JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS Vol. 59, No. 5, Aug. 2024, pp. 2099–2132

doi:10.1017/S0022109023000649

Labor Mobility and Loan Origination

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Abstract

We find that mortgage loans originated after the adoption of the inevitable disclosure doctrine (IDD; a mechanism discouraging loan officers' labor mobility) have a lower default probability, a higher loan modification rate, and a lower foreclosure rate. These effects are unaccompanied by any reduction in loan supply and contribute to more stable housing prices. Using the adoption of the Uniform Trade Secrets Act as an alternative identification generates consistent results. Overall, our findings suggest that restricting loan officers' labor mobility leads to better ex ante screening and ex post monitoring, improving the origination efficiency for U.S. residential mortgage loans.

I. Introduction

Mortgages are the largest loans on the balance sheets of most households and account for a large proportion of the bond market. In the U.S., for instance, as of the second quarter of 2016, mortgage-related loans accounted for 21.68% of the bond market, compared to 20.71% for corporate debt.[1] The subprime mortgage crisis at the end of 2007 led to the 2008–2012 global recession and stimulated numerous studies on the risks of subprime mortgages. Prior studies highlight significant deadweight losses for borrowers, lenders, taxpayers, and communities due to

The authorship is in alphabetical order, and the authors contributed equally. We thank Guodong Chen, Haoyu Gao, Jianwen Li, Yan Liu, Changcheng Song, and seminar participants at the 2018 AREUEA International Conference, 2021 AsRES-GCREC-AREUEA Joint Conference, and 2019 China Finance Annual Meeting for the helpful comments. We are grateful to Jarrad Harford (the editor) and the anonymous reviewer for their valuable comments and suggestions. This research is supported by the National Natural Science Foundation of China (Grant No. 72203112), the MOE (Ministry of Education in China) Project of Humanities and Social Sciences (Grant No. 20YJC790183), and Research Grants Council of Hong Kong (Grant No. 11503318). Lin acknowledges research support from the National University of Singapore and MOE Tier 1 Grant (Grant No. R-521-000-044-115).

[1]See the statistics provided by the Securities Industry and Financial Markets Association (SIFMA) as of July 2016, at http://www.sifma.org/research/statistics.aspx.

lax ex ante screening and lax ex post monitoring in mortgage loans (e.g., Keys, Mukherjee, Seru, and Vig (2010), Piskorski, Seru, and Vig (2010), Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff (2011), and Adelino, Gerardi, and Willen (2013)). These studies extensively focus on the negative effect of securitizations on loan officers' screening and monitoring incentives. Different from the prior literature, our article connects the labor market of loan officers to loan origination and renegotiation behaviors, showing the economically meaningful impacts of loan officers' labor mobility on the origination efficiency of mortgage loans.[2]

We hypothesize that loan officers' labor mobility will affect their incentives in both ex ante mortgage screening and ex post renegotiations. First, loan officers who can easily switch from one bank to another may extend credit to as many mortgage loan applicants as they can. The main reason is that both their current compensation and external job market opportunities are closely tied to the origination volume and the amount of client information they possess (which is also valuable to rival banks). Even though an aggressive expansion of credit could lead to an increase in mortgage default (Keys et al. (2010)), the default may occur several years after the origination. Loan officers who originated these mortgages may have moved on to another lending institution before the problem is revealed. Therefore, loan officers do not necessarily bear the entire cost of loan defaults, especially for those who enjoy an even higher job market value stemming from a longer client list (thus accelerated job mobility).[3]

Second, high mobility in the labor market also discourages loan officers from actively renegotiating mortgage contract terms upon borrowers' delinquencies. While successful mortgage modifications require costly renegotiations between a loan officer and her client, the long-run benefits resulting from the renegotiations cannot entirely accrue to the loan officer when she switches jobs frequently. A failure to internalize all costs and benefits of renegotiations will lead to an under-modification problem (Piskorski et al. (2010), Agarwal et al. (2011)). Therefore, another direct consequence of high job mobility for loan officers is a reduction in mortgage modification and an increase in mortgage foreclosures.[4]

To test the above hypotheses, we first rely on staggered adoptions of the inevitable disclosure doctrine (IDD) across states, which significantly increases

[2]"Loan officers" in our article refer to loan originators, branch managers, and other related staffs.

[3]A possible counterargument is that higher default rates of past clients can have an adverse effect on loan officers' outside opportunities. This effect can encourage loan officers to screen the applicants closely. However, the information about individual loan performance is highly proprietary in the banking industry. Rival banks are unlikely to have access to detailed information about individual loan officers' past performance. The job seeking loan officers do not have any incentive to voluntarily disclose negative information to potential employers. Therefore, the adverse effect of higher default rates of past clients on loan officers' outside job opportunities is expected to be limited. Nevertheless, this argument will only discourage us from finding a significant effect of IDD adoptions.

[4]We note that for a lending institution, the cost of training a loan officer can outweigh its benefit if this loan officer can easily move to another institution. In this situation, the lending institution is likely to invest less in human capital, leading to a lower ability of loan officers to modify loans (e.g., Garmaise (2011)). However, the in-house training or the loan officers' ability is unobservable. We thus do not distinguish whether our results are driven by loan officers' incentives or their ability.

the costs for loan officers to switch jobs.[5] Specifically, the IDD is a legal doctrine that inhibits employees with access to proprietary information of their employers from moving to a rival firm for a certain period after either voluntary resignation or layoff. Since the nature of the banking business allows employers to easily show a likelihood of proprietary information leakage/misappropriation by departing loan officers, adoptions of the IDD effectively increase the litigation risk for departing loan officers and restrict their job mobility. Anecdotal evidence suggests that legal disputes regarding the leakage of information upon loan officers' job switching are very prevalent in the mortgage business.[6]

To identify the causal effect, we employ a spatial regression discontinuity design (RDD) that focuses on counties that are located near state borders. This design helps us mitigate the concern that adoptions of the IDD could be driven by unobservable state-level factors. Our results show that adoptions of the IDD by state courts lead to a 5.3% decrease in the likelihood of mortgage default. The default-reducing effect is more pronounced where labor mobility is reduced to a greater extent or loan officers have more outside options (proxied by local banking competition), consistent with the notion that restricting loan officers' job mobility enhances their incentives to screen mortgage applicants. Further, the results are more pronounced for mortgage applicants that are subject to a severer lax screening problem, that is, applicants whose FICO scores are right above 620 (Keys et al. (2010)), suggesting that IDD adoptions indeed change loan officers' screening incentives and mitigates the lax screening problem associated with these applicants.

To substantiate our inferences, we further examine the extent to which a restriction on loan officers' labor mobility affects mortgage contract terms. Our results show that IDD adoptions are not associated with significantly higher borrower FICO scores by borrowers, suggesting that the positive effect of IDD adoptions on loan origination quality is not simply due to an increase in hard information collection but may be driven by loan offers' additional efforts in acquiring clients' soft information. In addition, we find a reduction in the loan-to-value (LTV) ratio and an increase in the interest rate, indicating that loan officers become more cautious in credit approvals if their external job opportunities are constrained.

Policymakers are concerned about both the lax screening problem and the undersupply of mortgage loans. A natural question arises as to whether the mitigation of lax screening due to IDD adoptions would unintendedly lead to a reduction in the mortgage supply. We find that IDD adoptions do not affect the likelihood of mortgage approval or the total volume of mortgages. In addition, we find that applicants' profiles do not change after the IDD adoption. Collectively, these findings suggest that improved mortgage screening is not associated with a reduction in the overall mortgage supply. In other words, it appears that lower labor mobility encourages loan officers to collect more soft information, achieve better

[5]This instrumental variable for job mobility is validated by the notion that the IDD is widely used to discourage employees from accepting jobs in competing firms following the termination of their current employment (e.g., Png (2017), Klasa, Ortiz-Molina, Serfling, and Srinivasan (2018), and Li, Lin, and Zhang (2018)).

[6]For example, *American Equity Mortgage INC v. First Option Mortgage LLC* (2006), *Lincoln Park Saving Bank v. Frank Binetti* et al. (2010), and 360 Mortgage Group LLC *v.* Stonegate Mortgage Corporation and Lisa Glenn (2014).

screening, and reallocate credit from lower-quality borrowers to higher-quality ones, rather than ration the credit to lower-quality borrowers.

Next, we examine the effect of IDD adoptions on loan modifications. We find that IDD adoptions increase the likelihood of modification by 1.1%, which suggests that lower loan officer job mobility not only mitigates the lax screening problem but also enhances loan officers' ex post monitoring incentives. Given that the average modification rate is 3.8% in our sample, such an improvement in the modification rate due to the IDD adoption is economically important. In addition, we find an overall reduction in the foreclosure rate after the IDD adoption, indicating a meaningful welfare improvement due to enhanced ex ante screening and ex post renegotiations. Finally, we show that a lower default rate, a higher modification rate upon mortgage delinquency, and a lower foreclosure rate due to the IDD adoption can translate into a lower housing price volatility.

Finally, we use the adoption of the Uniform Trade Secrets Act (UTSA), which discourages labor mobility as well, as an additional identification strategy. Unlike the IDD, the UTSA requires the employer to show an *actual incidence* (rather than a *likelihood*) of proprietary information leakage/misappropriation by departing loan officers. The results under the alternative setting consistently show that adoptions of the UTSA decrease mortgage defaults, increase modification rates, and reduce foreclosures. In sum, our results suggest that reducing loan officers' labor mobility can have meaningful economic impacts on the mortgage market.

Overall, our findings are consistent with the premise that labor mobility in the banking industry can discourage loan officers from screening or monitoring loan applicants, leading to multiple negative economic consequences. Anticipating the impact of labor market restrictions on loan officers' behavior, rational lending institutions may take actions such as changing the incentive structure for loan officers. Since actions taken by lending institutions are unobservable, our findings can reveal only the *net effects* of IDD adoptions.

Our work makes multiple contributions to the literature. First, our article adds to the growing literature on mortgage originations and modifications. Prior research articulates that lax screening and lax monitoring of mortgage loans contributed to the financial crisis (e.g., Mayer, Morrison, and Piskorski (2009), Posner and Zingales (2009), and Maturana (2017)). The focus of prior studies is on the negative effect of securitization on loan officers' incentives. For example, Keys et al. (2010) and Agarwal et al. (2011) show that loan securitization transfers the risk from originators to mortgage-backed securities (MBSs) and leads to lax screening. Deviating from extant studies, our article finds that an additional factor, the labor mobility of loan officers, has economically meaningful impacts on both the ex ante screening and the ex post renegotiation of mortgage loans. Our findings generate important implications that restricting loan officers' labor mobility improves loan origination efficiency without compromising the credit supply and stability of the housing market.

Second, our work contributes to the general literature on information acquisitions in the banking sector (e.g., Drucker and Puri (2005), (2009), Parlour and Winton (2013), Srinivasan (2014), and Even-Tov, Li, Williams, and Wang (2023)). The beneficial effects of proprietary information collection for banking businesses have been widely documented (e.g., Srinivasan (2014)). However, loan officers'

https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

labor mobility and the associated threat of information leakage have attracted far less attention. Our findings suggest that reducing loan officers' labor mobility encourages loan officers to acquire more proprietary/soft information, which in turn facilitates mortgage loan origination and modification.

Finally, our article is related to the growing literature on trade secret protections (e.g., Png (2017), Klasa et al. (2018), and Tang, Wang, and Zhou (2020)). Klasa et al. (2018) examine the effect of IDD adoptions on the capital structure of real-sector firms. Li et al. (2018) find that IDD adoptions affect firms' disclosure of customer identities. Unlike previous studies that focus on industrial firms, our article examines the economic consequences of IDD adoptions for loan officers' screening and monitoring incentives for residential mortgage loans.

The remainder of this article is organized as follows: Section II presents the institutional background. Section III discusses the data and methodology. The empirical results are reported and discussed in Section IV. We conclude the article in Section V.

II. Institutional Background

A. Inevitable Disclosure Doctrine

The IDD is a trade secret law that grants the employer (i.e., the plaintiff) an injunction to prevent a current or a former employee (i.e., the defendant) from working for another company, if the employer can establish a *likelihood* that the employee will inevitably disclose the employer's trade secrets in the performance of the new position. The IDD applies to all employees and all business secrets of an employer, and it does not require evidence of actual or even threatened misappropriation. In addition, the IDD provides incremental protection on trade secrets to existing contractual or legal arrangements in the United States, such as the covenant not to compete (CNC) and nondisclosure agreement (NDA) (Klasa et al. (2018)).

As secret keepers, banks collect a substantial amount of proprietary information from clients and use the information to facilitate their monitoring and provision of valuable banking services (e.g., Drucker and Puri (2005), (2009), Parlour and Winton (2013), and Srinivasan (2014)). Most of the proprietary information is directly collected by loan officers who interact with their clients regularly. The departing loan officers may inevitably divulge the proprietary information to their new employers, who are competitors of their previous employers. Therefore, the adoption of the IDD gives banks a stronger right to sue a departing employee who could inevitably leak proprietary information to the new bank. Such a legal liability would discourage employees from moving to a rival bank, effectively restricting the job mobility of employees (Gilson (1999), Samila and Sorenson (2011)). Png and Samila (2015) and Klasa et al. (2018) show that the IDD adoption leads to a reduction in job mobility in nonfinancial industries.

The impact of the IDD on job mobility varies across states and periods because the legislation related to the adoption of the IDD in a given state may change over time.[7] Fourteen changes were made in 12 states between 1992 and 2010.

[7]While empirical strategies relying on heterogeneous treatments across locations and time could introduce estimation bias, our empirical design mitigates this issue by matching treatment

B. The Mortgage Markets

The subprime crisis that began in 2007 caught the attention of both academicians and policymakers. A large number of studies have discussed the origins of the high default rates and some attribute it to lax screening. Loan officers play an important role in mortgage originations. They screen mortgage loan applications based on both the hard and soft information of the applicants.

When a borrower defaults on a mortgage, the borrower can apply for a mortgage modification, and the mortgage servicer can modify the terms of the mortgage: the interest rate may be reduced, the term may be extended such that the outstanding balance is amortized over a longer period, or a part of the principal may be written off. Modifications of these terms, individually or in combination, can change the borrower's monthly payments. Mortgage modifications may yield some losses for mortgage holders but reinstate their loan status to current and avoid the greater loss of foreclosure. If a modification application is rejected, the property will enter foreclosure. The borrower may also redefault after a modification. The subsequent foreclosures are costly to both individuals and the economy as a whole: mortgage borrowers and lenders suffer from substantial deadweight losses, and foreclosure of the property has negative externalities on the prices of nearby properties (Campbell, Giglio, and Pathak (2011)).

An originated loan may be kept on the originator's balance sheet or be resold and securitized into MBSs. Keys et al. (2010) document that mortgage applications with a FICO score above 620 are more likely to be accepted. The cause is a rule of thumb in mortgage securitizations that mortgages with 620+ FICO can be easily securitized and the mortgages' credit risk can be transferred to MBS investors. For securitized loans, mortgage servicers perform services associated with mortgages and MBSs. For example, they transfer payments from borrowers to MBS investors. The servicer may or may not be a subsidiary of the mortgage originator.

III. Sample and Methodology

A. Data and Sample

We use a data set containing micro-level information about residential mortgages collected by Blackbox Logic, a private data company that obtains data from mortgage servicers and securitization trustees. There are approximately 21 million mortgage loans in the data set, which account for approximately 90% of all privately securitized mortgages originating after Jan. 1, 1998 in the U.S. market. The data set covers not only subprime loans but also prime and Alt-A loans and contains information on both mortgage origination and subsequent performance. The data set provides information about the loan characteristics, including loan origination amount, interest rate, loan terms, whether it is the first lien, occupation status of the house during the loan period, borrower FICO score, and LTV ratio at origination. The location of the property is also provided. In addition, the data set tracks

counties with adjacent counties that are never treated and then controlling for pair-year fixed effects (see de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), and Baker, Larcker, and Wang (2022)).

mortgage performance after origination. For each month, this data set reports the outstanding balance of the loan, the remaining months to maturity, whether a mortgage becomes delinquent, and how long the mortgage has been delinquent.

Our sample consists of mortgages originated between Jan. 1, 1998 and Dec. 31, 2007, and includes mortgages' performance information through Dec. 31, 2016. We restrict the sample to this period for several reasons. First, only observations after 1998 are available in the Blackbox Logic database. Second, we want to cover the last change in the state-level IDD status in 2006. Third, the financial crisis broke out at the end of 2007, which may have changed lenders' behavior and imposed many confounding effects.

We use the HMDA database for the total amount of mortgage applications. The HMDA database reports information on the mortgage applications received by all kinds of mortgage originators, including some loan characteristics (the purpose and the amount, but not the interest rate) and the applicant's income.

We supplement the mortgage data with information from several other sources. The annual county-level housing price index comes from the Federal Housing Finance Authority (FHFA). We obtain the monthly unemployment rate from the Bureau of Labor Statistics (BLS) and annual per capita income and population from the Bureau of Economic Analysis (BEA) at both the county and state levels (Dambra, Even-Tov, and Naughton (2023)).[8] BEA also reports the number of employees by industry for each county. We also obtain household characteristics (monthly income, education level, number of members, and age) reported by the Current Population Survey (CPS), which is a monthly survey of approximately 60,000 representative households. We obtain job-switching information from the Survey of Income and Program Participation (SIPP), and introduce the detail in Section IV.B.1. Finally, we obtain information on the time each state adopted or rejected the IDD from Klasa et al. (2018). During our sample period, there were six changes in IDD status in six states, including IDD adoptions by Ohio (Sept. 2000), Missouri (Nov. 2000), and Kansas (Feb. 2006), respectively; and rejections by Florida (May 2001), Michigan (Apr. 2002), and Texas (Apr. 2003), respectively.

B. Research Design

To assess the impact of the IDD adoption on the risk of mortgage default, we need to estimate the counterfactual level of default in the absence of the adoption. A challenge to this identification is that state-level changes in IDD status may be correlated with unobserved macroeconomic conditions. To address this concern, we take advantage of the spatial RDD proposed by Holmes (1998), which has been widely used in economics research. In particular, we examine the changes in the default rate in counties lying at state borders. While there is a discontinuity in the IDD adoption at the border, the economic conditions are approximately the same on both sides of the border. Therefore, if we can find an abrupt change in the default

[8]For regressions at the annual frequency, we use the average monthly unemployment rate within a year.

rate after an IDD adoption or rejection on one side of the border relative to the other, we can attribute the change to the IDD adoption or rejection.[9] An important feature of the IDD is that it imposes a significant constraint on both within-state and cross-state job switching (e.g., Klasa et al. (2018)). Therefore, the possibility of regulatory arbitrage via cross-border job switching would not undermine the validity of our identification strategy relying on counties near state borders.

Our strategy is essentially a staggered difference-in-differences approach. The treatment states include states that changed their IDD status during the sample period, and the control states include states that are adjacent to the treated states but had no change in IDD status. We match treated states with adjacent control states without replacement, and generate the following six matched "*groups*": (FL: AL, GA), (KS: CO, NE, OK), (MI: IN, WI), (MO: AR, IA, IL, KY, TN), (OH: MI, PA, WV), and (TX: LA, NM). We further split each group into multiple "*pairs*" of counties that share the same border. For example, in the group (FL: AL, GA), Florida shares one border with Alabama and a different border with Georgia. Counties on both sides of the FL–AL border constitute one pair, and counties on both sides of the FL–GA border form another pair. In a pair, counties on either side of the border are defined as a "*district.*" For instance, the FL–AL pair has a treated district that includes the counties in Florida and a control district that includes the counties in Alabama. In total, we have 17 pairs and 34 districts. Following Holmes (1998), we consider counties whose centroids are within 50 miles of state borders without replacement.[10] A county can belong to only one pair whose border is the nearest to the county centroid. Figure 1 shows the location of these counties.

In our sample, whether an observation belongs to the treatment sample depends on the location of the borrower's property. If the property is located in a state that changed its IDD status during the sample period, the observation is classified into the treatment sample. Mortgage originators and their parent companies usually assign their branches near the location of borrowers to sell their mortgage products or financial services (such as financial advisory services) because the distance is critical in determining consumers' choice of financial services despite technological advances (see Khan (2004), Grzelonska (2005), Immergluck and Smith (2006), and Amel, Kennickell, and Moore (2008)). Therefore, when a borrower's state experiences an IDD change, it is reasonable to assume that the corresponding loan officer that originated the loan belongs to the same state and thus is subject to the restriction of the IDD.

To obtain consistent estimates, we use the following ordinary least squares (OLS) specification to estimate the effects of IDD adoptions on mortgage defaults:

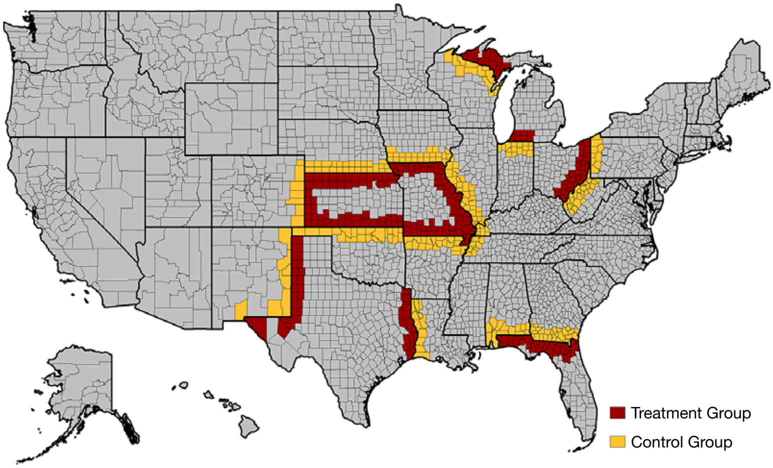
[9]We note that borrowers may strategically apply for cross-border loans. Such cross-border borrowing activities would only discourage us from finding any significant difference between two adjacent states.

[10]Holmes (1998) considers 25, 50, 75, and 100 miles. It seems reasonable to choose the smallest distance to make the observations in our sample as close to the border as possible. However, Holmes (1998) points out that using 25 miles would drop observations in many counties in western states because the counties in the west are so large that the distances from the centroids of some counties to the border are greater than 25 miles in some states, which would hurt the representativeness of the sample. Therefore, we choose the second smallest distance, 50 miles. However, our results are robust to varying distances to state borders.

FIGURE 1

Treatment and Control Groups: Based on IDD

Figure 1 shows the geographic distribution of the treated and control counties.



$$Y_{i,c,t} = \beta_0 + \beta_1 \text{IDD}_{d,t} + \beta_2 Z_{i,t} + \beta_3 X_{c,t} + \gamma_{p,t} + \mu_d + \varepsilon_{i,c,d,p,t}, \quad (1)$$

where i, c, d, p, and t represent loans, counties, districts, pairs, and origination years, respectively. $Y_{i,c,t}$ is a default dummy that equals 1 if the borrower ever defaults during the life of the loan, and 0 otherwise. We focus on newly originated loans rather than existing loans and therefore the default of the loans in question reflects the post-IDD screening of loan officers. Following the literature on mortgages, we define a loan as being defaulted if the loan is 60+ days past due. Following Klasa et al. (2018), $\text{IDD}_{d,t}$ equals 1 if the property is located in a district whose state has adopted the IDD and 0 if the state has not adopted the IDD or has rejected the IDD after a previous adoption or if the loan originated before the IDD status change. $Z_{i,t}$ represents loan characteristics, including whether the loan is classified as a low-document loan (LOW_DOC), interest rate at origination (INTEREST_ORG), FICO score (FICO), LTV ratio at origination (LTV_ORG), the natural logarithm of the loan origination amount (ln(AMT_ORG)), first lien (FIRST_LIEN), whether the property is occupied by the owner (NONOWNER), and whether the property type of the mortgage is a single family house (SINGLE_FAM). $X_{c,t}$ represents county-level controls, including the natural logarithm of per capita income (ln(PC_INC)) and total population (ln(POP)), and unemployment rate (%) (UNEMPLOY). Loan officers' incentive compensation can be a confounding factor (e.g., Tzioumis and Gee (2013), Behr, Drexler, Gropp, and Guettler (2020)). Prior to 2011, loan officers could be compensated based on the terms of the mortgage agreement. The amendment to Regulation Z in 2013 prohibits loan officers from receiving compensation linked to terms of the mortgage agreement. To control for the impact of loan officers' compensation on mortgage default, we obtain 269,835 data points of position-level total salary for loan officers in the U.S. from Revelio Labs. We define ln(SALARY) as the median value of the log of total salary for loan officers at the county-year level and then control for this variable in the regression.[11] We also control for the numbers of Republican (REP) and the Democratic (DEM) representatives, respectively, at the state-year level. $\gamma_{p,t}$ is pair-year fixed effects that control for time-varying shocks within each pair of districts. μ_d represents district fixed effects that absorb district-level time-invariant characteristics that affect mortgage originations. The IDD dummy (IDD) and various fixed effects constitute a difference-in-differences specification. Robust standard errors are clustered at the state level.

IV. Empirical Analysis

A. Summary Statistics

Panel A of Table 1 reports the descriptive statistics of the sample, covering loans whose corresponding properties are located within 50 miles of a state border. In total, there are 347,596 observations. The average IDD dummy is 0.48, indicating that 48% of observations are under the IDD-effective status. The average mortgage default rate is 49.1%, and the average foreclosure rate is 34.8%. It is not surprising that the default rate and foreclosure rate are high, since our sample consists of privately securitized mortgages whose quality is lower than other types, and the performance period covers a recession period (subprime loan crisis). A total of 72.5% of the loans are the first lien, and approximately 23.5% of the properties are nonowner occupied. The average FICO score is 654.7, the average LTV at origination is around 77%, and 56.3% of loans are low-document loans. We also present the characteristics of delinquent loans in Panel B.

B. The Effect of IDD Adoptions on Mortgage Default Risk

1. Validity of the IDD as an Exogenous Shock on Labor Mobility

Before proceeding to our baseline tests, we conduct two sets of empirical examinations to validate that the IDD represents an exogenous shock on loan officers' labor mobility. First, we examine whether adoptions of the IDD across states are driven by local economic, social, and political conditions. To this end, we conduct state-year-level regressions of IDD on state-level per capita income, population, unemployment rate, and the number of DEM and REP representatives, respectively. Our regression model also includes group-year fixed effects (a group consists of a treated state and its adjacent states and can be considered as a state-pair) and state fixed effects. The result, presented in Table A1, shows that none of the control variables is statistically significant except for that on DEM (significant at the 10% level). This evidence suggests that passages of the IDD are not entirely endogenous to local macroeconomic conditions. Nevertheless, given the significant

[11]The distribution of salary is skewed and therefore we rely on the median instead of the mean value of loan officer salary. Prior to 2000, Revelio Labs has a poorer coverage on some county-years. In such cases, we rely on state-level aggregation to preserve as many observations as possible when county-level data are missing.

TABLE 1

Descriptive Statistics

Table 1 reports the descriptive statistics of the sample (loan-level observations). The sample period is from Jan. 1, 1998 to Dec. 31, 2007. The detailed definitions of all variables are shown in the Appendix.

Variable	No. of Obs.	Mean	Std. Dev.
Panel A. Full Sample			
DEFAULT	347,596	0.491	0.500
FORECLOSE	347,596	0.348	0.476
INTEREST_ORG (%)	347,596	7.790	3.272
NONOWNER	347,596	0.235	0.424
FIRST_LIEN	347,596	0.725	0.447
SINGLE_FAM	347,596	0.825	0.380
IDD	347,596	0.480	0.500
FICO	347,596	654.7	71.41
LTV_ORG (%)	347,596	77.00	25.08
LOW_DOC	347,596	0.563	0.496
UNEMPLOY (%)	347,596	5.549	1.700
PC_INC	347,596	31,943	6,269
POP	347,596	600,462	636,910
AMT_ORG	347,596	115,455	175,581
DEM	347,596	5.960	3.044
REP	347,596	9.544	4.915
SALARY	347,596	64,431.07	811.633
Panel B. Delinquent Loans			
MODIFY	91,178	0.0377	0.191
FORECLOSE	91,178	0.702	0.457
LTV_ORG (%)	91,178	79.66	20.44
LTV_MTM (%)	91,178	75.59	21.34
INTEREST_CUR (%)	91,178	9.440	1.818
LOAN_AGE	91,178	19.88	15.21
NONOWNER	91,178	0.170	0.376
FIRST_LIEN	91,178	0.794	0.404
LOW_DOC	91,178	0.330	0.470
IDD	91,178	0.430	0.495
SINGLE_FAM	91,178	0.921	0.269
FICO	91,178	607.5	59.56
UNEMPLOY (%)	91,178	6.085	1.748
PC_INC	91,178	32,715	6,137
POP	91,178	809,347	758,877
BAL_OUT	91,178	98,213	86,171
DEM	91,178	5.556	2.363
REP	91,178	9.189	4.098
SALARY	91,178	63,981.96	590.355

coefficient on DEM, we control for both DEM and REP in all our regressions to alleviate possible influences of local partisan preferences.

Next, we provide evidence supporting the effectiveness of the IDD. In particular, we test whether adoptions of the IDD lead to lower job mobility in the banking industry. To this end, we construct a labor mobility measure based on the data from the SIPP, which surveys a national representative sample of U.S. households. The surveys are conducted in multiple waves for each state, and each survey is labeled with when and where the survey was conducted. The questions cover information about job switching of individual employees across waves and therefore allow us to measure the degree of labor mobility in a panel. We thus construct an individual-year level sample based on the data. If an employee switches jobs in a year, we assign a value of 1 to the dummy MOVE. Otherwise, MOVE equals 0.[12]

[12]SIPP uses the number of people represented by a survey subject (employee) in a month as the monthly weight of this employee. To account for the varying degrees of representativeness of the sample employees, we use the sum of the weight for an employee in a year as the weight in our individual-year level regression.

https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

We restrict the sample to industries related to loan originations (the SIPP industry codes 700 to 702) and employees above 20 years old. We then regress MOVE on IDD and state-level controls.

We control for group-year, state, industry (sub-categories within the banking industry), and individual employee fixed effects when estimating the regression of interfirm mobility on IDD. In Table A2, we find a negative and significant effect of IDD adoptions on employees' interfirm mobility in industries associated with loan origination. On average, adoptions of the IDD reduce the likelihood of interfirm job switching by 6.2% without controls, and by 6.6% after controlling for county-level variables, which are economically significant given the sample mean of 8.4%. Our findings are consistent with those of Png and Samila (2015) and Klasa et al. (2018), who find that IDD adoptions reduce labor mobility in different industries.

2. Baseline Results

We estimate equation (1) to analyze the effect of IDD adoptions on the mortgage default risk. Table 2 reports the results. In column 1, we control for only the IDD dummy and fixed effects. The coefficient on IDD is negative and statistically significant (−0.058, $t = -4.60$). Column 2 additionally controls for household, and county characteristics. We specifically control loan officers' compensation since the change in incentive compensation can be associated with IDD adoptions. We find a consistent result that the coefficient remains negative and significant (−0.028, $t = -2.52$).[13] In column 3, our baseline specification, we further control for characteristics of mortgage applicants and loans, and find a highly consistent result (−0.026, $t = -2.58$). Since the mean default rate of the sample is 0.491, this result suggests that the IDD adoption makes mortgage borrowers 5.3% less likely to default, which is an economically significant impact.[14] In column 4, we replace the district fixed effects with county fixed effects to account for more granular, county-level heterogeneities, and find a similar estimate with a slightly larger magnitude (−0.030, $t = -3.74$).

Time-varying characteristics of state laws could affect mortgage originations. In the U.S., there are two aspects of laws related to mortgage foreclosure, and they differ across states: whether a mortgage is a recourse loan and whether the foreclosure process is judicial (which is much lengthier) or nonjudicial. These two legal aspects can both affect mortgage origination. For example, mortgage origination in recourse-loan states may be lower since it is difficult for the lender to foreclose the property in the event of a default due to the lengthy foreclosure process (Ghent and Kudlyak (2011)). Besides, the anti-predatory lending laws (APLs) may also affect loan screening. To absorb these legal heterogeneities, we control for Recourse-Year, Judicial-Year, and APL fixed effects in column 5 of Table 2. The coefficient

[13] Kini, Williams, and Yin (2021) show that a stronger trade secret protection leads to higher incentive-based compensation. Therefore, IDD adoptions can be associated with higher incentive compensation, which in turn may cause an elevated mortgage default rate. Not controlling for this effect could lead to an underestimation of the effect of IDD adoptions on loan defaults.

[14] We note that adoptions of the IDD incentivize loan officers to select applicants based on applicants' characteristics. As such, controlling for observable applicant characteristics can lead to an underestimation on the treatment effect.

TABLE 2

Effect of the IDD on Mortgage Default Risk

Table 2 reports loan-level regressions estimating the effect of IDD adoptions on mortgage default probability. The dependent variable is an indicator that equals 1 if the borrower ever defaults during the life of the loan, and 0 otherwise. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2	3	4	5
IDD	−0.058***	−0.028**	−0.026**	−0.030***	−0.052***
	(−4.60)	(−2.52)	(−2.58)	(−3.74)	(−4.90)
LOW_DOC			−0.025***	−0.024***	−0.024***
			(−2.95)	(−2.86)	(−2.86)
LTV_ORG (%)			0.001***	0.001***	0.001***
			(18.40)	(20.67)	(20.87)
INTEREST_ORG (%)			−0.001	−0.001	−0.001
			(−1.33)	(−1.31)	(−1.30)
ln(AMT_ORG)			−0.029***	−0.030***	−0.030***
			(−7.85)	(−14.93)	(−14.94)
FIRST_LIEN			−0.008*	−0.008	−0.008
			(−1.79)	(−1.72)	(−1.71)
NONOWNER			0.060***	0.056***	0.056***
			(4.30)	(4.30)	(4.30)
FICO			−0.002***	−0.002***	−0.002***
			(−30.17)	(−29.31)	(−29.27)
SINGLE_FAM			−0.018	−0.017*	−0.016*
			(−1.69)	(−1.88)	(−1.85)
ln(PC_INC)		−0.086*	−0.002	0.097	0.099
		(−1.92)	(−0.06)	(1.59)	(1.51)
ln(POP)		0.018**	0.015**	0.181	0.130
		(2.44)	(2.62)	(0.82)	(0.67)
UNEMPLOY (%)		0.030***	0.020***	0.003	0.007
		(9.79)	(6.76)	(0.43)	(0.93)
REP		0.027*	0.026*	0.027*	0.022**
		(1.78)	(1.96)	(1.87)	(2.10)
DEM		0.018	0.016	0.017	0.011
		(1.27)	(1.33)	(1.32)	(1.17)
ln(SALARY)		0.006	0.017	0.057	0.066
		(0.13)	(0.30)	(0.98)	(1.12)
Pair-year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes		Yes
Recourse-year, judicial-year, APL FE					Yes
County FE				Yes	Yes
No. of obs.	347,596	347,596	347,596	347,590	347,590
R^2	0.082	0.087	0.179	0.184	0.184
Mean DV	0.491	0.491	0.491	0.491	0.491

magnitude gets greater with a higher significance level (−0.052, t = −4.90). Overall, the results reported in Table 2 are consistent with the argument that the lower labor mobility caused by the IDD adoption enhances loan officers' ex ante screening incentives on mortgage applicants.

3. Dynamic Effects

Better loan quality and adoption of the IDD could be simultaneously caused by banks' self-selection if banks can lobby for the passage of the IDD. To mitigate this concern, we test the dynamic effects of the IDD adoption by exploiting the notion that a change in loan quality will emerge well before the adoption of the IDD if banks lobby for trade secret protection.

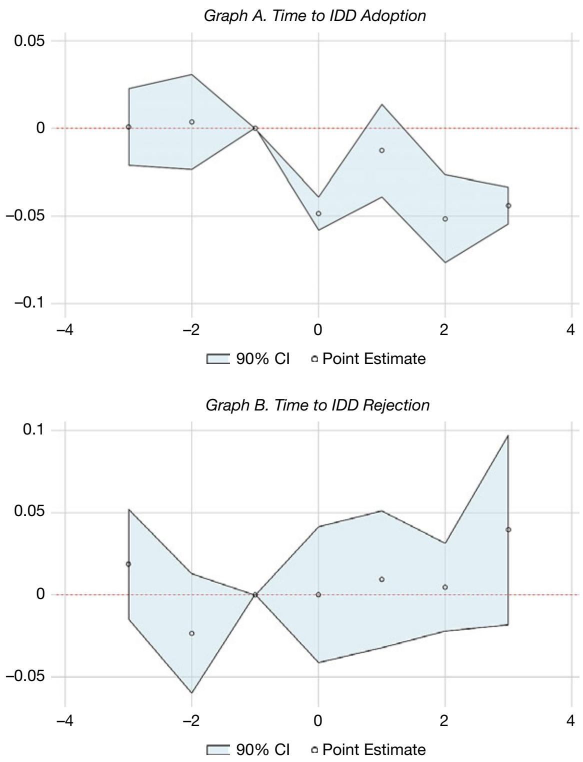
Due to the different natures of IDD adoptions and rejections, we estimate the dynamic analysis for IDD adoptions and rejections separately. Following Bertrand and Mullainathan (2003), we replace IDD with several alternative dummy variables: IDD_-N, a dummy variable that equals 1 for the state that will change the IDD status in N years; IDD_0, a dummy variable that equals 1 for the state that changes the IDD status in the current year; and IDD_N, a dummy variable that equals 1 for the state that changed the IDD status N years ago. The whole set of controls and high-dimensional fixed effects in equation (1) are considered in both regressions. Following the recent literature (e.g., Baker et al. (2022)), we restrict our sample to a 7-year window for each IDD change event instead of the whole sample period. We use IDD_-1 as the base year.

We visualize the dynamic analysis estimates in Figure 2, separately for IDD adoptions (Graph A) and rejections (Graph B). The graph for coefficient estimates of IDD adoptions shows indistinguishable pre-IDD trends between treated and control firms, a sharp decline in the IDD adoption year, and negative coefficients in the whole post-IDD window. The parallel pre-shock trends and the timing of the decline are consistent with the causal interpretation of our baseline results.

FIGURE 2

Dynamic Effects

Figure 2 shows the dynamics of the treatment effect. The horizontal axis represents the year relative to the IDD adoption/rejection year.



https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

However, the coefficients of IDD rejections, although showing an upward trend after the IDD rejection, are generally insignificant. The asymmetric effect of IDD rejections relative to IDD adoptions is probably due to the fact that IDD rejections are after years of IDD adoptions, during which the loan officers, with low job mobility, have accumulated experiences to better screen borrowers. The experiences acquired by loan officers will not be reversed by a subsequent IDD rejection, thus leading to an insignificant deterioration of loan origination quality.

A problem associated with staggered-shock difference-in-differences is that staggered shocks are time varying and heterogeneous, leading to biased estimates (de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021), and Baker et al. (2022)). Note that our empirical design is less subject to this concern because by controlling for pair-year fixed effects, our design essentially compares treated counties with paired neighbor counties that *never adopted IDD* (thus each treatment event is operationalized as a canonical, one-off difference-in-differences).

4. Alternative Distances to the State Border

For robustness, we include more or fewer counties in our sample by allowing alternative distances of counties to state borders. Specifically, we consider various distances ranging from 25 miles, the smallest one in Holmes (1998), to 50 miles, the one we use in the baseline specification, with a 5-mile interval. We also extend the distance beyond 50 miles with a 5-mile interval up to 70 miles. We control for the running variable DISTANCE, which is the distance between the county of the property and the state border. The results are reported in Figure 3. All the estimates are negative and statistically significant, and there is little change in the point estimates.

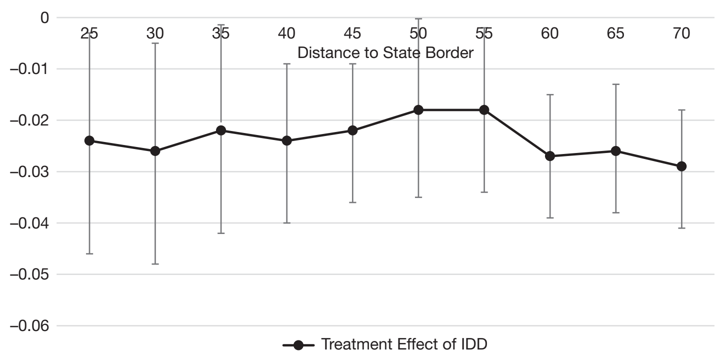
5. Cross-Sectional Tests

We argue that the adoption of the IDD can have a positive effect on loan origination because it restricts interfirm job switching and therefore mitigates the lax screening problem. Therefore, we need to pin down two underlying channels:

FIGURE 3

Effect of the IDD on Default Risk: Various Distances to the State Border

Figure 3 shows the point estimates and the 90% confidence intervals of loan-level regressions estimating the effect of IDD adoptions on mortgage default probability using different distances to the state border. For each regression, we control for the running variable (distance to state border) in addition to all the controls in the baseline regression.



i) it is the *reduced labor mobility* that affects loan officers' incentives, and ii) it is the *mitigated lax screening* that reduces the mortgage default probability.

We first test the labor mobility channel, which predicts that the magnitude of the IDD effect on mortgage default rate hinges on the extent to which the local labor mobility is reduced. In other words, we expect that states with a greater reduction in labor mobility in the banking industry are affected more strongly, thereby yielding a more pronounced effect of the IDD.

To test this conjecture, we calculate the weighted average MOVE at the state-year level based on the SIPP sample in Section IV.B.1. We then define a variable △MOBILITY as the change in the weighted average MOVE of the state from the period of low IDD enforcement to the period of high IDD enforcement. We include the interaction IDD × △MOBILITY in the regression specification.[15] Panel A of Table 3 reports the results. The first column excludes control variables and the second column includes the full set of controls. For brevity, we omit the presentation of coefficients on control variables (which are the same as in Table 2). In both columns, the coefficients of the interaction term are significantly negative. For example, in column 2, the coefficient is −1.573 ($t = -2.61$), suggesting that the default rates decrease to a greater extent in states with greater changes in mobility in banking industries. This result reinforces our argument that the restriction of inter-firm mobility is the channel through which the IDD adoption affects loan origination.

Besides the ex post measure of labor mobility above, we also construct an ex ante measure of labor mobility, as measured by local banking competition. Greater banking competition in the local area implies more outside options or higher labor mobility for loan officers. If IDD adoption leads to lower mortgage default probability by restricting loan officers' mobility, it should follow that the results are stronger when the loan officer has more outside options ex ante. To test this conjecture, we construct county-level HHI of local banks based on the number of mortgage-lending bank branches (HHI_BRAN) or the dollar volume of mortgage originations of local banks (HHI_MTG) as of the year prior to the IDD adoption. We then construct interaction terms by multiplying IDD with these two bank competition measures, respectively, and include them in our baseline model. The results are presented in Panel B of Table 3. The coefficients on both IDD × HHI_BRAN and IDD × HHI_MTG are significantly positive. Since higher HHI indicates a lower level of competition, our results suggest that greater competition among local banks will strengthen the effect of IDD adoptions, which is consistent with the labor mobility channel.

Next, we test the lax screening channel. A rule of thumb in the mortgage market is that loans with borrowers whose FICO scores are higher than 620 are easier to securitize than those of borrowers with lower scores, that is, 620 is the eligibility cutoff for mortgage securitization (Keys et al. (2010)). Therefore, loan officers may simply rely on the 620 FICO score in selecting borrowers, exercising close scrutiny over borrowers with a FICO score just below 620 (620− borrowers) but lax screening over borrowers with a score just above 620 (620+ borrowers). Such a nonlinear screening intensity likely leads to a jump in unobserved or

[15]△MOBILITY is absorbed by state fixed effects since it is time invariant.

https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

TABLE 3

Cross-Sectional Analyses

Table 3 shows loan-level regressions estimating the incremental effect of labor mobility, banking competition, and lax screening on the relationship between the IDD and mortgage default probability. The dependent variable is an indicator that equals 1 if the borrower ever defaults during the life of the loan, and 0 otherwise. In Panel A, we interact IDD with the change in state-level labor mobility. Panel B includes interactions between IDD and banking competition measures. In Panel C, we interact IDD with a dummy indicating 620+ borrowers. Panel C restricts the sample to loans with FICO scores between 590 and 650 to capture the discontinuity of the screening incentive around FICO scores of 620. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	1	2
Panel A. Full Sample		
IDD	−0.002	0.030
	(−0.07)	(1.23)
IDD × ΔMOBILITY	−1.445*	−1.573**
	(−1.91)	(−2.61)
Other controls	No	Yes
Pair-year FE	Yes	Yes
District FE	Yes	Yes
No. of obs.	347,596	347,596
R^2	0.082	0.179
Mean DV	0.491	0.491
Panel B. Banking Competition (Outside Option)		
IDD	−0.044***	−0.047***
	(−3.48)	(−4.07)
IDD × HHI_BRAN	0.339**	
	(2.65)	
HHI_BRAN	−0.295**	
	(−2.12)	
IDD × HHI_MTG		0.334***
		(3.90)
HHI_MTG		−0.256***
		(−3.19)
Other controls	Yes	Yes
Pair-year FE	Yes	Yes
District FE	Yes	Yes
No. of obs.	345,444	345,444
R^2	0.180	0.180
Mean DV	0.491	0.491
Panel C. The Effect of Ex Ante Lax Screening		
IDD	−0.066***	−0.044***
	(−7.09)	(−3.29)
IDD × FICO_620+	−0.023**	−0.027***
	(−2.49)	(−2.86)
FICO_620+	−0.045***	0.015**
	(−6.01)	(2.43)
Other controls	No	Yes
Pair-year FE	Yes	Yes
District FE	Yes	Yes
No. of obs.	114,516	114,516
R^2	0.094	0.102
Mean DV	0.579	0.579

unmodeled default risk around the 620 cutoff. A natural implication of the nonlinearity is that once loan officers put more effort into screening all applicants, the marginal effect (on reducing default risk) will be greater for 620+ borrowers. Therefore, if the IDD and the associated lower labor mobility do mitigate the lax screening problem, we can observe that the effect of the IDD on default risk is larger for loans with 620+ FICO scores.

We create a dummy, FICO_620+, which equals 1 if the FICO score of the borrower of the loan at origination is higher than 620, and 0 otherwise. In the regression model, we control for the interaction term, IDD × FICO_620+, to examine the heterogeneous effect of the IDD. We focus on borrowers whose FICO scores lie in a narrow band around 620 (i.e., between 590 and 650).[16] By comparing the two subsamples that have similar FICO scores, we can isolate the effect of a discrete change in lax screening from other unobserved factors.

Panel C of Table 3 reports the results. In column 1, the coefficient of the interaction term is significantly negative (−0.023, $t = -2.49$), suggesting that the default risk decreases more for loans with potential lax screening (620+ FICO scores). FICO scores also capture applicants' credit risk, which in turn affects loan performance. To further pin down the channel of lax screening, we control for other applicant characteristics in column 2, and find a consistent result (−0.027, $t = -2.86$). Once we control for these characteristics, the coefficient on FICO 620+ becomes positive, suggesting that the borrowers with credit scores just above 620 are more likely to default relative to those with scores just below 620. This result is consistent with Keys et al.'s (2010) finding that borrowers with scores just above 620 are not screened closely and therefore are associated with a higher likelihood of default.[17]

6. Ruling Out Demand Side Effect: Heterogeneities Based on Proportion of IDD-Sensitive Industries

It is possible that our documented results come from the demand side of the mortgage market: the IDD also changes the labor market faced by borrowers, not simply the loan officers at the bank, and the shifted labor mobility of local borrowers could drive our baseline results. If this is the case, we can hardly find any significant result if the local residents' job mobility is barely affected by the IDD. To test this, we define industries susceptible to the impact of the IDD as industries in which ordinary employees have more access to trade secrets. These industries include finance and insurance, wholesale trade, retail trade, information, real estate and rental and leasing, professional, scientific, and technical services, and management of companies and enterprises. We then divide the number of employees in these susceptible industries by the total number of employees of the same county

[16]The proportions of loans below and above FICO 620 within the bandwidth are balanced, with 32,891 observations between 590 and 619 and 33,371 observations between 620 and 650.

[17]There is another channel that could underlie our results: When banks expect longer services from their loan officers due to the IDD, they are encouraged to enhance loan officers' job skills by providing more professional training. These employees are now better able to screen mortgage borrowers thereby reducing future mortgage default probability. In untabulated results, we construct four measures of human capital investment (Organization Capital, Training, Employee Development, and Human Capital) from Compustat and Edgar. Using these measures, we find no evidence that banks increase their investment in human capital after IDD adoptions. One possible explanation for this insignificant effect is that banks' training systems are usually centralized and standardized, which is widely employed in service industries as to maximize the coordination benefits (Williamson (1996), Levin and Tadelis (2005)). Such a centralized training system would be less responsive to local shocks such as IDD adoptions. Nevertheless, due to the lack of a precise measure of bank human capital investment, we caution a strong interpretation on the insignificant finding.

https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

each year, and use this fraction as a proxy for the demand side effect.[18] Counties with a smaller fraction are supposed to be less affected by the IDD in general. Note that the supply side of these counties (loan officers who issue most of the local mortgages) is fully affected regardless of the fraction.

We run our baseline model in a set of subsamples with different fractions of trade secret-sensitive industries, and present the results in Table 4. The first three columns examine counties with a small fraction of trade secret-sensitive industries, that is, fractions lower than 5th, 10th, and 25th percentiles of the sample, respectively. For all three columns, we find negative and significant coefficients on IDD

TABLE 4

Ruling out Demand Side Effect

Table 4 reports loan-level regressions estimating the effect of the IDD adoptions on mortgage default probability across counties with different proportions of industries sensitive to the IDD, defined in Section IV.B. The dependent variable is an indicator that equals 1 if the borrower ever defaults during the life of the loan, and 0 otherwise. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Fraction of High-Mobility Industries	≤5%	≤10%	≤25%	(25%, 50%]	(50%, 75%]	(75%, 100%]
	1	2	3	4	5	6
IDD	−0.056**	−0.044*	−0.048***	−0.027*	0.007	−0.115*
	(−2.55)	(−2.02)	(−4.67)	(−1.78)	(0.41)	(−2.13)
LOW_DOC	−0.023***	−0.032***	−0.017	−0.029***	−0.043***	−0.008
	(−2.86)	(−3.33)	(−1.72)	(−5.08)	(−4.69)	(−0.71)
LTV_ORG (%)	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(3.97)	(7.11)	(9.78)	(18.69)	(28.10)	(16.82)
INTEREST_ORG (%)	0.001	0.001	−0.000	−0.001	−0.003	−0.001
	(0.49)	(0.93)	(−0.28)	(−1.46)	(−1.29)	(−1.06)
ln(AMT_ORG)	−0.009	−0.012	−0.018***	−0.027***	−0.030***	−0.043***
	(−1.30)	(−1.66)	(−3.57)	(−5.53)	(−11.12)	(−7.06)
FIRST_LIEN	−0.017	−0.022	−0.014*	−0.003	−0.018	0.007
	(−1.55)	(−1.39)	(−1.76)	(−0.56)	(−1.53)	(1.16)
NONOWNER	0.014	0.029*	0.018	0.055***	0.089***	0.057***
	(0.81)	(1.97)	(1.62)	(4.62)	(5.82)	(4.07)
FICO	−0.002***	−0.002***	−0.002***	−0.002***	−0.002***	−0.002***
	(−26.38)	(−33.73)	(−54.91)	(−50.60)	(−15.31)	(−13.17)
SINGLE_FAM	−0.053***	−0.034*	−0.042***	−0.019	0.007	−0.012
	(−3.02)	(−1.89)	(−4.97)	(−1.72)	(0.85)	(−1.12)
ln(PC_INC)	−0.020	−0.010	−0.089**	−0.032	0.057	0.314***
	(−0.38)	(−0.36)	(−2.59)	(−0.72)	(0.18)	(9.04)
ln(POP)	0.003	0.005	0.013**	0.031***	−0.031	−0.004
	(0.32)	(0.62)	(2.51)	(3.81)	(−0.64)	(−1.36)
UNEMPLOY (%)	0.008	0.005	0.008*	0.019***	0.009	0.013***
	(1.33)	(1.45)	(1.93)	(2.90)	(0.68)	(16.33)
REP	−0.011	−0.008	0.010	0.045	0.019**	−0.019
	(−0.80)	(−0.82)	(0.80)	(1.39)	(2.69)	(−1.26)
DEM	−0.007	−0.005	0.007	0.038	0.017**	−0.078***
	(−0.41)	(−0.41)	(0.53)	(1.17)	(2.85)	(−4.03)
ln(SALARY)	−0.023	−0.152	−0.013	−0.006	−0.519	−0.054
	(−0.19)	(−1.37)	(−0.24)	(−0.14)	(−1.13)	(−1.26)
Pair-year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	18,104	38,071	89,925	82,877	88,176	84,453
R^2	0.132	0.134	0.139	0.151	0.211	0.203
Mean DV	0.484	0.488	0.462	0.451	0.581	0.468

[18]The data are from BEA.

with large economic magnitudes. To illustrate the robustness of our finding, we also split the rest of the sample into three groups according to the quartile value of the proportion of trade secret-sensitive industries in local counties. We then estimate the baseline regression in these three groups, and present the result in columns 4–6. The coefficients on IDD are significantly negative for the (25%, 50%] group and the (75%, 100%] group. Notably, the default-reducing effect of the IDD is significantly lower among subsamples that have higher fractions of trade secret-sensitive industries, which is consistent with the notion that industries suffering from lower labor mobility and thus poorer quality employees might drive up the local mortgage default rate. Nevertheless, the significantly negative coefficients for most of the groups suggest the resilience of the default-reducing effect of IDD adoptions.

The overall evidence is inconsistent with the demand side effect (the lower default rate is caused by a shift in the local labor market of borrowers). In further analyses below, we find neither the mortgage demand nor the borrower characteristics have changed as a result of the IDD, reinforcing our argument on the supply-side effect.

C. Changes in Loan Characteristics

The next question is how loan terms are affected by the IDD. Loan characteristics will appear to be less risky if the loan officers screen and originate loans more carefully. However, it is also possible that loan officers scrutinize the risk profile of borrowers more carefully and price the risk attributes into the interest rate more sufficiently after the passage of the IDD.

We analyze how IDD adoptions affect four critical loan terms: the interest rate (INTEREST_ORG), origination amount (ln(AMT_ORG)), LTV at origination (LTV_ORG), and borrower FICO score (FICO). We use these characteristics as dependent variables and control for county-level characteristics, DEM and REP, and fixed effects. Table 5 reports the results.

The origination amount and FICO score change little (columns 1 and 2 of Table 5). The little change in the FICO score indicates that the better loan quality associated with the adoption of the IDD does not simply result from loan officers' stricter screening of hard information and may be driven by their greater efforts in soft information collection. Column 3 shows that LTV on average decreases by approximately 1.051 percentage points, which suggests that loan officers care more about leverage after the passage of the IDD. Column 4 shows that the average interest rate increases by 18.4 basis points, indicating that the loan officers charge a higher interest rate conditional on similar loan characteristics. These results reflect the lowered risk tolerance of loan officers subject to lower labor mobility. They are more likely to incorporate negative information into loan pricing or require a higher risk premium.

D. Does the Supply of Mortgage Loans Change?

We have shown that the IDD motivates loan officers to invest greater effort in loan screening and select borrowers of good credit quality. A natural question arises as to whether more careful screening due to the IDD leads to a reduction in the mortgage supply. To this end, we examine how the IDD affects the county-level

https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

TABLE 5

Effect of the IDD on Loan Characteristics

Table 5 reports loan-level regressions estimating the effect of IDD adoptions on various loan characteristics. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. The mean dependent variable is reported at the bottom to assess marginal effects. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	ln(AMT_ORG)	FICO	LTV_ORG	INTEREST_ORG
	1	2	3	4
IDD	−0.013	0.742	−1.051*	0.184*
	(−0.59)	(0.33)	(−1.78)	(2.04)
ln(PC_INC)	0.672***	30.066**	−1.823	−0.748***
	(3.01)	(2.27)	(−0.97)	(−3.00)
ln(POP)	−0.035	−0.722	−0.283	0.054
	(−1.02)	(−0.37)	(−1.04)	(1.03)
UNEMPLOY (%)	−0.035**	−3.476**	0.946***	0.091***
	(−2.43)	(−2.34)	(5.86)	(3.25)
REP	0.011	−0.740	−0.012	−0.026
	(1.46)	(−0.67)	(−0.03)	(−0.97)
DEM	0.000	−1.194	−0.501	0.042
	(0.01)	(−1.01)	(−1.09)	(1.25)
ln(SALARY)	0.013	1.764	−3.249	−0.443
	(0.11)	(0.09)	(−1.16)	(−1.21)
Pair-year FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
No. of obs.	347,596	347,596	347,596	347,596
R^2	0.099	0.057	0.026	0.073
Mean DV	11.307	654.708	77.000	7.790

aggregate annual origination volume. Since Blackbox includes only privately securitized loans, we turn to the HMDA data to gauge the total origination and application volume. Our regression analysis uses the natural logarithm of the total origination volume of a lender in a county (ln(ORG_VOL)) as the dependent variable and controls for county-level characteristics (ln(PC_INC), ln(POP), and UNEMPLOY) and state-level partisan preferences (DEM and REP). Given a certain level of mortgage demand (which is addressed in the next step), the origination amount is supposed to decrease under a stricter loan supply policy. However, the results reported in Table 6 show that adoptions of the IDD do not affect the origination amount (columns 1 and 2).

The assumption for the above argument is that the application amount does not change. To check whether this assumption holds, we replace the dependent variable with the log of aggregate application amount (ln(APP_AMT)) at the lender-county level in columns 3 and 4 of Table 6. The result indicates that IDD adoptions have no significant effect on the application amount, which further validates our arguments. In addition, in columns 5 and 6, we directly use the approval dummy (APPROVAL) as the dependent variable and control for applicants' characteristics (ln(APP_INC), MINORITY, and MALE) and loan characteristics (NONOWNER, FIRST_LIEN, and ln(AMT_ORG)), and find that the approval rate does not change as well, again inconsistent with loan officers reducing loan supply.

One may question why more careful screening, which reduces the default probability, does not lead to a reduction in the origination amount or the approval rate. One explanation is that loan officers devote more effort to collecting both hard

TABLE 6
Effect of the IDD on Origination and Application Volumes

Table 6 reports regressions estimating the effects of the IDD on mortgage origination amount, application amount, and approval rate. The regression is conducted at the lender-county-year level in columns 1–4 and the application level in columns 5 and 6. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	ln(ORG_VOL)		ln(APP_VOL)		APPROVAL	
	1	2	3	4	5	6
IDD	−0.029 (−1.39)	−0.023 (−1.13)	−0.030 (−1.70)	−0.020 (−1.17)	0.002 (0.85)	0.003 (1.31)
ln(APP_INC)						0.088*** (22.03)
ln(AMT_ORG)						0.023*** (15.04)
NONOWNER						−0.023*** (−3.63)
MINORITY						−0.044*** (−11.77)
MALE						0.025*** (15.19)
FIRST_LIEN						0.008 (1.31)
ln(PC_INC)		0.797*** (4.74)		0.715*** (4.33)		−0.002 (−0.20)
ln(POP)		0.629*** (31.98)		0.677*** (36.45)		0.004** (2.71)
UNEMPLOY (%)		−0.027** (−2.56)		−0.016 (−1.67)		−0.005*** (−3.50)
REP		−0.002 (−0.20)		0.003 (0.40)		−0.001 (−0.48)
DEM		−0.001 (−0.13)		0.001 (0.13)		0.000 (0.10)
ln(SALARY)		−0.327** (−2.12)		−0.352** (−2.43)		−0.012 (−0.53)
Pair-year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender-year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	384,851	384,851	487,957	487,957	8,763,585	8,763,585
R^2	0.301	0.482	0.316	0.506	0.252	0.275
Mean DV	5.640	5.640	5.750	5.750	0.729	0.729

and soft information and therefore can better allocate the credit to good borrowers. As loan officers' compensation is related to their origination amounts, they have the incentive to maintain the same level of credit supply. To reduce the credit risk without affecting the compensation, loan officers need to put more effort in distinguishing low-risk borrowers and offering mortgage credit to them. The second possible reason is that a decrease in the employee turnover rate reduces lending institutions' downside risk and thus their cost of capital, allowing them to supply more credit. This positive effect may offset the negative effect of strict screening on the credit supply.

E. Does the Composition of Residents Change?

Another alternative explanation is that the adoption of the IDD changes the composition of local residents. For example, labor force may migrate to states with fewer restrictions on inter-firm mobility. To test whether the composition changes after the IDD adoption, we exploit the CPS from the BLS database. CPS surveys approximately 60,000 households monthly and generates a representative sample of the U.S. household population. We examine whether the adoption of the IDD is correlated with monthly family income, size (number of members), residents' education level, and residents' age. Education level is average schooling years inferred from the education level categorized by BLS.

One issue with the CPS data is that the county information is missing for quite a few observations, for which we are not able to identify the distance from state borders. Hence, we expand the sample to all counties in each state in our baseline sample. We regress each dependent variable (the natural logarithm of family income (ln(FAM_INC)),[19] family size (ln(FAM_SIZE)), individual education level (ln(EDUCATION)), and individual age (ln(AGE)) at a monthly frequency) on state-level characteristics and fixed effects.[20] Columns 1–4 of Panel A of Table 7 present the results. The coefficient of IDD is neither statistically nor economically significant for each dependent variable. Therefore, the lower default rate after the adoption of the IDD is unlikely to be driven by changes in household characteristics.

However, it is possible that the composition of residents does not change only where most borrowers do not have access to trade secrets and therefore are not affected by IDD adoptions. Thus, we conduct an analysis similar to that in Table 4 by dividing our sample into groups with different fractions of industries that can be affected by IDD adoptions. Specifically, we regress the state-level average ln (FAM_INC), ln(FAM_SIZE), ln(AGE), and ln(EDUCATION) level on IDD, separately in four groups with varying proportions of trade secret-sensitive industries, and present the results in Panel B of Table 7. All regressions control for state-level characteristics whose coefficients are not tabulated. We find that after IDD adoptions, household income and family size remain qualitatively similar across different groups. The average age changes significantly only in the (50%, 75%] group, and the average education level changes significantly only in the group with the highest proportion of trade secret-sensitive industries. However, a reduction in education level is likely associated with an increase in mortgage default rate, against our finding.

Overall, the results imply that the reduction in mortgage default rate is not accompanied by notable demographic changes in states that are highly sensitive to trade secret protection, lending further support to our claim that the main finding is not driven by the demand-side effect.

F. The Effect of the IDD on Modifications and Subsequent Foreclosures

As we contend previously, higher labor mobility of loan officers can aggravate the under-renegotiation problem. We expect that lower labor mobility due to

[19]For each interval of income, an index is assigned by the survey, monotonically increasing in income, for example, index = 2 if income <5,000, =2 if (5,000, 7,500]. We use the mean of the interval as the income. For example, 6,250 for observations with an index of 2 (5,000 ~ 7,500]. For the highest level (>150,000), we use 150,000 + 25,000 (the half of the range for the second highest level (100,000, 150,000]) = 175,000 as the income.

[20]In state-level regressions, in which we do not observe counties and cannot define districts and pairs, we control for year fixed effects and state-level fixed effects.

TABLE 7

Effect of the IDD on Household Characteristics

Table 7 reports regressions estimating the effect of IDD adoptions on various household characteristics. Panel A uses the full sample, and Panel B examines different subsamples with different proportions of industries sensitive to the IDD. The regression is conducted at the household level for FAM_INC and FAM_SIZE and individual borrower level for AGE and EDUCATION. All regressions control for state-level characteristics (ln(PC_INC), ln(POP), UNEMPLOY, REP, and DEM), state fixed effects, and group-year fixed effects (year fixed effects for Panel B due to insufficient variations in IDD in subsamples). The sample period is from Jan. 1, 1998 to Dec. 31, 2007. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Full Sample Analysis

Dependent Variable	ln(FAM_INC)	ln(FAM_SIZE)	ln(AGE)	ln(EDUCATION)
	1	2	3	4
IDD	−0.001	−0.005	−0.006	−0.000
	(−0.15)	(−1.03)	(−1.50)	(−0.18)
ln(PC_INC) (state)	0.437**	0.053	−0.082*	−0.035
	(2.13)	(0.92)	(−2.03)	(−1.32)
ln(POP) (state)	0.579**	0.240***	−0.246***	−0.071**
	(2.72)	(3.32)	(−2.98)	(−2.27)
UNEMPLOY (state) (%)	−0.024***	0.002	−0.002	−0.003**
	(−3.44)	(0.82)	(−0.86)	(−2.34)
REP	0.001	−0.000	0.001	−0.000
	(0.22)	(−0.09)	(0.42)	(−0.15)
DEM	−0.004	−0.005***	0.003***	−0.000
	(−0.93)	(−2.95)	(3.08)	(−0.13)
Group-year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
No. of obs.	2,090,122	2,090,122	4,950,830	4,950,831
R^2	0.017	0.004	0.005	0.010
Mean DV	10.405	0.777	3.690	2.577

Panel B. Subsample by Sensitivities to Trade Secret Protection

	[0, 25%]	(25%, 50%]	(50%, 75%]	(75%, 100%]
	1	2	3	4
ln(FAM_INC)				
IDD	−0.008	−0.009	0.029	0.006
	(−0.75)	(−0.99)	(1.39)	(0.66)
Controls; FE	Yes	Yes	Yes	Yes
No. of obs.	561,579	502,059	580,341	446,143
R^2	0.010	0.011	0.027	0.011
Mean DV	10.342	10.465	10.389	10.436
ln(FAM_SIZE)				
IDD	−0.004	−0.002	0.015	0.011
	(−0.49)	(−0.24)	(1.06)	(1.63)
Controls, FE	Yes	Yes	Yes	Yes
No. of obs.	561,579	502,059	580,341	446,143
R^2	0.005	0.001	0.002	0.001
Mean DV	0.807	0.769	0.778	0.751
ln(AGE)				
IDD	0.007	0.006	−0.012*	−0.006
	(1.68)	(1.37)	(−2.45)	(−1.60)
Controls; FE	Yes	Yes	Yes	Yes
No. of obs.	1,295,647	1,314,782	1,255,803	1,084,598
R^2	0.003	0.004	0.001	0.001
Mean DV	3.664	3.682	3.691	3.728
ln(EDUCATION)				
IDD	−0.002	0.003	0.000	−0.008*
	(−0.96)	(1.62)	(0.00)	(−2.99)
Control; FE	Yes	Yes	Yes	Yes
No. of obs.	1,295,647	1,314,783	1,255,803	1,084,598
R^2	0.008	0.007	0.006	0.001
Mean DV	12.255	12.591	12.504	12.565

adoptions of the IDD can alleviate this problem and increase modification rates. We thus analyze delinquent mortgages (mortgages in default) to determine how adoptions of the IDD affect mortgage modification rates. Since delinquent loans can self-cure, one borrower may default multiple times. We focus on the last default of each borrower to eliminate the disturbance introduced by strategic default. We exclude outliers with a mark-to-market LTV higher than 500, which accounts for 0.14% of our sample. Then, we match the origination information of each delinquent loan to its performance data. As a result, we obtain a cross-sectional data set of all delinquent mortgages, and each observation includes information on loan characteristics, the defaulting date, and whether the loan is modified.

We employ a regression specification that resembles equation (1) but additionally controls for current interest rate (INTEREST_CUR), the natural logarithm of the outstanding balance (ln(BAL_OUT)), mark-to-market LTV (LTV_MTM), and loan age (LOAN_AGE).[21] The dependent variable is either a modification dummy (MODIFY) that equals 1 if the delinquent loan is modified, and 0 otherwise, or a foreclosure dummy (FORECLOSE) that equals 1 if the property of originated loan was ultimately foreclosed, and 0 otherwise.

Columns 1 and 2 of Table 8 present the results of the modification regressions. After adding all control variables, the IDD increases the likelihood of modification by 1.1%, which is economically significant because the average modification rate is 3.8% in our sample. In columns 3 and 4, we replace the dependent variable with the foreclosure dummy. After adding all control variables, the IDD leads to 3.5% lower foreclosure rates of delinquent loans, a 5% reduction compared to the sample mean, indicating a meaningful welfare improvement due to a higher modification rate.

Even though adoptions of the IDD reduce the mortgage default risk and increases modification rates, they do not necessarily reduce the foreclosure rate of all originated loans. We thus test the probability of foreclosure using the full sample (both delinquent and nondelinquent loans). The specification of control variables is the same as equation (1). Columns 5 and 6 of Table 8 report the results. Adoptions of the IDD reduce foreclosure rates by 3.6% compared with the sample mean of 34.8%, which is highly consistent with the result obtained using the delinquent loan sample.

G. The Volatility of Housing Price

We have shown that adoptions of the IDD lead to more cautious mortgage approval practices, higher modification rates upon delinquency, and lower foreclosure rates. We next examine if these effects of the IDD can translate into a lower housing price volatility. The regression model is specified below:

$$(2) \qquad \mathrm{HPI\_STD}_{c,t} = \theta_0 + \theta_1 \mathrm{IDD}_{d,t} + \theta_2 X_{c,t} + \gamma_{p,t} + \mu_d + \varepsilon_{c,d,p,t},$$

where $\mathrm{HPI\_STD}_{c,t}$ is the 5-year rolling-window (from year t to $t+4$) standard deviation of the FHFA housing price index for county c in year t. IDD, X, and fixed

[21] The value of the IDD depends on the default year (and the state) instead of the origination year, and the sample is restricted to the delinquent loans between 1998 and 2007.

TABLE 8

Effect of the IDD on Modifications and Subsequent Foreclosures: Delinquent Loans

Table 8 reports loan-level regressions estimating the effects of IDD adoptions on loan modification and foreclosure. The sample in the left four columns includes all 60+ days delinquent loans. The sample in the right two columns includes all loans. The sample period is between Jan. 1, 1998 and Dec. 31, 2007. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Delinquent Loans				All Loans	
Dependent Variable	MODIFY		FORECLOSE		FORECLOSE	
	1	2	3	4	5	6
IDD	0.008***	0.011*	−0.122***	−0.035*	−0.082**	−0.036*
	(4.14)	(1.83)	(−3.84)	(−1.77)	(−2.84)	(−1.89)
LOW_DOC		−0.002*		−0.006		−0.017**
		(−1.82)		(−1.23)		(−2.37)
FIRST_LIEN		0.011***		−0.028***		0.001***
		(3.32)		(−4.88)		(8.58)
NONOWNER		−0.010***		0.043***		−0.007***
		(−4.30)		(4.69)		(−8.03)
FICO		−0.000***		0.000**		−0.033***
		(−8.50)		(2.67)		(−7.14)
SINGLE_FAM		0.005*		−0.010		−0.063***
		(2.09)		(−1.36)		(−11.37)
LTV_ORG (%)		−0.000		−0.003***		0.115***
		(−0.53)		(−4.86)		(7.35)
INTEREST_ORG (%)		0.011*		0.039		−0.001***
		(1.80)		(1.28)		(−19.13)
ln(AMT_ORG)		0.000		0.003		−0.010
		(0.15)		(0.72)		(−0.76)
ln(PC_INC)		−0.000		0.022***		−0.005
		(−0.40)		(3.62)		(−0.08)
ln(POP)		−0.000		0.070***		0.020
		(−0.16)		(9.21)		(1.72)
UNEMPLOY (%)		−0.002		0.044***		0.020***
		(−0.81)		(8.07)		(4.45)
REP		0.002***		0.009**		0.034***
		(3.17)		(2.29)		(3.55)
DEM		0.000		0.004***		0.025**
		(0.76)		(6.67)		(2.76)
INTEREST_CUR (%)		0.014***		−0.009**		
		(7.31)		(−2.38)		
LTV_MTM (%)		0.000***		−0.002***		
		(3.51)		(−9.17)		
ln(BAL_OUT)		0.008		0.005		
		(0.57)		(0.13)		
LOAN_AGE		0.014***		−0.009**		
		(7.36)		(−2.41)		
ln(SALARY)		0.000***		−0.002***		0.011
		(3.51)		(−9.25)		(0.15)
Pair-year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	91,178	91,178	91,178	91,178	347,596	347,596
R^2	0.043	0.049	0.211	0.233	0.105	0.165
Mean DV	0.038	0.038	0.702	0.702	0.348	0.348

effects are defined in equation (1). Robust standard errors are clustered at the state level. The sample period is still between 1998 and 2007. θ_1 measures how adoptions of the IDD affect the volatility of housing prices.

Table 9 reports the results of estimating equation (2). After controlling for county-level characteristics, the adoption of the IDD leads to a 2.298-point reduction in the standard deviation of the housing price index. The effect is economically

TABLE 9

Effect of the IDD on Housing Price Volatility

Table 9 presents county-year-level regressions estimating the effect of IDD adoptions on housing price volatility. The dependent variable is the 5-year rolling window (from year t to $t+4$) standard deviation of the FHFA housing price index for county i in year t. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. Robust standard errors are clustered at the state level. The t-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: HPI_STD	
	1	2
IDD	−1.950***	−2.298**
	(−2.90)	(−2.53)
ln(PC_INC)		10.176*
		(1.86)
ln(POP)		3.174***
		(5.83)
UNEMPLOY (%)		0.023
		(0.08)
REP		0.739*
		(1.77)
DEM		1.245**
		(2.09)
Pair-year FE	Yes	Yes
District FE	Yes	Yes
No. of obs.	3,707	3,707
R^2	0.497	0.646
Mean DV	11.025	11.025

significant given the average standard deviation of 11.025. The result suggests that the various effects of restricting the job mobility of loan officers ultimately contribute to more stable housing prices. Note that the change in housing price volatility is likely driven by the reallocation effect since the total approved mortgage volume does not change (see Table 6). Therefore, we argue that restricting labor mobility via the IDD would encourage loan officers to reallocate credits from riskier borrowers to safer borrowers, thereby reducing the housing price volatility.

H. Adoptions of the UTSA and Mortgage Default Risk

In addition to relying on the IDD, companies can use the UTSA to protect trade secrets. In 1979, the National Conference of Commissioners on Uniform State Laws approved the UTSA and recommended it for enactment in all states. By 2014, 46 states, Washington, DC, and the U.S. Virgin Islands had adopted either the 1979 version of the UTSA or the 1985 amended version of the UTSA. The exceptions are Massachusetts, North Carolina, New York, and Texas. Note that recognition of the IDD by state courts does not require the adoption of the UTSA in the state and adoption of the UTSA does not imply recognition of the IDD. Therefore, the UTSA adoption is independent of the IDD adoption.[22]

[22]Under the IDD, firms can take action to prevent harm before it is done, whereas under the UTSA, the firm can act only after harm has already been done. In particular, under the UTSA, the employer needs to prove that the former employee has actually misappropriated a trade secret, whereas under the IDD, evidence for actual or threatened misappropriation is not required to obtain injunctive relief (Quinto and Singer (2014), Png and Samila (2015)). The plaintiff needs to show only that the employee would be employed in such a capacity that she would inevitably disclose the trade secrets.

In this section (IV.H), we use the UTSA as an alternative identification strategy and examine whether a reduction in labor mobility due to the passage of the UTSA affects mortgage default risk and modification. We identify passages of the UTSA in three states in our sample period (Panel A of Table 10) and apply the spatial RDD (see Figure 4 for matched districts). The regression model is exactly the same as equation (1) except that we replace the IDD dummy with the UTSA dummy, which equals 1 if the property is located in a district whose state has enacted the UTSA and 0 if the state has not enacted the UTSA.

Panel B of Table 10 reports the results. In column 1, where the default probability is the dependent variable, the coefficient on UTSA is negative and statistically significant (−0.049, $t = -7.05$). In column 2, we examine the effect of the UTSA on loan modification and find a positive and significant effect (0.017, $t = -25.79$). In column 3, we find that the coefficient on UTSA is negative and significant (−0.031, $t = -5.37$), suggesting that the adoptions of the UTSA reduce foreclosure rates.

Overall, using passages of the UTSA as an additional identification strategy, we consistently show that lower labor mobility caused by trade secret laws enhances loan officers' ex ante screening and ex post monitoring incentives on mortgage borrowers.

TABLE 10

Effect of the UTSA on Loan Default Risk and Modification

Table 10 reports loan-level regressions estimating the effect of UTSA adoptions on mortgage default, modification, and foreclosure. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. The mean dependent variable is reported at the bottom to assess marginal effects. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. States that Adopt the UTSA and Their Adjacent States

State	Year of Enactment	Adjacent States (Control States)
TN	2000	AL AR GA KY MO MS NC VA
PA	2004	DE MD NJ NY WV
WY	2006	CO ID MT NE SD UT

Panel B. Regression Analysis

Dependent Variable	DEFAULT	MODIFY	FORECLOSE
	1	2	3
UTSA	−0.049***	0.017***	−0.031***
	(−7.05)	(25.79)	(−5.37)
LOW_DOC	0.011	−0.002	0.013*
	(1.08)	(−0.99)	(1.91)
LTV_ORG (%)	0.001***	0.001	0.001***
	(9.93)	(1.81)	(3.96)
INTEREST_ORG (%)	0.001		−0.004***
	(1.50)		(−7.33)
ln(AMT_ORG)	−0.025***		−0.032***
	(−3.66)		(−4.56)
FIRST_LIEN	−0.004	0.017***	−0.075***
	(−0.69)	(5.84)	(−9.12)
NONOWNER	−0.010	−0.012***	0.047***
	(−0.88)	(−5.02)	(3.43)
FICO	−0.002***	−0.000***	−0.001***
	(−18.15)	(−6.60)	(−10.55)
SINGLE_FAM	−0.021**	0.012	−0.014*
	(−2.26)	(1.54)	(−1.78)

(continued on next page)

TABLE 10 (continued)

Effect of the UTSA on Loan Default Risk and Modification

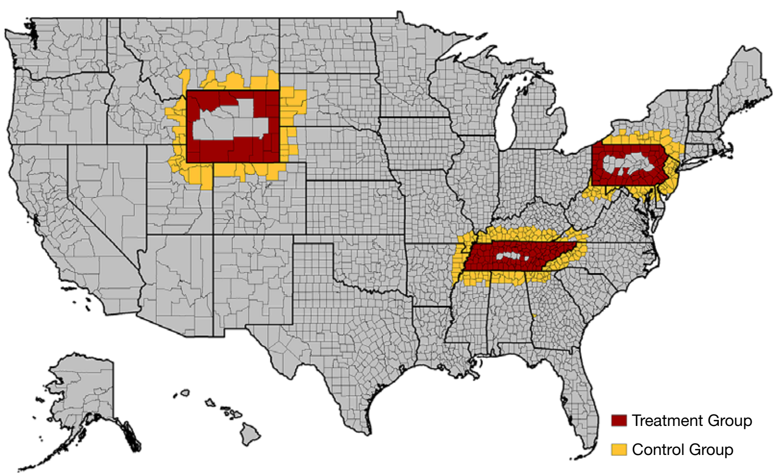
Panel B. Regression Analysis (continued)

Dependent Variable	DEFAULT	MODIFY	FORECLOSE
	1	2	3
ln(PC_INC)	0.031	0.010	0.057*
	(0.85)	(0.53)	(1.72)
ln(POP)	0.004	−0.000	0.000
	(0.96)	(−0.05)	(0.04)
UNEMPLOY (%)	0.014**	0.001	0.011
	(2.08)	(0.51)	(1.22)
REP	−0.017***	0.027***	−0.013**
	(−2.84)	(8.23)	(−2.70)
DEM	0.004	0.022***	0.002
	(0.82)	(19.94)	(0.63)
INTEREST_CUR (%)		0.003***	
		(4.88)	
LTV_MTM (%)		−0.001*	
		(−1.88)	
ln(BAL_OUT)		0.013**	
		(3.18)	
LOAN_AGE		0.000	
		(0.26)	
ln(SALARY)	0.003	−0.006	0.011
	(0.17)	(−0.12)	(0.66)
Pair-year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
No. of obs.	683,262	24,160	683,262
R^2	0.174	0.050	0.081
Mean DV	0.368	0.045	0.222

FIGURE 4

Treatment and Control Groups: Based on UTSA

Figure 4 shows the geographic distribution of the treated and control counties for the UTSA.



https://doi.org/10.1017/S0022109023000649 Published online by Cambridge University Press

V. Conclusion

In this article, we use a residential mortgage data set that contains detailed information on mortgage originations and performances to explore the effect of loan officers' labor mobility on the likelihood of mortgage defaults, modifications, and foreclosures. We use a spatial RDD, which exploits the discontinuity in IDD adoptions near state borders, to control for unmodeled local macroeconomic conditions. We find that adoptions of the IDD substantially reduce the likelihood of mortgage default. Such an effect is stronger for borrowers subject to a severe lax screening problem. We also find that adoptions of the IDD increase modification rates, reduce foreclosure rates, and lead to more stable housing prices. We use passages of the UTSA as an additional identification strategy and show a consistent result. Overall, our findings suggest that restricting loan officers' labor mobility induces stricter loan screening and monitoring, resulting in positive effects on the mortgage market.

Our article demonstrates that labor mobility in the banking industry can be an important factor impeding careful loan screening and thus adds to the growing literature on the determinants of mortgage risk. Moreover, our findings support the argument that trade secret laws can be used as a valuable mechanism to mitigate the externalities of labor mobility, thereby contributing to the literature on the costs and benefits of trade secret protection.

Appendix: Variable List

APPROVAL: Dummy that equals 1 if the loan application is approved.

DEFAULT: Dummy that equals 1 if a loan becomes 60+ days delinquent.

DEM: The number of democratic representatives at the state-year level.

District: Categorical variable. A district consists of counties on one side of a state border, which is defined in Section III.B.

FICO_620+: Dummy that equals 1 if the borrower's FICO is higher than 620.

FICO: Borrower's FICO credit score.

FORECLOSE: Dummy that equals 1 if the property of a mortgage loan is foreclosed.

FIRST_LIEN: Dummy that equals 1 if the mortgage loan is first lien.

HHI_BRAN: County-year-level Herfindahl–Hirschman Index based on the number of mortgage lender branches assuming each lender has one mortgage-lending branch in a census tract where it receives mortgage applications.

HHI_MTG: County-year-level Herfindahl–Hirschman Index based on mortgage origination volume.

HPI_STD: Standard deviation of the housing price index calculated using the FHFA annual county-level housing price index from the current year t to year $t + 4$.

IDD: Dummy that equals 1 if the property is located in a district whose state has adopted the IDD and 0 if the state has not adopted the IDD or has rejected the IDD after a previous adoption or if the loan is originated before the IDD status change.

INTEREST_ORG (%): Interest rate of the mortgage when it was originated.

INTEREST_CUR (%): Interest rate when the borrower defaults, used in the regression of modification.

ln(AGE): Natural logarithm of the resident age.

ln(AMT_ORG): Natural logarithm of the outstanding balance of the mortgage when it was originated.

ln(APP_AMT): Natural logarithm of the annual mortgage application volume by a lender in a county (reported in HMDA).

ln(APP_INC): Natural logarithm of the applicant's income (reported in HMDA).

ln(BAL_OUT): Natural logarithm of the outstanding balance of the mortgage when the borrower defaults.

ln(EDUCATION): Natural logarithm of the schooling years inferred from the education index defined by CPS (conducted by BLS).

ln(FAM_INC): Natural logarithm of the household annual income inferred from the income category index defined by CPS.

ln(FAM_SIZE): Natural logarithm of the total number of persons living in the household.

ln(ORG_VOL): Natural logarithm of the annual mortgage origination by a lender in a county.

ln(PC_INC): Natural logarithm of the county-level income per capita for each year.

ln(POP): Natural logarithm of the county-level population for each year.

ln(SALARY): Natural logarithm of the median value of loan officer salary for each county-year (or state-year).

LOAN_AGE: Number of months from the date of origination to the date of default.

LOW_DOC: Dummy that equals 1 if the mortgage loan is classified as a low-document loan.

LTV_ORG (%): Loan-to-value ratio when the mortgage was originated.

LTV_MTM (%): Loan-to-value ratio when the borrower defaulted.

MALE: Dummy that equals 1 if the applicant/borrower is male (reported in HMDA).

MINORITY: Dummy that equals 1 if the applicant/borrower is not white (reported in HMDA).

△MOBILITY: The changes in interfirm mobility at the state-year level as defined in Section IV.B.5.

MODIFY: Dummy that equals 1 if a defaulted loan is modified.

MOVE: Dummy that equals 1 if the subject has moved to a different firm since the previous wave.

NONOWNER: Dummy that equals 1 if the property of the mortgage is not occupied by the property owner (the borrower).

Pair: Categorical variable. A pair consists of two districts that are adjacent to each other, which is defined in Section III.B.

REP: The number of Republican representatives at the state-year level.

SINGLE_FAM: Dummy that equals 1 if the property of the mortgage is a 1–4 single family house.

UNEMPLOY (%): Annual county-level unemployment rate.

UTSA: Dummy that equals 1 if the property is located in a district whose state has enacted the UTSA and 0 if the state has not enacted the UTSA.

TABLE A1

Determination of the IDD

State-year-level regressions estimating the effects of macro variables on IDD adoptions. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. The dependent variable is the IDD dummy. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: IDD
ln(PC_INC) (state)	−0.939
	(−0.65)
ln(POP) (state)	−2.190
	(−0.75)
UNEMPLOY (state) (%)	0.065
	(0.83)
DEM	−0.017
	(−0.39)
REP	−0.127*
	(−1.92)
Group-year FE	Yes
State FE	Yes
No. of obs.	209
R^2	0.875
Mean DV	0.445

TABLE A2

The IDD and Employee Job Switching

Employee-year-level regressions estimating the effects of IDD adoptions on employees' interfirm mobility in the loan origination industries. We obtain the employee job switching information from SIPP and restrict the sample to loan origination industries. The sample period is from Jan. 1, 1998 to Dec. 31, 2007. The dependent variable is a dummy that equals 1 if the employee moves to a different firm in the year, and 0 otherwise. Robust standard errors are clustered at the state level. The *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: MOVE	
	1	2
IDD	−0.062**	−0.066**
	(−2.21)	(−2.25)
ln(PC_INC) (state)		0.261
		(1.59)
ln(POP) (state)		0.005
		(0.07)
UNEMPLOY (state) (%)		−0.002
		(−0.12)
REP		0.031*
		(1.92)
DEM		0.023*
		(2.00)
State FE	Yes	Yes
Employee FE	Yes	Yes
Industry FE	Yes	Yes
Group-year FE	Yes	Yes
No. of obs.	5,995	5,995
R^2	0.589	0.590
Mean DV	0.084	0.084

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