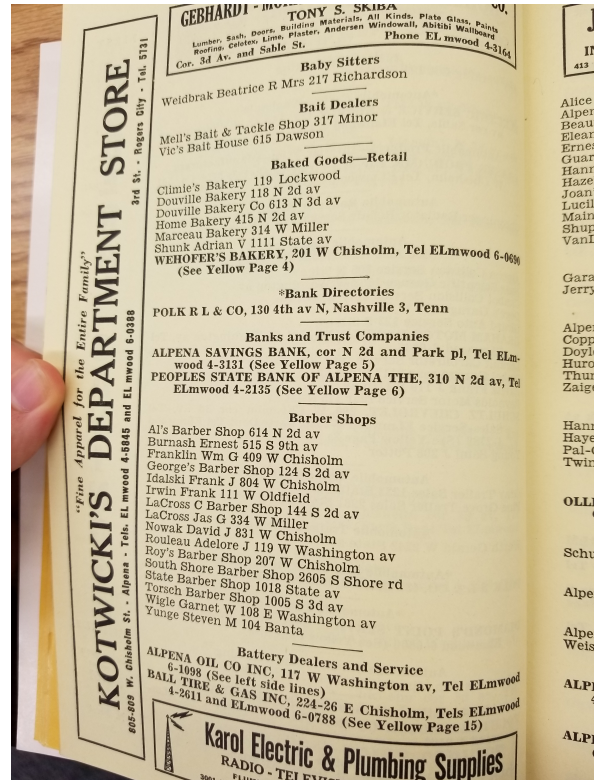
Figure 2(a) shows the cover page from one of the phone books in our sample. The cover page contains information related to the city/county and year covered by the book, and it

Figure 2: Example of Raw Phone Book Data

Panel (a) shows an example phone book cover page, which contains information about the directory type, publisher, publication date, covered location, and surrounding areas included in the book. Panel (b) shows a photographed Yellow Pages page containing financial-service listings. We photograph pages under financial-service headings, including banks, collections, credit unions, credit bureaus, finance companies, loans and loan subcategories, savings and loans/building and loans, and trusts. Section 2 details the sample collection process.



(a) Sample Phone Book Cover Page



(b) Sample Phone Book Data

also lists smaller cities included in the directory in addition to the larger municipality. Figure 2(b) shows a typical Yellow Pages entry under a financial-service heading. Together, the panels show how the source identifies the covered geography and how financial institutions appear in the underlying listings.

We sample directories at approximately five-year intervals from 1920 to 2000 for each town or county with available source material, then aggregate entries to county-period observations using the directory's stated geographic coverage. We choose this range for three reasons. First, directory coverage in our source collections becomes broad enough around 1920 to support a panel. Second, five-year intervals provide a more balanced historical panel than

annual collection would permit. Third, local financial architecture evolves slowly enough that five-year intervals capture the relevant medium-run variation in entry, exit, and branching. We stop in 2000, when internet search began to replace printed directories as the dominant local business directory.

After photographing or saving the relevant directory pages, we transcribe the financial-service entries into a structured dataset. Each record contains the directory identifier, source archive, image number, county, state, raw institution name, raw heading, listed branch address or addresses, listed ZIP code when available, and the standardized institution category assigned by our classification protocol.

We then geocode entries using the Google Maps API and retain geocoding metadata for audit and correction. Automatic geocoding is successful for most entries, but some historical addresses cannot be precisely located because the address is no longer valid. In these cases, we assign counties only when the directory coverage, municipality, address information, and manual inspection support the assignment. Occasionally, the same financial-institution branch appears in multiple directories because directory coverage areas overlap. We identify these duplicates using institution name, address, year, and directory-coverage rules, and retain one branch record.

We also link branch records and parent institutions within and across years. To do this, we follow a structure similar to the modern call-report format: each parent institution receives a unique identifier, and each branch receives a unique identifier. Branch links are based on standardized name and address; parent-institution links are based on standardized name and city or county. The result is a county-level panel of listed financial-provider branches and parent institutions that allows us to measure the level, composition, and persistence of local financial architecture over time. The real-economy analysis below merges this financial-architecture panel to Agricultural Census and land-value data. That merge yields a smaller 143-county panel during 1930-1985; the merge funnel and outcome-specific

samples are reported when the real-economy sample is introduced.

3 Financial Architecture: Descriptive Facts

In this section, we present several new facts about the local composition of the supply of finance, which we refer to as “financial architecture.” Local financial systems were not simply more or less banked; they differed sharply in the mix of banks, regulated nonbanks, and finance companies. We show that finance companies are economically significant, that financial architecture varies substantially across counties and over time, that these local differences are persistent, and that nonbanks are strongly positively correlated with bank presence.

3.1 Summary Statistics

The main financial-institution panel contains 1,478 county-period observations from an unbalanced panel of 171 counties in Indiana, Michigan, and Ohio, observed at five-year intervals from 1920 to 2000. Table 1 presents our summary statistics. For each financial-architecture measure, we report values at the branch level (Br) and at the institution level ($Firm$). An average county-period has 39.2 financial-institution branches and 24.5 distinct firms; the corresponding medians are 17.5 and 14.0.

In the sampled county-periods, banks are the largest single category, averaging 17.0 branches. But nonbanks are not peripheral: regulated nonbanks and finance companies together account for more branches than banks, and finance companies alone average 12.7 branches per county-period. Finance companies account for 33% of financial-institution branches and 39% of distinct financial firms on average. Thus, a bank-only measure misses both regulated nonbanks and finance companies; finance companies alone account for roughly one-third of the branch network and nearly two-fifths of distinct local providers captured in

Table 1: Summary Statistics

This table presents sample summary statistics. *Total Financial Institutions (Br)* is the total number of financial-institution branches in a county-period. *Total Financial Institutions (Firms)* is the total number of distinct financial institutions in a county-period. Measures labeled *Br* use branch counts; measures labeled *Firm* use distinct institution counts. *Reg'd Nonbanks* are regulated nonbank financial institutions, including savings/building-and-loan associations and credit unions. *Finance Cos* are non-depository finance companies listed in the directories and classified outside the bank, savings/building-and-loan, and credit-union categories. We report branch and firm counts for each county-period observation, as well as population-scaled measures per 10,000 residents. *Shadow Ratio* is finance-company branches as a share of total financial-institution branches for branch-count measures, and finance-company firms as a share of total financial-institution firms for firm-count measures.

	mean	sd	min	p25	p50	p75	max	count
Population (000s)	149.59	280.57	2.74	33.93	62.41	131.99	2670.37	1,478
Total Financial Institutions (Br)	39.23	61.03	1.00	10.00	17.50	42.00	507.00	1,478
Total Financial Institutions (Firms)	24.45	33.93	1.00	8.00	14.00	27.00	320.00	1,478
Banks (Br)	17.02	26.80	0.00	4.00	8.00	18.00	292.00	1,478
Banks (Firms)	6.02	5.62	0.00	3.00	4.00	7.00	69.00	1,478
Reg'd Nonbanks (Br)	9.51	21.55	0.00	1.00	3.00	9.00	234.00	1,478
Reg'd Nonbanks (Firms)	7.14	16.97	0.00	1.00	3.00	7.00	226.00	1,478
Finance Cos (Br)	12.70	22.40	0.00	2.00	6.00	13.00	206.00	1,478
Finance Cos (Firms)	11.29	18.08	0.00	2.00	5.00	13.00	186.00	1,478
Financial Institutions (Br) / 10k	3.26	1.73	0.15	1.97	3.10	4.36	8.18	1,478
Financial Institutions (Firms) / 10k	2.39	1.25	0.12	1.52	2.27	3.12	6.61	1,478
Banks (Br) / 10k	1.56	1.19	0.04	0.67	1.23	2.18	5.56	1,478
Banks (Firms) / 10k	0.84	0.67	0.03	0.37	0.66	1.13	3.37	1,478
Reg'd Nonbanks (Br) / 10k	0.67	0.59	0.00	0.24	0.52	0.95	2.66	1,478
Reg'd Nonbanks (Firms) / 10k	0.55	0.47	0.00	0.21	0.47	0.80	2.28	1,478
Finance Companies (Br) / 10k	1.02	0.80	0.00	0.41	0.89	1.48	3.86	1,478
Finance Companies (Firms) / 10k	0.98	0.78	0.00	0.40	0.85	1.38	3.83	1,478
Shadow Ratio (Br)	0.33	0.22	0.00	0.14	0.33	0.50	1.00	1,478
Shadow Ratio (Firms)	0.39	0.22	0.00	0.24	0.41	0.56	1.00	1,478

our data.

Finance companies cover a wide range of lending arrangements, but installment loans backing retail sales of consumer goods were an important part of the sector (National Commission on Consumer Finance, 1972). This lending spans the spectrum of consumer credit, including home mortgages, farm lending, auto lending, loans for furniture and household goods, salary-backed loans, and other unsecured lending. Prominent names in our dataset are Household Finance Corp. (eventually HSBC), Beneficial Finance Corp., and American General. Historical accounts emphasize that some of finance companies' growth in the early 20th century came from financing retailers' receivables and from supplying automobile credit that banks were hesitant to provide (Nugent, 1939).⁴ Our directory data identify their local

⁴ "Banks were extremely wary of loans secured by a commodity whose location could be shifted so readily

branch presence, not their loan portfolios, but this historical evidence helps explain why the category is economically meaningful. Consistent with this institutional distinction, National Commission on Consumer Finance (1972, p.115) describes finance companies as generally more willing than commercial banks to serve higher-risk borrowers.

Firm counts show a related but distinct margin. The average county-period has 24.5 distinct financial firms: 6.0 bank firms, 7.1 regulated-nonbank firms, and 11.3 finance-company firms. Comparing branches to firms reveals an important banking-network margin. Banks average 17.0 branches but only 6.0 firms per county-period, while finance companies average 12.7 branches and 11.3 firms. Thus, branch counts actually understate the relative presence of finance companies as distinct local providers: finance companies account for 39% of firms, compared with 33% of branches. The point is not just that nonbanks are large; it is that branch and firm measures capture different parts of local financial architecture, and finance companies are prominent on both margins.

3.2 Financial Architecture Over Time

Figure 3 plots the counts of banks, S&Ls, credit unions, and finance companies throughout our sample.⁵ The left panel reports total branch counts by institution type, and the right panel reports total distinct firms by institution type. In 1920, the sample contains 429 bank branches, 80 building-and-loan branches, and 436 finance-company branches, corresponding to 295, 78, and 389 distinct firms. By 1980, the sample contains 2,921 bank branches, 1,403 S&L branches, 666 credit-union branches, and 1,375 finance-company branches; the corresponding firm counts are 578, 392, 587, and 534.

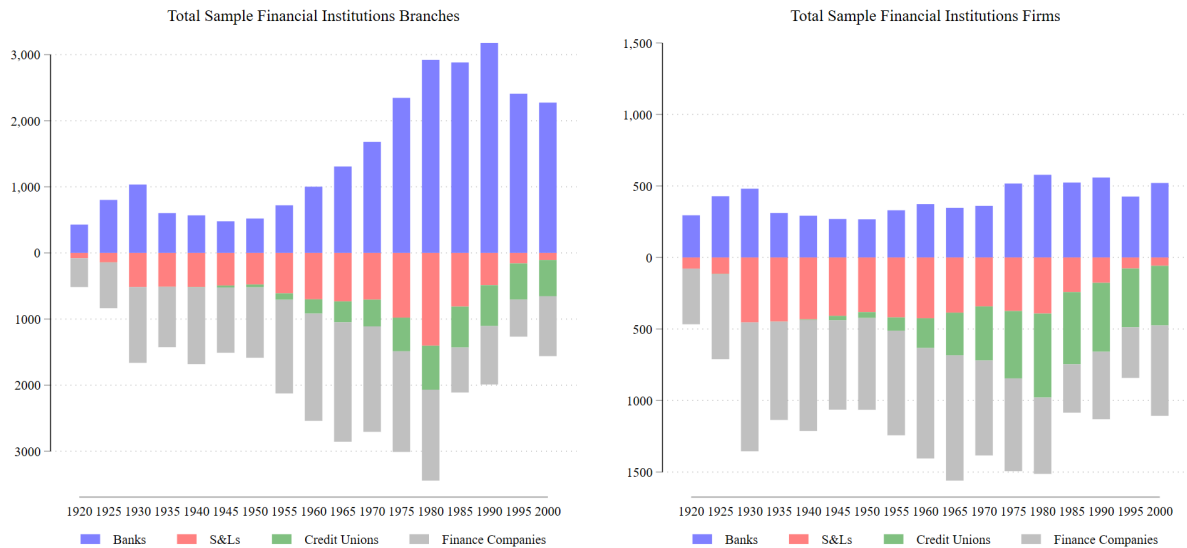
Figure 3 shows substantial changes in the composition of local financial providers over the

and they looked askance at long-term credits for the purchase of such luxuries. The installment finance company stepped into the breach between the consumer who wished to buy an automobile on partial payments and the dealer who wanted to make the sale but lacked the capital to finance it" (Nugent, 1939, p.80)

⁵The panel is unbalanced because directory availability varies across county-years. The same qualitative patterns are present in a more balanced subsample; Appendix Figure A.1 presents our sample coverage over time.

Figure 3: Financial Institutions Over Time

This figure presents total branch counts (left panel) and unique firm counts (right panel) by institution type. Nonbank categories are plotted below zero only to separate categories visually; magnitudes are counts. Section 2 details the sample collection process.



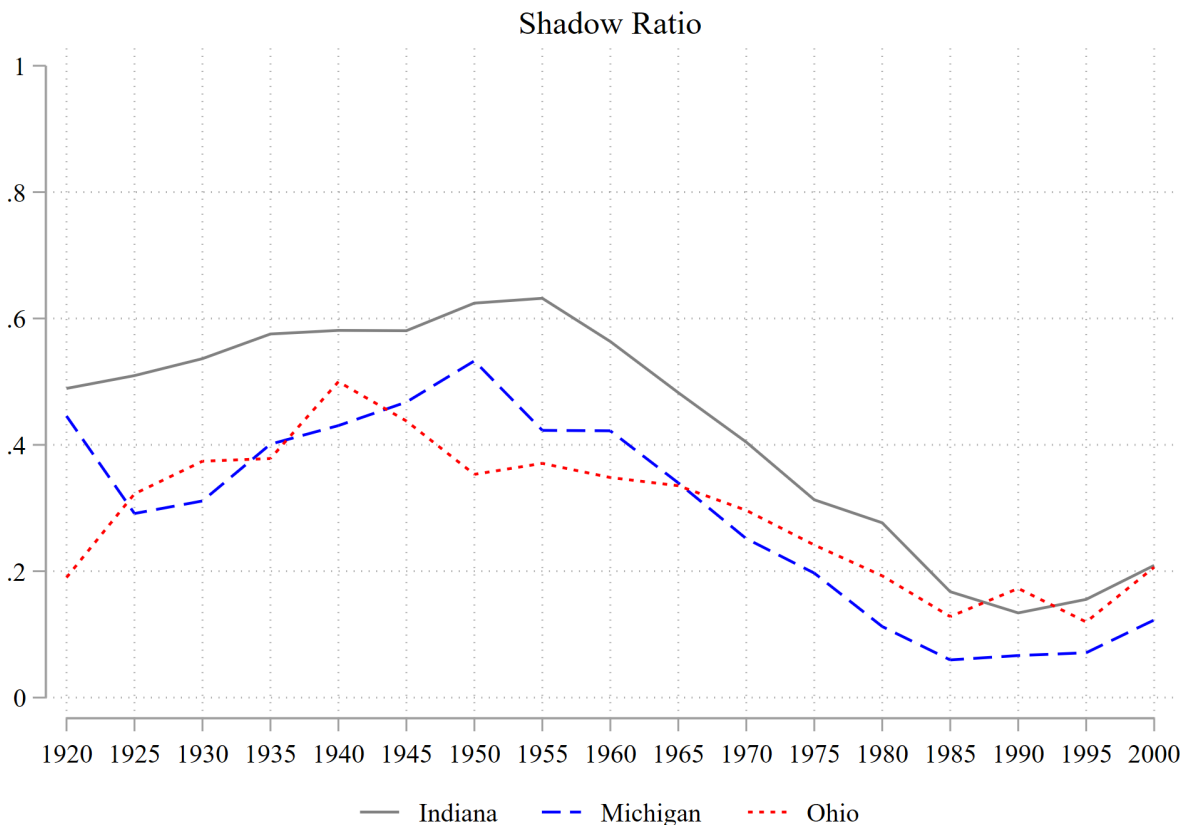
sample period. In this three-state panel, finance companies are present in every period and remain quantitatively important even as banks, S&Ls, and credit unions expand and contract. The figure also shows that branch counts rise much faster than firm counts after midcentury, especially for banks, indicating that growth in local banking partly reflects expansion of branch networks rather than proportional growth in independent firms. The increase in the number of branches between 1950–1980 came from the growth in regulated institutions, both banks and nonbanks.

Figure 4 plots the *Shadow Ratio (branch)* (i.e., the number of finance-company branches as a share of total financial-institution branches), over the sample period for each of the three states in our sample.⁶ Two striking patterns emerge. First, there is a strong time-specific element to financial architecture: all three states experience an increase in the share of finance companies in the initial three decades, followed by a decline and then flattening late in the sample. Second, there is substantial and persistent cross-sectional heterogeneity at

⁶The pattern for the ratio computed according to firm counts, *Shadow Ratio (firm)*, is similar but shifted slightly upward since banks typically have more branches than nonbanks.

Figure 4: Shadow Ratio Over Time

This figure presents the average county-level ratio of finance-company branches to total financial-institution branches (*Shadow Ratio*) for each state in our sample.



the state level. As we will show next, cross-sectional differences are most pronounced at the local county level. These patterns imply that bank-only or state-level measures collapse economically meaningful variation in local financial architecture. The relevant object for the rest of the paper is therefore the county-level mix of providers, not only the local number of banks.

3.3 Explaining Variation in Financial Architecture

What explains the variation in financial system architecture? While Figures 3 and 4 suggest strong time-series and state-specific components to the evolution of financial architecture, we now undertake a more systematic approach to examining the sources of this variation. We

estimate the following regression model for observations in county c , state s , at time t with different sets of fixed effects. The adjusted R-squared (R_a^2) is the statistic of primary interest.

$$y_{ct} = \alpha + \beta \log(\text{population}_{ct}) + \Gamma(\text{Fixed effects})_{sct} + \epsilon_{ct}. \quad (1)$$

The dependent variable y_{ct} represents either the logged number of financial-institution branches or firms in county c in year t , or the *Shadow Ratio*. We include $\log(\text{population}_{ct})$ in all specifications to account for baseline heterogeneity in local demand for financial services. We consider several combinations of fixed effects to shed light on which dimensions have the most explanatory power.

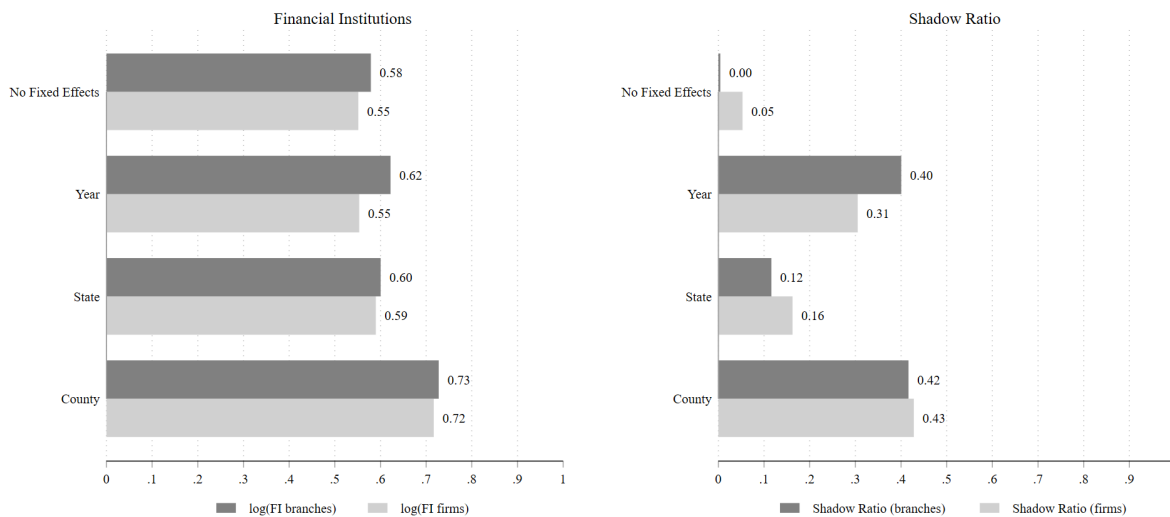
Figure 5 presents the results. Each bar in the figure presents the adjusted R^2 from the regression model (1), with the inclusion of the fixed effects noted on the y-axis. Without any fixed effects, we obtain an adjusted R^2 of 58% for the number of branches and almost 0% for the branch-based *Shadow Ratio*. Said differently, population is a key predictor of the depth of financial institutions, but not the mix of these institutions between finance companies and other providers.

The left panel shows that county-level heterogeneity plays an important role in explaining the number of local financial institutions. Including state or year fixed effects yields only a modest increase in R_a^2 from 0.58 to 0.60 and 0.62. However, including county fixed effects yields a sizable increase in the R_a^2 to 73%.

The right panel studies the *Shadow Ratio* and shows that including year fixed effects alone improves the model fit from 0% to 40%. Time-series variation, therefore, is an important component of the *Shadow Ratio*, as is shown visually in the earlier figures. County-specific variation is also a substantially stronger predictor of the *Shadow Ratio* than state-specific variation, increasing the R_a^2 from 12% for the model with only state fixed effects to 42% for the model with only county fixed effects. These results show substantial county-specific variation in financial architecture within the three-state sample. This matters for empirical

Figure 5: Explaining Financial Architecture

This figure presents adjusted R^2 values from regressions of local financial-architecture measures on log population and the fixed effects listed on the y-axis. The left panel reports adjusted R^2 values for log branch counts and log firm counts. The right panel reports adjusted R^2 values for branch-based and firm-based *Shadow Ratio*. Darker bars use branch-level measures, and lighter bars use firm-level measures. All regressions control for log population.



designs that rely on state-level banking variation as a proxy for local financial development: such designs can miss within-state differences in the composition of local finance.

We further show the importance of this county-specific effect by examining the persistence of financial architecture. Here we ask whether current financial depth (number of financial institutions, *FIs*) and the current *Shadow Ratio* are predicted by their lagged values at horizons up to 50 years. In these regressions, we control for year fixed effects, lagged population, and current population. Including current population helps account for interim changes in local economic conditions, allowing us to measure persistence in financial architecture after accounting for changes in baseline demand for financial services.

Table 2 presents results. Columns (1)-(4) use the log of the number of financial-institution branches as the dependent variable. At a lag of 20 years, we estimate an elasticity of 0.37 between current and past financial depth; the elasticity gradually declines but remains positive and significant at 0.14 even after 50 years. Columns (5)-(8) present the corresponding regression estimates for the branch-based *Shadow Ratio*. The branch-based ratio is also

Table 2: Persistence in Financial Architecture

This table presents regressions of $\log(\text{Financial Institution Branches})$ (columns 1-4) and Shadow Ratio (columns 5-8) on their lagged values, year fixed effects, contemporaneous population, and lagged population. In columns (1)-(4), the main independent variable is $\log(\text{Financial Institution Branches})$ lagged 10, 20, 35, and 50 years, respectively. Columns (5)-(8) are estimated similarly using lagged Shadow Ratio . Shadow Ratio (Br) is the number of finance-company branches as a share of total financial-institution branches. All regressions include year fixed effects. Parentheses report p -values computed using county-clustered standard errors.

	log(Financial Institution Branches)				Shadow Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
L10.log(FI Branches)	0.48*** (<0.01)							
L20.log(FI Branches)		0.37*** (<0.01)						
L35.log(FI Branches)			0.20*** (<0.01)					
L50.log(FI Branches)				0.14** (0.04)				
L10.Shadow Ratio (Br)					0.48*** (<0.01)			
L20.Shadow Ratio (Br)						0.35*** (<0.01)		
L35.Shadow Ratio (Br)							0.21*** (<0.01)	
L50.Shadow Ratio (Br)								0.15*** (<0.01)
log(population)	0.27** (0.05)	0.37** (0.03)	0.51*** (<0.01)	0.58*** (<0.01)	-0.01 (0.89)	-0.00 (0.98)	-0.03 (0.50)	-0.01 (0.73)
LX.log(population)	0.14 (0.33)	0.10 (0.57)	0.13 (0.45)	0.10 (0.58)	0.01 (0.75)	0.01 (0.84)	0.04 (0.42)	0.01 (0.78)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nobs	982	781	543	323	982	781	543	323
Adjusted R-squared	0.70	0.64	0.61	0.59	0.61	0.56	0.51	0.25

p -values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

persistent: the coefficient declines from 0.48 at a 10-year lag to 0.15 at a 50-year lag, and remains statistically significant throughout. In other words, cross-sectional differences in financial depth and in the finance-company branch share remain stable over long horizons within the sample. This persistence is useful for the empirical design below because lagged local financial architecture captures a durable component of the local financial environment rather than only short-run entry or exit.

3.4 The Interplay Between Banks and Nonbanks

How does the growth of the formal banking sector relate to the growth of nonbank institutions in the same area? It has been often argued that shadow banks fill in the gap left by regulated financial institutions as a result of relative differences in technology, regulation, or cost structure.⁷ In an early study, Nugent (1939) writes “The limited expansion of accommodation loans by banks was of the banks’ own choosing. Although many country banks increased their consumer loan balances as the demand for such loans increased, many urban banks were convinced by cost-accounting studies of the unprofitableness of small personal loans and restricted them so far as possible.” The willingness and ability of regulated banking institutions to expand in an area can directly influence the presence of shadow banks, but the direction of the relationship is unclear. Do they tend to be positively related to each other, negatively related to each other, or independent?

Theoretically, banks and nonbanks could be substitutes, complements, or completely independent of one another. For example, an oft-cited narrative against tighter bank regulation regarding certain activities is that it drives those activities “into the shadows.” On the other hand, increasing demand for financial services in an area can result in a simultaneous increase in both types of institutions, especially to the extent that they provide complementary services. Given the theoretical ambiguity, this is ultimately an empirical question. The answer has significant implications for potential biases in studies that use regulated banks alone as a proxy for financial development, as we discuss in detail in a later section. To examine this, we regress nonbank presence on contemporaneous bank presence using the following regression equation:

$$\log(\text{Nonbanks})_{cst} = \rho \log(\text{Banks}_{cst}) + \beta \log(\text{population}_{cst}) + \gamma_c + \tau_{st} + \epsilon_{cst}. \quad (2)$$

⁷For example, Buchak, Matvos, Piskorski, and Seru (2018) and Begley and Srinivasan (2022) show that such differences influence the industrial organization of the modern mortgage market.

The regressions control for population, county fixed effects γ_c , and state \times year fixed effects τ_{st} , each of which we have shown are important components of local financial architecture. The coefficient of interest is ρ , the elasticity of nonbanks to banks. We consider three measures of nonbanks in these regressions: all nonbank institutions, regulated nonbanks such as S&Ls and credit unions, and shadow banks (finance companies). For each measure, we present estimates using both branch counts and unique firm counts.

Table 3 presents the results. Column (1) shows a strong positive relationship between branches of all nonbank institutions and bank branches. The coefficient of 0.56 (p -value <0.01) indicates that a 10% increase in bank branches is associated with approximately 5.6% more all-nonbank branches. Column (2) produces similar results when considering the number of unique firms as the measure of interest: a 10% increase in bank firms is associated with approximately 5.9% more all-nonbank firms. In Columns (3)-(6), we use the same framework to separately examine regulated nonbanks including S&Ls and credit unions (columns 3 and 4) and shadow institutions (columns 5 and 6) as the dependent variable. These estimates show that the positive relationship appears for both types of nonbanks, including finance companies, the category typically missing from bank regulatory datasets. In Panel B, we perform the same analysis using Poisson regression. A benefit of Poisson regressions is they allow us to include observations with zero values, which have economic meaning in our setting. These are the county-year observations where a certain type of institution is absent. Overall, both estimation strategies in Panels A and B provide very similar results: nonbanks tend to evolve alongside banks.

The preceding analysis shows that shadow banks co-move positively with traditional banks, rather than simply appearing where banks are absent. This leads to the question of whether they play a distinct economic role. We propose that any distinct role is likely to stem from fundamental differences in their design. Unlike deposit-funded banks, finance companies were non-depository lenders outside the traditional bank prudential regime and may have had more flexibility in the contracts they offered. If this flexibility matters, counties with

Table 3: Interplay Between Banks and Nonbanks

Panel A of this table presents estimates from regressions of the log number of nonbanks in a county on the log number of banks, with each specification controlling for $\log(\text{Population})$, county fixed effects, and state \times year fixed effects. Columns (1)-(2) present estimates for all nonbanks; columns (3)-(4) for regulated nonbanks; and columns (5)-(6) for shadow banks. Odd columns use branch counts, and even columns use firm counts. Panel B performs analogous Poisson count regressions with counts of the respective financial institutions as the dependent variable. *Shadow Banks* are finance companies. *Reg'd Nonbanks* are regulated nonbank financial institutions, including S&Ls and credit unions. *Nonbanks* include regulated nonbanks and finance companies. Parentheses report p -values computed using county-clustered standard errors.

<i>Panel A: Linear Regression</i>						
	log(All Nonbanks)		log(Reg'd Nonbanks)		log(Shadow Banks)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Branches	Firms	Branches	Firms	Branches	Firms
log(Bank Branches)	0.56*** (<0.01)		0.46*** (<0.01)		0.54*** (<0.01)	
log(Bank Firms)		0.59*** (<0.01)		0.41*** (<0.01)		0.60*** (<0.01)
log(population)	-0.00 (0.99)	0.10 (0.57)	0.03 (0.87)	0.08 (0.70)	0.19 (0.25)	0.25 (0.14)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nobs	1,403	1,403	1,257	1,257	1,320	1,320
Adjusted R-squared	0.84	0.81	0.84	0.81	0.80	0.79
<i>Panel B: Poisson Regression</i>						
	All Nonbanks		Reg'd Nonbanks		Shadow Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
	Branches	Firms	Branches	Firms	Branches	Firms
log(Bank Branches)	0.56*** (<0.01)		0.61*** (<0.01)		0.54*** (<0.01)	
log(Bank Firms)		0.60*** (<0.01)		0.57*** (<0.01)		0.62*** (<0.01)
log(population)	0.10 (0.53)	0.28 (0.10)	0.08 (0.73)	0.31 (0.36)	0.23 (0.17)	0.40** (0.02)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nobs	1,446	1,446	1,429	1,429	1,437	1,437
Pseudo R-squared	0.84	0.79	0.80	0.75	0.76	0.72

p -values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

higher *Shadow Ratio* should respond differently when fundamentals change. The next section analyzes this question using changes in agricultural profitability.

4 Shadow Banks and the Real Economy

These facts lead directly to the paper’s main economic question: when fundamentals change, does the local mix of banks and finance companies affect how productive assets are reallocated? In standard intermediation models, finance can help assets move toward users who value them more by reducing information, matching, transaction-cost, and moral-hazard frictions (e.g., Leland and Pyle, 1977; Boot and Thakor, 1997; Ramakrishnan and Thakor, 1984; Subrahmanyam and Titman, 1999; Levine, 2002; Chen, Goldstein, and Jiang, 2007).

Shadow banks, which in our setting are finance companies, are hybrid institutions, sharing features of both traditional banks and financial markets. Like banks, they maintained local branch networks and supplied credit through local offices. Unlike banks, they were non-depository intermediaries funded outside insured retail deposits and operated outside the bank regulatory regime. These institutional differences gave finance companies scope to write, price, and adjust credit terms differently from banks, broadening the contracting space available to borrowers and lenders. This distinction should matter most when asset profitability changes: if finance companies expanded the contracts and credit availability locally, the same investment opportunity should produce different reallocation responses in counties where they are present in larger numbers.

4.1 Research Design

To empirically test how financial architecture affects asset reallocation, we require a setting with a locally financed asset that experiences plausibly exogenous shocks to its optimal ownership structure. We study farmland because its funding, use, and prices are inherently local. Specifically, we study corn-producing farmland and use fluctuations in the global price of corn as a shock that directly alters the profitability, economics, and thus the optimal allocation of farmland. As Dow and Gorton (1997) write “In a capitalist economy,

prices serve to equilibrate supply and demand for goods and services, continually changing to reallocate resources to their most efficient uses.”

Changes in corn prices naturally alter the value of reallocating land and capital across farms. When corn prices rise, farming becomes more profitable, and the return to scale, mechanization, and timely capital investment increases, consistent with real option investment models (Dixit and Pindyck, 1994). Farmers may respond by replacing older equipment with higher-capacity tractors, adding machinery, installing irrigation systems, or making other lumpy farm investments. These changes can make mismatches between land and operator more costly: land may become more valuable in the hands of operators who can finance the needed investment, exploit scale economies, and use the land more intensively. The first empirical prediction is therefore consolidation: fewer farms and larger average farm size in corn-producing counties when corn prices rise.

The use of corn price as a trigger for reallocation provides a distinct empirical advantage. Corn prices are determined on global markets and move with national and global supply and demand conditions, including wars, weather, and other forces far outside the control of any county in Indiana, Michigan, or Ohio (Rajan and Ramcharan, 2015; Carlin and Mann, 2024). These price movements therefore provide plausibly exogenous shocks to the profitability of corn production at the county level. The design uses this variation to test whether asset-reallocation responses to profitability shocks differ with lagged local financial architecture: do corn-producing counties with higher lagged *Shadow Ratio* consolidate differently when corn prices rise, relative to non-corn counties?

A key empirical challenge is to separate corn-price changes from other aggregate economic variables that could independently drive asset reallocation. To address this, our research design uses non-corn-producing counties as a benchmark for macroeconomic, regulatory, and agricultural-credit shocks that affect farming broadly rather than corn production specifically. This comparison is useful because those shocks should affect corn and non-corn counties in

the same direction, while corn-price shocks change the relative profitability of corn-producing land.⁸ We implement this strategy by classifying counties by corn exposure: corn-producing counties are those in the top quartile of average corn production over the relevant sample period.⁹

We combine the financial-architecture panel with detailed county-level U.S. Agricultural Census data and farmland-value data from Barnard and Jones (1987). The resulting sample is an unbalanced five-year county panel covering 143 counties from 1930 to 1985. This sample is smaller than the 171-county financial-architecture panel because the real-economy analysis also requires county-year agricultural outcomes, farmland values, and lagged financial-architecture measures in the relevant five-year windows. Using these data, we estimate the following triple-difference specification:

$$y_{ct} = \alpha_c + \alpha_t + \gamma(\text{CornCnty}_c \times \text{CornPriceReturn}_t \times \text{ShadowRatio}_{c,t-5}) + \Psi \mathbf{X}_{ct} + \epsilon_{ct} \quad (3)$$

y_{ct} represents an outcome in county c and year t . Outcomes include the number of farms, average farm size, tractors per farm, total tractors, land values per acre, and corn yields. County fixed effects α_c absorb time-invariant county characteristics, and year fixed effects α_t absorb shocks common to all counties in a year. CornCnty_c is an indicator for corn-producing counties; CornPriceReturn_t is the global corn-price return over the prior five years; and $\text{ShadowRatio}_{c,t-5}$ is the five-year-lagged finance-company branch share of total local financial-institution branches. The vector \mathbf{X}_{ct} includes the lower-order terms and

⁸One source of farm credit is the federal program called “The Farm Credit System (FCS)”. It is a congressionally chartered cooperative lending system that began with the 1916 Federal Farm Loan Act, which created Federal land banks and local associations to provide long-term farm mortgage credit. The system later expanded into short- and intermediate-term agricultural credit and remained national in scope. Because the FCS was national and not organized around local finance-company presence, it is unlikely to affect our regression estimates that compares outcomes across corn and non-corn producing counties. The design nevertheless relies on the standard assumption that broad program effects are not differentially correlated with corn exposure and lagged *Shadow Ratio* after the included controls. See <https://www.congress.gov/crs-product/RS21278>

⁹In robustness tests, we replace the binary corn-county indicator with the five-year-lagged share of county land in farms. This measure captures broader agricultural intensity rather than corn acreage, so we interpret it as a robustness check on agricultural exposure rather than as a continuous corn-share measure.

pairwise interactions not absorbed by the fixed effects. To separate corn-price variation from broad price-level movements, some specifications also control for the interaction of inflation with corn-price returns in corn-producing counties.

The coefficient γ measures whether the response of corn-producing counties to corn-price changes varies with lagged *Shadow Ratio*. Put differently, it asks whether local financial architecture changes the sensitivity of asset-reallocation outcomes to a profitability shock. The identifying assumption is that, absent the finance-company channel, corn counties with high and low *Shadow Ratio* would have had similar changes in reallocation outcomes relative to non-corn counties when corn prices move.¹⁰ County fixed effects, year fixed effects, and the lower-order interactions address time-invariant county differences, shocks common to all counties, and the component interactions. The coefficient of interest, γ , is the difference-in-differences-in-differences estimate that we are interested in.

4.2 Shadow Banks and Farm Consolidation

We begin with farm consolidation, the first real-economy margin in our analysis. Table 4 estimates equation (3) using log farm counts and log average farm size as outcomes. The table asks whether corn-producing counties with higher lagged *Shadow Ratio* consolidate more when corn prices rise.

Column (1) presents the baseline farm-count result: a higher five-year-lagged *Shadow Ratio* is associated with a larger decline in the number of farms when corn prices rise. The estimate remains similar in Column (2), which adds the inflation interaction. In Column (3), we re-estimate the model on a restricted sample in which finance companies are present but do not account for the entire local financial sector in both current and five-year-lagged

¹⁰In untabulated specifications, we also control for the corresponding triple interaction between corn-price returns, corn-county exposure, and lagged overall local financial depth, measured as log total financial-institution branches. The *Shadow Ratio* estimates are nearly unchanged across the main outcomes, indicating that the results are not driven simply by counties with deeper local financial systems responding differently to corn-price shocks.

Table 4: Farm Consolidation

This table presents estimates of regression (3), with $\log(\text{Farms})$ (columns 1-3) and $\log(\text{Average Farm Size})$ (columns 4-6) as the dependent variables. *Shadow Ratio* is finance-company branches as a share of total financial-institution branches, lagged five years. $(z)\text{CornPriceReturn}$ and $(z)\text{Inflation}$ are standardized five-year corn-price returns and inflation, $\log(\text{Farms})$ is the log of the number of farms in the county, and $\log(\text{Avg Farm Size})$ is the log of the average farm acreage in the county. All regressions include county and year fixed effects. Reported p-values, computed using county-clustered standard errors, are in parentheses.

	log(Farms)			log(Avg Farm Size)		
	(1)	(2)	(3)	(4)	(5)	(6)
L5.ShadowRatio \times (z)CornPriceReturn \times CornCnty	-0.14*** (0.01)	-0.15*** (0.01)	-0.25*** (<0.01)	0.06** (0.02)	0.08** (0.02)	0.10* (0.05)
L5.ShadowRatio \times (z)Inflation \times CornCnty		0.07 (0.28)	0.13* (0.08)		-0.09* (0.09)	-0.09 (0.17)
Sample	Full	Full	Restricted	Full	Full	Restricted
Nobs	988	988	698	976	976	694
Adjusted R-squared	0.94	0.94	0.93	0.95	0.95	0.94

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

observations. This test addresses whether the result is driven only by the difference between having and not having finance companies. The coefficient in Column (3) is -0.25 and is statistically significant. Economically, moving from the 25th to the 75th percentile of the *Shadow Ratio* is associated with about a 9 percent larger decline in farm counts after a one-standard-deviation increase in corn-price returns.

In Columns (4)-(6), we repeat the analysis using average farm size as the outcome. Across specifications, a higher lagged *Shadow Ratio* corresponds to a larger increase in average farm size, although the restricted-sample estimate is less precisely estimated than the farm-count result. Moving from the 25th to the 75th percentile of the *Shadow Ratio* is associated with roughly a 3.5 percent larger increase in average farm size after a one-standard-deviation corn-price return. Taken together, these results show that reallocation is occurring: positive corn-price shocks are followed by fewer and larger farms in counties with higher *Shadow Ratio*.

The next question is whether this reallocation is accompanied by evidence of more productive capital organization and higher land values. We therefore turn to two observable

implications: capital deepening and changes in land values.

4.3 Shadow Banks and Capital Deepening

A key motivation behind consolidation is to exploit economies of scale. Those scale economies become more important when rising corn prices increase the incentive for large, lumpy capital investment. This mechanism is particularly relevant in our sample period, which saw rapid advances in farm mechanization: available tractor horsepower quadrupled between 1947 and 1977 (Olmstead and Rhode, 2000). To study this margin, we estimate equation 3 using tractors per farm and log total tractors as outcomes. Tractors are only one form of farm capital, but they are a central input for mechanized production during this period.

Table 5 presents the results. Columns (1)-(3) show a positive relationship between the *Shadow Ratio* interaction term and tractors per farm. In Column (3), the estimate of 0.18 implies that moving from the 25th to the 75th percentile of the *Shadow Ratio* is associated with about 0.063 additional tractors per farm, roughly 4 percent of the sample mean, after a one-standard-deviation corn-price return. Columns (4)-(6) examine total tractors. The estimates are negative: the baseline and restricted-sample estimates are statistically significant at conventional levels, while the full-sample estimate with the inflation interaction is not statistically distinguishable from zero.

These results are consistent with capital becoming more concentrated on larger, surviving farms in areas with higher *Shadow Ratio*. Total tractor counts are flat to lower, while tractors per farm rise. This pattern fits the consolidation narrative: when corn prices rise, farm capital is organized over fewer and larger farms. In the next section, we examine whether this reorganization is reflected in asset prices.

Table 5: Capital Deepening

This table presents estimates of regression (3), with Tractors per Farm (columns 1-3) and $\log(\text{Tractors})$ (Columns 4-6) as the dependent variables. *Shadow Ratio* is finance-company branches as a share of total financial-institution branches, lagged five years. $(z)\text{CornPriceReturn}$ and $(z)\text{Inflation}$ are standardized five-year corn-price returns and inflation, *Tractors/Farm* is the number of tractors divided by the number of farms in the county, and $\log(\text{Tractors})$ is the log of the number of tractors in the county. All regressions include county and year fixed effects. Reported p-values, computed using county-clustered standard errors, are in parentheses.

	Tractors/Farm			$\log(\text{Tractors})$		
	(1)	(2)	(3)	(4)	(5)	(6)
L5.ShadowRatio \times $(z)\text{CornPriceReturn}$ \times CornCnty	0.08* (0.09)	0.12** (0.02)	0.18*** (0.01)	-0.10* (0.09)	-0.08 (0.21)	-0.13** (0.05)
L5.ShadowRatio \times $(z)\text{Inflation}$ \times CornCnty		-0.11 (0.36)	-0.18 (0.24)		-0.02 (0.84)	-0.01 (0.94)
Sample	Full	Full	Restricted	Full	Full	Restricted
Nobs	785	785	558	785	785	558
Adjusted R-squared	0.95	0.95	0.94	0.89	0.89	0.88

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.4 Shadow Banks and Land Values

We next examine farmland values. While we do not observe all inputs, outputs, costs, and revenues at the farm level, we do observe the market price of land. If the shift toward fewer, larger, and more capital-intensive farms raises the value of operating farmland, then corn-producing counties with higher lagged *Shadow Ratio* should experience larger increases in land values when corn prices rise.

Table 6 presents the results. Columns (1)-(3) show positive land-value estimates, with the strongest estimate in the restricted sample. Using the Column (3) estimate, moving from the 25th to the 75th percentile of the *Shadow Ratio* implies roughly a 6 percent larger land-value response to a one-standard-deviation increase in corn-price returns. The triple-difference design is important for interpreting this magnitude. It does not simply compare counties with high and low *Shadow Ratio*, nor does it rely on the unconditional effect of corn prices on corn-producing land. It compares the land-value response of corn-producing counties to contemporaneous changes in non-corn counties and asks whether that differential response is

Table 6: Land Values and Yield

This table presents estimates of regression (3), with $\log(\text{Land Value per Acre})$ (columns 1-3) and $\log(\text{Yield})$ (columns 4-6) as the dependent variables. *Shadow Ratio* is finance-company branches as a share of total financial-institution branches, lagged five years. $(z)\text{CornPriceReturn}$ and $(z)\text{Inflation}$ are standardized five-year corn-price returns and inflation, $\log(\text{Land Value per Acre})$ is the log of the average value of farmland in the county, and $\log(\text{Yield})$ is the log of the average corn yield (bushels per acre) in the county. All regressions include county and year fixed effects. Reported p-values, computed using county-clustered standard errors, are in parentheses.

	log(Land Value per Acre)			log(Yield)		
	(1)	(2)	(3)	(4)	(5)	(6)
L5.ShadowRatio \times (z)CornPriceReturn \times CornCnty	0.07** (0.05)	0.09** (0.01)	0.17*** (<0.01)	-0.04 (0.31)	-0.04 (0.41)	-0.04 (0.52)
L5.ShadowRatio \times (z)Inflation \times CornCnty		-0.09* (0.09)	-0.14** (0.05)		-0.03 (0.70)	-0.02 (0.78)
Sample	Full	Full	Restricted	Full	Full	Restricted
Nobs	988	988	698	973	973	688
Adjusted R-squared	0.99	0.99	0.99	0.91	0.91	0.91

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

stronger where lagged *Shadow Ratio* is higher.

Does the increase in land values reflect higher output per acre, or does it operate through other channels? To address this, Columns (4)-(6) estimate the same specification using corn yield as the outcome. The triple-interaction estimates are small, negative, and statistically indistinguishable from zero, indicating no detectable increase in corn yield per acre. This null result is not surprising if the relevant adjustment is organizational rather than agronomic: output per acre depends heavily on factors such as weather, soil, crop variety, planting decisions, tillage, and input regulation (Wonders, 2004). Land values, by contrast, capitalize expected net returns, which can rise through scale, cost, timing, or financing advantages even when bushels per acre do not. Taken together, the table shows that land prices respond while yields do not. This pattern points away from an output-per-acre channel and is consistent with capitalization of scale, cost, or financing advantages.

To assess sensitivity to the binary corn-county definition, we re-estimate the specifications using a continuous measure of agricultural exposure. We replace the corn-county indicator with the five-year-lagged percentage of county land in farms (*FarmingIntensity*), a measure

of agricultural exposure rather than corn-specific exposure, and re-estimate the main specifications. Appendix Table A.1 reinforces the main pattern: farm counts fall, while average farm size, tractors per farm, and land values increase. The total-tractor and yield estimates remain muted, which is consistent with the main tables.

4.5 Shadow Banks and Asset Volatility

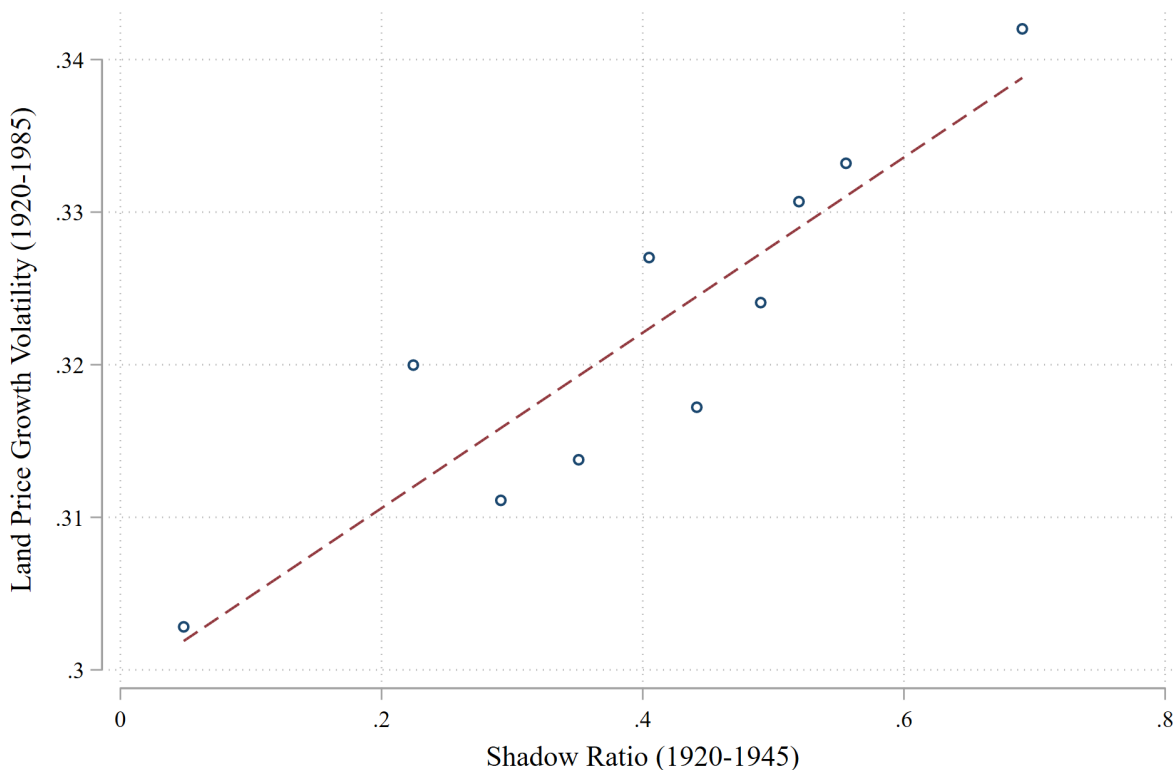
Finally, we examine whether the stronger consolidation and capitalization responses documented above have a risk-side counterpart. The preceding tables show that high-*Shadow Ratio* counties consolidate more, organize tractor capital over fewer farms, and experience larger land-value responses when corn prices rise. If these responses reflect greater financial flexibility and more intensive use of farm assets, the same architecture may also make land values more sensitive to shocks. Two channels are natural. First, more flexible local finance may raise financial leverage. Second, consolidation into larger, more capital-intensive farms can raise operating leverage. Both channels point to an efficiency-volatility trade-off: the local architecture associated with stronger reallocation responses may also leave asset values more exposed to changes in expected returns.

To measure volatility, we compute five-year land-value growth for each county and use the county-level standard deviation over 1920-1985 as the outcome. We then relate this long-run volatility measure to the average *Shadow Ratio* during 1920-1945, controlling for early-period county population. The persistence of the *Shadow Ratio* makes the early-sample measure informative about long-run financial architecture.

Figure 6 shows a positive relationship between early *Shadow Ratio* and land-value-growth volatility. The plotted regression coefficient is approximately 0.057, with a p-value of 0.014. Interpreted alongside the earlier consolidation, capital-concentration, and land-value results, this pattern is consistent with an efficiency-volatility trade-off: the counties where reallocation and valuation respond more strongly are also the counties with more volatile land-value

Figure 6: Shadow Ratio and Asset Volatility

This figure presents a 10-bin binscatter plot of county-level land-value-growth volatility over 1920-1985 as a function of average Shadow Ratio during 1920-1945, controlling for early-period county population. The regression coefficient is approximately 0.057 (p -value=0.014). *Shadow Ratio* is the finance-company branch share of total financial-institution branches, averaged over 1920-1945.



growth over the long run.

5 The Consequences of Bank-Only Measures of Financial Architecture

The reallocation evidence above uses the new panel to show one way local financial architecture matters for real outcomes. The same data also speak to a broader measurement problem in empirical literature in banking and finance. Many studies use banks as the observable proxy for local financial development because bank data are consistently available over long periods. But nonbank financial institutions are quantitatively important, vary

across counties and time, and co-move systematically with banks. We therefore use our data to ask what researchers miss when they measure local finance with banks alone. We focus on three consequences: (a) its effect on measures of local competition, (b) its effect on measurement error in studies involving the effect of financial development on real outcomes, and (c) its effect on omitted-variable bias in empirical specifications that require the inclusion of non-banks as a control variable. For brevity, we summarize our main findings on each of these three dimensions below, leaving the detailed derivations and estimates to Appendix A.

Bank-Only Measures Mismeasure Local Financial Competition

Competition in the supply of credit is of fundamental importance to policymakers and academics because it shapes access to credit, investment, bank-borrower relationships, consumer surplus, and the stability of financial markets (e.g., Petersen and Rajan, 1995). Measures of local financial competition computed only from banks miss a large set of credit suppliers. Using branch-counts in a county, the median banks-only HHI in our sample is 0.27, while the corresponding all-institution HHI is 0.09. The point is simply that once the broader set of financial intermediaries is observed, concentration of financial intermediaries is much lower than what the bank-only data imply.

The same issue appears in county rankings. For example, Clinton and Hamilton Counties, Indiana, had similar bank density in 1975, at about 1.6 bank branches per 10,000 residents. Once nonbanks are included, however, Clinton had about 4.1 total financial-institution branches per 10,000 residents, while Hamilton had about 2.5. A regulator or researcher using only banks would treat these counties as similar markets even though the broader financial-provider environment differs substantially. Appendix Section A.1. reports the full exercise.

Bank-Only Measures Create Non-Classical Measurement Error

Many empirical studies wish to estimate the effect of financial development (F_i) on real outcomes (Y_i) such as growth or capital formation using an empirical model of the following type:

$$Y_i = \alpha + \beta \times F_i + \epsilon_i \quad (4)$$

Since F_i is not available, the empirical researchers often estimate a model based on some parameterization of the number of banks B_i that measures F_i with some error:

$$Y_i = \gamma + \tilde{\beta} \times B_i + \nu_i \quad (5)$$

$$B_i = F_i + \eta_i \quad (6)$$

Given this measurement error, the relationship between the target coefficient β and the estimated coefficient $\tilde{\beta}$ is given by the following equations:

$$\tilde{\beta} = \beta \times (1 - \lambda) \quad (7)$$

$$\lambda = \frac{\sigma_\eta^2 + \sigma_{F,\eta}}{\sigma_\eta^2 + \sigma_F^2 + 2\sigma_{F,\eta}} \quad (8)$$

If the measurement error were classical ($\sigma_{F,\eta} = 0$), then $\lambda = \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_F^2}$, which constrains λ to be positive and bounded between zero and one. In that case, the consequence of mismeasurement is attenuation bias in the estimated coefficient. This is shown in Figure A.4 on the vertical line where $\sigma_{F,\eta} = 0$. Here, as the variance of the banks-only measurement error increases, so does λ , biasing the estimate from the mismeasured equation toward zero. Our estimates from Figure A.4 can be used by researchers to get a bound on attenuation bias if they suspect the measurement error to be classical.

What if $\sigma_{F,\eta} \neq 0$? This is exactly the possibility raised by our data, i.e., the measurement

error is likely to be non-classical in nature. We provide guidance to researchers by quantifying this bias based on our data.

We quantify the bias based on two parametrization of financial development that are often used in the literature: (a) the number of bank branches per 10,000 residents, also called the “bank density” (e.g., Rajan and Ramcharan, 2015), and (b) the log transform of the number of branches per 10,000 residents. Using bank density as the measure of choice, we show that the estimated coefficient $\tilde{\beta}$ from the mismeasured regression is 28% to 72% larger than the coefficient on total local financial development. If the researcher uses the log transform the direction of bias changes: $\tilde{\beta}$ from the mismeasured regression is now 13% to 35% smaller than the coefficient on total local financial development. Therefore, the nature and extent of bias depends critically on the empirical specification that the researchers employ. We provide the estimates of this bias as well as the variance-covariance estimates that drive the bias (e.g., $\sigma_{\eta}^2, \sigma_F^2, \sigma_{F,\eta}$) in the Appendix across several subsamples, as well as across rural and urban counties of the country to help future researchers with their own estimates.

Bank-Specific Regressions Face Omitted-Variable Bias

The prior subsection shows that excluding nonbanks leads to measurement-error bias when researchers use banks as a proxy for total local financial development. A separate problem arises when researchers want the bank-specific relationship rather than the relationship between total finance and outcomes. In this case, empirical researchers often estimate a form of the following regression:

$$y_i = \alpha + \psi_{short}(Banks_i) + \epsilon_i \tag{9}$$

However, such a regression excludes the presence of nonbanks. The omitted-variable issue

can be seen by comparing that short regression with the following longer specification:

$$y_i = \xi + \psi_{long}(Banks_i) + \gamma(Nonbanks_i) + \nu_i \quad (10)$$

The omitted-variable bias can be written as follows:

$$\psi_{short} = \psi_{long} + \gamma \times \delta_{Nb-B} \quad (11)$$

ψ_{short} is the estimate from the “short” regression that omits nonbanks; ψ_{long} represents the target bank-specific relationship; γ is the coefficient on nonbanks in the long regression; and δ_{Nb-B} is the coefficient from a regression of nonbanks on banks.

For outcomes in which nonbanks provide related credit products, it is natural to consider the case $\gamma > 0$. Further, as we have shown in Section 3.4, there is a strong positive relationship between bank and nonbank presence, which suggests that $\delta_{Nb-B} > 0$. In Panel A of Table A.5, we regress county-level $\log(\text{Nonbanks})$ on $\log(\text{Banks})$ for each major time period: PreWar, PostWar, and Modern. We find coefficient estimates in the range of [0.90, 1.02] if we only include state \times year fixed effects in the model (columns 1-3), and [0.47, 0.73] when we also control for local population (columns 4-6). These estimates imply that the omitted-variable term can be large when nonbanks affect the outcome in the same direction as banks.

5.0.1 Regression Calibration

We now provide a regression-calibration benchmark for settings in which researchers observe banks but not the broader local financial system. We do so by estimating $E[F|B]$ based on our data. For branch counts over the full sample, the fitted relationship is:

$$E[\text{Total FI branches}/10k \mid \text{Bank branches}/10k] \approx 1.44 + 1.17 \times \text{Bank branches}/10k.$$

We also provide period-specific estimates as well as estimates for rural and urban counties separately in Appendix A.3.1. These estimates give researchers a concrete way to translate bank counts into expected total financial-institution density in settings where broader data are not available.

6 Conclusion

Finance shapes who can acquire and use productive assets, but empirical work usually observes local finance through banks alone. This paper addresses that measurement problem by constructing a county-level panel from Yellow Pages in Indiana, Michigan, and Ohio from 1920 to 2000. The data measure local branch networks of banks, regulated nonbanks, and finance companies, and they show that finance companies, i.e., shadow banks, were not peripheral. They accounted for roughly one-third of local financial-institution branches and varied substantially across counties and over time. Strikingly, local financial architecture was persistent even at 50-year horizons, despite dramatic changes in regulation, technology, and the organization of credit markets over the twentieth century. Evaluating their relationship with banks reveals another important fact: the two types of intermediaries co-moved positively across local markets rather than simply substituting for one another.

We use this measurement to study reallocation when agricultural fundamentals change. We find that corn-producing counties with higher lagged *Shadow Ratio* respond more strongly when corn prices rise: farms consolidate, average farm size increases, and tractor capital becomes more concentrated on surviving farms. Consistent with the market capitalizing the value of this reallocation, land values rise more in these counties. Taken together, the results point to a reallocation channel in which local shadow-bank presence is associated with the movement of land and capital toward operators who can use them at larger scale.

The broader implication of our work is that banks are not sufficient statistics for local

finance. The same data that make the reallocation analysis possible show that banks-only measures can distort measures of financial development, local financial competition, and regression estimates that attempt to tease out the effect of finance on real outcomes. Bank-only data miss not only a large class of intermediaries, but a class that varies across counties, persists over time, and co-moves with banks.

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Appendix Figures and Tables

Figure A.1: Sample Coverage over Time

This figure reports the number of sample counties observed in each five-year period, stacked by state. Section 2 describes the sample construction process.

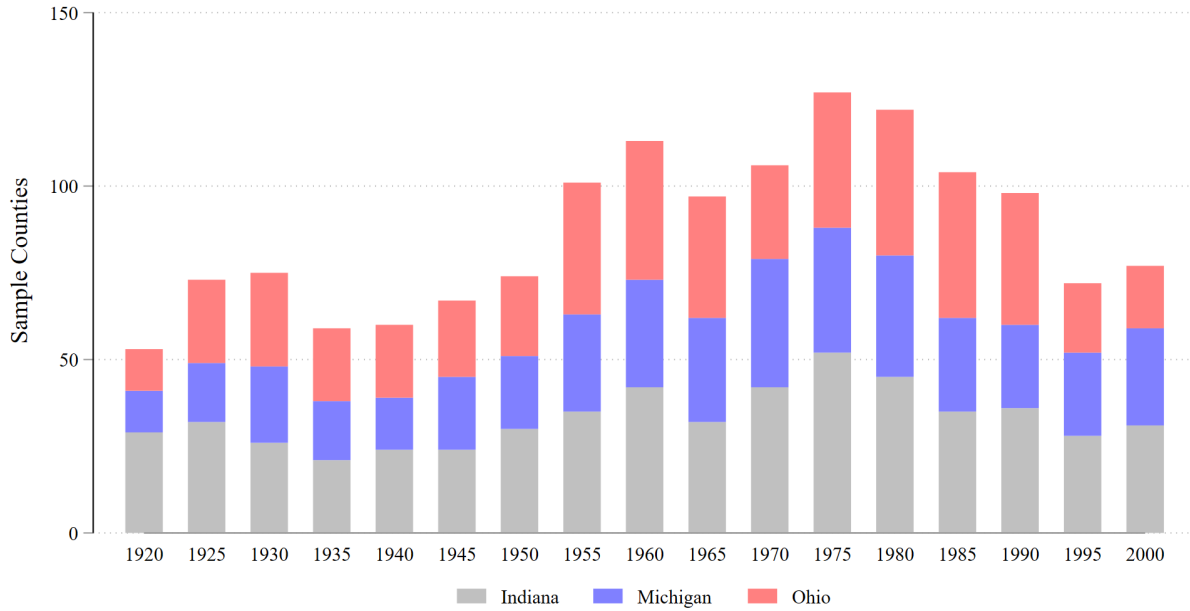


Table A.1: Robustness: Farm Intensity

This table presents estimates of regression (3) using five-year-lagged *FarmingIntensity* as the measure of local agricultural exposure. *Shadow Ratio* is finance-company branches as a share of total financial-institution branches, lagged five years. $\log(\text{Farms})$ is the log of the number of farms in the county, and $\log(\text{Avg Farm Size})$ is the log of the average farm acreage in the county. *Tractors/Farm* is the number of tractors divided by the number of farms in the county, $\log(\text{Tractors})$ is the log of the number of tractors in the county, $(z)\text{CornPriceReturn}$ and $(z)\text{Inflation}$ are standardized five-year corn-price returns and inflation, $\log(\text{Land Value per Acre})$ is the log of the average value of farmland in the county, and $\log(\text{Yield})$ is the log of the average corn yield (bushels per acre) in the county. *FarmingIntensity* is the percent of county land in farms, lagged five years. All regressions include county and year fixed effects. Reported p-values, computed using county-clustered standard errors, are in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	log(Farms)	log(AvgFarmSize)	Trac/Farm	log(Tractors)	log(LandValue)	log(Yield)
L5.ShadowRatio \times (z)CornPriceReturn \times L5.FarmingIntensity	-0.63*** (<0.01)	0.37*** (<0.01)	0.35*** (<0.01)	-0.33 (0.11)	0.40*** (<0.01)	0.06 (0.59)
L5.ShadowRatio \times (z)Inflation \times L5.FarmingIntensity	0.35* (0.08)	-0.27 (0.13)	-0.64* (0.08)	-0.79* (0.08)	-0.30 (0.20)	0.22 (0.23)
Sample	Restricted	Restricted	Restricted	Restricted	Restricted	Restricted
Nobs	698	694	558	558	698	688
Adjusted R-squared	0.95	0.95	0.95	0.90	0.99	0.91

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Regression Calibration for Financial Institution Firms

This table presents OLS estimates of the number of financial-institution firms per 10,000 residents on the number of banking firms per 10,000 residents. Panels A, B, and C estimate the regressions for all counties, urban counties, and rural counties, respectively. Column (1) of each panel includes observations from the entire sample period, while columns (2), (3), and (4) use data from the PreWar, PostWar, and Modern periods, respectively. We define counties as urban or rural based on whether their median population is above or below the sample median. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000. Reported p-values, computed using county-clustered standard errors, are in parentheses.

<i>Panel A: All Counties</i>				
	(1)	(2)	(3)	(4)
Banks (Firms) / 10k	1.24*** (<0.01)	1.48*** (<0.01)	1.44*** (<0.01)	1.16*** (<0.01)
Constant	1.35*** (<0.01)	1.16*** (<0.01)	1.41*** (<0.01)	1.16*** (<0.01)
Sample Period	All	PreWar	PostWar	Modern
Nobs	1,478	387	618	473
Adjusted R-squared	0.44	0.51	0.38	0.51
<i>Panel B: Urban Counties</i>				
	(1)	(2)	(3)	(4)
Banks (Firms) / 10k	1.56*** (<0.01)	1.49*** (<0.01)	2.42*** (<0.01)	1.99*** (<0.01)
Constant	1.16*** (<0.01)	1.11*** (<0.01)	1.11*** (<0.01)	0.72*** (<0.01)
Sample Period	All	PreWar	PostWar	Modern
Nobs	732	236	288	208
Adjusted R-squared	0.36	0.45	0.33	0.42
<i>Panel C: Rural Counties</i>				
	(1)	(2)	(3)	(4)
Banks (Firms) / 10k	1.10*** (<0.01)	1.40*** (<0.01)	1.31*** (<0.01)	1.08*** (<0.01)
Constant	1.54*** (<0.01)	1.31*** (<0.01)	1.50*** (<0.01)	1.27*** (<0.01)
Sample Period	All	PreWar	PostWar	Modern
Nobs	746	151	330	265
Adjusted R-squared	0.35	0.43	0.30	0.43

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Analysis Appendix

A. The Consequences of Mismeasurement

This appendix supports Section 5. We begin by showing the basic inaccuracy that arises when local competition in financial services is measured with banks alone. We then characterize the measurement-error bias that comes from using banks as the proxy for total financial development. Finally, we show how excluding nonbanks introduces omitted-variable bias in estimates that seek to isolate the bank-specific relationship with real outcomes. The exercises use the same county-level financial-architecture panel described in Section 2.

A.1. Bank-Only Measures and Local Financial Competition

Competition in the supply of credit is central to both policy and academic research. The issue here is that bank-only measures miss nonbank financial institutions that supply many related credit products. We first compare bank and nonbank branch density in each county-period. Figure A.2 plots nonbank financial-institution branches per 10,000 residents against bank branches per 10,000 residents. The solid lines trace combinations with the same total number of bank and nonbank branches per 10,000 residents. Moving northeast therefore corresponds to a larger total local financial presence.

The same issue appears in county comparisons. Clinton and Hamilton Counties, Indiana, each had about 1.6 bank branches per 10,000 residents in 1975. Including nonbanks changes their relative positions: Clinton had about 4.1 total financial-institution branches per 10,000 residents, while Hamilton had about 2.5. A researcher using only banks would treat these counties as similarly banked local markets even though the broader listed-provider environment differs substantially.

A second common way of examining local competition is the Herfindahl-Hirschman Index (HHI). Because nonbanks provide several of the same broad categories of credit products as banks, a banks-only HHI can overstate local concentration relative to a measure that includes all listed financial institutions. We compute a local HHI measure based on the number of branches each financial firm has in a county. For example, if a bank has three branches and a finance company has one branch, the bank receives three times the local branch weight of the finance company.

Figure A.3 plots each county's HHI measured with banks only on the x-axis against the HHI measured using all local financial institutions on the y-axis. Counties on the diagonal have no listed nonbank financial institutions, so the two measures coincide. Counties below the diagonal have lower concentration once nonbanks are included. In our sample, the median banks-only HHI is 0.27, while the median all-institution HHI is 0.09. The difference between the two measures ranges from 0.05 at the 10th percentile to 0.40 at the 90th percentile. The conclusion is that ignoring nonbank suppliers of credit can substantially mismeasure local financial competition.

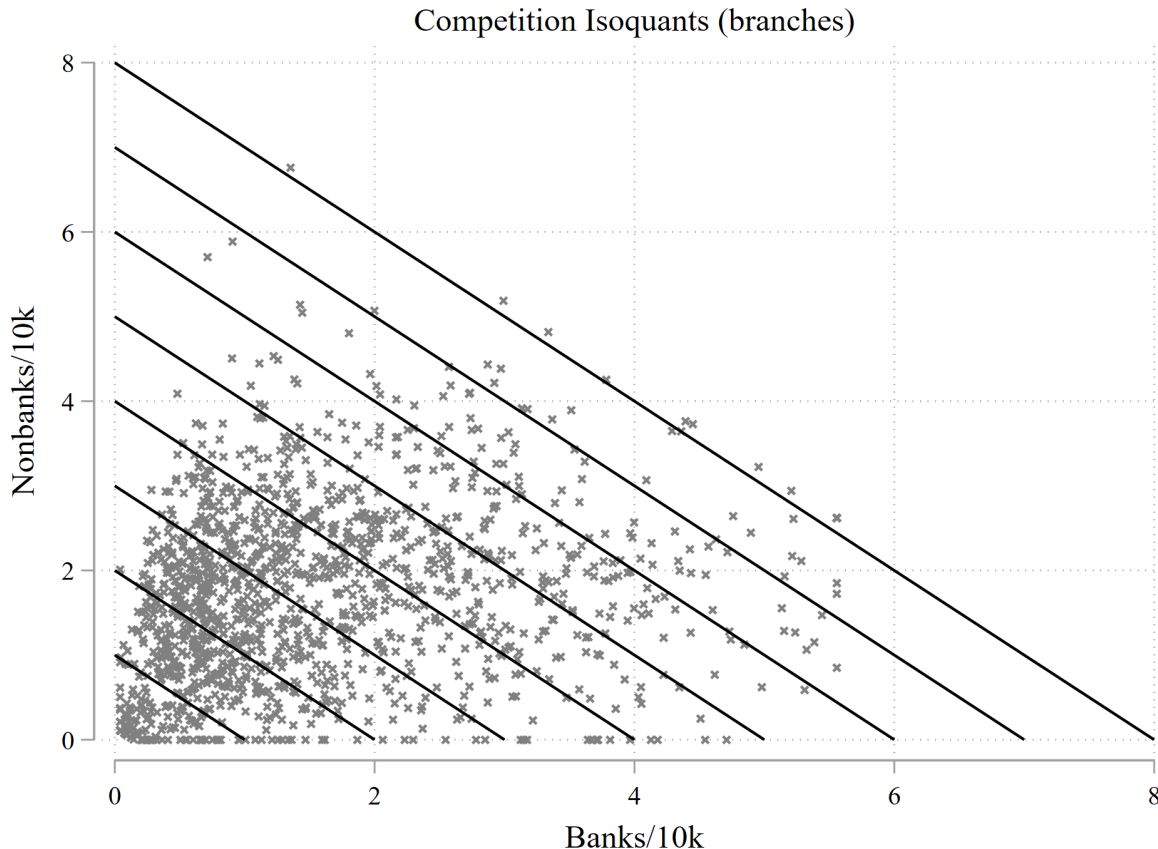
A.2. Measurement Error Bias

A.2.1. Characterizing the Bias from Mismeasuring Financial Architecture

In light of the results presented so far, omitting nonbank financial institutions from a study that links financial development to economic outcomes can be problematic for at least three reasons: nonbanks represent a large fraction of local financial institutions, their presence varies meaningfully

Figure A.2: Bank and Nonbank Branch Density

This figure plots the number of nonbank financial-institution branches per 10,000 residents against bank branches per 10,000 residents for each county-period observation in the sample. The solid lines trace combinations with the same total number of bank and nonbank branches per 10,000 residents.



over time and across counties, and banks and nonbanks evolve together. The last point is critical. It means omitted nonbanks are not just a noise term in analyses that measure financial development with banks alone. We now quantify the extent of this bias using our data.

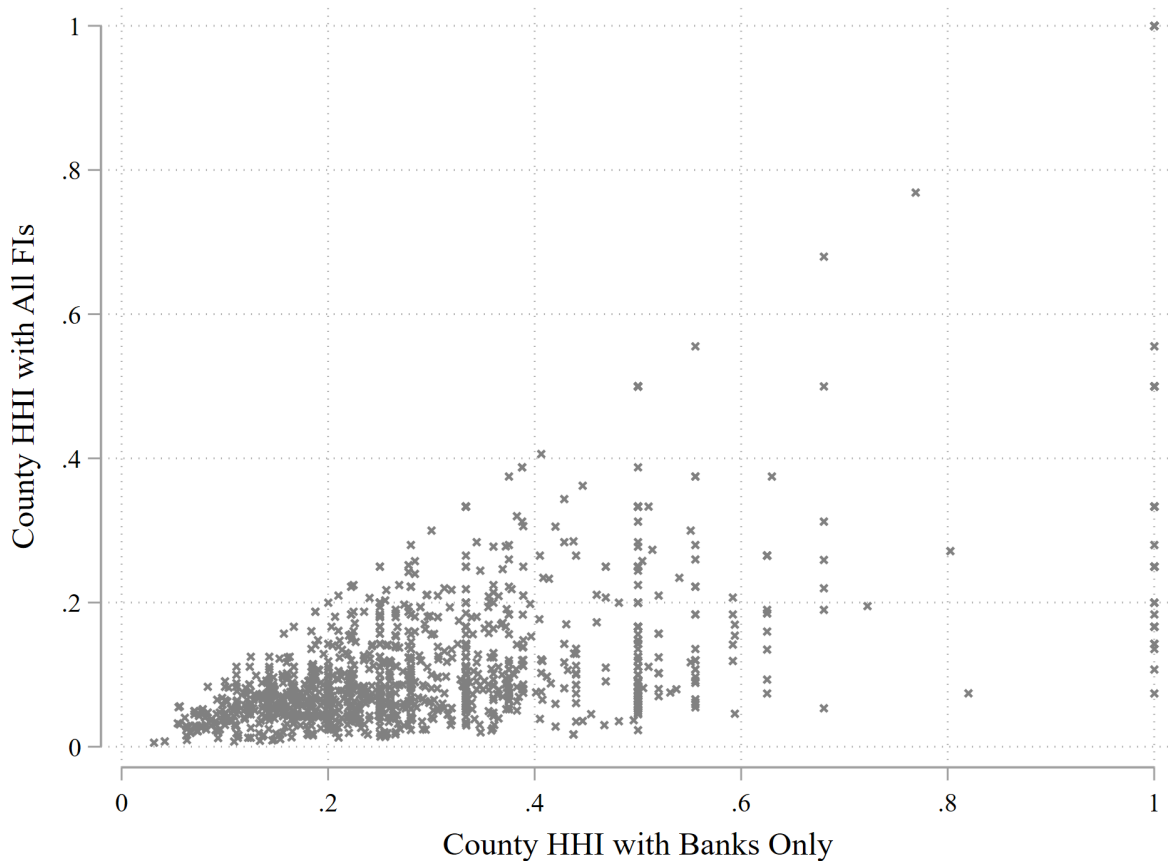
Many empirical studies estimate how financial development relates to real outcomes such as growth or capital formation. Let F_i denote total local financial development and B_i the bank-only proxy observed by the researcher. The target regression is:

$$Y_i = \alpha + \beta \times F_i + \epsilon_i \tag{12}$$

Since the total measure of financial development, which can include banks, credit unions, savings and loans, finance companies, and other nonbank lenders, is not usually available, empirical researchers often estimate a model based on some parameterization of the number of banks B_i , for

Figure A.3: Local Financial Competition: Branch-Count HHI

This figure plots each county's branch-count HHI using banks only on the x-axis and all financial institutions on the y-axis. To compute the HHI, we weight each financial-institution branch equally and aggregate branches to the firm level for each county. For example, a bank with three local branches receives three times the branch weight of a finance company with one local branch.



which data are more readily available:

$$Y_i = \gamma + \tilde{\beta} \times B_i + \nu_i \quad (13)$$

$$B_i = F_i + \eta_i \quad (14)$$

Here $\eta_i = B_i - F_i$ is the banks-only measurement error. When F_i includes nonbank financial institutions, omitted nonbanks enter as $-\eta_i$.

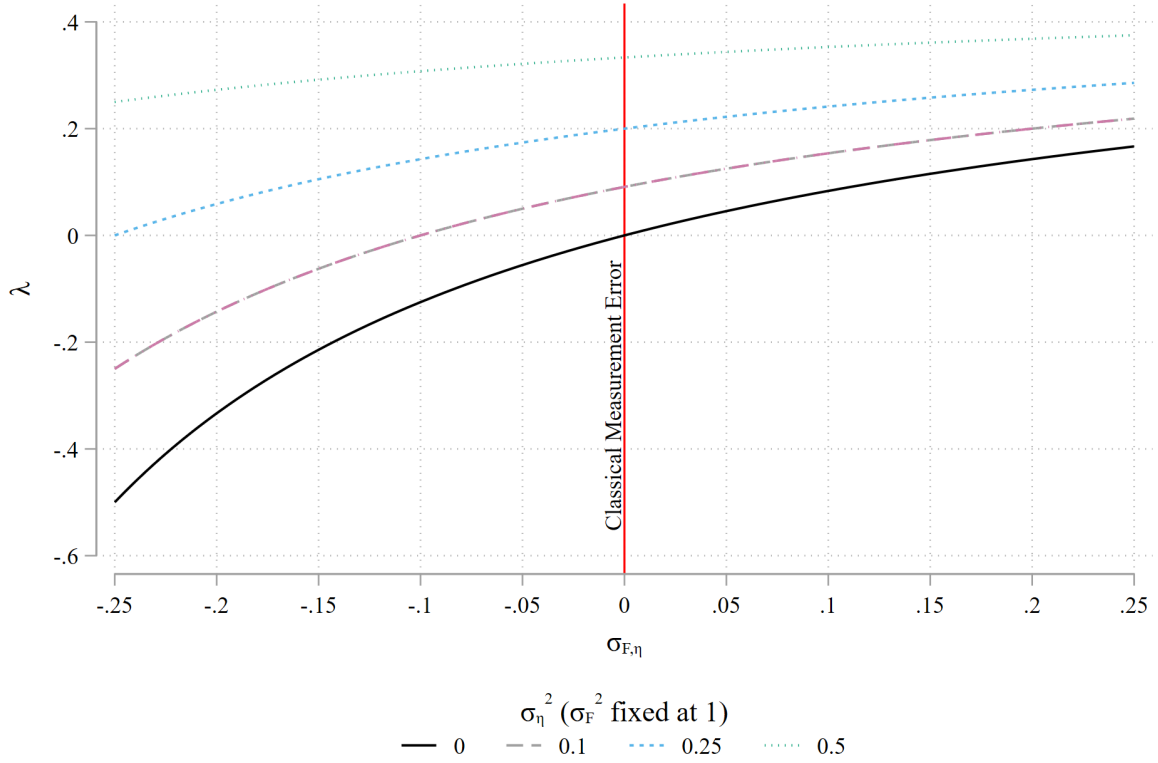
Given this measurement error, the relationship between the target coefficient β in (12) and the estimated coefficient $\tilde{\beta}$ from the bank-only regression (13) is given by the following equations:

$$\tilde{\beta} = \beta \times (1 - \lambda) \quad (15)$$

$$\lambda = \frac{\sigma_\eta^2 + \sigma_{F,\eta}}{\sigma_\eta^2 + \sigma_F^2 + 2\sigma_{F,\eta}} \quad (16)$$

Figure A.4: Mismeasurement and the Extent of the Bias

This figure presents the value of the measurement-error factor λ as a function of $\sigma_{F,\eta}$, the covariance between total local financial development and the banks-only measurement error $\eta_i = B_i - F_i$. Each line plots this relationship for a given σ_η^2 . The plot is normalized such that the variance of total local financial development σ_F^2 is one.



The bias factor λ depends on the relative importance of the variance in the measurement error (σ_η^2), the variance of total local financial development (σ_F^2), and the covariance between the total measure and the banks-only measurement error ($\sigma_{F,\eta}$).

Figure A.4 illustrates how the interaction of these components translates to bias in the empirical estimates in the mismeasured equation by plotting equation (16) for λ with σ_F^2 normalized to one for various degrees of the variance of the error (σ_η^2) and the covariance between total local financial development and the measurement error ($\sigma_{F,\eta}$).

If the measurement error were classical ($\sigma_{F,\eta} = 0$), then $\lambda = \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_F^2}$, which constrains λ to be positive and bounded between zero and one. In that case, the consequence of mismeasurement is attenuation bias in the estimated coefficient. This is shown in Figure A.4 on the vertical line where $\sigma_{F,\eta} = 0$. Here, as the variance of the banks-only measurement error increases, so does λ , biasing the estimate from the mismeasured equation toward zero.

What if $\sigma_{F,\eta} \neq 0$? This is exactly the possibility raised by our data. If total local financial development F is correlated with the banks-only measurement error η , then λ is no longer bounded between zero and one. In this case, using banks rather than total local financial development can attenuate the coefficient toward zero ($0 < \lambda < 1$), move it away from zero ($\lambda < 0$), or even reverse

its sign ($\lambda > 1$) under the stated model. In the next subsection, we use our data to estimate these bias components during our sample period.

A.2.2. Quantifying the Measurement-Error Bias

Section 3.4 shows a strong positive correlation between banks and nonbank institutions. Since $F_i = B_i + N_i$ and $\eta_i = B_i - F_i = -N_i$, this comovement makes the banks-only measurement error correlated with true financial development rather than classical. Because systematic local nonbank data have generally been unavailable, prior work has had limited ability to quantify this source of bias. Our data allow us to observe F more directly, estimate the key variances and covariance in equation (16), and quantify the likely bias in studies that estimate the relationship between economic outcomes and banks (equation 13) rather than total local financial development (equation 12).

As shown earlier in the paper, there is substantial time-series variation in the measures of F and B in our sample. We therefore quantify the bias decade by decade, so the variance and covariance components are estimated within the period in which the researcher would be estimating the outcome relationship. The first measure of financial development we consider is branch density: the number of total financial-institution branches or bank branches per 10,000 residents. This is often referred to as “bank density” (e.g., Rajan and Ramcharan, 2015).

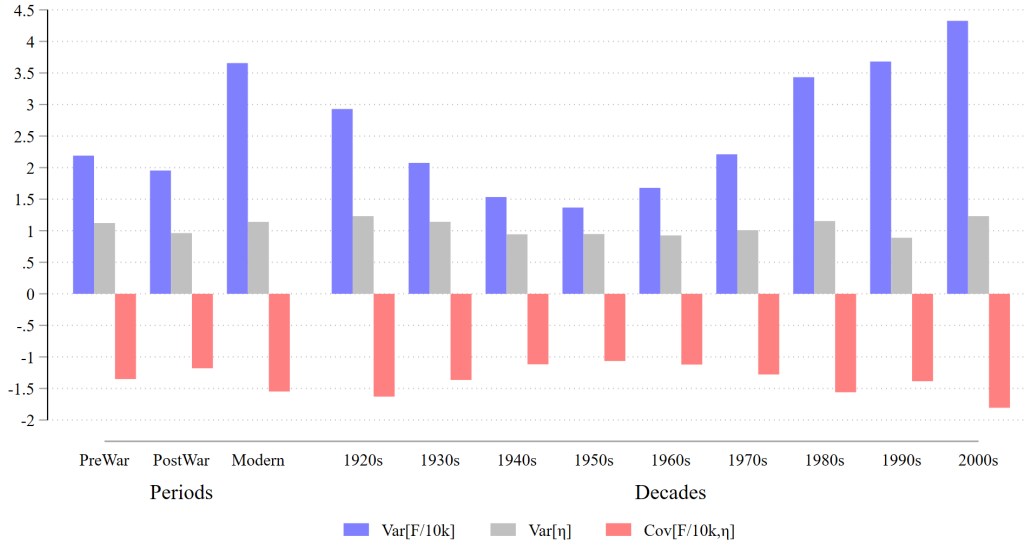
The estimates of relevant variances and covariance are shown in Figure A.5a on a decade-by-decade basis as well as for three distinct periods: (a) PreWar, 1920-1945, (b) PostWar, 1950-1980, and (c) Modern, 1985-2000. We aggregate the data across these three periods based on the aggregate trend in the branch-based *Shadow Ratio* documented in Figure 4. Two observations emerge. First, the variance of the measurement-error term (σ_η^2) is broadly of the same order of magnitude as the variance of total financial development (σ_F^2). Second, the covariance term ($\sigma_{F,\eta}$) is negative and sizable. The first observation implies that the extent of bias can be large even if measurement error were classical. The second shows that the measurement error is non-classical, making the sign as well as the magnitude of the bias an empirical question.

Using these estimates, Figure A.5b presents the decade-by-decade estimate of λ when finance is measured by the number of financial-institution branches per 10,000 residents, which we denote as $\lambda_{F/10k}$ (also shown in column (1) of Table A.3). This figure shows that $\lambda_{F/10k}$ is negative for each decade in the sample, ranging from -0.72 in the 1940s to roughly -0.28 in the late-sample decades; the pooled Modern estimate is -0.24. Plugging the decade-by-decade values into equation (15) indicates that the estimated coefficient $\tilde{\beta}$ from the mismeasured regression (13) is 28% to 72% larger than the coefficient on total local financial development.

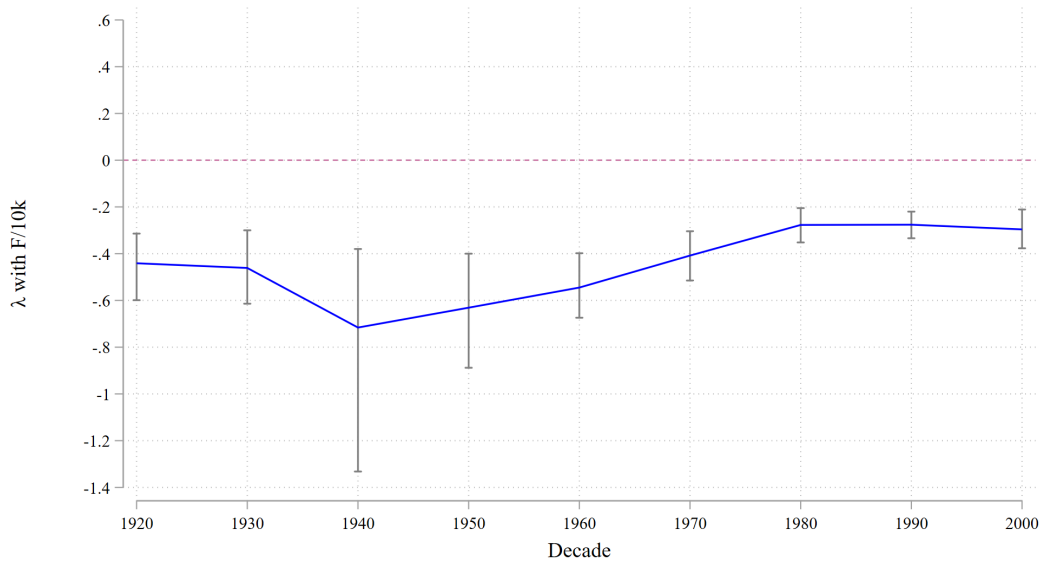
We next consider an alternative parameterization of banking depth by taking the log transform of the number of branches per 10,000 residents. Figure A.6a shows that, relative to the variance in total financial development ($\log(F/10k)$), the variance of the banks-only measurement error (σ_η^2) is smaller and the covariance term ($\sigma_{F,\eta}$) remains negative but smaller in magnitude. Figure A.6b presents the decade-by-decade estimate of $\lambda_{\log(F/10k)}$, showing a positive value that declines over time. This example shows that a common change in parameterization, from $F/10k$ to $\log(F/10k)$, flips the sign of the bias: plugging the decade-by-decade values into equation (15), $\tilde{\beta}$ from the mismeasured regression (13) is 13% to 35% smaller than the coefficient on total local financial development. The figure also shows that during the Modern period, the covariance term becomes relatively small, making the measurement problem closer to classical attenuation. Quantifying the

Figure A.5: Measurement Error Components: Finance Branch Density

This figure presents estimates of the measurement-error components from equation (16) in Panel (a) and the bias factor λ in Panel (b) when the measure of financial development is the number of financial-institution branches per 10,000 residents ($F/10k$). The components are estimated within the specific time periods in the figure and include the variance of total local financial development $\sigma_{F/10k}^2$, the variance of the banks-only measurement error σ_{η}^2 , and their covariance $\sigma_{F/10k,\eta}$. The measurement-error bias factor is computed as $\lambda_{F/10k} = \frac{\sigma_{\eta}^2 + \sigma_{F/10k,\eta}}{\sigma_{\eta}^2 + \sigma_{F/10k}^2 + 2\sigma_{F/10k,\eta}}$, with 90 percent bootstrapped confidence intervals. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000.



(a) Measurement Error Components ($F/10k$)



(b) Lambda ($F/10k$)

Table A.3: Lambdas for Different Measures of Financial Development

This table presents estimates of the measurement-error bias factor λ for bank-based proxies of total local financial development. This factor is computed using equation (16): $\lambda_{F/10k} = \frac{\sigma_\eta^2 + \sigma_{F/10k,\eta}}{\sigma_\eta^2 + \sigma_{F/10k}^2 + 2\sigma_{F/10k,\eta}}$. Columns (1) and (2) use bank branches per 10,000 residents and the log of bank branches per 10,000 residents as proxies for total financial development. Columns (3) and (4) use all regulated financial institutions, including banks, S&Ls, and credit unions. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000.

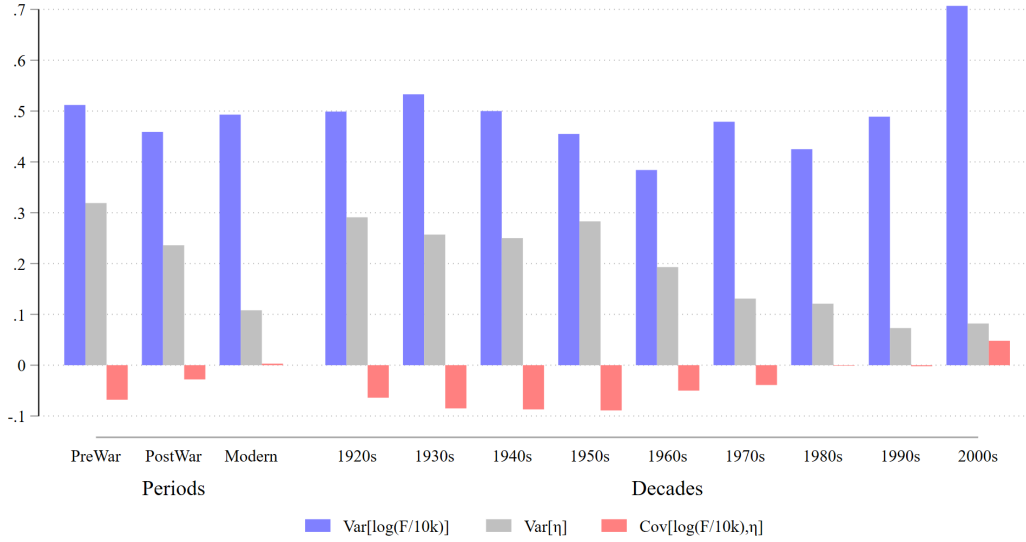
Measure:	Banks		All Regulated Financial Institutions	
	Banks/10k (1)	log(Banks/10k) (2)	RegdFIs/10k (3)	log(RegdFIs/10k) (4)
PreWar	-0.377	0.361	-0.219	0.174
PostWar	-0.390	0.326	-0.149	0.185
Modern	-0.240	0.183	-0.114	0.088
1920s	-0.441	0.343	-0.228	0.186
1930s	-0.461	0.277	-0.206	0.140
1940s	-0.716	0.283	-0.266	0.156
1950s	-0.631	0.347	-0.195	0.223
1960s	-0.545	0.300	-0.325	0.091
1970s	-0.408	0.172	-0.250	0.038
1980s	-0.277	0.220	-0.107	0.105
1990s	-0.276	0.128	-0.114	0.079
2000s	-0.296	0.147	-0.143	0.069

size of that attenuation bias, however, still requires measuring σ_η^2 .

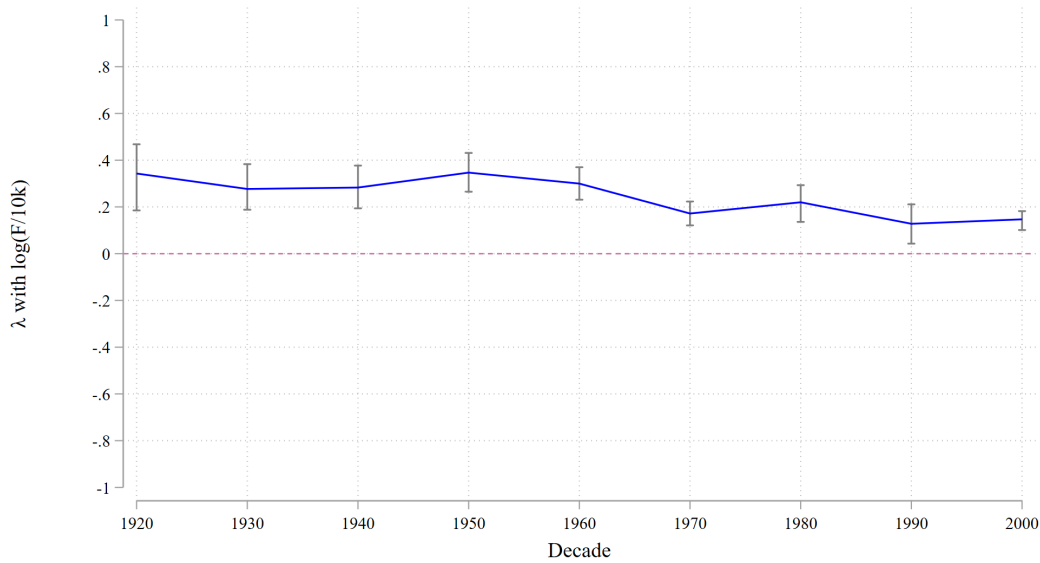
These exercises have focused on branches as the measure of financial development. Alternatively, we compute the degree of bias using the number of unique financial-institution firms as our measure. Appendix Figure A.7 plots the results. We find qualitatively similar patterns, but the size of the bias is amplified for both parameterizations, $F/10k$ and $\log(F/10k)$. This is driven by aggregation to the firm level within county, which disproportionately affects banks and lowers σ_F^2 , shrinking the denominator of λ in equation (16).

Figure A.6: Measurement Error Components: Log Finance Branch Density

This figure presents estimates of the measurement-error components from equation (16) in Panel (a) and the bias factor λ in Panel (b) when the measure of financial development is the log of financial-institution branches per 10,000 residents ($\log(F/10k)$). The components are estimated within the specific time periods in the figure and include the variance of total local financial development $\sigma_{\log(F/10k)}^2$, the variance of the banks-only measurement error σ_{η}^2 , and their covariance $\sigma_{\log(F/10k),\eta}$. The measurement-error bias factor is computed as $\lambda_{\log(F/10k)} = \frac{\sigma_{\eta}^2 + \sigma_{\log(F/10k),\eta}}{\sigma_{\eta}^2 + \sigma_{\log(F/10k)}^2 + 2\sigma_{\log(F/10k),\eta}}$, with 90 percent bootstrapped confidence intervals. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000.



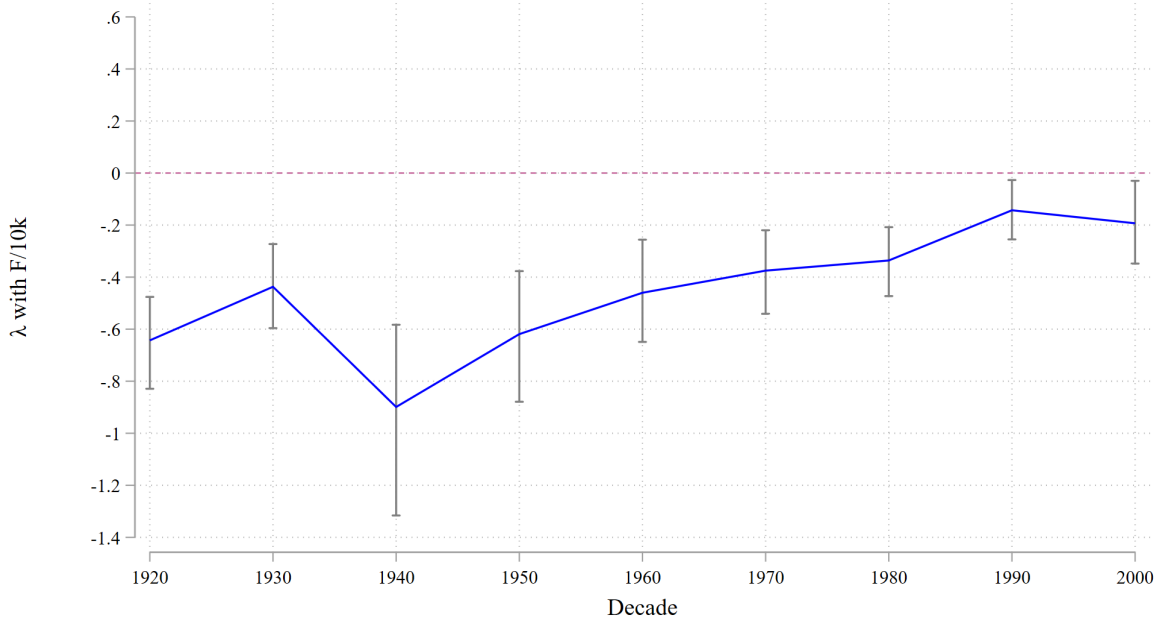
(a) Measurement Error Components ($\log(F/10k)$)



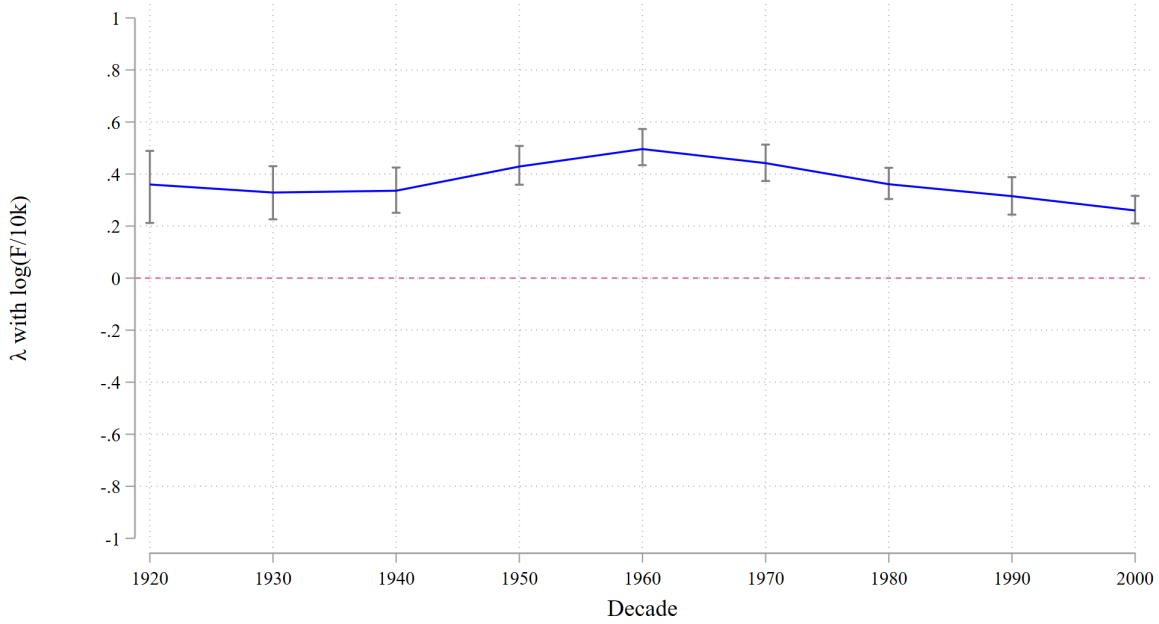
(b) Lambda ($\log(F/10k)$)

Figure A.7: Measurement Error When Using Unique Financial Firms

This figure presents estimates of the bias factor λ from equation (16) over the sample period. Panels (a) and (b) present the results when the measure of financial development is the number of distinct financial-institution firms per 10,000 residents and its log, respectively. The components of λ are estimated within each decade, with 90 percent bootstrapped confidence intervals.



(a) Lambda: $F/10k$



(b) Lambda: $\log(F/10k)$

A.2.3. Cross-Sectional Heterogeneity in Measurement Error Bias

The results above show substantial time-series variation over the 1920-2000 sample and corresponding changes in the bias from using incomplete measures of financial development. What about the cross-section? Given the interest of policymakers and academics in how finance relates to different areas and populations, it is important to understand whether the severity of the measurement-error problem differs across groups.

We begin by splitting counties into urban and rural groups based on whether the county’s median sample population is above or below the sample median. In Panel A of Table A.4, we present estimates of λ across urban and rural counties for each of the main time periods: PreWar, PostWar, and Modern. Across all periods, $\lambda_{F/10k}$ is substantially more negative for urban than rural counties. The negative $\lambda_{F/10k}$ indicates that studies using *Banks/10k* as the measure of financial development will overstate the coefficient on total local financial development, and this overstatement is substantially larger for urban areas. For example, the overstatement in the PostWar period is 18% for rural areas and 80% for urban areas. Panel A again highlights the role of parameterization. When using $\log(\text{Banks}/10k)$, $\lambda_{\log(F/10k)} > 0$ indicates attenuation relative to the coefficient on total local financial development, and the attenuation is larger for rural areas for most of the sample. For example, using $\log(\text{Banks}/10k)$ leads to an understatement in the PostWar period of 37% for rural areas compared to 26% for urban areas.

In Panel B of Table A.4, we perform a similar exercise by splitting counties into above- and below-median per capita income (“Rich” versus “Poor”). Because income data begin in 1960, the income split excludes the PreWar period. The overstatement when using *Banks/10k* is larger for high-income counties than low-income counties ($\lambda_{F/10k}^{Rich} < \lambda_{F/10k}^{Poor} < 0$), while the understatement when using $\log(\text{Banks}/10k)$ is greater for lower-income counties ($\lambda_{\log(F/10k)}^{Poor} > \lambda_{\log(F/10k)}^{Rich} > 0$). This heterogeneity implies that bank-only measures can distort comparisons across places, not just average effects.

A.3. Omitted Variable Bias

The prior subsection shows that excluding nonbanks leads to measurement-error bias when researchers use banks as a proxy for total local financial development. A separate problem arises when researchers want the bank-specific relationship rather than the relationship between total finance and outcomes. In this case, empirical researchers often estimate a form of the following regression:

$$y_i = \alpha + \psi_{short}(\text{Banks}_i) + \epsilon_i \tag{17}$$

However, such a regression excludes the presence of nonbanks. The omitted-variable issue can be seen by comparing that short regression with the following longer specification:

$$y_i = \xi + \psi_{long}(\text{Banks}_i) + \gamma(\text{Nonbanks}_i) + \nu_i \tag{18}$$

The omitted-variable bias can be written as follows:

$$\psi_{short} = \psi_{long} + \gamma \times \delta_{Nb-B} \tag{19}$$

Table A.4: Lambdas Across Urban/Rural and High/Low Income

This table presents estimates of the measurement-error bias factor λ for urban versus rural areas over the periods in our sample for our two main measures of financial development in Panel A, and similarly for high-income (Rich) versus low-income (Poor) counties in Panel B. We define counties as urban or rural based on whether their median population is above or below the sample median. Rich and Poor counties are defined analogously using county income; because income data begin in 1960, the income split excludes PreWar observations and uses only 1960 onward within the PostWar period. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000.

<i>Panel A: Urban versus Rural</i>			
	PreWar (1)	PostWar (2)	Modern (3)
Banks/10k – Urban	-0.510	-0.804	-0.496
Banks/10k – Rural	-0.233	-0.182	-0.163
log(Banks/10k) – Urban	0.301	0.264	0.174
log(Banks/10k) – Rural	0.463	0.373	0.154
<i>Panel B: High versus Low Income</i>			
	PreWar (1)	PostWar (2)	Modern (3)
Banks/10k – Rich	.	-0.700	-0.395
Banks/10k – Poor	.	-0.198	-0.141
log(Banks/10k) – Rich	.	0.196	0.113
log(Banks/10k) – Poor	.	0.374	0.282

ψ_{short} is the estimate from the “short” regression (17) that omits nonbanks; ψ_{long} represents the target bank-specific relationship in (18); γ is the coefficient on nonbanks in the long regression; and δ_{Nb-B} is the coefficient from a regression of nonbanks on banks.

For outcomes in which nonbanks provide related credit products, it is natural to consider the case $\gamma > 0$. Further, as we have shown in Section 3.4, there is a strong positive relationship between bank and nonbank presence, which suggests that $\delta_{Nb-B} > 0$. In Panel A of Table A.5, we regress county-level $\log(\text{Nonbanks})$ on $\log(\text{Banks})$ for each major time period: PreWar, PostWar, and Modern. We find coefficient estimates in the range of [0.90, 1.02] when only including state \times year fixed effects (columns 1-3), and [0.47, 0.73] when controlling for local population (columns 4-6). These estimates imply that the omitted-variable term can be large when nonbanks affect the outcome in the same direction as banks.

For example, consider the auto loan market. If more banks are associated with lower auto loan rates or greater access, nonbanks that provide related credit products could plausibly move the same outcome in the same direction. Under the illustrative assumption that nonbanks and banks have similar outcome effects ($\gamma \approx \psi_{long}$), the omitted-variable formula implies an overstatement of 47%–73% in this calibration.

Panel B of Table A.5 separately estimates the relationship between nonbanks and banks for urban counties (columns 1-3) and rural counties (columns 4-6). The estimates show that the increasingly larger relationship between nonbanks and banks over time holds for both urban and rural areas. However, the relationship is 2–4 times larger for urban areas than rural areas. Under the same illustrative assumption that nonbanks and banks have similar outcome effects, the short bank-only coefficient would be overstated by 45% in rural areas and 93% in urban areas in the Modern period.

Table A.5: Quantifying Omitted Variable Bias–Nonbanks on Banks

This table presents the estimates of the regression of the log of nonbanks (finance companies, savings and loans, credit unions) on the log of banks for various time periods. These estimates provide a necessary input to quantify the degree of omitted variable bias (equation 19). Specifically, it estimates the parameter δ_{Nb-B} for the omitted variable bias formula $\psi_{short} = \psi_{long} + \gamma \times \delta_{Nb-B}$. Panel A shows the estimates using all counties and Panel B divides the counties into Urban and Rural. We define counties as urban or rural based on whether their median population is above or below the sample median. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000. Reported p-values, computed using county-clustered standard errors, are in parentheses.

<i>Panel A: All Counties</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
	PreWar	PostWar	Modern	PreWar	PostWar	Modern
log(Bank branches)	0.90*** (<0.01)	0.96*** (<0.01)	1.02*** (<0.01)	0.47*** (<0.01)	0.55*** (<0.01)	0.73*** (<0.01)
log(population)				0.58*** (<0.01)	0.50*** (<0.01)	0.37*** (<0.01)
State*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nobs	364	598	441	364	598	441
Adjusted R-squared	0.66	0.64	0.73	0.76	0.72	0.76

<i>Panel B: Urban versus Rural</i>						
	Urban			Rural		
	(1)	(2)	(3)	(4)	(5)	(6)
	PreWar	PostWar	Modern	PreWar	PostWar	Modern
log(Bank branches)	0.53*** (<0.01)	0.79*** (<0.01)	0.93*** (<0.01)	0.28** (0.02)	0.20* (0.05)	0.45*** (<0.01)
log(population)	0.51*** (<0.01)	0.21* (0.09)	0.12 (0.11)	0.33 (0.15)	0.62*** (<0.01)	0.53*** (<0.01)
State*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nobs	227	283	203	137	315	238
Adjusted R-squared	0.74	0.70	0.78	0.54	0.29	0.40

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In sum, omitting nonbanks from empirical research relating financial architecture to real outcomes can create large biases. When researchers use banks to measure local financial architecture, the sign of measurement-error bias depends on basic empirical choices such as whether to log transform the measure of local bank presence. When researchers instead interpret a banks-only coefficient as the independent bank effect, that interpretation requires assumptions about omitted nonbank activity.

A.3.1. Regression Calibration

Overall, a clear message emerges from this analysis. Estimates of the relationship between finance and outcomes that use only banks as the measure can be substantially biased, and the direction of the bias depends on the parameterization. The bias also varies across population density and income. Our analytical framework and empirical estimates provide guidance on the sign and magnitude of bias as well as settings where the bias is likely to be higher.

We now provide a regression-calibration benchmark for settings in which researchers observe banks but not the broader local financial system. We do so by estimating $E[F|B]$ based on our data. Under standard regression-calibration assumptions, replacing B with $E[F|B]$ can reduce measurement-error bias; the estimates below provide a benchmark for settings similar to ours.

Table A.6 provides regression estimates with the number of financial-institution branches per 10,000 residents ($F/10k$) as the dependent variable and bank branches per 10,000 residents ($B/10k$) as the explanatory variable. Panel A provides full-sample estimates in column (1) and PreWar, PostWar, and Modern subsamples in columns (2), (3), and (4). We also present estimates separately for urban counties (Panel B) and rural counties (Panel C). The relationship between banks and total institutions is larger in magnitude for urban areas than rural areas, so the calibration is not one-size-fits-all. One interpretation, consistent with Nugent (1939), is that urban banks relied more on hard information and collateral, leaving more room for finance companies, whereas rural banks had greater scope to lend on soft information.

Appendix Table A.2 presents analogous firm-count estimates. Depending on the specific application, these estimates can be used as benchmarks for approximating broader financial-institution presence in comparable settings.

Table A.6: Regression Calibration for Financial Institution Branches

This table presents OLS estimates of the number of financial-institution branches per 10,000 residents on the number of bank branches per 10,000 residents. Panels A, B, and C estimate the regressions for all counties, urban counties, and rural counties, respectively. Column (1) of each panel includes observations from the entire sample period, while columns (2), (3), and (4) use data from the PreWar, PostWar, and Modern periods, respectively. We define counties as urban or rural based on whether their median population is above or below the sample median. *PreWar* is 1920-1945, *PostWar* is 1950-1980, and *Modern* is 1985-2000. Reported p-values, computed using county-clustered standard errors, are in parentheses.

<i>Panel A: All Counties</i>				
	(1)	(2)	(3)	(4)
Banks (Br) / 10k	1.17*** (<0.01)	1.38*** (<0.01)	1.39*** (<0.01)	1.24*** (<0.01)
Constant	1.44*** (<0.01)	1.24*** (<0.01)	1.39*** (<0.01)	1.00*** (<0.01)
Sample Period	All	PreWar	Post War	Modern
Nobs	1,478	387	618	473
Adjusted R-squared	0.65	0.53	0.55	0.71
<i>Panel B: Urban Counties</i>				
	(1)	(2)	(3)	(4)
Banks (Br) / 10k	1.39*** (<0.01)	1.51*** (<0.01)	1.80*** (<0.01)	1.50*** (<0.01)
Constant	1.19*** (<0.01)	1.08*** (<0.01)	1.07*** (<0.01)	0.63*** (<0.01)
Sample Period	All	PreWar	Post War	Modern
Nobs	732	236	288	208
Adjusted R-squared	0.69	0.48	0.68	0.77
<i>Panel C: Rural Counties</i>				
	(1)	(2)	(3)	(4)
Banks (Br) / 10k	1.06*** (<0.01)	1.23*** (<0.01)	1.18*** (<0.01)	1.16*** (<0.01)
Constant	1.64*** (<0.01)	1.51*** (<0.01)	1.61*** (<0.01)	1.09*** (<0.01)
Sample Period	All	PreWar	Post War	Modern
Nobs	746	151	330	265
Adjusted R-squared	0.61	0.48	0.45	0.69

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$