# Trust in Banks and Borrower Behavior: Evidence from Supervisory Actions and Local Information[⋆]

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**Abstract**

We study how consumers' trust in banks influences their borrowing decisions. We use bank enforcement actions as a shock to bank reputation, undermining consumers' trust in banks. Utilizing granular loan data from a credit reporting agency that links borrowers to banks, we find a decline in borrower and loan quality for loans issued by banks under enforcement. Notably, this decline is absent in news deserts, counties experiencing newspaper closures, and those with fewer newspaper establishments, reinforcing a trust mechanism as poor local information environments mitigate the reputational damage from enforcement. Our findings are not attributable to supply-side factors such as increased lending volume or loosened credit terms. Additionally, survey data indicate that enforcement actions are associated with declining local trust in banks and bankers. Overall, our results suggest that trust in banks influences borrower behavior.

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## 1. Introduction

In the aftermath of the global financial crisis, trust in banks has been an increasing topic of concern among policymakers and academics (Guiso, 2012; FDIC, 2021; Sapienza & Zingales, 2012; Stevenson & Wolfers, 2011). The percentage of adults who state they have high confidence in US banks fell from 53% in 2004 to just 22% in 2009 as a result of bank failures and the 2008 economic recession (McCarthy, 2016). The recent high-profile collapse of Silicon Valley Bank, First Republic Bank, and Signature Bank has revived such discussion, with an AP-NORC poll showing only 10% of US adults having high confidence in the nation's banks in 2023 (Wiseman & Fingerhut, 2023). These economy-wide trends in public trust in banks have raised the stakes for individual banks to maintain positive reputations, as it influences the degree of confidence that consumers place in banks and consequently their decisions to use banks' services (Osili & Paulson, 2014; Van Der Cruijsen et al., 2012).[1]

Based on Gambetta et al. (2000) and Sapienza & Zingales (2012), we define trust as "the expectation that another person (or institution) will perform actions that are beneficial, or at least not detrimental, to us regardless of our capacity to monitor those actions." Reputation is the "long-lived information about an agent's type" (Diamond, 1991). Reputation can influence trust because significant and highly publicized events can alter consumers' expectations about an agent's type, thereby changing their belief in the agent's propensity to act in their best interest. While extant literature has explored changes in consumers' trust in banks and its determinants, much less work has studied how trust influences consumers' financial decisions.[2] The limited research primarily concentrates on depositors' behavior while neglecting borrowers, understandably due to depositors' direct concern with entrusting their funds to banks (Osili & Paulson, 2014; Van Der Cruijsen et al., 2012). However, studying borrowers' responses is crucial to understanding the full implications of trust in banks, as borrowers are a vital part of a bank's business and their actions directly impact banks' financial health. We address this gap in the literature by examining how borrowers respond

[1]Following the literature, we use the terms "trust" and "confidence" interchangeably (Giannetti & Wang, 2016; Sapienza & Zingales, 2012).

[2]See, for example, Fungáčová et al. (2019); Jansen et al. (2015); Van Der Cruijsen et al. (2023) who explore factors that determine consumers' trust in banks and public institutions, and Knell & Stix (2015); Sapienza & Zingales (2012); Stevenson & Wolfers (2011) who study how trust is affected by the global financial crisis.

to an adverse event that negatively affects bank reputation and lowers confidence in banks.

An empirical challenge in identifying the relationship between trust in banks and borrower behavior is that measures of trust, which are generally survey-based, tend to be regional or economy-wide rather than bank-specific. Furthermore, survey-based measures of trust may not always be reliable, as respondents might choose to give answers that are socially acceptable rather than truthful. We take a novel approach to address these challenges by examining enforcement actions (also referred to as enforcement decisions and orders, or EDOs) issued by US bank supervisors against financial institutions. Bank regulators use enforcement actions as a measure of last resort to address unsafe and unsound banking practices by requiring banks to take corrective actions (Curry et al., 1999; Eisenbach et al., 2017; Hirtle et al., 2020).[3] Importantly, EDOs are publicly disclosed and often covered in the local news media, leading to significant reputational damage for banks (Delis et al., 2020; Dyck et al., 2008; Kleymenova & Tomy, 2022). Consequently, informed individuals' confidence in banks that receive enforcement actions should decline. This is consistent with prior research that, apart from severe systemic events like the global financial crisis, a range of adverse developments such as negative media coverage, declining stock prices, lack of transparency in product information, and excessive executive compensation are linked to diminished confidence in banks (Fungáčová et al., 2019; Jansen et al., 2015; Van Der Cruijsen et al., 2023).

Our approach of using bank enforcement actions to measure confidence in banks follows prior research that utilizes adverse events to measure declining confidence in firms and other institutions. For example, Giannetti & Wang (2016) use the incidence of corporate fraud as a measure of loss of trust in stock markets, Gurun et al. (2018) use exposure to the Madoff ponzi scheme as a measure of trust in investment advisors, and Yang (2023) uses the county-level share of Wells Fargo's deposits following revelation of their fraudulent sales practices as a measure of trust in banks versus Fintech lenders. Relative to these studies, we use bank enforcement actions to mea-

[3] Issuing an enforcement action starts when bank examiners assign low CAMELS ratings of 4 or 5 and recommend regulatory action. CAMELS stands for composite and component ratings assigned by bank examiners, evaluating six key areas: capital adequacy, asset quality, management, earnings, liquidity, and sensitivity to risk. Regulators typically begin with informal measures, such as bank board resolutions or memoranda of understanding, before issuing formal enforcement actions. The primary goal of an EDO is to compel corrective actions. However, banks can contest an EDO before an administrative law judge. The bank supervisor issues a termination order after the bank satisfactorily meets the requirements of the EDO. If a bank fails to comply, the regulator can enforce the order in court or terminate deposit insurance. For a detailed description of the EDO issuing process, please see Section 2 in Kleymenova & Tomy (2022).

sure changes in consumers' confidence in banks. Unlike studies that utilize spatial variation in the level of trust, using a bank-specific measure allows us to isolate the impact of declining trust on particular banks, mitigating concerns that other contemporaneous region-specific factors might drive borrowers' actions. Also, we exploit spatial variation in the media-driven awareness of enforcement actions to identify changes in trust. This approach is relevant because consumer awareness of enforcement actions leads to a decline in bank reputation, affecting consumers' trust in banks. We leverage research that highlights the media's vital role in disseminating firm-specific information, leading to greater visibility and stakeholder monitoring, thereby impacting firms' reputations (Dai et al., 2015; Dyck et al., 2008; Miller, 2006; Miller & Skinner, 2015). We validate EDOs as a measure of changes in trust by utilizing survey data from the Chicago Booth/Kellogg School Financial Trust Index database. We find that the incidence of EDOs at the county-year level is significantly associated with declining local trust in banks and bankers.

There are several reasons why trust in banks might deter borrowers, particularly higher-quality borrowers who have a larger choice set, from transacting with a bank that receives an enforcement action. First, a loan agreement is a complex financial contract, and borrowers might be reluctant to enter into such contracts with banks they do not trust. For instance, these contracts might have terms that require specific financial knowledge that borrowers may not possess. Borrowers may thus have to rely on the integrity of the bankers to act in the borrowers' best interest. Second, operational instability at EDO banks might reduce consumers' confidence that these banks would act in their best interest. Banks subject to EDOs frequently incur regulatory fines and may have to implement costly and significant operational modifications to comply with EDO stipulations. Prospective borrowers might avoid the bank, anticipating subpar service or increased loan rates, especially if the EDO is prominent and widely publicized. Also, customers risk losing favorable relationship-based terms if the bank collapses. Outside banks might charge higher interest rates to all new customers, including high-quality ones, due to difficulty discerning their creditworthiness. This risk could prompt high-quality customers to switch to more reliable banks preemptively.

Third, individuals often borrow from banks where they have deposits. If consumers' declining trust leads them to remove their deposits from the bank, they are also less likely to borrow from that bank. Indeed, prior research has shown that confidence in banks matters to depositors who tend to withdraw from rule-breaking banks (Das et al., 2024; Osili & Paulson, 2014). Finally, borrowers might have ethical reservations about doing business with a bank known for violating rules.

Using granular data on auto loans from a credit reporting agency (TransUnion), which provided us with a link between borrowers and lenders, we study variations in the quality of loans originated for banks that receive enforcement actions relative to those that do not. Utilizing survival models to estimate the time to delinquency, we find that new loans originated by EDO banks while an EDO is active become delinquent 28% sooner than those originated by control banks. These results are robust to including time-varying bank and local economic controls, year-month fixed effects to account for systemic time-varying factors that contribute to loan delinquencies, and stratifying by state to account for time-invariant regional differences. Our results are also robust to an entropy-matched sample, accounting for variation in banks' regional lending at the state level. We also estimate an OLS model that considers only the probability of delinquency (and does not include information on the time of delinquency as in the survival model) and find consistent results. In particular, loans originated by EDO banks while an EDO is in effect are 2.4% more likely to become delinquent than the control sample of non-EDO banks. The OLS model also includes the more granular county×year-month fixed effects. Importantly, in all of our estimations, we find that loan quality is not significantly different for EDO versus non-EDO banks prior to receiving the EDO, allaying concerns that declining loan quality might have led to the enforcement action. Furthermore, we find an insignificant and smaller difference in delinquency between loans originated by non-EDO banks and EDO banks in the five years after EDO termination.

We use loan performance to assess borrower quality because it provides a clear indication of whether borrowers meet their repayment obligations. Banks' initial assessments of borrower quality might not accurately reflect future loan performance. This is especially relevant for banks that receive enforcement actions as such banks tend to have weaker loan policies and internal governance mechanisms, leading to deficiencies in credit assessments and lending decisions (An et al., 2024). Nonetheless, we also explore borrower characteristics at loan origination for EDO banks relative to non-EDO banks. Consistent with our loan delinquency results, we find that borrower quality is worse for EDO banks while an EDO is open. Specifically, during the EDO period, borrowers of EDO banks have on average 0.8% lower credit scores, are 3.5% more likely to have ever had a loan in collections, have 0.94 percentage points higher loans ever delinquent, have 10% higher loans past due in the last 12 months, are 3% more likely to have ever defaulted, and are 0.9% more likely to have ever filed for bankruptcy compared to borrowers of non-EDO banks. Overall, these results indicate that banks originate worse loans while an enforcement action is open.

We hypothesize that a trust mechanism drives the decline in borrower quality for EDO banks when an enforcement action is open. Trust is closely linked to reputation—A strong reputation suggests competence, sound risk management, and adherence to ethical standards, encouraging consumers to trust in the bank's ability to safeguard their funds and provide dependable financial services. Banks subject to enforcement actions suffer reputational damage and a subsequent loss in consumer trust, prompting customers to avoid using the services of the EDO bank. As discussed above, this scenario is particularly likely to drive away higher-quality borrowers with more options.

To identify the mechanism, we rely on the quality of the local information environment, as reputational damage and subsequent declines in trust are less likely if consumers are not aware of enforcement actions. Higher quality local news implies that individuals would be more aware of the local events that directly affect them, such as bank misconduct. Also, a higher quality local information environment facilitates greater community interaction as community members are more likely to know of and attend community events (Abernathy, 2020; Mathews, 2022; Mathews & Ali, 2023). Greater community interaction allows consumers to get information through informal channels and might also amplify the negative sentiment towards EDO banks.

We use counties that are news deserts to identify regions that have poor local information quality. These are counties where local newspaper coverage is inadequate either because there are none or very limited local newspapers in circulation or because the existing local newspapers do not publish community-relevant information because of diminished newsrooms (Abernathy, 2020). Thus, news deserts are an unambiguous measure of poor local information quality. If a trust mechanism is at work, high-quality borrowers in news deserts are unlikely to exit EDO banks, as information about EDOs is less likely to be disseminated in these regions. Consequently, bank reputation and borrowers' confidence in EDO banks would be less impacted. Consistent with our conjecture and contrary to our main results, in news deserts, the quality of loans originated by EDO banks while an EDO is open is no different from loans originated by non-EDO banks. These results are robust to dynamically controlling for several potential economic and demographic determinants of news deserts that may correspond to loan delinquency.

We conduct several sensitivity analyses to assess the robustness of our results. First, we show that our results are unlikely to be driven by financial distress in news deserts. Second, we utilize additional measures of poor quality local information, including newspaper mergers and closures and the number of newspaper publishing establishments in a county, and find consistent results. Third, consistent with reputation effects being more pronounced immediately after an EDO is issued (due to greater media coverage) we find that our results are concentrated in the first year of EDO issuance. Fourth, we find that our results are concentrated in regions with more financially sophisticated borrowers who are more likely to consume financial news or regulatory disclosures related to bank enforcement. Finally, our results are stronger for more severe enforcement actions that are likely to attract greater negative attention.

One might argue that the effects we observe stem only from an information mechanism rather than trust in banks. Borrowers who know more about declines in a bank's financial health may avoid its services out of concern that the bank could fail to deliver quality services. However, such beliefs are related to the trust mechanism as well. When borrowers have trust in a bank, they are more likely to believe that the bank will fulfill its obligations, provide reliable services, and safeguard their interests. Conversely, if trust is compromised, borrowers may question the bank's ability to meet these expectations, leading them to avoid its services or withdraw deposits. To more clearly tie enforcement actions to a decline in trust, we utilize survey data on trust in banks and bankers. We find that, consistent with a decline in trust, counties with greater exposure to EDO banks witness a greater county-level decline in trust in banks and bankers. We also find that relative to the pre-EDO period, county-level trust declines in the years that an EDO is open. These survey-based results support enforcement actions as a significant shock to trust in banks.

We also explore the alternative that supply-side changes might drive our results. Following the enforcement action, EDO banks might have expanded credit and loosened their lending standards, thus shifting the distribution toward lower-quality borrowers. Inconsistent with this alternative, we find that EDO banks, in fact, contract lending. In addition, although we find a modest decrease in loan length, we find no change in interest rates or loan size conditional on borrower quality.

Our paper makes several important contributions. We extend the literature on how confidence in banks influences borrowers' decisions, a subject that is topical because consumer confidence in banks is declining, making it critical for consumer decision-making. Literature in this area generally focuses on depositors' actions, as it is more apparent that depositors would factor confidence in banks into their deposit decisions (Anastasiou & Drakos, 2021; Das et al., 2024; Osili & Paulson, 2014). We extend this literature by showing that bank reputation and confidence impact borrowers' decisions, which has important implications for banks' financial health. Our work complements a recent working paper on borrowers' mortgage choices between banks and Fintech providers (Yang, 2023). Unlike Yang (2023), who uses geographic variation in Wells Fargo's deposit share as a proxy for declining bank trust, we employ bank-specific enforcement actions as a shock that reduces confidence in banks. We find that reduced confidence shifts the borrower distribution towards lower-quality borrowers.

Our work is also conceptually related to literature that explores how trust in institutions shapes investor behavior and asset allocation decisions, including individuals' stock market participation (Giannetti & Wang, 2016; Guiso et al., 2008), future credit outcomes (Brown et al., 2019), and the use of professional money managers and investment advisors (Gennaioli et al., 2015; Gurun et al., 2018; Kostovetsky, 2016). We extend this literature by showing how trust in banks affects the behavior of borrowers, one of their largest stakeholder groups. Finally, we contribute to the bank enforcement action literature by using local information environments to isolate the consequences of reputational damage and declining trust in banks (Berger et al., 2022; Caiazza et al., 2018; Delis et al., 2019; Deli et al., 2019; Kleymenova & Tomy, 2022).

## 2. Data and sample

### *2.1. Bank and enforcement action data*

We obtain commercial banks' financial data from Call Reports collected by the Federal Financial Institutions Examination Council (FFIEC). We source data on enforcement actions from the S&P Global SNL Financial database. Following prior research on bank enforcement (An et al., 2024; Kleymenova & Tomy, 2022), we only include severe enforcement actions issued by federal banking regulators. These include cease and desist (C&D) orders, formal or supervisory agreements, consent orders, and prompt corrective action (PCA) orders. We merge our sample of enforcement actions with the TransUnion borrower data (described below).[4] Our sample only includes loans issued by commercial banks. Our final sample consists of 3,056 banks of which 610 received enforcement actions.

*2.2. Borrower data*

We source loan-level data from TransUnion, which is a nationally representative 10% random sample of consumer records from their credit archives. We observe individuals whose credit records appear in the TransUnion database as of July 2000 in addition to 10% of subsequent new borrowers and excluding those who drop out of the sample due to reasons such as death. We use both loan-month and consumer-month panels. The loan-month panel includes time-variant account-level information, including loan amount, origination date, closure date, delinquency indicators, and time-series tracking of payment behavior. The consumer-month panel is an array of consumer details over time, such as credit score, date of birth, location, and term loan history by type.

These data are available starting from July 2000, but data prior to 2009 contain fewer variables including the ones necessary for our analysis. Thus, we restrict our sample to 2009 through 2020. We use auto loans throughout our analysis because they are sufficiently short term for us to observe these loans from initiation to completion or delinquency within our sample period. Finally, auto loans make up a large portion of the TransUnion dataset along with mortgages and credit card loans.[5] While we do not directly observe interest rates in our sample, we impute them using loan principal, maturity, and scheduled payment amount (Granja & Nagel, 2023).

*2.3. Demographic data*

Our data on population demographics is sourced from the US Census Bureau American Community Survey (ACS). This database provides local demographic information for each US census tract, including variables related to education, income, unemployment, and population.

[4] TransUnion provided us with the list of lenders who report credit data to them. We merged these with our sample of enforcement actions and bank financial data. Then, TransUnion matched these observations to the lender key used in their loan panel, removing the bank names to ensure confidentiality.

[5] About 80% of new and 40% of used vehicles are financed. As of 2020, banks accounted for 31% of the market share for new and 34% of used auto loans. The larger portions of the remaining loan volumes were accounted for by captive finance (51% new and 7% used), credit unions (13% new and 25% used), and other independent lenders (Zabritski, 2024). Also, mortgages would be unsuitable for our analysis given that they typically have 20- or 30-year terms and we only observe data for 14 years. Credit card loans are very volatile in amount and experience large interest rate fluctuations, making them also unsuitable for our analysis.

Since ACS 1-year data is only available for areas with populations of 65,000 or more, we use ACS 5-year estimates, which are available for all census tracts. ACS 5-year data is also more representative of regional demographics over time as it averages annually collected data over the current and preceding four years. Except for observations in 2009 (when ACS 5-year data begins), we lag these variables by one year. Finally, we obtain time-invariant census-tract-level urbanization data from the 2010 U.S. Census.

### *2.4. News deserts data*

Our data on news deserts is sourced from the UNC Hussman School of Journalism and Media's Center for Innovation and Sustainability in Local Media. The database identifies local newspapers that conduct journalism serving the public interest. Importantly, it excludes shoppers, newsletters, specialized publications, promotional inserts, and certain zoned editions that do not represent public interest journalism. We combine the database with a comprehensive list of U.S. counties to identify 'news deserts,' or counties in which no local newspapers operate. This data is collected in rounds, which occur in 2004, 2014, 2016, 2018, and 2020. Given that our sample spans 2009 to 2020, we take an ex ante time-invariant definition of news deserts from 2004 data.[6]

### *2.5. Descriptive statistics*

Descriptive statistics for our main sample are presented at the loan level in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles of their respective distributions in each sample year. We provide detailed definitions of all variables in Appendix A. The average loan in our sample has an imputed APR of 6.3%, amount of $14,660, and term length of 50 months.[7] For the 8.6% of loans that go delinquent, an average of 20 months elapses between initiation and delinquency, and the distribution of time to delinquency is slightly right-skewed. Looking at borrower characteristics at loan origination, the average borrower is 42 years old and has a credit score of 691. In addition, 19.4% of borrowers have ever had a loan in collections, 26.6% have ever defaulted on a loan, and 3.5% have ever experienced a bankruptcy. Geographically, 1.6% of loan

[6] The data collection staff indicated that data from the 2014 and 2016 collection rounds are less accurate.

[7] The periodic interest rate is imputed using the initial loan amount, the number of periods, and the payment per period. The APR is then calculated as the number of periods multiplied by the periodic interest rate.

borrowers reside in news deserts, 27.2% in top-tercile undergraduate-educated census tracts, and 28.4% in bottom-tercile income census tracts.

In terms of EDOs, 7.5% of loans in our sample are issued by EDO banks, with 1.8% issued before EDOs, 2.7% issued during EDOs, and 3.0% issued after EDOs. Figure 1 shows the geographic distribution of loans made by EDO and non-EDO banks in the contiguous US, and Figure 2 shows the distribution of news desert countines. EDOs and news deserts seem to be uniformly scattered across the country and do not appear clustered in any particular area. As for bank characteristics, TransUnion only allows us to match decile values of bank-specific variables in order to ensure bank anonymity. Therefore, we utilize deciles, calculated from the distribution of all commercial banks, as our bank controls. We lag these variables by one year in order to capture the knowledge that borrowers have about the bank at the time of loan origination.

## 3. Hypothesis development and empirical results

### *3.1. EDOs and time to delinquency*

We hypothesize that enforcement actions compromise bank reputation and thus individuals' trust in banks, affecting their likelihood of using banks' services. Enforcement actions are publicized in the news media and on regulators' websites, and represent significant adverse events for banks. Research has found that adverse banking events influence individuals' confidence in banks. For example, several studies find that confidence in the banking sector declined following the great financial crisis of 2008 (Fungáčová et al., 2019; Knell & Stix, 2015; Sapienza & Zingales, 2012; Van Der Cruijsen et al., 2023). In the same vein, Jansen et al. (2015) find that adverse media reports, falling stock prices, opaque product information, and excessive executive compensation are associated with low trust in banks. Research also distinguishes between broad-scope trust (trust in institutions in general) and narrow-scope trust (trust in a specific institution, e.g., in one's own bank). Our approach of using enforcement actions focuses on narrow-scope trust. However, Van Der Cruijsen et al. (2023) find that broad-scope and narrow-scope trust are highly correlated (correlation coefficient of 0.72).[8]

[8]In analyses discussed in Section 5.1, we find that EDOs are correlated with survey-based measures of local trust in banks and bankers.

Only a few studies extend this line of work to examine if individuals' financial decisions are impacted by their loss of confidence in banks, but most studies focus on the behavior of depositors rather than borrowers. For example, Osili & Paulson (2014) find that immigrants who experienced a banking crisis in their home country are less likely to have checking accounts with US banks relative to others who emigrated from the same country but did not live through a banking crisis. Similarly, Anastasiou & Drakos (2021) show that depositors' crisis sentiment drives bank deposit flows in the European Union. Specifically exploring bank enforcement, Das et al. (2024) find that news of supervisory penalties on some banks in India leads depositors to withdraw funds from both the penalized and neighboring nonpenalized banks' branches. Consistent with trust in banks influencing depositor behavior, they find that withdrawals are more pronounced in regions with lower trust in public institutions (i.e., courts and banks) relative to regions with higher trust. Also, the 2021 FDIC National Survey of Unbanked and Underbanked Households reveals that 33% of unbanked households cite a lack of trust in banks as a reason for not having a bank account (FDIC, 2021).

The decline in a bank's reputation and the subsequent loss of confidence could also affect borrowers' behavior, particularly higher-quality borrowers who have more options. First, loan agreements are complex financial contracts containing specialized financial terms, and borrowers often depend on the integrity of bankers to act in their best interest. Thus, they would likely hesitate to enter such contracts with banks they do not trust. Second, EDO banks face operational instability because of regulatory penalties and disruptive changes necessary to satisfy EDO requirements (An et al., 2024), which casts doubt on banks' ability to act in their customers' best interests. For example, enforced banks might be less able to offer superior rates and service to borrowers. Also, if the bank fails, high-quality customers would lose the favorable terms they have built up over time based on their relationship with the bank and the soft information the bank has acquired (Slovin et al., 1993), likely facing relatively higher interest rates from uninformed outside banks. As a result of this potential for adverse selection, high-quality customers might flee the EDO bank in anticipation, seeking to establish more stable banking relationships elsewhere. Third, individuals often initiate loans with the same bank where they maintain a deposit account. As depositors withdraw their funds from EDO banks (Delis et al., 2019), it is likely that these individuals also shift their future borrowing away from the bank. Finally, borrowers may also have ethical considerations about transacting with a rule-breaking bank (Rivoli, 1995), which may be significant if they are explicitly aware of a bank's EDO status.

To assess changes in the credit quality of originated loans over the various stages of enforcement actions, we follow prior literature to estimate survival models of loan delinquency (Gross & Souleles, 2002; Shumway, 2001). Estimating survival models allows us to utilize information on both the occurrence of and time to delinquency. Specifically, we estimate the following lognormal accelerated failure time (AFT) model:

$$\begin{aligned}\log(\textit{Time to Delinquency})_{itbcs} = \beta_0 + \beta_1 EDO_b + \beta_2 \textit{EDO Cohort}_b \\ + \beta_3 Bank_{(t-1)b} + \beta_4 Econ_{(t-1)c} + \delta_t + \gamma_s + \varepsilon_{itbcs}\ .\end{aligned} \tag{1}$$

The model predicts how the covariates accelerate or decelerate the time to delinquency for a loan. In Equation 1, $i$ indexes the loan, $t$ the year-month of loan origination, $b$ the bank, $c$ the local geographic region (census tract), and $s$ the state. The dependent variable, *Time to Delinquency*, is the survival time and represents the number of months a loan remains current before it goes delinquent.[9] *EDO Cohort* represents indicators for the following groups: *Pre EDO*$_{[\tau-3,\tau-1]}$, *During EDO*$_{[\tau]}$, and *Post EDO*$_{[\tau+1,\tau+5]}$, where $\tau$ represents all years that an EDO is active. Specifically, *Pre EDO* is an indicator for loans issued one to three years before EDO issuance, *During EDO* is an indicator for loans issued while an EDO is active, and *Post EDO* is an indicator for loans issued one to five years after EDO termination. The variables *Bank* and *Econ* are lagged controls for bank characteristics and local economic conditions, respectively, which are expected to influence the likelihood of and time to delinquency. *Bank* includes yearly deciles of bank size, profitability, liquidity, capital ratio, and nonperforming assets. *Econ* includes census-tract-level unemployment rate and yearly percent growth in per capita income. We include year-month fixed effects ($\delta_t$) to account for time-varying factors that contribute to loan delinquencies and stratify by state ($\gamma_s$) to

[9]Loans are considered delinquent in the first month of nonrepayment and drop out of the sample thereafter (Gross & Souleles, 2002). For loans that are marked delinquent after the termination date, we consider them delinquent in the last period before termination if they are marked delinquent within six months after termination. This is a reasonable assumption, as there is often a delay between the loan delinquency and the bank reporting it to the credit agency, and the majority of these delayed delinquencies in our sample are documented within six months of termination. Delinquencies showing longer delays could be due to other unrelated errors, so we exclude those from the sample. Observations are at the loan-month level, and the survival model uses the delinquency indicator to implicitly calculate loan-level *Time to Delinquency*.

account for time-invariant regional differences. Finally, $\varepsilon$ is the error term.

Our sample includes loans originated by all EDO and non-EDO banks in the random sample of loans provided by TransUnion. For the years of interest in our study, we include the full sample of loans in our analyses because the survival model implicitly assumes that all loans have some non-zero probability of delinquency and thus contribute some information, increasing the accuracy of our estimates (Singer & Willett, 2003).[10] For EDO banks, we drop observations following *Post* $EDO_{[\tau+1,\tau+5]}$. If EDO banks experience reputational costs, and a subsequent loss of trust that cause them to lose high-quality borrowers, we expect loans originated in the *During EDO* period to go delinquent sooner than loans originated by non-EDO banks. Then, if trust in the bank improves after EDO termination, the quality of loans originated in the *Post EDO* period should be higher than *During EDO* loans. Finally, we are agnostic about the sign of the coefficient on *Pre EDO*. This coefficient could be negative if the bank had declining borrower health caused by irresponsible lending before the EDO, which could be the reason for the issuance of the EDO. On the other hand, if the bank received an EDO for other reasons, the coefficient could be close to zero. Thus, including the *Pre EDO* variable in our analysis helps us evaluate the potential alternative explanation that the cause of the bank receiving the enforcement action was risky and irresponsible lending which may have continued after EDO issuance.

In Figure 3, we plot the hazard function, which shows how the instantaneous risk of delinquency changes over time for loans originated in each *EDO Cohort*. The risk of delinquency increases from loan inception, reaches its maximum around 20 months after inception, and declines after that. Importantly, throughout the life of the loan, the probability of delinquency is highest for loans originated when an EDO is in effect (that is, in the *During EDO* period). The next highest probability of delinquency is for the cohort of loans originated in the five years following EDO termination (the *Post EDO* period), consistent with banks gradually recovering from the impact of enforcement. We also observe that the probability of delinquency is similar for loans originated

[10] A general form of the likelihood function is:

$$L = \prod_{j=1}^{N} [f(t_j)]^{d_j} [S(t_j)]^{1-d_j} \ .$$

The delinquent (uncensored) observations ($d_j = 1$) contribute to the density $f$, whereas the non-delinquent (censored) observations ($d_j = 0$) contribute to the survival function $S$.

in the three years before an EDO (the *Pre EDO* period) and the control sample of loans originated by non-EDO banks.

In Table 2, we tabulate the results from estimating Equation 1. Consistent with our prediction, Column (1) shows a statistically significant coefficient of $-0.324$ for *During EDO*, suggesting that loans originated by EDO banks while an EDO is active become delinquent 28% sooner than those originated by control banks.[11,12] In column (2) of Table 2, we use entropy balancing to mitigate the concern that EDO banks may systematically differ from non-EDO banks in their lending footprint, resulting in differences in the long-term loan outcomes.[13] Specifically, we match EDO banks and non-EDO banks on the first two moments of the number of auto loans originated at the bank-state level, and we re-balance our sample in each year. Using the entropy-balanced sample, Column (2) shows a statistically significant coefficient of $-0.309$ for *During EDO*, suggesting that loans originated by EDO banks while an EDO is active become delinquent 27% sooner than those originated by control banks.[14] Additionally, in both columns, the coefficient of *Post EDO* has the same sign but a lower magnitude than the coefficient of *During EDO*, consistent with banks gradually recovering after EDO termination.

One potential concern is that a decline in loan quality could have resulted in bank enforcement, thereby confounding our empirical inferences. This reverse causality concern is somewhat alleviated because the insignificant coefficients on *Pre EDO* and *EDO* suggest that, conditional on bank characteristics, EDO banks are no different from non-EDO banks in loan delinquencies prior to receiving the EDO. Furthermore, we study delinquency for *originated* loans by *EDO Cohort*, implying that loans originated in the *EDO* or *Pre EDO* period cannot show up as delinquent in the *During EDO* period.[15]

[11] In all subsequent discussions, we define control banks as non-EDO banks and EDO banks before the *Pre EDO* years.

[12] These magnitudes are calculated as follows: $(1 - e^{-0.324}) \times 100$. This is illustrative of the approach we use in all subsequent analogous cases.

[13] Entropy balancing utilizes an optimization algorithm to construct weights for treated and control units under a set of balancing constraints (Hainmueller, 2012). Unlike most other matching methods, entropy balancing does not simply discard unmatched observations, thus preventing the loss of information arising from a reduction in sample size.

[14] We utilize the entropy-balanced sample in all subsequent analyses unless otherwise noted. In our main analysis, we match EDO and non-EDO banks on the first two moments of the number of auto loans granted at the bank-state level. In additional robustness checks, we balance the sample based on the first two moments of the amount of auto loans granted at the bank-state level and find consistent results.

[15] Nonetheless, we acknowledge that this concern persists—for instance, delinquency might have been gradually increasing in subtle ways that our tests have not detected. In later analyses (discussed in Section 4), we conduct

Using a survival model enables us to incorporate more information in our estimation, specifically both the occurrence of delinquency and the time to delinquency. However, this model statistically limits the number of fixed effects we can include. To address this limitation, we additionally estimate an OLS model that captures only the occurrence of delinquency. Specifically, we re-estimate Equation 1 where the dependent variable is *Delinquency*, an indicator for whether a loan goes delinquent at any point during its entire life, instead of log(*Time to Delinquency*). In the OLS specification, we include granular county × year-month fixed effects, allowing us to remove unobservable factors that are unique to each county in each month. We further augment time-varying bank-level controls with bank fixed effects that capture unobservable time-invariant characteristics unique to each bank. With more stringent fixed effects, we find consistent results—loans originated by EDO banks in the *During EDO* period are 2.4% more likely to become delinquent relative to the control sample of loans originated by non-EDO banks (see Table A1 of Appendix B). Also, consistent with the survival model results, loans originated in the *Pre EDO* and *Post EDO* periods are not significantly different in their likelihood of delinquency compared to loans originated by control banks.

### *3.2. Borrower characteristics at loan origination*

We next study whether the borrower characteristics at loan origination are systematically different for EDO versus non-EDO banks. Specifically, we estimate variations of the following OLS model:

$$\begin{aligned} Borrower\ Characteristic_{i(t-1)bc} =& \beta_0 + \beta_1 EDO_b + \beta_2 EDO\ Cohort_b \\ &+ \gamma_1 Borrower\ Age_{it} + \gamma_2 Econ_{(t-1)c} + \eta_c + \alpha_b + \delta_t + \varepsilon_{itbc}\ , \end{aligned} \tag{2}$$

where *Borrower Characteristic* represents various credit and financial history variables for a borrower as of the month before loan origination. These include the borrower's credit score (*Credit Score*), whether the borrower ever had a loan in collections (*Collections*), the percentage of loans that have ever been delinquent for the borrower (*% Delinquent*), the total past-due loan amount in the last 12 months for the borrower (*Past Due (12mo)*), whether the borrower has ever defaulted

additional tests to more directly assess reputational effects.

on a loan (*Defaults*), and whether the borrower has ever filed for bankruptcy (*Bankruptcy*).[16] The variable *Borrower Age* represents the natural log of the borrower's age at the time of loan origination. $\eta_c$, $\alpha_b$, and $\delta_t$ represent county, bank, and year-month fixed effects, respectively. The remaining variables are defined as before.

Similar to the survival analysis, we include local macroeconomic variables (i.e., per capita income growth and unemployment rate) to account for changes in census-tract-level economic conditions that might influence borrowers' credit characteristics. We also control for borrower age because it likely impacts the length of credit history available, credit utilization rates, and experience in managing credit effectively. Finally, we include bank fixed effects to control for any time-invariant bank-specific factors.

In Table 3, we present results from the estimation of Equation 2. Column (1) shows that the borrowers of loans originated in the *During EDO* period have worse credit scores compared to loans originated by control banks. On average, borrower credit scores are 0.8% lower in the *During EDO* period, which translates into a score difference of 6 points.[17] Around an already high average, this is a modest but economically significant magnitude. Consistent with the results for credit score, Columns (2)–(4) show that, relative to the control banks, borrowers of EDO banks in the *During EDO* period are 3.5% more likely to have ever had a loan in collections, have 0.94 percentage points higher loans ever delinquent, and have 10% higher loans past due in the last 12 months. Further, Columns (5) and (6) indicate that borrowers of EDO banks are 3% more likely to have ever defaulted and 0.9% more likely to have ever filed for bankruptcy relative to non-EDO banks. Overall, the results in Table 3 are consistent with those in Table 2, indicating that borrowers of loans originated while an EDO is in effect are generally of worse quality compared to borrowers of loans originated by control banks.

## 4. Exploring the mechanism: Trust in banks and borrower quality

Our findings of faster time to delinquency and worse borrower quality for loans originated during an EDO could be explained by EDO-driven declines in bank reputation and subsequent losses

[16]Bankruptcies only include those reported to the credit agency.

[17]We calculate this as the pre-EDO average score of 696 multiplied by 0.8%, which yields a difference of 5.57 points.

of consumers' trust. As discussed in Section 3.1, this should differentially impact higher-quality borrowers because they possess a larger choice set.

To identify the effect of consumers' declining trust in banks, we rely on variations in the local information environment, specifically the local news media. Local news outlets can affect a bank's reputation, and consequently consumers' trust in banks, by creating and disseminating news about banks (Dai et al., 2015; Dyck et al., 2008; Miller, 2006; Miller & Skinner, 2015). While news content is often created by other parties (e.g., regulators who issue enforcement actions), news outlets play an active role in disseminating this information in an accessible format to the general public. They also contribute to the forming of public opinion by contextualizing news—for example, by highlighting the importance of a bank to the local economy or discussing violations by specific bank executives.[18] Furthermore, research suggests that local news plays an important role in creating awareness of local community events, thereby increasing community interaction (Mathews, 2022). Even if the general public is not attentive to individual instances of enforcement, a richer local information environment can enhance collective negative sentiment towards offending banks. Thus, declines in trust in a bank involved in wrongdoing would be more severe in areas with a richer news environment.

We use counties that are news deserts to identify regions with inferior local information quality. Abernathy (2020) defines a news desert as "a community, either rural or urban, where residents have very limited access to the sort of credible and comprehensive news and information that feed democracy at the grassroots level." We rely on news deserts as our main measure of low quality local information for two reasons.[19] First, news deserts are an unambiguous measure of poor local information. These are counties where local newspaper coverage is inadequate either because there are none or very limited local newspapers in circulation, or because the existing local newspapers do not publish community-relevant information because of diminished newsrooms (Abernathy, 2020). Even though people might obtain news digitally, most online sources of local news are operated by local newspaper providers and nationally available news does not typically cover local events.[20] Thus, obtaining local information in a news desert requires significant effort

[18] Please see Appendix B for examples of media coverage of bank enforcement actions.

[19] As described later in this section, we also utilize newspaper closures and mergers, and the number of newspaper publishing establishments as additional measures of the local information environment.

[20] This also applies to news obtained through social media, which are typically also maintained by local newspaper

(Mathews & Ali, 2023), such as accessing news from the nearest city, and such news may not cover important local events or opinions deemed inconsequential by larger news outlets. Second, research finds that a lack of local newspapers dilutes bonds between community members because newspapers often act as a central source of shared information, fostering a sense of community identity and engagement (Mathews, 2022). Without newspapers, residents lose a common platform for local news and events, leading to reduced communal interaction and local awareness. Therefore, word-of-mouth information about financial matters (such as dealing with a poorly managed bank) is also less likely to transmit between individuals in news deserts. For these reasons, enforcement actions are less likely to lead to trust declines in news deserts because borrowers would be less informed about bank-related issues. Thus, in news deserts, we expect a less salient or nonexistent effect of EDOs on loan quality.

We estimate variations of the following model:

$$
\begin{aligned}
\log \mathit{(Time\ to\ Delinquency)}_{itbcs} = {} & \beta_0 + \beta_1 EDO_b + \beta_2 EDO\ Cohort_b + \beta_3 News\ Desert_c \\
& + \beta_4 EDO \times News\ Desert_c + \beta_5 EDO\ Cohort_b \times News\ Desert_c \\
& + \gamma_1 Bank_{(t-1)b} + \gamma_2 Econ'_{(t-1)c} + \delta_t + \gamma_s + \varepsilon_{itbcs}\ ,
\end{aligned}
\tag{3}
$$

where *News Desert* is an indicator for a county that has no local newspapers in operation as of 2004. $Econ'$ includes the *Econ* variables as defined before in addition to several other local economic and demographic variables at the census tract level that could be associated with the incidence of news deserts. Since economically disadvantaged or rural counties may be unable to sustain a local newspaper through subscriptions and advertising revenue, these counties are more likely to be news deserts. Therefore, in addition to per-capita income growth and unemployment rate defined as before, we also include the natural log of population size, level of per-capita income, average age of the population, and level of urbanization as economic controls. Additionally, we interact *News Desert* with these control variables to address the concern that economic and demographic conditions differentially impact loan quality in news deserts relative to other areas. The remaining variables are as defined before.

providers. Social media from other sources may contain opinions and biases that are not conducive to the local information environment.

Results from the estimation of Equation 3 are presented in Table 4. Column (1) includes the *Econ* and *Bank* controls, similar to the main results in Table 2. Column (2) adds the remainder of the $Econ'$ controls, which may be associated with the occurrence of news deserts. Column (3) further interacts *News Desert* with all of the $Econ'$ variables. The economic health of an area is closely tied to residents' income and their likelihood of loan delinquency. Thus, interacting *News Desert* with local economic and demographic controls mitigates concerns that our results for news deserts reflect factors other than the reputational costs of bank enforcement.

Contrary to our main results in Table 2, the findings in Table 4 show a slower time to delinquency for EDO banks when borrowers are located in news deserts. Specifically, based on the results presented in column (1), when the borrower is not located in a news desert (i.e., *News Desert* $= 0$), delinquency for loans originated while an EDO is open is 27% faster relative to control banks (based on the coefficient estimate of $-0.320$ on *During EDO*). However, when the borrower is located in a news desert (*News Desert* $= 1$), the time to delinquency is not significantly different from that of control banks. The insignificant difference in loan delinquencies for loans originated in news deserts while an EDO is open is assessed with a test of whether the sum of coefficients on *During EDO* and *During EDO* $\times$ *News Desert* equals zero. The test fails to reject the null ($z$-statistic $= 1.316$). We find results of similar magnitude and statistical significance in Columns (2) and (3).

One potential concern is that banks receiving EDOs might operate in economically distressed areas, which are systematically correlated with news deserts. Indeed, the negative coefficient on *News Desert* in Column (3) of Table 4 suggests that loans originated in news deserts do become delinquent sooner relative to loans originated in non-news deserts. Economic distress could cause banks to take undue risk, resulting in overly risky lending practices and, ultimately, receiving an EDO. However, this alternative is inconsistent with the insignificant coefficients on *EDO* $\times$ *News Desert* and *Pre EDO* $\times$ *News Desert*. These coefficients suggest that, conditional on covariates, loan delinquency trends in news deserts are not significantly different for pre-EDO bank loans relative to non-EDO bank loans. Another potential concern is that borrower quality in news deserts declines at the same time that banks receive EDOs. However, under this alternative, the coefficient of *During EDO* $\times$ *News Desert* would be negative and not positive as we find. That is, borrower quality should be worse in news deserts during an EDO, not better as the positive coefficient of

*During EDO* $\times$ *News Desert* indicates.

We conduct four additional robustness tests. First, we run further analysis to rule out the alternative that news deserts might merely coincide with low-income areas, and economic distress rather than bank reputation could be driving our results. Specifically, we reestimate Equation 3 adding *Low Income*, an indicator for lowest-tercile individual or household median income in a census tract, and its interactions with *EDO Cohort*. This analysis determines whether *News Desert* or *Low Income* have higher explanatory power for *Time to Delinquency.* If a significant correlation with local economic distress primarily drives the news desert result, then we should observe a significant positive coefficient for *During EDO* $\times$ *Low Income* but not for *During EDO* $\times$ *News Desert*. In contrast, Table 5 shows that *During EDO* $\times$ *Low Income* is insignificant while *During EDO* $\times$ *News Desert* continues to be significant and positive. For borrowers located in news deserts, the time to delinquency for *During EDO* loans remains insignificantly different from that of control loans. These results suggest that local financial distress is unlikely to drive our findings.

Second, we perform a county-level propensity score matching to address the concern that other economic and demographic factors correlated with news deserts, such as population size or urbanization, could be driving our results. We first estimate a logit model that regresses *News Desert* on the $Econ'$ variables. Then, we calculate the propensity score for each county and pair each news-desert county with ten non-desert counties with the smallest differences in propensity scores.[21] Table A2 Panel A, shows that news-desert and non-desert counties are balanced along each of the six variables in $Econ'$. We reestimate Equation 3 using the new matched sample and present the results in Table A2 Panel B. Column (1) does not include matched-pair fixed effects while Column (2) does. The results are consistent with those in Table 4, indicating that our results are not driven by county-specific factors correlated with news deserts.

Third, we exploit a different source of variation in the local information environment induced by local newspaper closures and mergers. Closures of local newspapers are significant occurrences that directly reduce the amount of information available on local events. Newspaper mergers distort the operations of all newspapers involved and potentially lower the competition among the

[21] We perform propensity score matching with replacement. Specifically, we have 178 news-desert counties, so we pair with 1780 non-desert counties.

local newspaper publishing industry, similarly leading to the deterioration of the local information environment.[22] An advantage over the news deserts measure is that, while newspaper closures and mergers are not entirely random, they are less likely to be correlated with the time to delinquency for auto loans. In particular, Ma et al. (2023) provide evidence that the deterioration of local economies is not a major force driving newspaper closures. Instead, they suggest that newspaper closures are more likely driven by evolution in the newspaper publishing industry, which alleviates concerns that factors other than local information quality drive our results. Also consistent with lower local information quality, the literature finds various negative consequences of local newspaper closures. These include increased profitability from insider trading (Kyung & Nam, 2023), more frequent corporate misconduct (Heese et al., 2022), greater toxic emissions (Jiang & Kong, 2024), reduced regulatory activity (Leonelli, 2024), higher interest spreads for corporate borrowers (Ma et al., 2023), and higher municipal borrowing costs (Gao et al., 2020).

We obtain data on local newspaper closures and mergers from UNC's Center for Innovation and Sustainability in Local Media. When there are multiple newspaper closures or mergers in the same county and year, we consider them as one treatment event. For each county, we keep only the first such treatment event within our sample period and restrict our sample to the 27 counties that experienced at least one newspaper closure or merger.[23] We reestimate Equation 3 after replacing *News Desert* with *Post Newspaper Closure*, an indicator variable that takes the value of one if an EDO is issued following a local newspaper closure or merger, and zero otherwise.[24]

We present the results in Table 6. Similar to Table 4, Column (1) only includes the *Econ* controls, while Columns (2)–(3) include increasingly stringent controls. Based on the results presented in column (1), if an EDO is issued when there are no newspaper closures or mergers (i.e., *Post Newspaper Closure* = 0), delinquency for loans originated while an EDO is open is 65% faster relative to control banks (based on the coefficient estimate of −1.059 on *During EDO*). However, when an EDO is issued following a newspaper closure or merger in the county (i.e., *Post Newspaper*

[22] Our inferences remain unchanged if we exclude newspaper mergers from our analysis (untabulated).

[23] Because we use a restricted sample, we do not employ entropy balancing for this analysis.

[24] *Post Newspaper Closure* is defined based on the issuance date of EDOs since we focus on the media coverage of EDOs in this analysis. This variable is thus defined only for EDO banks, which means the two-way interaction term *EDO* × *Post Newspaper Closure* is collinear with *Post Newspaper Closure*. Consequently, the coefficient of *EDO* × *Post Newspaper Closure* is not estimated.

*Closure* $= 1$), the time to delinquency is not significantly different from that of control banks. The insignificant difference is assessed with a test of whether the sum of coefficients on *During EDO* and *During EDO* $\times$ *Post Newspaper Closure* equals zero. The test fails to reject the null ($z$-statistic $= -0.482$). Estimates in Column (2) and (3) lead to similar inferences.

Finally, we also measure the local information environment using data on newspaper publishing establishments from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (Allee et al., 2024). Specifically, we reestimate Equation 3 replacing *News Desert* with *Low Establishment Count*, an indicator for whether a county is within the bottom tercile of the number of newspaper publishing establishments. Results are presented in Table A3. Consistent with Table 4 and Table 6, we find that the reduced time to delinquency for *During EDO* loans is not observed in counties with fewer newspaper establishments. Overall, the findings in this section suggest that a decline in local information quality attenuates the reputational damage from enforcement actions and the subsequent loss of consumer confidence.

## 5. Additional analyses and robustness

### *5.1. Validating EDOs as a measure of trust*

Our analyses build on prior research that finds adverse events such as the financial crisis or negative news lead to a decline in consumers' trust in banks (Jansen et al., 2015; Sapienza & Zingales, 2012). Enforcement actions are also adverse events covered in the news media and, therefore, should result in a decline in trust. In this section, we validate this assumption using the Chicago Booth/Kellogg School Financial Trust Index data. Available from 2009 to 2021, the index is constructed by surveying over 1,000 randomly selected American households about their trust in various financial institutions. The survey question is:

> *On a scale from 1 to 5 where one means "I do not trust them at all" and five means "I trust them completely," can you please tell me how much do you trust... [Banks, Bankers, Brokers, Mutual funds, Stock market, Insurance companies, The Government, Large corporations, The market system, The Federal Reserve Bank, Other people in general]?*

We utilize these survey responses to create two measures, *Trust (Banks)* and *Trust (Bankers)*, which are indicators for an average response of 4 or higher for trust in banks and bankers, respectively, in each county-year.

We conduct two sets of analyses. First, we assess whether higher county-level exposure to EDO banks is associated with greater declines in trust in banks and bankers. Specifically, we define *Exposure to EDO* as an indicator for the top tercile of the proportion of total loan volume within a county issued by banks under active EDOs. We estimate an OLS model regressing trust in banks and bankers on our EDO exposure indicator in addition to year and state fixed effects. Results presented in Table 7 Panel A indicate that high county-level exposure to EDO banks is associated with a county-level decline in trust in banks and bankers.[25]

Second, we test whether county-level trust declines during bank EDOs relative to before EDO initiation. Specifically, we identify survey responses within the county of each EDO bank (by bank headquarters) and restrict our sample to responses in the *Pre EDO* and *During EDO* periods. We again estimate an OLS model regressing trust in banks and bankers on a *During EDO* indicator. Results presented in Table 7 Panel B show a 6.0% and 3.6% decline in trust in banks and bankers, respectively, in the *During EDO* period relative to the *Pre EDO* period. Overall, both tests demonstrate that EDOs significantly and negatively impact local levels of consumer trust in banks and bankers.

### *5.2. Supply-side changes*

Our analyses thus far consider the demand-side changes: enforcement actions result in significant reputational damage and, consequently, a decline in the confidence in banks. High-quality borrowers, who have more options, choose not to use the services of the EDO banks, leading to a shift in the distribution of borrowers towards lower-quality borrowers. In this section, we consider the alternative that our results are not driven by higher-quality borrowers exiting EDO banks, but rather by EDO banks expanding total credit provided thereby shifting the distribution of borrower quality towards lower quality borrowers.

To explore this alternative, we examine whether banks expand credit after receiving an enforcement action. Specifically, we estimate variations of the following OLS model at the bank-

[25] In an untabulated robustness test, we also measure exposure to EDO banks using county-level deposits and find similar results.

county-year level:

$$
\begin{aligned}
Loan\ Volume_{tbc} = \beta_0 + \beta_1 EDO\ Cohort_b + \gamma_1 Bank_{(t-1)b} + \gamma_2 Econ_{(t-1)c} \\
+ \eta_c + \alpha_b + \delta_t + \varepsilon_{tbc}\ .
\end{aligned} \tag{4}
$$

*Loan Volume* represents one of the following measures: the total amount of loans issued by bank $b$ in a county-year (*Loan Amount*), the total number of loans issued by bank $b$ in a county-year (*Loan Count*), the share of total loan amount in a county-year that is issued by bank $b$ (*Loan Amount Share*), the share of total loan count in a county-year that is issued by bank $b$ (*Loan Count Share*). We present results from estimating Equation 4 in Table 8. Across all specifications, we find that EDO banks in fact contract lending relative to non-EDO banks.[26] These findings are inconsistent with the alternative explanation that a supply-side credit expansion drives the observed shift in distribution towards lower-quality borrowers.

Although we cannot cleanly parse out demand versus supply-side effects, we conduct additional analysis to understand whether EDO banks changed their loan terms conditional on borrower credit quality. We reestimate Equation 4 using the following loan-specific outcome variables: the imputed APR (*Interest Rate*), the natural log of the loan size (*Loan Size*), and the natural log of the initial loan term (*Loan Length*). We include *Credit Score* and *Borrower Age* as additional control variables. Table 9 presents results from this estimation. Columns (1)–(4) show insignificant and negligible changes in interest rates and loan size during EDOs. However, Columns (5) and (6) indicate that loan length decreases by 1.7–2.9% in the *During EDO* period relative to the *Pre EDO* period, which translates into a modest 1–2 month decrease relative to the pre-EDO average term of 57 months. Overall, we find that EDO banks did not expand lending or materially change their loan terms conditional on borrower quality, which suggests that supply-side changes are unlikely to drive the decline in loan quality that we find in the *During EDO* period.

### *5.3. Time to delinquency in the first year of EDO issuance*

Reputational damage from an enforcement action is likely to be more pronounced immediately after it is issued, as news outlets typically cover enforcement actions soon after its issuance.

[26] In additional analyses, we aggregate *Loan Amount* and *Loan Count* at the bank-year level instead of the bank-county-year level. The results are presented in Table A4 and yield similar inferences.

Declines in trust are also likely to be most salient at this time. If news coverage is not sustained after the initial shock, high-quality borrowers might begin returning to the bank even while the EDO is still active. To test this conjecture, we re-estimate Equation 1 partitioning the *During EDO* indicator into *During EDO 1*, an indicator for the first year of EDO issuance, and *During EDO 1+*, an indicator for all subsequent years an EDO is active. Table 10 presents the results, showing that the decline in loan quality is concentrated in the first year of EDO issuance. This is consistent with media coverage of EDOs driving lower trust in enforced banks and decreased loan quality.

### *5.4. Robustness: Borrower financial sophistication*

We conduct additional robustness tests to strengthen our evidence for reputational damage from EDOs and declining trust in banks If a decline in trust in EDO banks drives borrower actions, our results should be concentrated among borrowers who are more financially sophisticated, who are better able to access and comprehend information related to bank enforcement actions. Financially unsophisticated borrowers generally pay less attention to regulatory disclosures, EDO-specific financial news, and informal word-of-mouth communications that lead to impression formation about banks. They are also less able to interpret and use such information when deciding which banks to transact with, making it less likely that bank reputation lead to declining trust in banks and influence their borrowing decisions. Following prior work, we utilize geographic variation in education levels to proxy for local financial sophistication (Forman et al., 2012; Kumar, 2009).[27] Specifically, we reestimate Equation 3 replacing *News Desert* with *High Education*, an indicator of whether a census tract is within the top tercile of the proportion of high school-, college-, or postbaccalaureate-educated individuals.

Table 11 presents results from this estimation. The reduction in time to delinquency for loans originated during an EDO is concentrated in census tracts where borrowers are more financially sophisticated. In census tracts with lower financial sophistication (*High Education*=0), delinquency trends during EDOs are insignificantly different from those of control banks. However, in census tracts with high financial sophistication (*High Education*=1), loans originated during EDOs show a

[27] Regions with more financially sophisticated populations are often wealthier. Although we control for the unemployment rate and annual per capita income growth in our estimations, we recognize that our measures of financial sophistication may also reflect underlying wealth differences. A trust mechanism is also likely to be more salient in wealthier regions, which tend to have a greater proportion of high-quality borrowers.

statistically significant 37%–39% decrease in time to delinquency compared to control banks.[28] The insignificant coefficient on $EDO \times High\ Education$ is inconsistent with EDO banks systematically operating in high-education areas. Also, the significant and positive coefficient on *High Education* supports our expectation that loans in financially sophisticated localities have a higher time to delinquency. Overall, results from this test indicate more salient reputational damage and declines in trust from EDOs in areas with higher financial sophistication.

*5.5. Robustness: EDO severity*

Finally, we study how EDO severity influences changes in time to delinquency. More severe EDOs would attract greater negative attention locally, as these events are more likely to be covered (perhaps repeatedly) by news outlets or communicated informally by word of mouth. Thus, the decline in borrower quality during EDOs should be stronger for more severe EDOs. Consistent with prior research, we proxy for EDO severity using loan length, as more severe EDOs likely take a longer time to resolve (An et al., 2024; Kleymenova & Tomy, 2022). Specifically, we reestimate Equation 3 replacing *News Desert* with *Long EDO*, an indicator for whether an EDO is four years or longer (approximately 80th percentile among all EDOs).

Results from the estimation are presented in Table A5. For short EDOs (i.e., *Long EDO*=0), we find no significant effect on delinquency trends when an EDO is open. However, because *Long EDO* is only defined for the EDO sample, we only observe within-EDO bank temporal variation.[29] For long EDOs (*Long EDO*=1), loans originated the *During EDO* period are associated with a 42% decrease in the expected time to delinquency, compared to short EDOs in the benchmark period (years before the *pre EDO* period). The decrease in the expected time to delinquency is based on the significant and negative coefficient on *During EDO* × *Long EDO*. These results are consistent with more severe EDOs inducing more salient reputational damage to the bank, resulting in lower borrower quality in the *During EDO* period.

[28] These magnitudes are calculated from the sum of coefficients on *During EDO* and *During EDO* × *High Education*. Specifically, $(1 - e^{-0.455}) \times 100$ and $(1 - e^{-0.502}) \times 100$.

[29] Because we only use the EDO bank sample, this analysis does not support entropy balancing.

## 6. Conclusion

We examine how consumers' trust in banks affects their borrowing behavior, addressing a timely and underexplored issue given the ongoing decline of consumer trust in the banking sector (Wiseman & Fingerhut, 2023). Measures of trust in banks, typically derived from public surveys, are generally not bank-specific, presenting a major challenge for linking trust in banks with borrower behavior. To address this challenge, we use enforcement decisions and orders (EDOs), issued by bank supervisors, as a potential negative shock to consumers' trust in banks. As a measure of last resort, regulators can issue EDOs to force banks to correct their unsafe or unsound banking practices. EDO issuance is publicly announced by regulators and often also gains media coverage. Such publicity can lead to substantial reputational damage and subsequently diminished public confidence for banks receiving EDOs. Consistent with this, survey data shows that EDOs are followed by county-level declines in trust in banks and bankers.

While prior literature has documented depositor responses to declining trust, borrowers are also likely to respond to a decline in trust in banks. Typically, higher-quality borrowers have more options and are more likely to exit an EDO bank. Consistent with this, we find that loans originated by EDO banks while the EDO is active become delinquent sooner than loans originated by control banks. Importantly, loan quality is not significantly different for loans issued by EDO banks prior to or after the EDO compared to loans issued by control banks, suggesting that the effect we document indeed coincides with enforcement actions. We also assess borrower characteristics at loan issuance and find that borrowers of loans originated while a bank is under enforcement have worse credit scores and histories.

We hypothesize that bank reputation and the resultant effect on borrowers' trust in banks drives the decline in loan quality during EDOs. Diminished reputation due to enforcement actions adversely affects borrowers' trust in enforced banks, making them less likely to transact with enforced banks. To provide evidence on the reputational channel, we utilize news deserts, areas where poor-quality local information leads to reduced awareness of local events. In news deserts, we argue that EDOs are less likely to adversely affect bank reputation and borrowers' trust in banks. Consistent with the trust mechanism, we find no significant differences in loan quality between EDO and non-EDO banks in news deserts. Our results are robust to multiple sensitivity tests which allay concerns that other factors correlated with news deserts might be driving our findings. We also find similar results when utilizing alternative measures of poor quality local information, such as newspaper mergers and closures and the number of newspaper publishers in a county. Also supporting the trust mechanism, the decline in borrower quality is most pronounced in the initial year of EDO issuance when the most media coverage occurs, in counties with financially sophisticated borrowers who are more likely to consume and understand information about bank enforcement, and for more severe EDOs that attract greater negative attention.

We also address the potential alternative that supply-side bank decisions drive the decline in borrower quality during EDOs. For example, the alternative that EDO banks may have loosened lending standards and expanded credit to lower-quality borrowers. On the contrary, we find that EDO banks contracted lending during enforcement actions. Additionally, we find no material changes in interest rate or loan size for EDO banks' loans relative to control banks' loans, which further contradicts the supply-side explanation for our results. Our work extends the literature on consumer confidence in banks and its effect on consumers' financial choices by examining how bank reputation and trust affects borrowers' decisions.

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## Appendix A. Variable definitions

| **Variable** | **Definition** | **Source** |
|---|---|---|
| **Dependent Variables** | | |
| *Bankruptcy* | Indicator variable equal to 1 if the borrower has ever had a tradeline bankruptcy, lagged by one month. | TransUnion |
| *Collections* | Indicator variable equal to 1 if the borrower has ever had a loan in collections, lagged by one month. | TransUnion |
| *Credit Score* | Natural log of the VantageScore 3.0 of the borrower (scale of 300 to 850), lagged by one month and winsorized at the 1st and 99th percentile by loan origination year. (Also a control variable.) | TransUnion |
| *Defaults* | Indicator variable equal to 1 if the borrower has ever had a loan more than 90 days past due, lagged by one month. | TransUnion |
| *Interest Rate* | Annual Percentage Rate (APR) charged on the loan, imputed from loan amount, loan length, and monthly payment amount, winsorized at the 1st and 99th percentile by loan origination year. | Calculation |
| *Loan Amount* | Natural log of the total amount of auto loans issued by a bank in a county-year, winsorized at the 1st and 99th percentile by loan origination year. | TransUnion |
| *Loan Amount Share* | The share of auto loans issued by a bank in a county-year in terms of loan amount. | TransUnion |
| *Loan Count* | Natural log of the total number of auto loans issued by a bank in a county-year, winsorized at the 1st and 99th percentile by loan origination year. | TransUnion |
| *Loan Count Share* | The share of auto loans issued by a bank in a county-year in terms of loan count. | TransUnion |
| *Loan Length* | Natural logarithm of the number of months of the initial loan, winsorized at the 1st and 99th percentile by loan origination year. | TransUnion |
| *Loan Size* | Natural logarithm of the highest amount ever owed on the loan (initial loan amount), winsorized at the 1st and 99th percentile by loan origination year. | TransUnion |
| *Past Due (12mo)* | Natural log of the borrower's total past due amount in the past 12 months, lagged by one month and winsorized at the 1st and 99th percentile by loan origination year. | TransUnion |

| | | |
|---|---|---|
| *Time to Delinquency* | Number of months until the manner of payment for the loan is no longer "paid or paying as agreed". Calculated implicitly in the survival model from a variable indicating first month of delinquency. | TransUnion |
| *Trust (Bankers)* | Indicator variable equal to 1 if the average response in a county-year to the question "How much do you trust bankers?" is 4 or higher on a 1-5 scale, where 1 means "I do not trust them at all" and 5 means "I trust them completely." | Chicago Booth/Kellogg School Financial Trust Index |
| *Trust (Banks)* | Indicator variable equal to 1 if the average response in a county-year to the question "How much do you trust banks?" is 4 or higher on a 1-5 scale, where 1 means "I do not trust them at all" and 5 means "I trust them completely." | Chicago Booth/Kellogg School Financial Trust Index |
| *% Delinquent* | Percent of the borrower's loans ever delinquent, lagged by one month. | TransUnion |
| **Independent Variables** | | |
| *EDO* | Indicator variable equal to 1 if loan is originated by an EDO bank. | SNL |
| *Exposure to EDO* | Indicator for the top tercile of the amount of loans issued by EDO banks while an EDO is open divided by the total amount of loans in a given county-year. | TransUnion and SNL |
| *During EDO* | Indicator variable equal to 1 if loan is originated while the lender experiences an EDO. | SNL |
| *During EDO 1* | Indicator variable equal to 1 if loan is originated in the first year in which the lender experiences an EDO. | SNL |
| *During EDO 1+* | Indicator variable equal to 1 if loan is originated in any year following the first year of EDO while the EDO is still in effect. | SNL |
| *Post EDO* | Indicator variable equal to 1 if loan is originated in the 5 years after the lender experiences an EDO. | SNL |
| *Pre EDO* | Indicator variable equal to 1 if loan is originated in the 3 years before the lender experiences an EDO. | SNL |
| **Bank Controls** | | |
| *Capital Ratio* | Total equity divided by total assets for the lender, lagged by one year and expressed in deciles (1 to 10). | Call Reports |
| *Liquidity* | Cash and cash equivalents divided by total assets for the lender, where cash is defined as the sum of interest-bearing balances, noninterest-bearing balances, and currency and coin, lagged by one year and expressed in deciles (1 to 10). | Call Reports |

| | | |
|---|---|---|
| *NPA* | Accruing and nonaccruing loans in the past 90 days divided by net total loans for the lender, lagged by one year and expressed in deciles (1 to 10). | Call Reports |
| *ROA* | Net income divided by total assets for the lender, lagged by one year and expressed in deciles (1 to 10). | Call Reports |
| *Size* | Natural logarithm of the lender's total assets, lagged by one year and expressed in deciles (1 to 10). | Call Reports |
| **Economic Controls** | | |
| *PC Income Growth* | One-year growth in per-capita income at the census-tract level, lagged by one year and winsorized at the 1st and 99th percentile by loan origination year. | ACS |
| *Unemployment Rate* | Unemployment rate at the census-tract level, lagged by one year and winsorized at the 1st and 99th percentile by loan origination year. | ACS |
| **Additional Economic Controls** | | |
| *Median Age* | Natural logarithm of median age at the census-tract level, lagged by one year and winsorized at the 1st and 99th percentile by loan origination year. | ACS |
| *PC Income* | Natural logarithm of per-capita income at the census-tract level, lagged by one year and winsorized at the 1st and 99th percentile by loan origination year. | ACS |
| *Population* | Natural logarithm of population at the census-tract level, lagged by one year and winsorized at the 1st and 99th percentile by loan origination year. | ACS |
| *Urbanization* | Urban population divided by total population at the census tract level in 2010. | 2010 Census |
| **Other Controls** | | |
| *Borrower Age* | Natural logarithm of the borrower's age at loan origination, winsorized at the 1st and 99th percentile by loan origination year. | TransUnion |
| **Cross-Sectional Variables** | | |
| *High Education (College)* | Indicator variable equal to 1 if borrower resides in a census tract which is in the top tercile of average college graduation in the US one year before the loan origination. | ACS |
| *High Education (High School)* | Indicator variable equal to 1 if borrower resides in a census tract which is in the top tercile of average high school graduation in the US one year before the loan origination. | ACS |

| | | |
|---|---|---|
| *High Education (Postbacc.)* | Indicator variable equal to 1 if borrower resides in a census tract which is in the top tercile of average postbaccalaureate graduation in the US. | ACS |
| *Long EDO* | Indicator variable equal to 1 if an EDO is in effect for four years or longer. | SNL |
| *Low Establishment Count* | Indicator variable equal to 1 if borrower resides in a county which is in the bottom tercile of the number of newspaper publishing establishments in the US one year before the loan origination. | BLS-QCWE |
| *Low Income (Household)* | Indicator variable equal to 1 if borrower resides in a census tract which is in the bottom tercile of median household income one year before the loan origination. | ACS |
| *Low Income (Individual)* | Indicator variable equal to 1 if borrower resides in a census tract which is in the bottom tercile of median individual income one year before the loan origination. | ACS |
| *News Desert* | Indicator variable equal to 1 if borrower resides in a county without a local newspaper in 2004. | UNC |
| *Post Newspaper Closure* | Indicator variable equal to 1 if an EDO is issued after a county experiences a newspaper closure and/or merger. | UNC |

## Appendix B. Local news coverage examples

*Example 1. FDIC Enforcement Action on Peoples Bank*

> **Peoples Bank enters consent order with Federal Deposit Insurance Corp.**
> **The Times of Northwest Indiana – January 2, 2024**
>
> Peoples Bank agreed to a consent order with the Federal Deposit Insurance Corp. and the Indiana Department of Financial Institutions that alleges "unsafe and unsound banking practices" with regard to the Bank Security Act, a federal law that requires banks to report suspicious transactions that could be signs of money laundering.
>
> Munster-based Finward Bancorp, the parent company of Peoples Bank, entered into the consent order without "admitting or denying the charges of unsafe or unsound banking practices and violations of law or regulation relating to the Bank Secrecy Act," according to the FDIC's order.
>
> "Following a routine regulatory exam in March, Peoples Bank was cited for a violation related to BSA guidelines," Peoples Bank Senior Vice President and Director of Marketing Sarah Ricciardi said. "The violation was identified by the bank during an audit prior to the examination. Because we were able to identify these issues, the bank proactively began correcting them and began implementing new systems and processes to prevent future reporting errors." (...)

*Example 2. OCC Enforcement Action on Three Texan Banks*

> **Three subsidiary banks of Texas-based Industry Bancshares fighting allegations leveled by regulator**
> **San Antonio Express-News – January 22, 2024**
>
> Three subsidiary banks of Texas-based Industry Bancshares Inc. have been declared in "troubled condition" over "unsafe or unsound banking practices" by one of their regulators.
>
> The Office of the Comptroller of the Currency recently issued notices of charges seeking orders to cease and desist from those practices to the three national banks — First National Bank of Shiner, First National Bank of Bellville and Bank of Brenham.
>
> The banks have reported significant negative net worth as a result of huge losses in their bond portfolios. Industry Bancshares told its shareholders it's going to fight the OCC's charges before an administrative law judge. "We look forward to our day in court," a company spokesperson said in a statement. (...)

*Example 3. Federal Reserve Enforcement Action on Nano Banc*

**Nano Banc Gets Order from Fed Reserve**
**Orange County Business Journal – January 18, 2022**

Irvine-based Nano Banc, a fast-growing bank started in 2018, received an order from the Federal Reserve to hire a new chief executive, chief financial officer and chief credit officer by the end of the month.

"Such executive officers shall have the qualifications and experience necessary to fulfill their duties and responsibilities, restore the Bank to a safe and sound condition, comply with applicable laws and regulations, and comply with the provisions of this Order," the Federal Reserve said.

It also required the bank to achieve the minimum number of directors required under applicable state law, with a majority being outside directors. It gave a deadline of 10 days after the order, which was dated Jan. 18. (...)

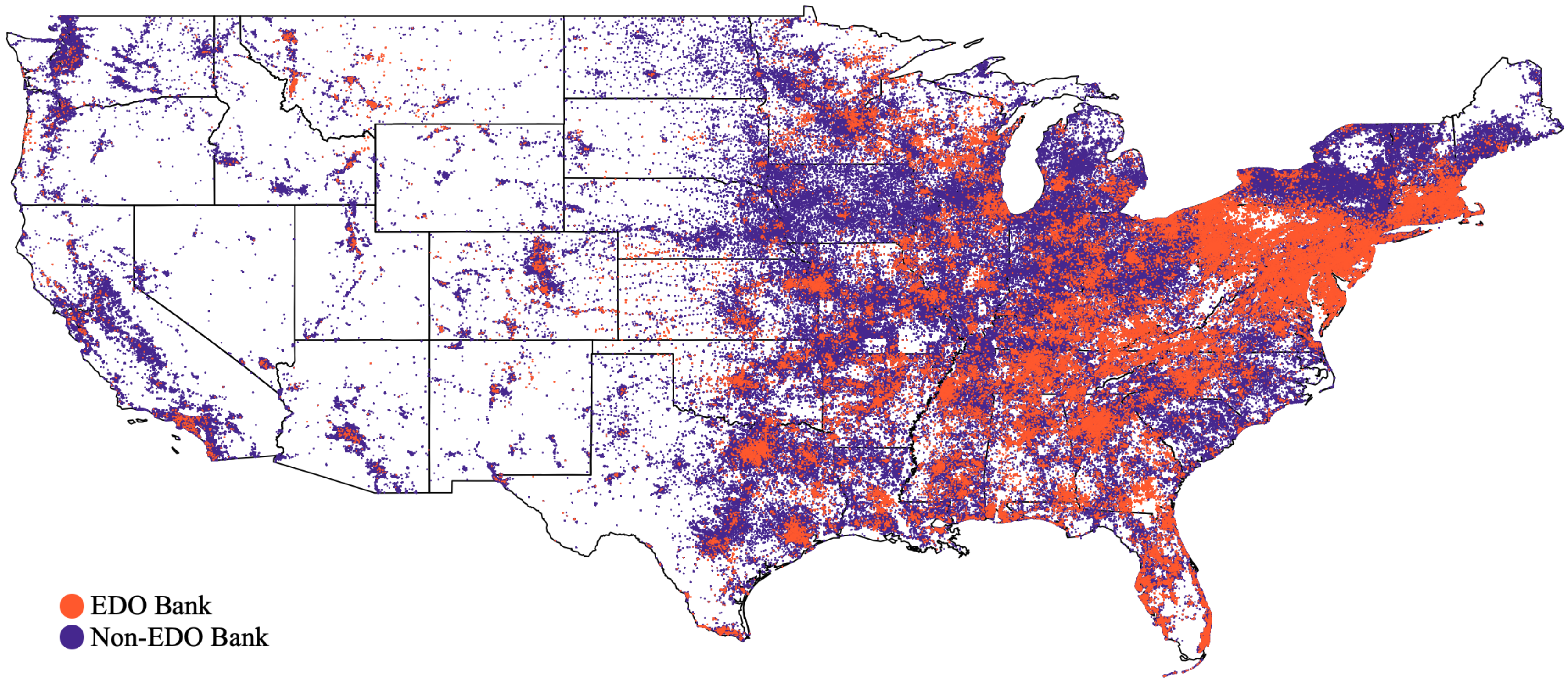


Figure 1: Geographic distribution of EDO and non-EDO auto loans

This figure shows the geographic distribution of auto loans by the lending bank's EDO status across the contiguous United States. When overlapping, EDO loans are plotted over non-EDO loans.

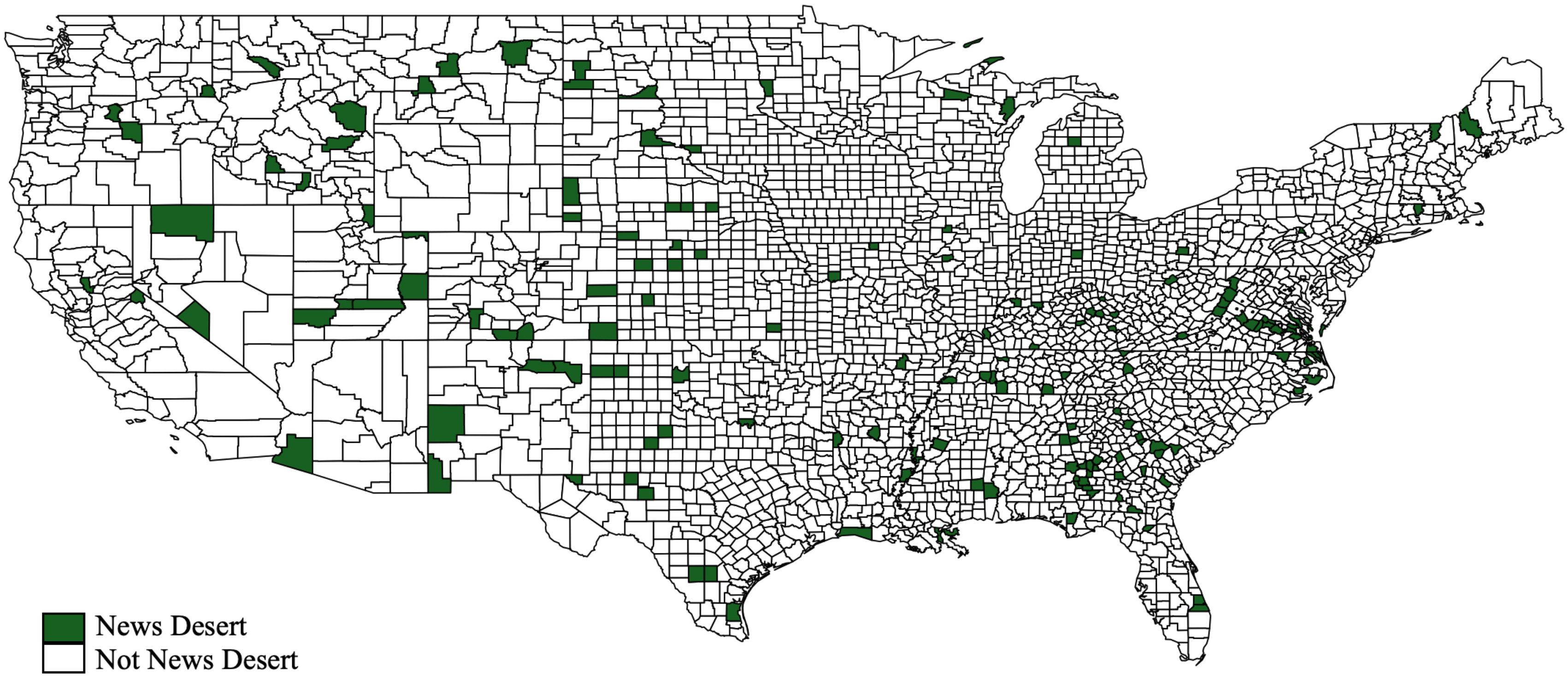


Figure 2: Geographic distribution of news deserts

This figure shows the geographic distribution of news deserts across the contiguous United States.

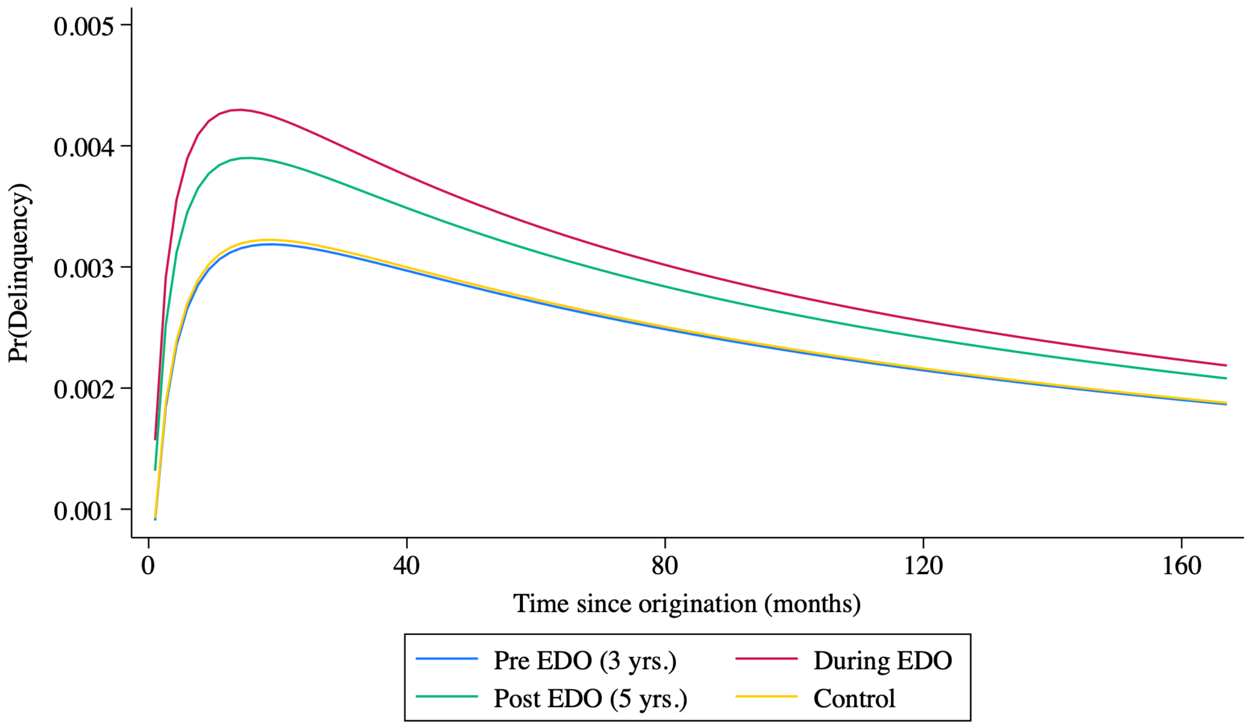


Figure 3: Time to delinquency by *EDO Cohort*

This figure shows the change in probability of delinquency over the life of auto loans by *EDO Cohort*. We estimate the model from Table 2 and graph the average probability of delinquency of loans over months of age for each aggregate cohort. All variables are defined in Appendix A.

Table 1: Descriptive statistics

This table presents the summary statistics for the variables we use in our analyses. These are presented at the *loan level* for ease of interpretation. All variables are defined in Appendix A.

| Variable | Obs | Mean | Std. Dev. | P1 | P25 | Median | P75 | P99 |
|---|---|---|---|---|---|---|---|---|
| ***Dependent Variable (Lognormal AFT Model)*** | | | | | | | | |
| *Time to Delinquency* | 153,752 | 20.068 | 15.384 | 3.000 | 8.000 | 16.000 | 28.000 | 69.000 |
| ***Dependent Variables (OLS Model)*** | | | | | | | | |
| *Delinquent* | 1,788,436 | 0.086 | 0.280 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *Credit Score* | 1,788,436 | 6.539 | 0.137 | 6.146 | 6.469 | 6.557 | 6.639 | 6.719 |
| *Collections* | 1,788,436 | 0.194 | 0.396 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *Defaults* | 1,788,436 | 0.266 | 0.442 | 0.000 | 0.000 | 0.000 | 1.000 | 1.000 |
| *Bankruptcy* | 1,788,436 | 0.035 | 0.184 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *% Delinquent* | 1,788,436 | 9.995 | 17.225 | 0.000 | 0.000 | 0.000 | 13.000 | 80.000 |
| *Past Due (12mo)* | 1,788,436 | 0.582 | 1.970 | 0.000 | 0.000 | 0.000 | 0.000 | 9.016 |
| *Loan Amount* | 246,274 | 10.576 | 1.465 | 7.601 | 9.569 | 10.428 | 11.498 | 14.432 |
| *Loan Count* | 246,274 | 1.036 | 1.186 | 0.000 | 0.000 | 0.693 | 1.792 | 4.489 |
| *Loan Amount Share* | 246,274 | 14.613 | 20.752 | 0.047 | 1.409 | 5.571 | 18.725 | 100.000 |
| *Loan Count Share* | 246,274 | 14.613 | 20.157 | 0.126 | 1.887 | 5.882 | 18.478 | 100.000 |
| *Interest Rate* | 1,693,426 | 0.063 | 0.034 | 0.017 | 0.038 | 0.055 | 0.080 | 0.180 |
| *Loan Size* | 1,787,975 | 9.593 | 0.781 | 7.378 | 9.146 | 9.723 | 10.157 | 10.948 |
| *Loan Length* | 1,781,988 | 3.936 | 0.483 | 2.079 | 3.871 | 4.094 | 4.277 | 4.431 |
| ***Independent Variables*** | | | | | | | | |
| *EDO* | 1,788,436 | 0.075 | 0.264 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *Pre EDO* | 1,788,436 | 0.014 | 0.119 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *During EDO* | 1,788,436 | 0.027 | 0.162 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *During EDO 1* | 1,788,436 | 0.006 | 0.077 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| *During EDO 1+* | 1,788,436 | 0.021 | 0.143 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *Post EDO* | 1,788,436 | 0.030 | 0.170 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| ***Bank Controls*** | | | | | | | | |
| *Size* | 1,788,436 | 7.707 | 2.265 | 0.000 | 7.000 | 9.000 | 9.000 | 9.000 |
| *ROA* | 1,788,436 | 4.885 | 2.615 | 0.000 | 3.000 | 5.000 | 7.000 | 9.000 |
| *Liquidity* | 1,788,436 | 4.111 | 3.083 | 0.000 | 1.000 | 4.000 | 7.000 | 9.000 |
| *NPA* | 1,788,436 | 4.975 | 2.145 | 0.000 | 3.000 | 6.000 | 7.000 | 9.000 |
| *Capital Ratio* | 1,788,436 | 3.896 | 2.701 | 0.000 | 1.000 | 4.000 | 6.000 | 9.000 |
| ***Economic Controls*** | | | | | | | | |
| *PC Income Growth* | 1,788,436 | 0.022 | 0.061 | -0.124 | -0.016 | 0.020 | 0.057 | 0.191 |
| *Unemployment Rate* | 1,788,436 | 0.070 | 0.040 | 0.010 | 0.041 | 0.063 | 0.091 | 0.200 |
| ***Additional Economic Controls*** | | | | | | | | |
| *Population* | 1,788,436 | 8.450 | 0.432 | 7.352 | 8.176 | 8.470 | 8.737 | 9.513 |
| *PC Income* | 1,788,436 | 10.399 | 0.338 | 9.653 | 10.172 | 10.372 | 10.604 | 11.307 |
| *Median Age* | 1,788,436 | 3.684 | 0.167 | 3.170 | 3.586 | 3.706 | 3.800 | 4.027 |
| *Urbanization* | 1,788,414 | 0.631 | 0.422 | 0.000 | 0.080 | 0.880 | 1.000 | 1.000 |
| ***Other Controls*** | | | | | | | | |
| *Borrower Age* | 1,788,436 | 3.766 | 0.343 | 3.045 | 3.497 | 3.807 | 4.025 | 4.382 |
| ***Cross-Sectional Variables*** | | | | | | | | |
| *News Desert* | 1,788,436 | 0.016 | 0.127 | 0.000 | 0.000 | 0.000 | 0.000 | 1.000 |
| *High Education* (High School) | 1,788,434 | 0.334 | 0.472 | 0.000 | 0.000 | 0.000 | 1.000 | 1.000 |
| *High Education* (College) | 1,788,434 | 0.272 | 0.445 | 0.000 | 0.000 | 0.000 | 1.000 | 1.000 |
| *High Education* (Postbacc.) | 1,788,434 | 0.275 | 0.447 | 0.000 | 0.000 | 0.000 | 1.000 | 1.000 |
| *Low Income* (Individual) | 1,788,436 | 0.284 | 0.451 | 0.000 | 0.000 | 0.000 | 1.000 | 1.000 |
| *Low Income* (Household) | 1,788,301 | 0.282 | 0.450 | 0.000 | 0.000 | 0.000 | 1.000 | 1.000 |

Table 2: Time to delinquency for EDO banks

This table shows the effect of EDOs on auto loan delinquencies. Columns (1)–(2) are estimated using a lognormal survival (AFT) model. Column (1) is estimated using the full sample, while Column (2) is estimated using the entropy balanced sample. The dependent variable is *Time to Delinquency.* All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* | |
|---|---|---|
| | Full Sample | Entropy Balanced |
| | (1) | (2) |
| *EDO* | 0.032 | 0.202 |
| | (0.193) | (1.283) |
| *Pre EDO* | -0.017 | -0.091 |
| | (-0.170) | (-1.206) |
| *During EDO* | -0.324* | -0.309* |
| | (-1.702) | (-1.785) |
| *Post EDO* | -0.236 | -0.300 |
| | (-1.154) | (-1.507) |
| *Per Capita Income Growth* | -0.198*** | -0.105 |
| | (-4.280) | (-1.146) |
| *Unemployment Rate* | -4.130*** | -3.296*** |
| | (-9.977) | (-10.784) |
| *Size* | 0.136*** | 0.102*** |
| | (7.814) | (6.820) |
| *ROA* | -0.000 | -0.015 |
| | (-0.029) | (-1.108) |
| *Liquidity* | 0.039** | 0.004 |
| | (2.383) | (0.247) |
| *NPA* | -0.016 | -0.036*** |
| | (-0.908) | (-2.586) |
| *Capital Ratio* | -0.047*** | -0.027* |
| | (-2.784) | (-1.698) |
| Observations | 47,247,006 | 47,247,006 |
| Wald $\chi^2$ | 18012*** | 17046*** |
| Year-Month FE | Yes | Yes |
| Strata | State | State |
| Model | AFT | AFT |

Table 3: Borrower composition during EDOs

This table shows changes in borrower characteristics over the various stages of an EDO, utilizing the entropy-balanced sample. All columns include year-month, bank, and county fixed effects. The dependent variables are various elements of borrower credit quality and history. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $t$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Credit Score* | *Collections* | *% Delinquent* | *Past Due (12mo)* | *Defaults* | *Bankruptcy* |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Pre EDO* | -0.002 | 0.016 | 0.115 | 0.036 | 0.009 | 0.003 |
| | (-0.555) | (0.889) | (0.187) | (0.676) | (0.543) | (0.827) |
| *During EDO* | -0.008** | 0.035** | 0.938* | 0.100** | 0.030** | 0.009*** |
| | (-2.080) | (2.122) | (1.793) | (1.998) | (2.112) | (2.758) |
| *Post EDO* | -0.002 | 0.026 | 0.484 | 0.041 | 0.017 | 0.009* |
| | (-0.403) | (1.465) | (0.798) | (0.750) | (1.070) | (1.820) |
| *Borrower Age* | 0.100*** | -0.030*** | -2.532*** | -0.003 | 0.024*** | 0.032*** |
| | (24.292) | (-4.350) | (-5.838) | (-0.124) | (3.416) | (15.678) |
| Observations | 1,788,295 | 1,788,295 | 1,788,295 | 1,788,295 | 1,788,295 | 1,788,295 |
| Adjusted $R^2$ | 0.216 | 0.147 | 0.110 | 0.105 | 0.090 | 0.035 |
| Bank controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Lender FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Model | OLS | OLS | OLS | OLS | OLS | OLS |

Table 4: Local news environment

This table shows the effect of EDOs on auto loan delinquencies, differentiating counties by whether they are news deserts. Columns (1)–(3) are estimated using a lognormal survival (AFT) model and include increasingly stringent control variables and fixed-effects structures. This analysis utilizes the entropy-balanced sample. All standard errors are clustered at the bank level. The dependent variable is *Time to Delinquency*. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| *EDO* | 0.204 | 0.191 | 0.190 |
| | (1.293) | (1.198) | (1.193) |
| *Pre EDO* | -0.096 | -0.084 | -0.083 |
| | (-1.247) | (-1.121) | (-1.115) |
| *During EDO* | -0.320* | -0.298* | -0.297* |
| | (-1.853) | (-1.697) | (-1.694) |
| *Post EDO* | -0.310 | -0.291 | -0.290 |
| | (-1.555) | (-1.422) | (-1.420) |
| *News Desert* | -0.133 | -0.113 | -3.463* |
| | (-1.478) | (-1.264) | (-1.654) |
| *EDO* × *News Desert* | -0.106 | -0.137 | -0.121 |
| | (-0.465) | (-0.606) | (-0.503) |
| *Pre EDO* × *News Desert* | 0.212 | 0.224 | 0.189 |
| | (0.779) | (0.835) | (0.712) |
| *During EDO* × *News Desert* | 0.616** | 0.637*** | 0.592** |
| | (2.441) | (2.592) | (2.344) |
| *Post EDO* × *News Desert* | 0.461* | 0.459* | 0.411 |
| | (1.728) | (1.720) | (1.506) |
| *During EDO* + *During EDO* × *News Desert* | 0.297 | 0.339 | 0.295 |
| | (1.316) | (1.601) | (1.382) |
| Observations | 47,247,006 | 45,439,185 | 45,439,185 |
| Wald $\chi^2$ | 17434*** | 18268*** | 18516*** |
| Bank controls | Yes | Yes | Yes |
| Economic controls | Yes | Yes | Yes |
| Additional Economic controls | No | Yes | Yes |
| News Desert × All Economic controls | No | No | Yes |
| Year-Month FE | Yes | Yes | Yes |
| Strata | State | State | State |
| Model | AFT | AFT | AFT |

Table 5: Local news environment and financial distress

This table shows the effect of EDOs on auto loan delinquencies, differentiating counties by whether they are news deserts and controlling for low income census tracts. Columns (1)–(4) columns are estimated using a lognormal survival (AFT) model. Columns (1)–(2) use average individual income as the interacted control variable and include increasingly stringent control variables and fixed-effect/stratification structures. Columns (3)–(4) use average household income as the interacted control variable and include increasingly stringent control variables and fixed-effect/stratification structures. All standard errors are clustered at the bank level. The dependent variable is *Time to Delinquency*. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* | | | |
|---|---|---|---|---|
| | Income: Individual | | Income: Household | |
| | (1) | (2) | (3) | (4) |
| *EDO* | 0.167 | 0.165 | 0.166 | 0.162 |
| | (0.866) | (0.854) | (0.878) | (0.853) |
| *Pre EDO* | -0.062 | -0.059 | -0.073 | -0.070 |
| | (-0.672) | (-0.641) | (-0.789) | (-0.756) |
| *During EDO* | -0.332* | -0.327 | -0.337* | -0.329* |
| | (-1.693) | (-1.639) | (-1.751) | (-1.691) |
| *Post EDO* | -0.286 | -0.292 | -0.301 | -0.305 |
| | (-1.212) | (-1.199) | (-1.267) | (-1.251) |
| *News Desert* | -0.110 | -3.216 | -0.105 | -3.155 |
| | (-1.278) | (-1.606) | (-1.232) | (-1.576) |
| *Low Income* | -0.246*** | -0.087** | -0.275*** | -0.127*** |
| | (-5.798) | (-2.257) | (-7.426) | (-4.082) |
| *EDO* × *News Desert* | -0.177 | -0.179 | -0.164 | -0.175 |
| | (-0.748) | (-0.700) | (-0.703) | (-0.698) |
| *Pre EDO* × *News Desert* | 0.233 | 0.207 | 0.214 | 0.199 |
| | (0.844) | (0.741) | (0.792) | (0.726) |
| *During EDO* × *News Desert* | 0.661** | 0.630** | 0.642** | 0.625** |
| | (2.513) | (2.309) | (2.467) | (2.344) |
| *Post EDO* × *News Desert* | 0.520* | 0.473* | 0.505* | 0.462* |
| | (1.894) | (1.684) | (1.889) | (1.682) |
| *EDO* × *Low Income* | 0.044 | 0.045 | 0.050 | 0.053 |
| | (0.337) | (0.351) | (0.408) | (0.437) |
| *Pre EDO* × *Low Income* | -0.036 | -0.038 | -0.011 | -0.007 |
| | (-0.341) | (-0.342) | (-0.081) | (-0.050) |
| *During EDO* × *Low Income* | 0.113 | 0.123 | 0.122 | 0.130 |
| | (1.077) | (1.094) | (1.222) | (1.265) |
| *Post EDO* × *Low Income* | 0.009 | 0.026 | 0.045 | 0.061 |
| | (0.059) | (0.166) | (0.308) | (0.398) |
| *During EDO* + *During EDO* × *News Desert* | 0.329 | 0.304 | 0.305 | 0.296 |
| | (1.428) | (1.351) | (1.346) | (1.340) |
| Observations | 46,884,698 | 45,077,491 | 46,881,022 | 45,074,043 |
| Wald $\chi^2$ | 18167*** | 18930*** | 18220*** | 19147*** |
| Bank controls | Yes | Yes | Yes | Yes |
| Economic controls | Yes | Yes | Yes | Yes |
| Additional Economic controls | No | Yes | No | Yes |
| News Desert × All Economic controls | No | Yes | No | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes |
| Strata | State | State | State | State |
| Model | AFT | AFT | AFT | AFT |

Table 6: Local newspaper closures and mergers

This table shows the effect of EDOs on auto loan delinquencies exploiting the staggered closures and mergers of local newspapers across counties. Columns (1)-(3) estimated using a lognormal survival (AFT) model and include increasingly stringent control variables. The dependent variable is *Time to Delinquency*. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; ${}^{*}p < 0.1$; ${}^{**}p < 0.05$; ${}^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| *EDO* | 0.353 | 0.429** | 0.432** |
| | (1.606) | (1.985) | (1.997) |
| *Pre EDO* | -0.037 | -0.058 | -0.057 |
| | (-0.329) | (-0.565) | (-0.552) |
| *During EDO* | -1.059*** | -1.085*** | -1.085*** |
| | (-7.617) | (-7.527) | (-7.533) |
| *Post EDO* | -0.513 | -0.593 | -0.596 |
| | (-1.417) | (-1.539) | (-1.546) |
| *Post Newspaper Closure* | -0.432 | -0.526* | 2.619 |
| | (-1.521) | (-1.829) | (0.629) |
| *Pre EDO* × *Post Newspaper Closure* | 0.249 | 0.241 | 0.272 |
| | (0.977) | (0.960) | (1.174) |
| *During EDO* × *Post Newspaper Closure* | 0.915*** | 0.972*** | 0.914** |
| | (3.128) | (3.032) | (2.438) |
| *Post EDO* × *Post Newspaper Closure* | 0.429 | 0.635 | 0.610 |
| | (1.123) | (1.505) | (1.430) |
| *During EDO* + *During EDO* × *Post Newspaper Closure* | -0.144 | -0.113 | -0.171 |
| | (-0.482) | (-0.358) | (-0.470) |
| Observations | 3,157,016 | 3,016,546 | 3,016,546 |
| Bank controls | Yes | Yes | Yes |
| Economic controls | Yes | Yes | Yes |
| Additional Economic controls | No | Yes | Yes |
| *Post Newspaper Closure* × All Economic controls | No | No | Yes |
| Year-Month FE | Yes | Yes | Yes |
| Strata | County | County | County |
| Model | AFT | AFT | AFT |

Table 7: Validating EDOs as a measure of trust

This table relates EDOs to survey-based measures of trust in banks (*Trust (Banks)*) and bankers (*Trust (Bankers)*) from the Chicago Booth/Kellogg School Financial Trust Index database. In Panel A, *Exposure to EDO* is a county-year level measure of exposure to EDO banks based in loans. The economic controls include unemployment rate and yearly percent growth in per capita income. Panel B shows changes in trust in the *During EDO* period relative to *Pre EDO*. All standard errors are clustered at the county level. All variables are defined in Appendix A. The *t*-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

Panel A: Exposure to EDOs and trust in banks

| | *Trust (Banks)* | | *Trust (Bankers)* | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Exposure to EDO* | -0.058*** | -0.057*** | -0.057*** | -0.056*** |
| | (-5.754) | (-5.668) | (-6.441) | (-6.350) |
| Observations | 10,726 | 10,713 | 10,658 | 10,645 |
| Adjusted $R^2$ | 0.033 | 0.033 | 0.027 | 0.026 |
| Economic controls | No | Yes | No | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Model | OLS | OLS | OLS | OLS |

Table 7: Validating EDOs as a measure of trust, continued

Panel B: Changes in county-level trust

| | *Trust (Banks)* | *Trust (Bankers)* |
|---|---|---|
| | (1) | (2) |
| *During EDO* | -0.060*** | -0.036* |
| | (-2.914) | (-1.912) |
| Observations | 2,963 | 2,953 |
| Adjusted $R^2$ | 0.137 | 0.157 |
| EDO FE | Yes | Yes |
| Model | OLS | OLS |

Table 8: Changes in loan amount at the extensive margin

This table shows the effect of EDOs on the amount of loans issued at the extensive margin. This analysis is conducted at the bank-county-year level. The dependent variable is the total amount of loans issued by a bank in a county-year in Columns (1)–(2), the total number of loans issued by a bank in a county-year in Columns (3)–(4), the share of loans issued by a bank in a county-year in terms of loan amount in Columns (5)–(6), and the share of loans issued by a bank in a county-year in terms of loan count in Columns (7)–(8). The odd columns include year, county, and bank FE, while the even columns include county × year and bank FE. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $t$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Loan Amount* | | *Loan Count* | | *Loan Amount Share* | | *Loan Count Share* | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Pre EDO* | -0.018 | -0.066 | -0.064*** | -0.100*** | -3.956* | -2.799** | -3.732*** | -2.427*** |
| | (-0.389) | (-1.189) | (-2.651) | (-3.237) | (-1.951) | (-1.992) | (-2.776) | (-2.933) |
| *During EDO* | -0.132** | -0.202*** | -0.128*** | -0.180*** | -6.463*** | -5.589*** | -5.979*** | -4.729*** |
| | (-2.316) | (-3.342) | (-4.173) | (-4.689) | (-4.223) | (-4.522) | (-4.880) | (-4.551) |
| *Post EDO* | -0.106* | -0.166** | -0.120*** | -0.162*** | -5.265*** | -4.450*** | -5.220*** | -4.143*** |
| | (-1.685) | (-2.497) | (-4.123) | (-3.834) | (-3.517) | (-3.223) | (-4.639) | (-3.442) |
| Observations | 245,965 | 243,376 | 245,965 | 243,376 | 245,965 | 243,376 | 245,965 | 243,376 |
| Adjusted $R^2$ | 0.248 | 0.208 | 0.177 | 0.117 | 0.340 | 0.241 | 0.342 | 0.241 |
| Bank controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic controls | Yes | No | Yes | No | Yes | No | Yes | No |
| Year FE | Yes | No | Yes | No | Yes | No | Yes | No |
| Bank FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | No | Yes | No | Yes | No | Yes | No |
| Year × County FE | No | Yes | No | Yes | No | Yes | No | Yes |
| Model | OLS | OLS | OLS | OLS | OLS | OLS | Bank | Bank |

Table 9: Loan terms

This table shows the effect of EDOs on the terms of the auto loans. Columns (1)–(6) are estimated using an OLS model. The dependent variables are interest rate, loan amount, and loan length. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $t$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Interest Rate* | | *Loan Size* | | *Loan Length* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Pre EDO* | -0.002 | -0.001 | 0.020 | 0.022 | -0.001 | -0.005 |
| | (-1.334) | (-0.588) | (1.349) | (1.326) | (-0.058) | (-0.537) |
| *During EDO* | -0.002 | -0.002 | -0.004 | -0.016 | -0.017* | -0.029*** |
| | (-1.094) | (-1.428) | (-0.281) | (-1.047) | (-1.784) | (-2.804) |
| *Post EDO* | -0.002 | -0.002 | 0.017 | -0.003 | -0.014 | -0.023* |
| | (-1.088) | (-1.293) | (0.846) | (-0.152) | (-1.128) | (-1.677) |
| *Borrower Age* | 0.000 | 0.000 | -0.044** | -0.029 | -0.065*** | -0.061*** |
| | (0.503) | (0.059) | (-2.289) | (-1.399) | (-6.473) | (-6.536) |
| *Credit Score* | -0.075*** | -0.077*** | 0.470*** | 0.443*** | 0.157*** | 0.135** |
| | (-11.340) | (-10.185) | (3.971) | (3.680) | (2.713) | (2.396) |
| Observations | 1,693,285 | 1,623,770 | 1,693,285 | 1,623,770 | 1,693,285 | 1,623,770 |
| Adjusted $R^2$ | 0.424 | 0.513 | 0.352 | 0.458 | 0.485 | 0.573 |
| Bank controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Bank FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | No | Yes | No | Yes | No |
| Year-Month FE | Yes | No | Yes | No | Yes | No |
| County × Year-Month FE | No | Yes | No | Yes | No | Yes |
| Model | OLS | OLS | OLS | OLS | OLS | OLS |

Table 10: Effect of EDOs in the year of issuance

This table shows the effect of EDOs on auto loan delinquencies in the year of issuance. Column (1) is estimated using the entropy balanced sample. *During EDO 1* is an indicator which switches on in the year of EDO issuance, and *During EDO 1+* is an indicator which switches on in all subsequent years while an EDO is in effect. All other variables are defined in Appendix A. All standard errors are clustered at the bank level. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* |
|---|---|
| | (1) |
| *EDO* | 0.202 |
| | (1.264) |
| *Pre EDO* | -0.091 |
| | (-1.205) |
| *During EDO 1* | -0.316*** |
| | (-2.683) |
| *During EDO 1+* | -0.307 |
| | (-1.523) |
| *Post EDO* | -0.300 |
| | (-1.469) |
| *PC Income Growth* | -0.105 |
| | (-1.150) |
| *Unemployment Rate* | -3.297*** |
| | (-10.882) |
| *Size* | 0.102*** |
| | (6.812) |
| *ROA* | -0.015 |
| | (-1.101) |
| *Liquidity* | 0.004 |
| | (0.240) |
| *NPA* | -0.036*** |
| | (-2.601) |
| *Capital Ratio* | -0.027* |
| | (-1.700) |
| Observations | 47,247,006 |
| Wald $\chi^2$ | 17246*** |
| Year-Month FE | Yes |
| Strata | State |
| Model | AFT |

Table 11: Borrower sophistication

This table shows the effect of EDOs on auto loan delinquencies, differentiating counties by the average education level of their residents. Columns (1)–(3) are estimated using a lognormal survival (AFT) model. Column (1) uses average high school graduate rate as the cross-sectional variable. Column (2) uses average college graduate rate as the cross-sectional variable. Column (3) uses average postbaccalaureate graduate rate as the cross-sectional variable. All standard errors are clustered at the bank level. The dependent variable is *Time to Delinquency*. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* | | |
|---|---|---|---|
| | Education: High School | Education: College | Education: Postbacc. |
| | (1) | (2) | (3) |
| *EDO* | 0.182 | 0.179 | 0.186 |
| | (1.303) | (1.247) | (1.279) |
| *Pre EDO* | -0.053 | -0.044 | -0.050 |
| | (-0.725) | (-0.615) | (-0.684) |
| *During EDO* | -0.236 | -0.232 | -0.242 |
| | (-1.598) | (-1.489) | (-1.505) |
| *Post EDO* | -0.254 | -0.243 | -0.253 |
| | (-1.414) | (-1.312) | (-1.373) |
| *High Education* | 0.263*** | 0.295*** | 0.270*** |
| | (4.707) | (4.137) | (3.955) |
| *EDO* × *High Education* | 0.039 | 0.045 | 0.024 |
| | (0.314) | (0.356) | (0.233) |
| *Pre EDO* × *High Education* | -0.094 | -0.146* | -0.128* |
| | (-1.014) | (-1.826) | (-1.690) |
| *During EDO* × *High Education* | -0.219** | -0.270*** | -0.225*** |
| | (-2.076) | (-3.015) | (-3.406) |
| *Post EDO* × *High Education* | -0.126 | -0.197 | -0.154 |
| | (-1.013) | (-1.580) | (-1.316) |
| Observations | 46,884,569 | 46,884,569 | 46,884,569 |
| Wald $\chi^2$ | 17348*** | 17518*** | 17483*** |
| Bank controls | Yes | Yes | Yes |
| Economic controls | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes |
| Strata | State | State | State |
| Model | AFT | AFT | AFT |

## Additional Results

Table A1: Incidence of delinquency for EDO banks

This table shows the effect of EDOs on auto loan delinquencies. Columns (1) is estimated using an OLS model. The dependent variable is *delinquent*. This analysis utilizes the entropy balanced sample. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The *t*-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Delinquency* |
|---|---|
| | (1) |
| *Pre EDO* | 0.011 |
| | (1.289) |
| *During EDO* | 0.024** |
| | (2.104) |
| *Post EDO* | 0.012 |
| | (0.933) |
| *PC Income Growth* | -0.010 |
| | (-1.020) |
| *Unemployment Rate* | 0.347*** |
| | (10.221) |
| *Size* | -0.001 |
| | (-0.297) |
| *ROA* | 0.000 |
| | (0.457) |
| *Liquidity* | 0.001 |
| | (1.347) |
| *NPA* | 0.002*** |
| | (2.937) |
| *Capital Ratio* | -0.000 |
| | (-0.539) |
| Observations | 1,764,815 |
| Adjusted $R^2$ | 0.196 |
| Lender FE | Yes |
| County × Year-Month FE | Yes |
| Model | OLS |

Table A2: Local news environment with propensity score matched sample

Panel A shows the results of two sample t-test for the propensity score matched news deserts and control counties. Panel B shows the effect of EDOs on auto loan delinquencies using the propensity score matched sample, differentiating counties by whether they are news deserts. Columns (1)–(2) are estimated using a lognormal survival (AFT) model. Column (1) includes year-month fixed effect, and column (2) additionally includes matched-pair fixed effects. All standard errors are clustered at the bank level. The dependent variable is *Time to Delinquency*. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

Panel A: Two-sample t-tests

| | Non-News Deserts | | News Deserts | | t-test | |
|---|---|---|---|---|---|---|
| | N | Mean | N | Mean | Difference | $p$-value |
| *PC Income Growth* | 1780 | 0.018 | 178 | 0.020 | -0.002 | 0.544 |
| *Unemployment Rate* | 1780 | 0.069 | 178 | 0.069 | 0.000 | 0.986 |
| *Population* | 1780 | 8.930 | 178 | 8.894 | 0.036 | 0.740 |
| *PC Income* | 1780 | 10.150 | 178 | 10.135 | 0.017 | 0.409 |
| *Median Age* | 1780 | 3.680 | 178 | 3.681 | 0.012 | 0.884 |
| *Urbanization* | 1780 | 0.193 | 178 | 0.186 | 0.007 | 0.761 |

Table A2: Local news environment with propensity score matched sample, continued

Panel B: Survival models

| | *Time to Delinquency* | |
|---|---|---|
| | (1) | (2) |
| *EDO* | 0.312* | 0.265* |
| | (1.930) | (1.662) |
| *Pre EDO* | -0.283*** | -0.265*** |
| | (-2.695) | (-2.650) |
| *During EDO* | -0.595*** | -0.556*** |
| | (-3.522) | (-3.255) |
| *Post EDO* | -0.391* | -0.334 |
| | (-1.939) | (-1.618) |
| *News Desert* | -0.073 | 0.043 |
| | (-1.217) | (0.841) |
| *EDO* $\times$ *News Desert* | -0.049 | -0.070 |
| | (-0.213) | (-0.290) |
| *Pre EDO* $\times$ *News Desert* | -0.022 | -0.013 |
| | (-0.116) | (-0.060) |
| *During EDO* $\times$ *News Desert* | 0.487** | 0.401* |
| | (2.370) | (1.855) |
| *Post EDO* $\times$ *News Desert* | 0.198 | 0.213 |
| | (0.707) | (0.771) |
| Observations | 7,142,080 | 7,142,080 |
| Bank controls | Yes | Yes |
| Year-Month FE | Yes | Yes |
| Matched-Pair FE | No | Yes |
| Strata | State | State |
| Model | AFT | AFT |

Table A3: Local news environment: Newspaper publishing establishments

This table presents coefficient estimates from a lognormal survival (AFT) model for changes in auto loan delinquencies by *EDO Cohort*. The variable *Low Establishment Count* is an indicator for a county within the bottom tercile of the number of newspaper publishing establishments. The dependent variable is *Time to Delinquency*. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* | |
|---|---|---|
| | (1) | (2) |
| *EDO* | 0.248 | 0.236 |
| | (1.361) | (1.292) |
| *Pre EDO* | -0.123 | -0.110 |
| | (-1.337) | (-1.221) |
| *During EDO* | -0.399** | -0.378* |
| | (-2.035) | (-1.914) |
| *Post EDO* | -0.403* | -0.389* |
| | (-1.804) | (-1.714) |
| *Low Establishment Count* | -0.070* | -0.131 |
| | (-1.783) | (-0.188) |
| *EDO* × *Low Establishment Count* | -0.249 | -0.235 |
| | (-1.189) | (-1.202) |
| *Pre EDO* × *Low Establishment Count* | 0.159 | 0.135 |
| | (0.924) | (0.851) |
| *During EDO* × *Low Establishment Count* | 0.404* | 0.386** |
| | (1.925) | (1.964) |
| *Post EDO* × *Low Establishment Count* | 0.434** | 0.434** |
| | (2.011) | (2.130) |
| Observations | 45,215,301 | 43,504,183 |
| Wald $\chi^2$ | 17338*** | 19441*** |
| Bank controls | Yes | Yes |
| Economic controls | Yes | Yes |
| Additional Economic controls | No | Yes |
| *Low Establishment Count* × All Economic controls | No | Yes |
| Year-Month FE | Yes | Yes |
| Strata | State | State |
| Model | AFT | AFT |

Table A4: Changes in loan amount at the extensive margin (Bank-year analysis)

This table shows the effect of EDOs on the amount of loans issued at the extensive margin. This analysis is conducted at the bank-year level. The dependent variable is the total amount of loans issued by a bank in a year in Column (1) and the total number of loans issued by a bank in a year in Column (2). Both columns include year and bank FE. All standard errors are clustered at the bank level. All variables are defined in Appendix A. The $t$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Loan Amount* | *Loan Count* |
|---|---|---|
| | (1) | (2) |
| *Pre EDO* | -0.263** | -0.211** |
| | (-2.475) | (-2.334) |
| *During EDO* | -0.561*** | -0.397*** |
| | (-4.739) | (-3.989) |
| *Post EDO* | -0.505*** | -0.374*** |
| | (-4.024) | (-3.613) |
| Observations | 22,561 | 22,561 |
| Adjusted $R^2$ | 0.748 | 0.801 |
| Bank controls | Yes | Yes |
| Year FE | Yes | Yes |
| Bank FE | Yes | Yes |
| Model | OLS | OLS |

Table A5: EDO severity

This table shows the effect of EDOs on auto loan delinquencies, differentiating EDOs based on their severity as proxied by EDO length. Column (1) is estimated using a lognormal survival (AFT) model. All standard errors are clustered at the bank level. The dependent variable is *Time to Delinquency*. All variables are defined in Appendix A. The $z$-statistics are presented in parentheses; $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$ (two-tailed).

| | *Time to Delinquency* |
|---|---|
| | (1) |
| *Pre EDO* | -0.160** |
| | (-2.366) |
| *During EDO* | -0.045 |
| | (-0.549) |
| *Post EDO* | -0.103 |
| | (-0.998) |
| *Long EDO* | 0.871*** |
| | (5.174) |
| *Pre EDO* × *Long EDO* | -0.084 |
| | (-0.915) |
| *During EDO* × *Long EDO* | -0.551*** |
| | (-4.864) |
| *Post EDO* × *Long EDO* | -0.438** |
| | (-2.270) |
| Observations | 4,105,160 |
| Wald $\chi^2$ | 117596*** |
| Bank controls | Yes |
| Economic controls | Yes |
| Year-Month FE | Yes |
| Strata | State |
| Model | AFT |