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Abstract

We study how the 1990s adoption of now widely-used automated mortgage underwriting systems affected credit supply, house prices and their comovement across locations. The effects go well beyond processing improvements. By implementing more complex, statistically-informed lending rules, the systems allowed households to borrow more, pushing up house prices. Furthermore, by transmitting a common set of credit standards across lenders, the new technology increased credit and house price synchronization. Together, our results illustrate how new lending technology can generate correlated credit supply shocks, contributing to house price booms and increasing market interconnectedness.

JEL Classification: G21, L85, R21, R31

Keywords: mortgage credit, financial technology, automated underwriting, housing prices, credit supply, house price comovement

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1 Introduction

With advances in technology, automated decision making has become increasingly prevalent across credit markets. In the U.S. mortgage market, automated underwriting is currently used for the majority of applications. In 2023, around two thirds of U.S mortgage originations were underwritten using Fannie Mae's Desktop Underwriter (DU) system or Freddie Mac's Loan Prospector (LP) system, including many loans that were not directly sold to Fannie or Freddie. In this paper, we study the real implications of lending automation by looking back to the mid-1990s when Fannie and Freddie first released their automated underwriting systems. The systems may have had consequences beyond processing improvements. Automation also facilitated implementation of more complex, statistically-informed underwriting rules. Both new rules and increased consistency of rules across lenders can have important real effects. A large expansion in lending standards can potentially contribute to housing boom and bust episodes. When a small number of systems are widely used, rule updates also propagate nationally in a short period of time, potentially increasing comovement and systemic risk.

To quantify these effects, we use a difference-in-differences research design that leverages heterogeneity in adoption timing of the DU and LP systems. We collect names of early adopters from contemporaneous news articles, match these to mortgage data and also use trade journals as a source of narrative evidence on usage of the systems. We take advantage of initial differences between the DU and LP systems to separately quantify the effects of processing and lending algorithms. Freddie rolled out a new set of statistically-informed rules with LP, leading to early adopters using different rules from other lenders selling to Freddie (Maselli (1994); Straka (2000)). In contrast, early versions of Fannie's DU system automated the application of its existing manual rules. Early adopters of DU used an automated process but continued to apply the same rules as other lenders selling to Fannie. Lenders' choice of system was determined by pre-existing selling relationships with Fannie or Freddie, mitigating concerns about strategic selection of a system conditional on early adoption. We find that adoption of Freddie's LP system had a large effect on local house prices by substantially expanding credit access.

Narrative evidence indicates that the shift in standards involved a large increase in the maximum loan amount for many borrowers, by reducing the emphasis on mortgage payment-to-income ratios. We find support for this in the data, showing a large increase in lending at high loan-to-income ratios by early LP adopters relative to early DU adopters in the same zip code. Using Home Mortgage Disclosure Act data, we find that lenders increased high loan-to-income lending by 17% after adopting LP. This increase was broad-based across the borrower income distribution.

First, we present, to our knowledge, the first causal estimates of the effect of automated mortgage underwriting systems on loan processing time. We find a statistically significant but modest reduction in processing time. The time from application to closing declines by about 2 days, with a larger reduction for denied applications.

To estimate a house price response we use variation in zip code exposure to initial LP adopters. We find that a one standard deviation increase in market share of LP users (4.4 pp) leads to a cumulative house price increase of 1.8 per cent by 1996 and 3.9 per cent by 2000. The long-run response in particular is large relative to the implied increase in borrowing capacity. Using a simple model we show that the expansion in the effect over time could be explained by adaptive expectations feedback (as has also been documented in Armona, Fuster and Zafar (2019), Bailey, Cao, Kuchler and Stroebel (2018), and Case, Shiller and Thompson (2012)). After observing elevated recent price growth, market participants expect future price growth to be higher and this lowers their perceived user cost, raising demand.

Finally, we study the effect of adoption on house price growth correlation. When two lenders adopt the same underwriting system, their lending standards should become more similar. Both lenders also roll out future changes in standards at the same time. To test this, we compute two co-herfindahl indexes to measure lending integration for each county pair (Landier, Sraer and Thesmar, 2017). The first treats each lender as distinct. The second aggregates initial adopters of the same system (LP or DU) as a single "standards group". The difference between these two indexes is a measure of the potential increase in lending integration between two counties occurring when the automated system is rolled out. We find that both lending and house price correlation increase with adoption, in line with an increase in standards integration.

Our paper contributes to two main streams in the literature. First, a growing literature shows that technology can have important real effects on credit allocation, for example, through improved statistical modelling techniques (Fuster, Goldsmith-Pinkham, Ramadorai and Walther, 2022), additional data sources that can help thin credit file borrowers (Berg, Burg, Gombović and Puri (2019); Lee, Yang and Anderson (2025); Di Maggio, Ratnadiwakara and Carmichael (2022); Blattner and Nelson (2021)) and by reducing information frictions (Petersen and Rajan (2002); Jiang and Zhang (2025); Jansen, Nguyen and Shams (2025)) and biases (Howell, Kuchler, Snitkof, Stroebel and Wong, 2024).

In our setting, statistically-informed rules expand credit access, but push up the value of the underlying asset by increasing demand when supply is inelastic. This can partly offset anticipated gains in affordability and access. We argue that this direct effect is then further amplified by adaptive expectations, creating a localized housing boom. We also show that automated underwriting makes lending and house price growth more highly correlated across locations, as lenders apply increasingly similar rules to each other. This is related to the idea that banking integration leads to increased comovement (Landier et al. (2017)), but with the further implication that common technology usage can make lending highly correlated even among a large number of small lenders.

Several papers study the effect of fintech on convenience and processing speed in lending and real estate (Fuster, Plosser, Schnabl and Vickery (2019); Buchak, Matvos, Piskorski and Seru (2018); Berg, Fuster and Puri (2022); Buchak, Matvos, Piskorski and Seru

(2020)).[1] We show that automated underwriting primarily affected the housing market through an accompanying change in lending standards and their coordinated transmission, rather than processing advantages directly.

Other papers studying real effects of mortgage technology include Gao, Yi and Zhang (2023), Lewellen and Williams (2021) and Foote, Loewenstein and Willen (2019). Foote et al. (2019) provide a narrative overview of the history of technological innovation and AUS adoption in the mortgage market during the 1990s. They argue that the relationship between mortgage size and income gradually weakened nationally during the 1990s, and suggest this implies a relative expansion for lower income households.[2]

Unlike our paper, Foote et al. (2019) find that underwriting technology did not affect house prices. The main source for this difference is that our paper leverages lender-level variation in exposure to underwriting technology. Using a difference-in-differences approach, we find that the credit expansion induced by automated underwriting adoption was broad-based across the income distribution and had a large effect on house prices. We also show that after accounting for gradual adoption by lenders, the aggregate rise in systems usage coincides with the start of the national housing boom.

Second, we contribute to the mortgage and housing literature connecting lending standards and house prices, often in the context of the causes of the 2000s housing boom.[3] Greenwald (2018), Greenwald and Guren (2025) and Kaplan et al. (2020) specifically model the effect of a sudden relaxation of payment-to-income limits in the late 1990s, but do not provide a fundamental explanation for that shift. Davis, Larson, Oliner and Smith (2023)

[1] The effect of automated underwriting on processing is a related but distinct question (for example, automated underwriting is also used by lenders who do not offer fully online applications).

[2] Gao et al. (2023) use an FHA underwriting change applied to high-risk applicants to study effects on financial inclusion and relocation. Lewellen and Williams (2021) examine the effects of the Mortgage Electronic Registration System (MERS) that reduced the cost and time associated with secondary mortgage sales. They show that the introduction of MERS led to an expansion in mortgage credit supply that was primarily fueled by non-bank lenders originating mortgages to low-income borrowers.

[3] Adelino, Schoar and Severino (2016); Foote, Loewenstein and Willen (2020); Mian and Sufi (2009); Favilukis, Ludvigson and Van Nieuwerburgh (2017); Greenwald (2018); Greenwald and Guren (2025); Justiniano, Primiceri and Tambalotti (2019); Kaplan, Mitman and Violante (2020); Favara and Imbs (2015); Acharya, Bergant, Crosignani, Eisert and McCann (2022); Di Maggio and Kermani (2017); Adelino, Schoar and Severino (2025); Loutskina and Strahan (2015); Johnson (2020).

also show that mortgage risk had already risen in the 1990s. In this paper we argue that the acceleration of automated underwriting adoption during the late 1990s refinancing boom provides a natural explanation for this shift in payment-to-income limits. Combining our difference-in-differences estimates with national system usage statistics we argue in Section 4.6 that automated underwriting adoption can explain a large share of house price growth in the boom.[4]

2 Institutional background

2.1 Early automated underwriting systems and their usage

In the early 1990s, most mortgage applications were manually underwritten using guidelines set out by lenders, Fannie and Freddie, or other secondary market participants. This reliance on human underwriters posed personnel challenges when dealing with large fluctuations in application volumes, for example during refinancing booms (Straka, 2000). Manual underwriting also arguably limited the time available to spend on difficult files. Automation was, therefore, expected to deliver benefits to both lenders and borrowers.

Automated systems also facilitated implementation of complex rules. Historically, lenders had based their underwriting rules on direct experience – for example, observed poor performance of loans with low down payments (Straka, 2000). Subsequent increases in standardization, data availability and computing power facilitated sophisticated statistical analysis of the determinants of mortgage default. The systems not only made these complex, statistically-informed rules easier to apply, but they also allowed for proprietary algorithms. A lender could use another party's system without observing the underlying rules directly.

[4]Our 1990s empirical setting predates much of the reduced-form work examining the housing boom. Our sample period overlaps with Favara and Imbs (2015), who argue that bank branching deregulation was an important driver of house price growth in the late 1990s and early 2000s. The adoption of automated underwriting may have affected an even larger part of the market. The removal of branching restrictions only directly affected commercial banks, but lenders of all types adopted Fannie and Freddie's systems.

Freddie Mac was at the forefront of loan performance analysis and incorporated new statistical rules when it developed its Loan Prospector system. Early in 1994, a number of lenders participated in a pilot program, and, in 1995, LP was publicly released. Fannie Mae's system Desktop Underwriter was also piloted and released along a similar timeline. Although DU had the potential to reduce processing time, it was initially not statistically based. Instead, the system simply applied Fannie Mae's existing manual guidelines (Straka (2000); Nixon (1995)).[5,6]

During our sample period, Fannie and Freddie's systems were used to determine eligibility and did not provide lenders with a detailed risk measure (Temkin, Johnson and Levy, 2002).[7] For most applications, Loan Prospector generated one of only two recommendations "accept" and "caution". Loans with a "caution" recommendation could be found to be eligible but would need to be manually underwritten.[8] Risk-based pricing was rare in the 1990s, with most lenders using average cost pricing and charging all their borrowers the same rate. The market was segmented into prime and subprime lenders, with subprime lenders charging higher rates to all borrowers reflecting higher average risk. Specialized subprime lenders showed little initial interest in Fannie and Freddie's

[5]Some large lenders and mortgage insurers also developed systems around this time. The Countrywide Loan Underwriting Expert System (CLUES) was one of the earliest systems used on a large scale and was rolled out in 1993. The rules used by CLUES were developed not through statistical analysis, but by observing the decisions of expert underwriters. Countrywide started developing the system in 1991 with the primary goal of increasing the number of loans per employee – not changing lending standards (Talebzadeh, Mandutianu and Winner, 1995). PMI Mortgage Insurance Co. had been working on its Automated Underwriting Risk Analysis (AURA) system since the 1980s. Unlike CLUES, the system was statistically based and generated a risk score between 1 and 100 (Mikel and Baker, 1992).

[6]The early 1990s was a period of relaxation of lending standards. One example was the "GSE Act", a 1992 law mandating that the GSEs help promote credit access and homeownership opportunities for low-income households. For more information, see (Bhutta, 2009).

[7]Freddie Mac was also active in promoting the use of FICO scores around the same time, publishing a study in 1992 showing that general FICO scores had substantial predictive power for mortgage default. While general credit scores ultimately became input into Loan Prospectors' recommendations, Freddie Mac also strongly recommended the use of credit scores to lenders using manual underwriting. The take-up of credit scores as an underwriting input during this period was, therefore, not unique to lenders using automated underwriting systems. Fannie Mae also soon followed suit in recommending the use of FICO scores in manual underwriting (Pierzchalski, 1996).

[8]For government loans (i.e. FHA or VA loans) Loan Prospector generated either an "accept" or "refer" recommendation. Loans receiving a "refer" recommendation also needed to be manually underwritten in order to be eligible (Temkin et al., 2002).

systems because they were thought to be less applicable to high-risk borrowers. Temkin et al. (2002) provide an extensive discussion of Fannie and Freddie's eventual move into subprime lending and the potential of their systems to be used for risk-based pricing. These developments occurred after the end of our sample period.

DU and LP were marketed as general underwriting tools, and lenders also used them for loans they did not intend to sell to Fannie or Freddie. Some lenders reported running every application through LP or DU, and then manually underwriting applications that were not accepted (LaMalfa (1998); LaMalfa (1999); LaMalfa (1999); Jones (1997)). Fannie and Freddie's underwriting guidelines represented an industry standard that was widely used for both portfolio loans and loans sold to other secondary market participants. This gave Fannie and Freddie's systems a competitive advantage. According to Dennis and Robertson (1995):

> *"To a great extent, the underwriting guidelines of both Fannie Mae and Freddie Mac are the core standards that most lenders attempt to follow. Even those lenders who don't intend to sell loans to these two secondary mortgage market players should attempt to follow these well-conceived underwriting guidelines." (pp. 116-117)*

The ability to automatically certify that a loan met these standards was valuable to lenders. An "accept" recommendation from the Fannie or Freddie's systems could arguably be taken as a general indication that the loan was prime. While competing systems were able to automatically underwrite loans using Fannie or Freddie's public manual rules, the ability to sell certain loans was tied to the proprietary automated rules, and therefore to the use of Fannie or Freddie's own underwriting systems.

2.2 Effect on lending standards

Freddie's statistical approach to lending standards had the potential to expand credit access without a substantial increase in default risk (Maselli, 1994). Credit access could in principle be expanded in a low-risk way by optimizing underwriting cutoffs and allowing for more complex interactions of risk factors. An automated system made these interactions of risk factors easier to apply, and removed the need to directly disclose underwriting rules.[9] Unlike Freddie's manual rules, which were public, LP applied a proprietary algorithm. Our understanding of how standards changed therefore relies partly on narrative evidence from lenders who used the systems.

Lenders' comments to trade journals point to an expansion in the allowable ratio of debt payments to income subject to other risk factors. This expansion was first noted in 1994 by lenders participating in the LP pilot (Maselli, 1994). Harney (1996) reports that the system accepted debt-to-income ratios up to 72 per cent, at a time when manual underwriting guidelines typically limited the debt-to-income ratio to less than 36 per cent (Maselli (1994), Irwin (1992)), with some discretion (the back-end debt-to-income ratio is defined as the ratio of monthly debt payments and other financial obligations to gross monthly income).[10] Both Harney (1996) and Maselli (1994) suggest that LP eligibility at high debt-to-income ratios was initially limited to borrowers with offsetting factors, such as good credit, a large down payment or substantial cash reserves. In contrast, by 1999 high debt-to-income ratios were allowed in a much broader range of cases by both Fannie and Freddie. Appendix Figure A.1 shows the debt-to-income distribution of loans acquired in 1999 by both Fannie and Freddie (1999 is the earliest year for which this dataset is available). Although mortgage data from the mid-1990s are not detailed enough to back out precise changes in underwriting standards, in Section 4 we document responses consistent with a large relaxation of debt-to-income limits.

[9] More information on the performance of scorecards can be found in Straka (2000), Gates, Perry and Zorn (2002) and Foote et al. (2019).

[10] For context, around 7 per cent of 2018 HMDA applications had a back-end debt-to-income ratio above 60 per cent (the ratio is top-coded at 60 per cent), with most being denied. We expect that a limit of 72 per cent would not be binding in most cases. In contrast, around 58 per cent of 2018 HMDA applications had a debt-to-income ratio above 36 per cent. These statistics are based on the subset of HMDA applications for which the debt-to-income ratio is known. We use 2018 data as information on the debt-to-income ratio was not collected for prior years.

2.3 Adoption timing

Table A.1 provides statistics on lenders' usage of Fannie and Freddie's systems over time. In the first half of 1997, less than a quarter of eligible loans were processed using DU or LP (Foster, 1997). Adoption increased substantially for both systems after 1997, coinciding with a refinancing boom. Historically, lenders needed to hire a large number of additional underwriters to cope with refinancing demand, so automation offered greater benefits during these periods (according to Talebzadeh et al. (1995), Countrywide's development of CLUES was motivated by a 'serious shortage of qualified underwriters' during the previous refinancing boom).

Why were lenders slow to adopt Fannie and Freddie's systems? Trade journals highlight a number of concerns that lenders had. Even the initial DU and LP users we focus on here acknowledged it would take some time for the gains to be realized. For example, a representative of Flagstar Bank noted that "It isn't cheap: there are transaction costs, equipment costs, training costs. And there's a learning curve. The efficiencies are starting to materialize now" (LaMalfa, 1996). A representative of InterFirst stated, "We love LP, but it's still not cost-effective" (LaMalfa, 1997). Another lender noted that after licensing and usage fees Loan Prospector "doesn't appear to net any cost saving", with the caveat that "the Freddie Mac and Fannie Mae processes can ultimately decrease your cost in volatile periods" (LaMalfa, 1996). In November 1995, the per loan cost of a Loan Prospector approval was around $400 (Sullivan, 1995).[11] Fannie and Freddie also used proprietary data standards (Markus, Dutta, Steinfield and Wigand, 2008) and according to Oliver and McDonald (1997) lenders "did not make full use of AU systems (i.e., use AUS at the point of sale) owing in part to lack of integration with back-end systems".

Furthermore, Freddie would not accept a DU decision and Fannie would not accept a LP decision. This lack of reciprocity meant that lenders would need to run a loan through

[11] Sullivan (1995) provides some pricing details disclosed by Fannie and Freddie, but it is not possible to do a full cost comparison. Although Freddie's per loan fees were slightly higher, Fannie charged a number of additional fees that were not disclosed.

both LP and DU to compare pricing, paying double the fees (Foster, 1997). Many lenders felt it was too costly to use multiple systems, and either chose just one of the two systems or continued to apply Fannie and Freddie's manual guidelines, possibly using an alternative system (LaMalfa, 1997). Perceived costs of switching and the fact that the systems locked them into a buyer led to lenders having sticky relationships with either Fannie or Freddie (DeMuth (1999); Johnson (2020)). Switching costs may also have led some lenders to strategically delay adoption.

According to Fannie and Freddie's annual reports, usage of DU and LP stabilized at just over 60 per cent in 2001 (Table A.1). This is likely an underestimate of overall system usage and the prevalence of relaxed rules. Starting in the late 1990s, both Fannie and Freddie made agreements with some very large sellers to purchase loans underwritten using other systems. Data on Fannie and Freddie's acquisitions available starting in 1999 show that high debt-to-income loans continued to be purchased from the sellers with whom agreements had been reached to accept alternative systems. Prior to these agreements, Freddie Mac was predicting higher stabilized LP usage of 80–85 per cent. Reported usage of DU and LP by larger community banks in 2004 was also around 85 per cent – consistent with earlier forecasts (Costanzo, 2004).

3 Data

We use data on mortgage lending from the Home Mortgage Disclosure Act (HMDA). HMDA provides fairly comprehensive coverage of the U.S. mortgage market, particularly for properties located in metropolitan areas. The dataset includes mortgage originations and loan purchases, as well as applications that did not lead to an origination. We also use confidential supervisory data collected under the HMDA to construct a measure of processing time from application dates and closing/decision dates. The confidential dataset contains more comprehensive information than what is disclosed in the public version of the data. We use FHFA zip code price data to study the effect on house prices.[12]

3.1 Lender treatment and statistics

Table 1 lists initial users of Loan Prospector or Desktop Underwriter. These lenders were already using the systems at the time of public release in 1995. We identify HMDA IDs associated with these lenders based on their names and locations provided in the source articles. We also use NIC data on institutional relationships and transformations to ensure that the set of HMDA IDs we use for each lender captures as consistent an institution as possible over our sample.

Because DU initially applied Fannie's manual rules, we can use initial users of DU as a control group to quantify the effect of adopting statistical lending standards on credit on house prices. As both groups of lenders adopted a system at the same time, the direct effect of automation is differenced out. Any remaining selection concerns relate to the choice of LP relative to DU, rather than the decision to adopt a system early. Below we show that the choice between LP and DU is driven by relationships lenders had with Fannie or Freddie before the development of the systems, rather than anticipation of the different rules applied by LP.

Table 2 reports estimates from a linear probability model that relates the choice of the lender's system to lender characteristics. Each variable is divided by its standard deviation. The main difference between initial LP and DU users is that lenders choosing LP sold a much larger share of their loans to Freddie prior to the release of the two underwriting systems. This is consistent with research conducted by Mortech in 1996 that "revealed that AU decisions are primarily based on which GSE the lender does the most business with" (Strickberger, 1999).[13] Coefficients on other variables, including the loan-to-income

[12]We also explored other house price indices but found them to be less reliable and with lower effective coverage than the FHFA house price indices during the 1990s.

[13]Consistent with this, a representative of Fleet Mortgage stated in 1999 that: "It is impractical for us to have two AU systems" and "we elected to go with DU first. We typically sell about 60 per cent of our business to Fannie Mae. That had a lot to do with it." (DeMuth, 1999).

ratio, are insignificant.

To estimate effects on processing time, we use a set of matched control lenders. The matching procedure targets the following variables: loan purchases as a share of total purchases and originations; refinance originations as a share of total originations; portfolio originations (not sold in the year of origination) as a share of total originations. We also require matched lenders to be in the same broad size class based on total originations and loan purchases. We use HUD's classification of prime and subprime lenders to ensure matched lenders are of the same type. We match without replacement and select matches based on the Mahalanobis distance.

Table A.2 compares DU and LP users with matched control lenders. We see that the initial share sold (directly) to Fannie or Freddie is positive and significant correlated with the probability of being DU users. However, there is no significant difference in the portfolio loan share suggesting that initial DU users do not sell a larger share of loans in general. Coefficients on other variables are insignificant. In the case of LP users, there is no significant relationship with any of the lender characteristics.

3.2 ZIP exposure and statistics

In Section 4.3, we use variation in zip code exposure to initial adopters of Loan Prospector to study the effect on house prices:

$$LP_z = \frac{\text{\# Loans reported in zip code } z \text{ by LP lenders (Table 1; Column 1)}}{\text{\# Loans reported in zip code } z \text{ by all HMDA reporters}} \tag{1}$$

We report estimates conditional on total exposure to early adopters of either Loan Prospector or Desktop Underwriter:

$$AUS_z = \frac{\text{\# Loans reported in zip code } z \text{ by all lenders in Table 1}}{\text{\# Loans reported in zip code } z \text{ by all HMDA reporters}} \tag{2}$$

Conditioning on AUS_z is consistent with the loan-level analysis where we compare loans made by initial LP users with initial DU users (rather than all other lenders). We compute market shares (1) and (2) using data from 1993 – before lenders started using the systems.

Figure 1 shows the distribution of the 5-digit zip code market share of initial Loan Prospector users conditional on control variables we include in our regressions (interacted with time dummies where appropriate). Table 3 shows the relationship between exposure to early Loan Prospector users and zip code characteristics. Both dependent and independent variables are normalized by dividing by the standard deviation. Note that there is a mechanical positive relationship with the combined market share of early LP and DU users (AUS_z). We condition on this to strengthen identification. We also expect a positive relationship with the overall zip code share of loans sold to Freddie given how lender-level system choices are driven by pre-existing relationships. While it's not clear that the general Fannie/Freddie relationship would drive correlations with other zip code outcomes, we conservatively condition on these shares anyway.

We show two sets of house price estimates: conditional on census division by year fixed effects and conditional on 3-digit zip code by year fixed effects. Figure A.2A shows residualized zip market shares of initial LP users in the New England census division as an example. Figure A.2B shows residualized zip market shares of initial LP users in 3-digit zip codes that overlap the Dallas-Fort Worth-Arlington MSA. While identification arguably may be stronger when comparing a small set of physically close zip codes, this strategy raises other concerns. The close substitutability of nearby properties may restrict how much prices can diverge across these locations in equilibrium, leading to responses being understated.

Although there are correlations with some variables in Table 3 when we use division fixed effects, conditional on these variables, we see no evidence of different house price trends prior to adoption. This is consistent with our claim that additional price growth in the mid-1990s is due to early adoption of new statistical underwriting criteria, not due to other characteristics of these lenders or the areas in which they operate. For example, if affected lenders were, in general, more aggressive, their locations may be expected to experience stronger price growth prior to system adoption.

4 Results

4.1 Credit response

In this section, we measure how the new rules rolled out with Loan Prospector influenced lending. Using HMDA, we compare loan characteristics of initial LP users with initial DU users before and after 1994, when these lenders started using the systems. This differences out the effects of automation alone. Comparing loans in the same 5-digit zip code in the same year, we find that initial LP adopters lend more relative to income after adopting the system. Because we compare loans in the same zip code at the same point in time, differences in credit outcomes are unlikely to be driven by differences in house price growth.

We follow lending outcomes up until 1997. As we move further in time from the press releases listing initial adopters, the less likely it is that these lenders exist in the same form throughout the HMDA sample. We create consistent institutions over the 1992-1997 period to account for mergers and acquisitions during that time.[14] DU also started to apply very similar rules to LP after 1997, which would affect the interpretation of our difference-in-differences estimates.

First, we look at the effects on high loan-to-income lending. We define $HighLTI_i$ equal to one if the loan-to-income ratio exceeds 2.5 and zero otherwise. We estimate the approximate proportional response using Pseudo Poisson Maximum Likelihood.[15] We include

[14] We start our sample in 1992, as it is the first year to use the 1990 census tracts. A consistent tract definition is important for the mapping to zip codes. In addition, several early adopters are not present in 1991 HMDA data.

[15] Unlike linear regression where the dependent variable is the log of the outcome of interest, Pseudo Poisson Maximum Likelihood gives consistent estimates in the presence of heteroskedasticity and avoids dropping observations where the outcome is equal to zero (Wooldridge (2010); Cohn, Liu and Wardlaw

ZIP5-year, income quartile-year and lender-income quartile fixed effects. For loan i reported by lender l in income quartile n in zip z in year t:

$$HighLTI_i = \exp\left(\alpha_{l,n} + \gamma_{n,t} + \delta_{z,t} + \sum_{k \neq 1993} \mathbb{1}_{t=k}\beta_k LP_l\right) + \epsilon_i \tag{3}$$

where LP_l is an indicator equal to one for early LP users and zero for early DU users. β_k is interpreted as the log change in number of loans between the base year (1993) and year k.

We focus on a loan-to-income ratio cutoff of 2.5, as this approximately corresponds to the manual underwriting debt-to-income cutoff of 36 per cent for a household with a relatively high level of existing debt payments. Setting the loan-to-income cutoff too high may miss the effect of the new rules on households with high other debt obligations, as these households have a lower loan-to-income ratio at a given debt-to-income ratio.[16] Figure 2A shows that the high loan-to-income loan share increases by around 17 per cent following adoption. Using linear regression we find a peak 3.4 percentage point increase in the high loan-to-income share of originations relative to an average high loan-to-income share of 16 per cent (Figure 2C).

To show how responses vary with borrower income, we further interact LP_l with indicators for each income quartile n and include ZIP5 by income quartile by year fixed effects. Low income households are not necessarily the only beneficiaries of expanding

(2022)).

[16]To inform the cutoff, we looked at data on components of the debt-to-income ratio from the mid 1990s. Property taxes in the 1995 Survey of Consumer Finances average around 1.3 per cent of the property value, and homeowner's insurance in the 1995 American Housing Survey averages around 0.45 per cent of the property value. The average 30 year mortgage rate in 1995 was around 8 per cent. At a loan-to-value ratio of 80 per cent, the debt-to-income ratio is then approximately:

$$\begin{aligned} DTI &= \frac{OtherObligations}{Income} + \left(\frac{1.3+0.45}{0.8} + 1200 \times \frac{\frac{0.08}{12}\cdot(1+\frac{0.08}{12})^{360}}{(1+\frac{0.08}{12})^{360}-1}\right)\cdot LTI \\ &= \frac{OtherObligations}{Income} + 8.805 \cdot LTI \end{aligned}$$

A household with an LTI of 2.5 will therefore have a DTI of around 36 per cent if their other financial obligations, such as debt payments and child support, are around 14 per cent of income. This is about the 90th percentile of other financial obligations for households in the 1995 Survey of Consumer Finances who bought their home in the last 5 years.

debt-to-income limits, especially where the expansion is conditional on other risk factors. If housing demand and down payments scale with income (and the mortgage is the primary source of debt) we might expect both the numerator and the denominator of the debt-to-income ratio to increase at a similar rate with income. In recent HMDA data the debt-to-income distribution is very similar for households in the bottom three-quarters of the income distribution, and high debt-to-income borrowing spans the entire income distribution (Appendix Figure A.3).[17] Consistent with this, we find that the response is not limited to lower income households (Figures 2B and 2D).

Figure 2E shows approximate percentage effects on the average loan-to-income ratio. Average loan-to-income increases gradually with a peak effect of 8%, and again we see similar responses across the income distribution, with the exception of the bottom income quartile which sees smaller effects (Figure 2F). Overall, we do not find evidence of larger effects for households in the bottom income quartile and conclude that the shift to statistical lending standards led to a broad-based increase in credit access. As all specifications compare loans made in the same year and in the same 5-digit zip code, we believe the differences are unlikely to be explained by differential house price or demand growth.

4.1.1 Additional tests

We report a number of additional results in the appendix. We look at effects on lending volumes, but are unable to use location fixed effects given that the location of the property is more frequently missing for a substantial share of HMDA loans prior to 1996.[18] We estimate the approximate percentage change in the number of loans made by lender l in

[17] Unfortunately debt-to-income was only added recently and is not reported for our sample period.

[18] Prior to 1996, depository institutions were only required to report locations for properties in MSAs where the lender had a home or branch office. This did not include offices of affiliates such as brokers or correspondents, or non-branch locations which accepted applications. Non-depositories were considered to have a branch office in any MSA where they had at least 5 reportable loans or applications for home purchase or home improvement in the previous calendar year, and therefore reported locations for a larger share of loans. From 1996 onward, banks or thrifts with at least $250 million in assets (or that were subsidiaries of a holding company with banking or thrift assets of at least $1 billion) no longer received exemptions from reporting property locations and location reporting improved considerably.

income quartile n in year t using Pseudo Poisson Maximum Likelihood:

$$\#Loans_{l,n,t} = \exp\left(\alpha_{l,n} + \gamma_{n,t} + \sum_{k\neq 1993} \mathbb{1}_{t=k}\beta_k LP_l\right) + \epsilon_{l,n,t} \tag{4}$$

Appendix Figure A.4A plots estimates of $\{\beta_k\}$ from Equation 4. Initial LP adopters start to grow originations relative to initial DU adopters in 1994. The cumulative log change is 0.8 by 1996, meaning the number of loans made by early Loan Prospector adopters was about 117 per cent higher in 1996 than 1993 (relative to the corresponding change for initial DU adopters). The response is large, but it could reflect initial LP users expanding at the expense of other lenders, and does not necessarily represent loans that would not otherwise have been made. Without location by year fixed effects, the response also reflects feedback effects from the housing market. The change in lending volumes is similar across income quartiles (Appendix Figure A.4B).

We also repeat our main analysis without conditioning on location, allowing us to include loans with missing location data (Appendix Figure A.5). The responses are qualitatively similar but larger. As with the volume response, this could reflect feedback effects from stronger house price growth to credit demand. This feedback channel is absorbed by ZIP5 $\times$ year fixed effects in our main specification.

4.2 Processing time response

We adopt a different empirical approach to quantify effects on processing time, as this requires comparing initial users of either system with other lenders (given that both DU and LP adopters should experience similar processing benefits). Comparing lenders who chose to participate in Fannie or Freddie's pilot programs with other lenders raises the possibility of selection. To mitigate this we construct a control group of three matched lenders for each initial DU and LP user. There is a large pool of potential control lenders to choose from. The matching procedure targets three variables with the goal of finding lenders with similar business models in 1993: the share of refinance loans, the share of originations held in portfolio and the share of loans that were purchased (rather than originated). We also only match to lenders in the same broadly-defined size class, based on the combined number of originations and loan purchases. For purchase application i in income quartile n submitted to lender l with action a taken in year t we estimate:

$$Time_i = \alpha_{l,n,a} + \gamma_{g(l),a,t} + \delta_{z,t} + \beta_0 AUS_l + \beta_1 DU_l \cdot Post_t + \alpha_1 X_i + \epsilon_i \tag{5}$$

where $Time_i$ is the number of days between application and closing for originated loans, and the number of days between application and denial for denied application. Actions a include origination and denial. Lender group $g(l)$ includes lender l as well as the three matched control lenders. AUS_l is an indicator equal to one for initial DU or LP adopters and zero for control lenders. $Post_t$ is an indicator equal to one for 1994-1997 (the sample period is 1992-1997). We condition on log loan amount and log income and exclude FHA and VA loans. In specifications with only originated loans we also include loan purchaser type fixed effects.

Table 4 shows estimates of β_1 from Equation 5. The time from application to closing/denial declines by 2-3 days relative to matched lenders. When we estimate Equation 5 separately for originated and denied loans we find a processing time reduction of 2 days and 1-2 weeks, respectively.[19]

The overall effect is smaller than the 9 day reduction in processing time for purchase applications to 'Fintech' lenders documented by Fuster et al. (2019), and considerably smaller than potential reductions from automated underwriting that were projected in the mid 1990s (Maselli, 1994). Fuster et al. (2019) focus on lenders with a fully online

[19]We also find a significant reduction in processing time for refinance denials, but not for originated loans. One possible reason is that borrowers arguably have more incentive to complete a purchase loan as fast as possible given deadlines associated with the property transaction, making it easier to detect an effect of automation.

application process, which is a different technology than automated underwriting (it is likely that most 'non-Fintech' lenders in their sample also use an automated underwriting system given the more recent setting).

Our historical setting is useful for measuring the effect of underwriting automation on processing time, despite the fact that more recent HMDA data provide information about whether an automated system was used and which system was used for a given loan. Most lenders now use automated underwriting to some extent and its usage for a given application is likely subject to considerable selection bias. To illustrate this, we re-estimate the processing time relationship with 2018-2019 HMDA data and replace AUS_l with an indicator for whether an automated system was actually used to underwrite application i (AUS_i).

Appendix Table A.3 shows that using an automated system is associated with a four day *increase* in processing time on average. Conditional on denial, processing time increases by 11 days. Conditional on origination, automation is associated with a 3 day reduction in processing time – similar to what we find in our historical setting. This difference could be because denials following automated underwriting are more likely to occur for reasons that emerge later in the process. To explore this further, we separately estimate the relationship between automated approval and automated non-approval in Columns 2, 4, and 6. In the case of loans that are ultimately made, if the system does not recommend approval this is associated with an additional 6 days to closing, perhaps because the loan then needed to go through an additional underwriting process. Use of an automated system is always associated with longer time to denial for denied loans, but this is more extreme for loans the system initially approved. This could be consistent with the systems being more likely to be used for loans that are not clear cut denials, and therefore take longer to deny.

Overall, using our historic setting, we find that AUS allows for faster denials and reduces total processing time for originated loans as well. However, the average reduction is less than one week, suggesting that the automated task likely accounts for a modest proportion of work and time needed to successfully close a loan.

4.3 House price response

Next we estimate the effect of the new rules applied by the automated systems on house prices. Given the substantial expansion in eligibility, we expect that house prices could rise along with system usage. To establish a causal relationship, we take a difference-in-differences approach using the fact that initially only LP applied new rules, with DU automating the application of existing rules. We compare 5-digit zip codes with different exposure to initial LP adopters within the same census division, conditional on the zip code characteristics in Table 3 interacted with year dummies:

$$\log(Price_{z,t}) = \delta_z + \gamma_{div,t} + \sum_{\substack{t=1990 \\ k \neq 1993}}^{2005} \mathbb{1}_{t=k} \left(\beta_k \frac{LP_z}{SD(LP_z)} + \alpha_k X_z \right) + \epsilon_{z,t} \tag{6}$$

where $\log(Price_{z,t})$ is the log of the FHFA county house price index and LP_z is the measure of zip code exposure to early LP adopters defined in Section 3.[20] We divide the exposure measure by its standard deviation, so the coefficient of interest β_k is interpreted as the cumulative house price response to a one standard deviation increase in exposure. We further show that we can detect a house price response even when we compare 5-digit zip codes within the same 3-digit zip code:

$$\log(Price_{z,t}) = \delta_z + \gamma_{ZIP3,t} + \sum_{\substack{t=1990 \\ k \neq 1993}}^{2005} \mathbb{1}_{t=k} \left(\beta_k \frac{LP_z}{SD(LP_z)} + \alpha_k X_z \right) + \epsilon_{z,t} \tag{7}$$

Figure 3 plots the cumulative house price response estimated using Equation 6. A one standard deviation increase in 1993 exposure to Loan Prospector (4.4pp) raises prices by around 1.8% by 1996 with a (cumulative) effect of 3.9% by 2000. We do not observe a

[20] Using Poisson regression instead has a negligible effect on the estimates.

significant pre-trend. To help address remaining concerns about correlations with other zip-level factors driving differential house price growth, we estimate Equation 7, which includes 3-digit zip code by year fixed effects. The disadvantage of this specification is that there is less variation in the exposure measure and housing is much more substitutable across these locations, likely reducing the divergence in prices that can reasonably occur due to our shock. Nonetheless, we still find a positive and statistically significant effect on house prices coinciding with adoption. Here we find a cumulative response of 0.7% by 1996 and 1.8% by 2000 (Figure 4).

In both specifications, the house price response grows for several years after the original shock. This expansion is much more pronounced when we use variation across less substitutable locations and is also followed by a relative contraction. The shape of the cumulative price response in Figure 3 looks like a (relative) boom-bust cycle. While some further relaxation in the rules applied by LP is possible and consistent with the credit analysis and narrative evidence, this seems unlikely to fully account for the magnitude of the long-run response. We believe price momentum is a plausible explanation, for example, through feedback channels such as adaptive expectations (Armona et al. (2019); Bailey et al. (2018); Case et al. (2012)). We explore this channel further in Section 4.5 below.

4.3.1 Additional tests

To test whether the effect is monotonic we plot the relationship between 1993-1996 house price growth and the zip code exposure measure conditional on a number of characteristics. The relationship between the exposure measure and house price growth looks broadly linear (Appendix Figure A.6).

We expect that adoption of automated underwriting has a smaller effect on housing permits and a larger effect on prices in supply inelastic areas (Glaeser and Gyourko (2005); Glaeser, Gyourko and Saiz (2008)). Table 5 explores the effect of interacting exposure to early LP adopters with supply elasticity. Firstly, we note that using historic FHFA repeat sales house price data limits the set of zip codes to higher population zip codes in denser areas. Columns 1 and 2 use decennial census data on owner-occupied home values to show the effect of expanding the sample to a broader set of zip codes. Both subsamples show large and statistically significant responses, but the response in the larger sample is smaller, consistent with the inclusion of less dense areas. Column 3 shows the interaction with Baum-Snow and Han (2024) elasticity measure (space) centered on a value of 1. Column (4) shows the effect on supply, as measured by occupied housing units. The interaction term with Baum-Snow and Han (2024) elasticity measure in Columns 3 and 4 shows a significant negative effect on FHFA house prices and a positive significant effect on the supply of occupied housing units, consistent with theory. Table A.4 shows that the magnitude of the housing shock itself does not vary with supply elasticity. Therefore, the differential price response by supply elasticity is more likely to be related to supply responsiveness than to different shock intensity.

4.4 Magnitude of Short-run House Price Response

To better compare our house price response with other studies, we estimate the short-run elasticity of zip code house prices to credit using zip code exposure to early LP adopters as an instrument. This requires us to estimate zip-level credit responses. Given changes to HMDA location reporting rules over our sample, we do not estimate the response of aggregate lending and instead focus on the average loan-to-income ratio and share of high loan-to-income lending. Table A.5 shows first and second stage regressions. A 1% increase in average loan-to-income leads to an 0.39% increase in house prices. A one percentage point increase in the share of loans with a loan-to-income ratio about 2.5 leads to a 2% increase in house prices. The magnitude of the response is somewhat larger than, but still broadly similar to, estimates of the response of house prices to mortgage credit obtained in other settings.[21]

4.5 Can Expectations Feedback Explain the Price Response Profile?

Next, we propose an explanation for our estimated long-term price response based on feedback to expectations and demonstrate its plausibility using a simple theoretical framework. We show that combining adaptive expectations with the short-run response can approximately replicate our price response profile. Intuitively, households in more exposed locations observe higher recent house price growth following system adoption. If these households naively extrapolate, past growth lowers their perceived cost of housing and increases housing demand on top of the initial direct effect of automated underwriting.

We assume that the price response up until 1995 is the direct effect of adopting automated underwriting. Starting in 1996, we update expectations according to an adaptive rule. We also estimate the housing supply response using the Census Building Permits Survey and incorporate this when updating house prices. By assumption, the house price response is zero up until 1993, and then matches the estimated price effect to 1995.

4.5.1 Housing demand

We assume nominal housing demand of unconstrained households with income y_i is given by:

$$P \cdot H_i = \frac{\alpha_i y_i}{\theta + r + \delta - g^e} \tag{8}$$

That is, each (unconstrained) household has a constant housing budget share equal to α_i. The denominator on the right hand side of 8 is the user cost: θ is property taxes and insurance as a share of property value; r is the mortgage interest rate, which we also assume is equal to the opportunity cost of home equity for convenience; δ is the depreciation rate; and g^e is expected annual house price growth.

[21] Favara and Imbs (2015) find an elasticity of house prices to loan-to-income ratio on impact of 0.12, with a peak elasticity of 0.2 two years out. Di Maggio and Kermani (2017) find an elasticity of house prices to loan amount of 0.33.

Next we compute aggregate nominal housing demand. For this, we also need to consider the demand of households who are constrained. We denote the observed housing budget share by $\hat{\alpha}_i$. For unconstrained households $\alpha_i = \hat{\alpha}_i$, but for constrained households $\alpha_i > \hat{\alpha}_i$. For simplicity we assume $y_i = \bar{y}\ \forall i$:

$$P \cdot H = P \sum_{i=1}^{N} H_i = N \frac{\bar{y} \frac{\sum_{i=1}^{N} \hat{\alpha}_i}{N}}{\theta + r + \delta - g^e} = N \frac{\alpha \bar{y}}{\theta + r + \delta - g^e} \tag{9}$$

From here on we use α to denote the aggregate observed housing budget share.

4.5.2 Using the short-run price response to back out the change in fundamentals

When Loan Prospector allows households to borrow more relative to their income, this increases α and, therefore, housing demand. We take our estimated short-run price response and use it back out the implied change in α for 'treated' locations. We distinguish between 'treated' locations ($\tilde{\alpha}$) and control locations (α). We start with the following decomposition of the change in log nominal housing demand:

$$\Delta \log(PH) = \Delta \log P + \Delta \log H \tag{10}$$

We then substitute our estimated short-run price and housing supply responses as follows:

$$\Delta_{1993-1995} \log(PH) = \hat{\beta}^{P}_{1995} + \hat{\beta}^{H}_{1995} \tag{11}$$

Where $\hat{\beta}^{P}_{1995}$ is from Equation 6 and $\hat{\beta}^{H}_{1995}$ is described when we discuss calibration below. Assuming that there is no short-run effect on the user cost, $\theta + r + \delta - g^e$, we use $\Delta \log(PH) = \Delta \log(\alpha)$ from Equation 9 combined with a calibrated value for $\alpha_{1993} (= \tilde{\alpha}_{1993})$ to obtain $\tilde{\alpha}_{1995}$. We then keep $\tilde{\alpha}_t = \tilde{\alpha}_{1995}$ permanently. That is, we assume our natural experiment generates a one-off permanent relaxation of borrowing constraints between 1993 and 1995 for the 'treated' group. We further assume that the 'control' group (initial DU users) shift to using the same lending standards as the 'treatment' group in

1998, consistent with narrative evidence.

4.5.3 House price growth expectations

Beginning in 1996, we update households' growth expectations. We assume that households form expectations at the start of year t about price growth during the year as follows:

$$g_t^e = \lambda \sum_{j=1}^{t-t_0} (1-\lambda)^{j-1} g_{t-j} \tag{12}$$

As we will be making a comparison with our difference-in-differences estimates, we next rewrite Equation 12 to separate out the part of expectations that comes from the 'treatment'. We denote expectations in the 'treated' location by $\tilde{g}_t^e$ and write it as a function of our estimated price response Effect_t (expressed as an annual percentage change):

$$\begin{aligned} \tilde{g}_t^e &= \lambda \sum_{j=1}^{t-t_0} (1-\lambda)^{j-1} g_{t-j} + \lambda \sum_{j=1}^{t-t_0} (1-\lambda)^{j-1} \text{Effect}_{t-j} \\ \Rightarrow \tilde{g}_t^e &= g_t^e + \Delta_t \end{aligned} \tag{13}$$

That is, expectations in the 'treated' location are equal to expectations in the 'control' location plus $\Delta_t = \lambda \sum_{j=1}^{t-t_0} (1-\lambda)^{j-1} \text{Effect}_{t-j}$. Recalling our estimated responses are cumulative log changes, we compute the annual percentage price response as:

$$\text{Effect}_t = e^{\frac{\hat{\beta}_t}{\hat{\beta}_{t-1}}} - 1 \tag{14}$$

4.5.4 Implied difference-in-differences house price response

Next, we compute the cumulative implied (difference-in-differences) effect on nominal housing demand relative to 1993:[22]

$$\Delta \log(PH)_t = \log\left(\frac{\tilde{\alpha}_t}{\alpha_t}\right) + \left[\log(\theta_t + r_t + \delta - g_t^e) - \log(\theta_t + r_t + \delta - g_t^e - \Delta_t)\right] \quad (15)$$

The first term on the right hand side of Equation 15 is the direct effect due to Loan Prospector adoption. This diminishes over time due to eventual adoption in the 'control' group. The term in square brackets reflects feedback to growth expectations. Finally, we combine Equations 10 and 15 with our estimated supply response to back out the cumulative log price response at the end of year $t \geq 1996$:

$$\Delta \log P_t = \Delta \log(PH)_t - \Delta \log H_t = \Delta \log(PH)_t - \hat{\beta}_t^H \quad (16)$$

4.5.5 Calibration

Growth expectations (λ, g_t^e)

Our goal here is to show that our estimated response profile *could* be generated from a one-off relaxation of constraints due to Loan Prospector adoption. We perform a search for the value of the expectations parameter λ that best matches our response profile using a least squares criterion. For each value of λ, we first compute g_t^e using the FHFA U.S. All Transactions HPI before computing $\Delta \log P_t$ as described above.[23] We find that $\lambda = 0.085$ best matches the data response profile.

[22] In 'treated' locations the cumulative log change in demand is: $\log y + \log(\frac{\tilde{\alpha}_t}{\tilde{\alpha}_{1993}}) + \log(\theta_{1993} + r_{1993} + \delta - g_{1993}^e) - \log(\theta_t + r_t + \delta - g_t^e - \Delta_t)$. In 'control' locations the cumulative log change in demand is: $\log y + \log(\frac{\alpha_t}{\alpha_{1993}}) + \log(\theta_{1993} + r_{1993} + \delta - g_{1993}^e) - \log(\theta_t + r_t + \delta - g_t^e)$. Taking the difference between treated and control locations gives Equation 15.

[23] When computing g_t^e using observed price data, we use a modified formula is needed so that weights sum to one when working with a finite price history: $g_t^e = \frac{\lambda}{1-(1-\lambda)^{t-t_0}} \sum_{j=1}^{t-t_0} (1-\lambda)^{j-1} g_{t-j}$. As we have national price data back to 1975, the difference relative to Equation 12 is not very large by the mid 1990s.

Non-growth user cost components (θ_t, r_t, δ)

We use data on property tax and insurance costs from the American Housing Survey to calibrate θ_t. We divide each household's total property tax and insurance costs by their property value and set θ_t equal to the average value for owner-occupiers. $\theta_{1993} = 0.018$ and is broadly similar over the sample period. We assume an interest rate (and opportunity cost of home equity) equal to the prevailing Freddie Mac interest rate on a 30-year fixed rate mortgage. This is 7.31% in 1993.[24] We assume an annual depreciation rate of $\delta = 0.02$.

Initial housing budget share (α_{1993})

We compute $\alpha_{1993} = \left(\frac{P \cdot H}{y}\right)_{1993} \cdot (\theta_{1993} + r_{1993} + \delta - g^e_{1993})$. We set $\left(\frac{P \cdot H}{y}\right)_{1993}$ equal to the 1993 AHS average house price to income ratio of 3.12. We use θ_{1993}, r_{1993}, δ and g^e_{1993} as described above. This gives us $\alpha_{1993} = 0.18$.

Housing supply response ($\hat{\beta}^H_t$)

We use county Census data on permits issued for new housing units to estimate the supply response using Equation 6 with $\log(Permits_{c,t})$ as the dependent variable and county level controls. We use building permits data as we only observe the outstanding housing stock at the 1990 and 2000 censuses. Figure A.8A shows an increase in the flow of building permits.[25]

We use the path of housing supply from Figure A.8B to calibrate the model. To obtain this we convert the flow estimates from Figure A.8A into the cumulative percentage change in implied total housing units. The cumulative increase in outstanding housing stock for a one standard deviation increase in exposure from 1993 to 2003 is 0.33%. Incorporating the county level price response of 1.2% for a one standard deviation increase in exposure, this implies a decade housing supply elasticity of around 0.3. While this

[24] The timeseries can be downloaded here: `https://fred.stlouisfed.org/series/MORTGAGE30US`.

[25] Figure A.7 shows the relationship between actual and imputed growth in housing units from the 1990 to 2000 censuses.

is lower than the MSA elasticities estimated in Saiz (2010); our period of analysis is also shorter than the 30-year period used there. We note that Baum-Snow and Han (2024) have estimated more modest supply elasticities from 2000 to 2010.[26]

4.5.6 Results

Figure 5 compares our estimated response from Figure 4 (solid line) with the response generated by applying adaptive expectations to the short-run effect (dashed line). We are broadly able to match the shape of the data response. Although the model generated response is conditional on a number of strong simplifying assumptions, we believe this exercise enhances the plausibility of our main estimates. It also illustrates how our large estimated long-run response could reflect a fundamental direct response combined with feedback through an expectations channel, consistent with Chodorow-Reich, Guren and McQuade (2024).

4.6 Effect of systems adoption on national house prices

We document a substantial relative house price boom in locations with greater exposure to initial Loan Prospector users. Assuming adoption by other lenders stimulated a similar house price response as we see for early adopters, automated underwriting technology could have played an important role in the start of the 2000s housing boom. Consistent with this, aggregate adoption rates from Fannie and Freddie's annual reports line up with the start of the national housing boom in the late 1990s (Figure A.9).

We combine aggregate adoption data from Fannie and Freddie with our difference-in-differences estimates to compute a back-of-the-envelope house price response from 1993 to 2005. A lender adopting in year t contributes to price growth between year t and 2005. The contribution is assumed to be equal to the cumulative price response to

[26] Baum-Snow and Han (2024) explore the elasticity of housing supply at the neighborhood level and find a region-level unit elasticity of 0.41. As neighborhoods become stronger demand substitutes, a shock affecting labor market opportunities in one location affects housing demand in a wider range of nearby areas. Our paper is more similar to Baum-Snow and Han (2024) than to Saiz (2010), given the time frame of our paper, and the focus on the local observed heterogeneity across neighborhoods.

automated underwriting adoption at horizon $h = 2005 - t$ estimated in Section 4.3. This assumes that later adopters trigger the same house price response as early adopters. For this exercise, we assume that the cumulative response is flat after 5 years (corresponding to 1998 for the early adopters on Figure 3). To incorporate the later rollout of similar expanded rules with DU, we compute annual changes in adoption assuming that DU did not apply the new rules prior to 1998. Then, starting in 1998, we incorporate DU usage statistics as well as LP. This leads to a large jump in adoption in 1998. Incremental adoption is negligible after 2001 and we assume it is equal to zero after 2004 (see Table A.1).

We find that gradual adoption of Fannie and Freddie's automated underwriting systems, implicitly including likely expectations feedback discussed in Section 4.5, can explain around half of cumulative price growth (for the zip codes included in our sample) from 1993 to 2005. If we restrict attention to the short-run price response to 1995, which is likely close to the direct effect, systems adoption explains around 20% of price growth from 1993-2005.[27]

The magnitude of our aggregate response is highly consistent with Greenwald (2018), who models the effect of an announced relaxation of debt-to-income limits from 36% to 58% occurring in 1998Q1, motivated by an observed relaxation of debt-to-income limits in mortgage data. In this paper, we argue gradual adoption of Fannie and Freddie's systems provides a plausible explanation for the relaxation Greenwald (2018) observes. Greenwald (2018) finds a resulting 21% increase in the price-to-rent-ratio to the peak of the boom in 2006Q1. To make an appropriate comparison with Greenwald (2018)'s model,

[27]We note that our back-of-the-envelope calculation relies on the strong assumption that that our difference-in-differences estimates can be applied at the national level. A primary concern with aggregation in our setting relates to the supply of funds. If lending standards are relaxed but the supply of funds is fixed, it is expected that aggregate interest rates will rise. If true, we might still expect to see divergence in local house prices due to differences in local lending standards, but with a smaller effect on national prices. Justiniano et al. (2019) consider this argument and note that the empirical facts suggest an expansion in the supply of funds over the housing boom period. Such an expansion of funds, for example due to securitization or a global savings glut, would allow changes in lending standards to have large aggregate effects on house prices. We also note that agency MBS has broad appeal both in the U.S. and internationally. A large expansion in GSE lending criteria need not necessarily imply a large increase in mortgage rates.

we compute the aggregate price response using only our short-run estimates to 1995, abstracting from likely adaptive expectations feedback. Under this assumption, gradual adoption of Fannie and Freddie's systems leads to a very similar increase in house prices of 18%.

4.7 House price correlation

If two lenders adopt an automated underwriting system and use it to underwrite all applications, the lending standards applied by these two lenders become the same as each other (and other lenders who use the same system for all loans). A change in underwriting rules then immediately propagates to all lenders using the system. Automated underwriting may therefore affect house price co-movement in a similar way to banking integration (Landier et al., 2017).

However, even in the absence of automated underwriting, Fannie, Freddie (and FHA) rules should themselves contribute to comovement. For example, if all lenders made loans in accordance with one of these three sets of rules, the result is similar to that of highly concentrated banking system, even if individual lenders remain small.

We argue that automated underwriting can lead to stronger integration in addition to this loan product effect. First, the manual underwriting process involved more discretion on the part of lenders, even for fairly standardized products. By removing this idiosyncratic component, automation would lead to stronger correlation. Secondly, if system rules are sufficiently favorable or efficiency gains are large, lenders are incentivized to apply the system beyond those loans to which they would have applied Fannie or Freddie's manual rules. We have direct narrative evidence that early adopters applied the systems broadly, including to portfolio loans (see Section 2). Furthermore, using post 2018 HMDA data, which contains information on system usage, we observe Fannie and Freddie's systems being used for loans that are not sold to them. Therefore, automated underwriting can directly increase correlation by reducing discretion and because it incentivizes lenders to apply a given set of rules more broadly.

As discussed in Section 2, early versions of Desktop Underwriter encoded Fannie's manual rules but early versions of Loan Prospector implemented new, statistically informed rules. It is likely that DU implemented new rules similar to LP by 1998, and certainly by 1999. In any case, it is possible that even early versions of DU could increase co-movement through discretion in manual underwriting and application to a broader set of loans (though the incentive for broader application would be limited to processing efficiencies). For this analysis, we therefore evaluate the effect of greater lender integration coming from either LP or DU.

Given the large number of pairwise zip code combinations, we focus on county correlations. Our dependent variable $C_{i,j,t}$ is correlation of annual house price growth between counties i and j for years t to $t+4$ (i.e. 5 years). We use 5-year non-overlapping windows covering 1984-2003, so each county pair has 4 observations, two in the pre period (1984-1988; 1989-1993) and two in the post period (1994-1998; 1999-2003).

We next measure the increase in integration between counties i and j implied by a set of lenders adopting the same automated underwriting system. We compute the co-Herfindahl measure of Landier et al. (2017), firstly, treating initial LP users as distinct lenders (S_i^k is the market share of lender k in county i):

$$H_{i,j}^1 = \sum_{k \in LP} S_i^k S_j^k + \sum_{l \notin LP} S_i^l S_j^l \tag{17}$$

and, secondly, treating initial LP users as if they were the same lender:

$$H_{i,j}^2 = (\sum_{k \in LP} S_i^k)(\sum_{k \in LP} S_j^k) + \sum_{l \notin LP} S_i^l S_j^l \tag{18}$$

We then take the difference between the two measures:

$$\Delta H_{i,j} = H_{i,j}^2 - H_{i,j}^1 = (\sum_{k \in LP} S_i^k)(\sum_{k \in LP} S_j^k) - \sum_{k \in LP} S_i^k S_j^k \tag{19}$$

We compute 19 using 1993 HMDA data and repeat it for early DU adopters.

To condition on integration arising from common loan products as discussed above, we also compute:

$$H_{i,j}^{p} = S_i^{Fannie} \cdot S_j^{Fannie} + S_i^{Freddie} \cdot S_j^{Freddie} + S_i^{FHA} \cdot S_j^{FHA} + \sum_l \tilde{S}_i^l \tilde{S}_j^l \qquad (20)$$

Where S_i^{Fannie} ($S_i^{Freddie}$) is the share of loans in county i with Fannie Mae (Freddie Mac) listed as the purchaser, S_i^{FHA} is the share of FHA loans in county i and $\tilde{S}_i^l$ is lender l's market share in county i computed using only loans not in Fannie, Freddie or FHA categories. This captures integration arising from common rules applied to Fannie, Freddie and FHA loans, and (potentially) common rules for other loans due to the presence of the same lender in multiple counties. We compute this measure for each time period.

We estimate for county i and county j (in Combined Statistical Area $a(j)$) and year t:

$$\begin{aligned} Corr_{i,j,t} = \alpha_{i,j} + \gamma_{i,a(j),t} + \beta_1 Post_t \cdot \Delta H_{i,j}^{LP} + \beta_2 Post_t \cdot \Delta H_{i,j}^{DU} \\ + Controls_{i,j,t} + \epsilon_{i,j,t} \end{aligned}$$

Where $Corr_{i,j,t}$ is the annual house price growth correlation between county i and county j for years t to $t+4$. The post indicator is equal to 0 for periods starting in $t = 1985$ to $t = 1989$ ($t = 1989$ includes house price growth up until 1993) and 1 for later years. Controls include the per capita income growth and population growth correlation between county i and county j in the time period starting with year t and the loan type integration measure discussed above. Standard errors are clustered by Combined Statistical Area pairs. All specifications include fixed effects for time period by first county by the CSA of the second county. So for example, when looking at determinants of the correlation between a particular county i in Houston-Pasadena CSA and a county j in Dallas-Fort Worth CSA, we use variation across counties within Dallas-Fort Worth (when paired with county i). We standardize the three control variables, so each coefficient is interpreted as a one standard deviation increase. We multiply $\Delta H^{LP}_{i,j}$ and $\Delta H^{DU}_{i,j}$ by 10,000. So if two locations i and j respectively contain two different lenders A and B each with a market share of 10%, who are both LP adopters, then $\Delta H^{LP}_{i,j} = 10,000 * 0.1 * 0.1 = 100$.

Column 1 of Table 6 shows estimates without controlling for loan product type integration, $H^{p}_{i,j}$. Both LP and DU-induced integration raise house price correlation from 1994-2003. A one unit increase in LP-integration raises house price correlation by 0.16 percentage points. As described above, in the case where each county contains one lender with 10% market share adopting LP, the implied increase in house price correlation is 16pp. A one standard deviation increase, 3.89, increases house price correlation by 0.62pp, comparable to a one standard deviation increase in per capita income correlation. In Column 2, we also control for the loan type integration measure. This has little effect on the coefficients on system integration. We find that a 1 standard deviation increase in loan product type integration increases house price correlation by about 1 percentage point. This is equivalent to an 1.5 standard deviation increase in per capita income correlation.

Columns 3-6 show effects on lending correlation. We construct aggregated county lending measures from the National Neighborhood Data Archive (Edlebi, Mitchell, Richardson, Meier, Chen, Noppert and Gypin, 2024). In Columns 3 and 4 the dependent variable is pairwise county correlation in loan-to-income growth (average county loan size divided by average county per capita income). In Columns 5 and 6 the dependent variable is the pairwise correlation in home purchase loan growth. We find that all lending integration measures (DU, LP and loan product type) increase the correlation of growth in average loan to income and the number of home purchase loan originations.

5 Conclusion

We use the 1990s rollout of Fannie and Freddie's automated underwriting systems to study the effects of automated mortgage underwriting on processing time, lending standards, house prices and price comovement. Automation also implied the adoption of more complex, statistically-informed underwriting rules. Freddie Mac's system Loan Prospector allowed households to take on larger mortgage payments relative to their income, and Fannie Mae later incorporated similar rules into Desktop Underwriter. We show that locations with early exposure to these rules experienced an early housing boom starting around 1995. Using a simple model, we show that the dynamic house price response to system adoption is consistent with an adaptive expectations feedback channel amplifying the direct effect of standards relaxation.

Overall, we find that automated underwriting systems do not simply streamline processing, but can lead to sudden and highly correlated rollouts of underwriting rules, contributing to housing boom (and potentially bust) episodes, and increasing house price co-movement across locations where lenders use the same systems. This has important implications. Finally, extrapolating from our setting, we note that the broader adoption timeline of Fannie and Freddie's systems (and coincident relaxation in lending standards) lines up with the start of the 2000s housing boom.

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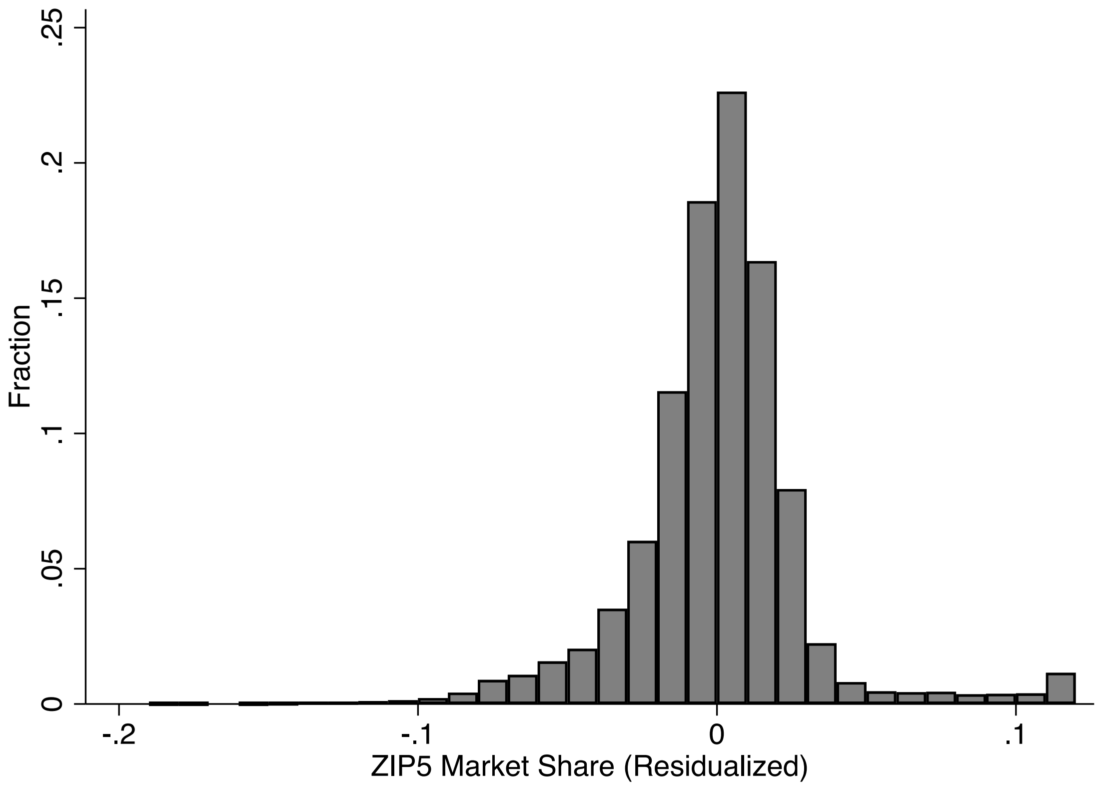
FIGURE 1

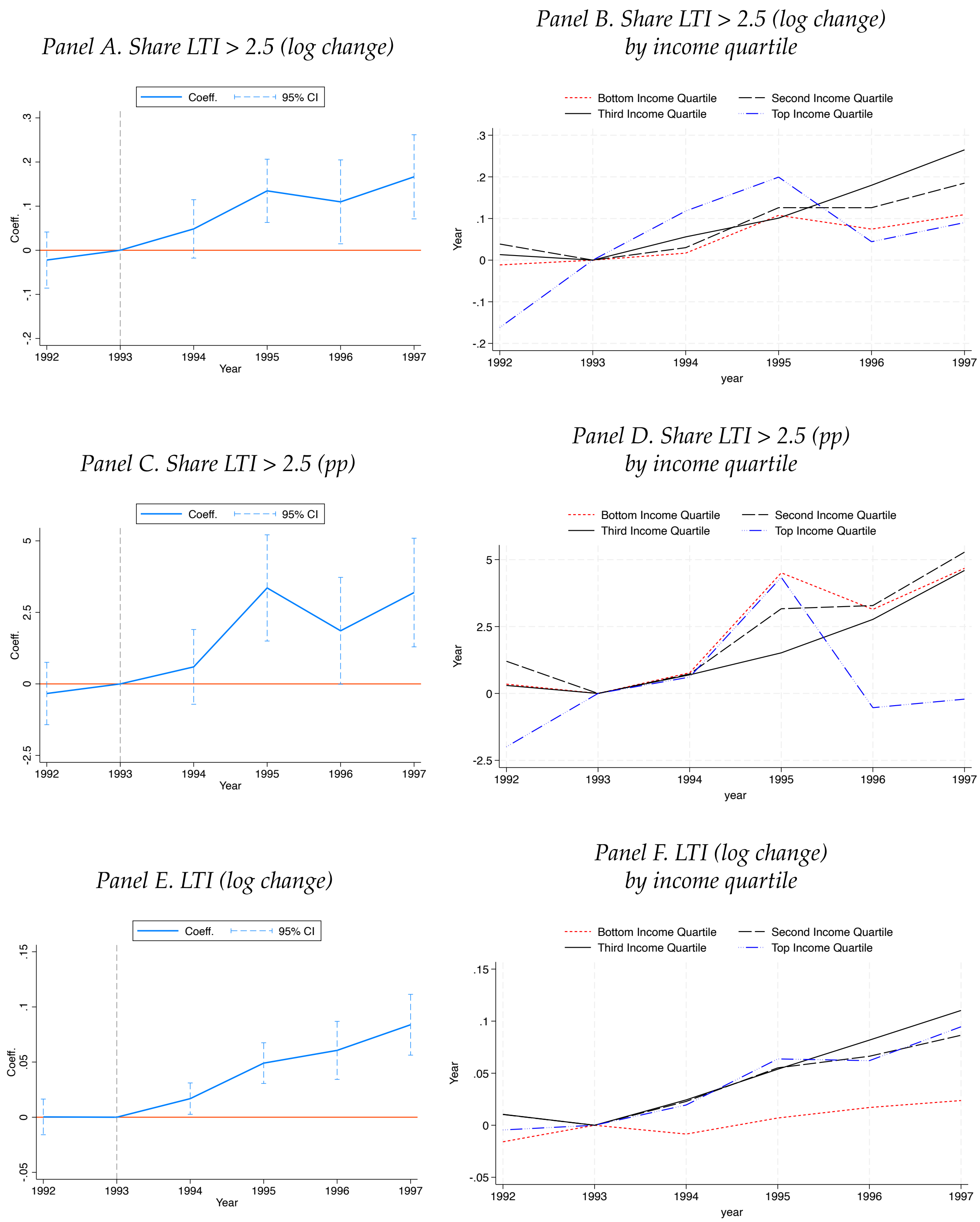
MARKET SHARE OF INITIAL LOAN PROSPECTOR USERS



NOTES: Figure 1 plots the residualized exposure measure. The raw exposure measure is: $EarlyLP_c = \frac{\text{\# Loans reported in zip } z \text{ by LP lenders in Table 1}}{\text{\# Loans reported in zip } z \text{ by all HMDA reporters}}$, computed using 1993 HMDA originations and loan purchases and includes both purchase and refinance loans. We condition on the following variables, at the zip code level unless otherwise indicated: (county) coastal indicator, number of lenders, large lender market share, thrift market share, commercial bank market share, share of originations sold to either Fannie or Freddie, share of originations sold to Freddie, log median household income, combined share of initial LP and DU users. The sample includes zip codes in metropolitan counties with non-missing house price data. The measure is winsorized at the 99th percentile. Sources: HMDA and authors' calculations.

FIGURE 2

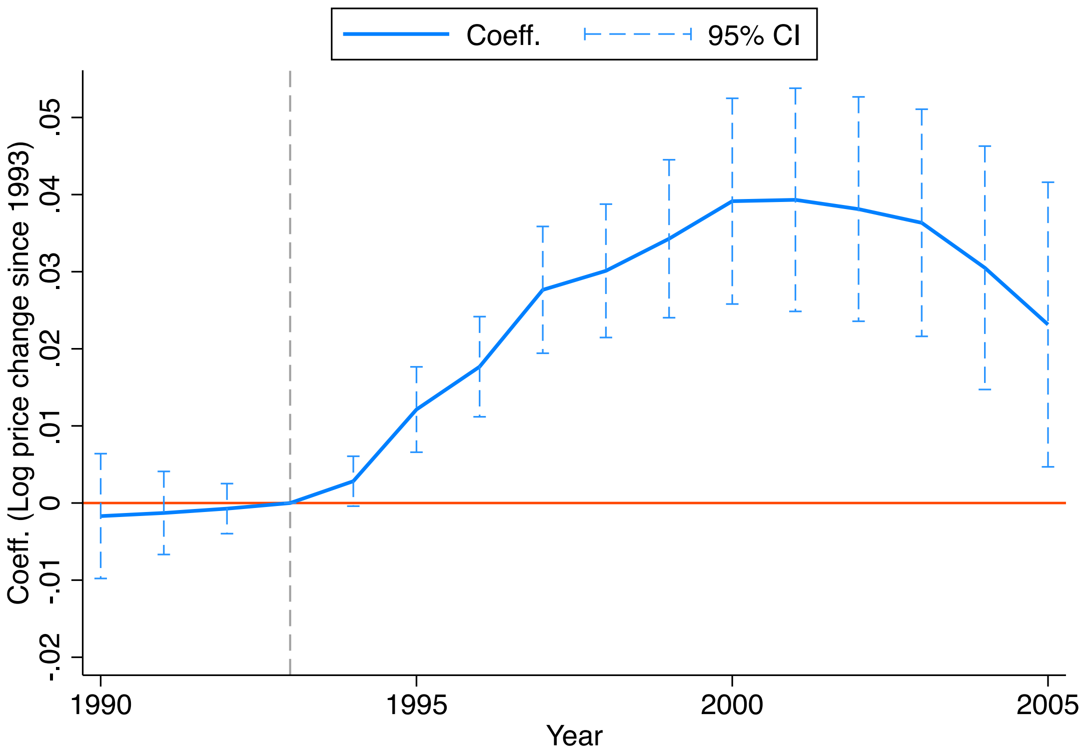
MORTGAGE CREDIT OUTCOMES FOR INITIAL LOAN PROSPECTOR USERS RELATIVE TO INITIAL DESKTOP UNDERWRITER USERS IN SAME ZIP5



NOTES: Figures 2A plots estimates of $\{\beta_k\}$ from Equation 3. The dependent variable is an indicator equal to one for originations with a loan-to-income ratio (loan size divided by income) above 2.5 and zero otherwise. Figures 2C plots the response of the share with LTI $>$ 2.5 from a linear specification. Figure 2E plots estimates of $\{\beta_k\}$ from Equation 3 where the dependent variable is the LTI ratio. Figures 2B, 2D and 2F plot the response by borrower income quartile. The sample is HMDA purchase and refinance originations reported by initial users of Loan Prospector or Desktop Underwriter. The coefficients are interpreted as changes relative to 1993. Standard errors are clustered by lender $\times$ income quartile. Sources: HMDA.

FIGURE 3

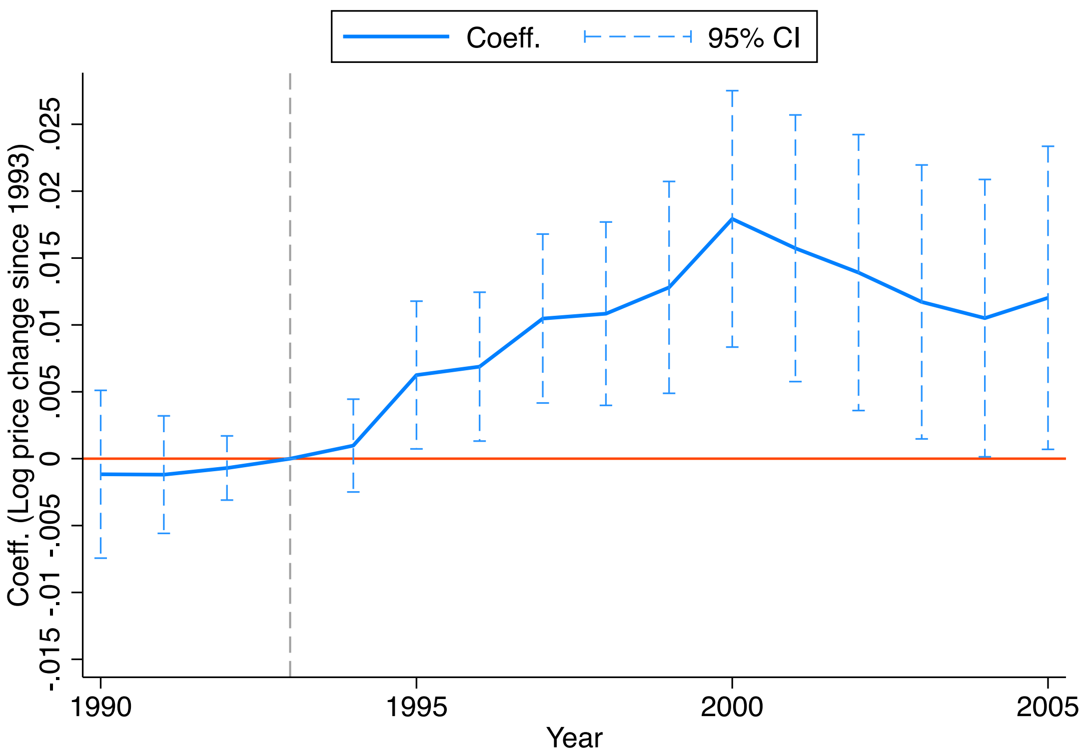
CUMULATIVE EFFECT OF LOAN PROSPECTOR ADOPTION ON HOUSE PRICES



NOTES: Figure 3 plots estimates of $\{\beta_k\}$ from Equation 6. We include census division by year fixed effects and condition on the following variables interacted with year dummies, at the zip code level unless otherwise indicated: (county) coastal indicator, number of lenders, large lender market share, thrift market share, commercial bank market share, share of originations sold to either Fannie or Freddie, share of originations sold to Freddie, log median household income, Saiz (2010) housing supply elasticity, the combined zip code share of initial DU and LP users $EarlyAUS_z$. The sample includes zip codes in metropolitan counties with FHFA house price data available continuously from 1990 to 2005. Coefficients are interpreted as cumulative log price changes relative to 1993. Standard errors are clustered by ZIP3. Sources: FHFA HPI; HMDA 1990 decennial census; BEA; NOAA list of coastal counties; and authors' calculations.

FIGURE 4

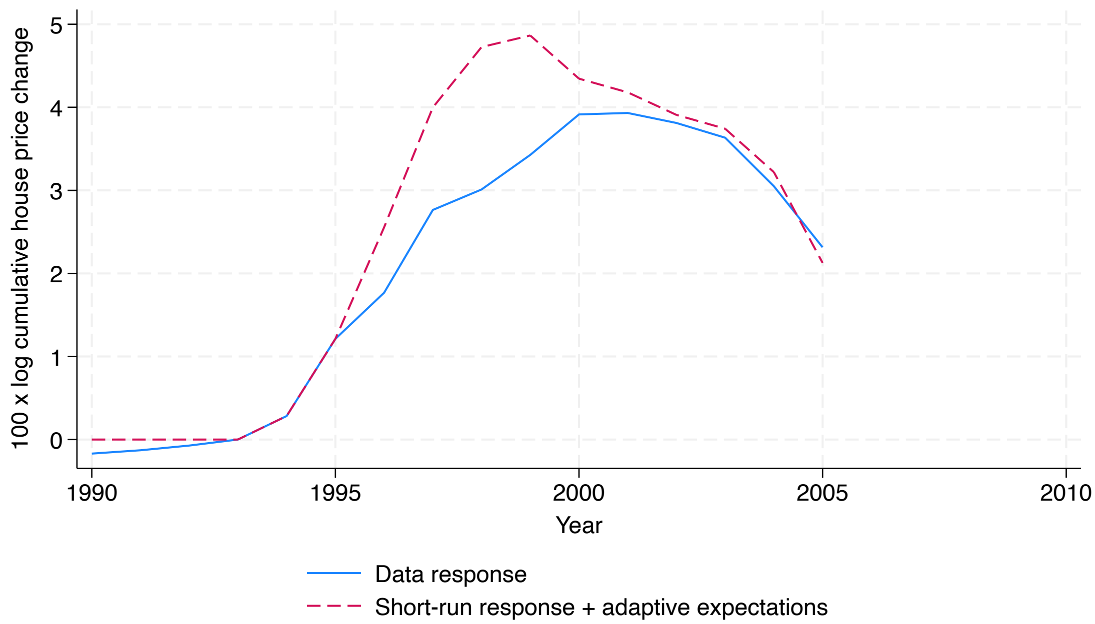
CUMULATIVE EFFECT OF LOAN PROSPECTOR ADOPTION ON HOUSE PRICES WITHIN LOCAL AREA



NOTES: Figure 4 plots estimates of $\{\beta_k\}$ from Equation 7. We include three-digit zip code by year fixed effects and condition on the following variables interacted with year dummies, at the zip code level unless otherwise indicated: (county) coastal indicator, number of lenders, large lender market share, thrift market share, commercial bank market share, share of originations sold to either Fannie or Freddie, share of originations sold to Freddie, log median household income, the combined zip code share of initial DU and LP users $EarlyAUS_c$. The sample includes zip codes in metropolitan counties with FHFA house price data available continuously from 1990 to 2005. Coefficients are interpreted as cumulative log price changes relative to 1993. Standard errors are clustered by ZIP3. Sources: FHFA HPI; HMDA 1990 decennial census; BEA; NOAA list of coastal counties; and authors' calculations.

FIGURE 5

EXPLAINING THE LONG-RUN PRICE RESPONSE WITH ADAPTIVE EXPECTATIONS



NOTES: The blue line in Figure 5 shows the data response from Figure 3. The red line plots the model price response implied by combining (1) the short-run estimated price response (to 1995); (2) adaptive expectations applied to the short-run response from 1996 onwards; (3) the housing supply response estimated from data on building permits over the entire sample (4) eventual AUS adoption in the 'control' group.

TABLE 1

INITIAL USERS OF FREDDIE MAC AND FANNIE MAE'S AUTOMATED UNDERWRITING SYSTEMS

Loan Prospector (Freddie Mac)	Desktop Underwriter (Fannie Mae)
Citicorp Mortgage	American City Mortgage Corp.
First Security Savings Bank (Flagstar Bank)	BancBoston Mortgage Corp.
Midland Financial Mortgages (Amerus)	BrooksAmerica Mortgage Corp.
Mission Hills Mortgage Corp.	Crestar Mortgage Corp.
Monument Mortgage	Crossland Mortgage Corp.
Mortgage America	Fleet Mortgage Corp.
Old Kent Mortgage Co.	Headlands Mortgage Co.
PHH Mortgage Corp.	ICM Mortgage Corp. (Pulte Mortgage)
Standard Federal Bank (InterFirst)	National Pacific Mortgage Corp.
	Phoenix Mortgage and Investment
	Residential Funding Corp. (GMAC-RFC)
	Seattle Mortgage Co.
	State Savings Bank
	Temple-Inland Mortgage Corp.
	Trustmark National Bank
	Universal American Mortgage Co.
	Washtenaw Mortgage Co.

NOTES: This table shows initial Loan Prospector and Desktop Underwriter users. The two lists were obtained from American Banker (1995) and (PR Newswire, 1995). We track mergers, acquisitions and name changes over the sample period using data from the National Information Center (NIC). We exclude West Jersey Community Bank, which was acquired by Sovereign Bank early in our sample period. Flagstar Bank was an initial user of both LP and DU. We exclude Flagstar from the set of Desktop Underwriter adopters as they reported relying mainly on Loan Prospector up until at least the late 1990s (LaMalfa (1997); LaMalfa (1998); LaMalfa (1999)).

TABLE 2

HOW IS SYSTEM CHOICE RELATED TO PRE-ADOPTION LENDER CHARACTERISTICS?

Dependent variable: Indicator equal to 1 for LP users and 0 for DU users.	(1)	(2)
Share sold to Freddie	0.33**	0.57**
	(0.13)	(0.21)
Average loan-to-income ratio		0.10
		(0.18)
Portfolio share		0.11
		(0.14)
Thrift or thrift subsidiary		-0.14
		(0.13)
Share bottom quartile income		0.04
		(0.29)
Share LTI > 2.5		-0.19
		(0.17)
Conventional share of originations		0.06
		(0.17)
Refinance share of originations		-0.24
		(0.20)
Number of Observations	27	27

NOTES: This table shows estimated coefficients from $\text{LP}_l = \alpha + \beta X_{l,1991-1993} + \epsilon_l$. LP_l is an indicator equal to 1 for initial Loan Prospector users listed and zero for initial Desktop Underwriter users (see Table 1). Flagstar Bank is classified as a Loan Prospector user as it reported relying mainly on Loan Prospector during the period we analyze. Share sold to Freddie is $\frac{\text{\#Loans Sold to Freddie}}{\text{\#Loans Sold to Fannie or Freddie}}$. Portfolio share is the share of loans originated by the institution that were not sold in the the calendar year of origination. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: HMDA and authors' calculations.

TABLE 3

HOW IS THE INITIAL LP SHARE RELATED TO ZIP CHARACTERISTICS?

Dependent variable: ZIP5 market share of early LP users in 1993.	(1)	(2)
Share of population living in NOAA coastal county	0.11***	0.01
	(0.03)	(0.03)
MSA housing supply elasticity (Saiz,2010)	-0.07*	
	(0.03)	
# HMDA Respondents	-0.14***	-0.03
	(0.03)	(0.02)
Market share of large HMDA respondents	-0.03	0.02
	(0.04)	(0.03)
Share of originations by thrift (1993)	0.03	0.06*
	(0.03)	(0.03)
Share of originations by commercial bank (1993)	-0.07**	-0.02
	(0.03)	(0.03)
Share of originations sold to either Fannie or Freddie	-0.10**	-0.07**
	(0.04)	(0.03)
Share of originations sold to Freddie in calendar year (1993)	0.14***	0.09***
	(0.04)	(0.03)
Log median HH income	0.01	0.00
	(0.02)	(0.01)
Market share of early LP or DU users by # of loans	0.75***	0.51***
	(0.10)	(0.07)
Division FE	X	
ZIP3 FE		X
Number of ZIP5	6,419	6,362
Number of Counties	587	562
Number of ZIP3	551	494
Within R-squared	0.62	0.43
Number of Observations	6,419	6,362

NOTES: Column 1 shows estimated coefficients from: $EarlyLP_z = \alpha_d + \beta X_z + \epsilon_z$. Column 1 shows estimated coefficients from: $EarlyLP_z = \alpha_{zip3} + \beta X_z + \epsilon_z$. $EarlyLP_z$ is the 1993 county market share of Loan Prospector users listed in Table 1 by number of HMDA loans (see Equation 1). All variables are normalized by dividing by the standard deviation. Standard errors are clustered by ZIP3. The sample is restricted to counties in metropolitan areas with non-missing FHFA house price data. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: HMDA; 1990 decennial census; NOAA list of coastal counties; authors' calculations.

TABLE 4

EFFECT OF AUS ON PROCESSING TIME

Dependent variable: Time in days from application to closing/denial	(1)	(2)	(3)	(4)	(5)	(6)
	Originated		Denied		All	
Early DU User X Post	-2.198**		-7.286***		-2.325**	
	(-2.30)		(-6.14)		(-2.58)	
Early LP User X Post		-1.951***		-13.35***		-3.252***
		(-2.88)		(-6.38)		(-4.90)
Lender × Income Quartile × Action FE	X	X	X	X	X	X
Purchaser Type FE	X	X				
Number of Observations	1,536,136	777,773	248,509	57,946	1,789,032	841,319

NOTES: This table shows estimates of β from Equation 5. The sample includes originated loans and denied applications reported by the initial Desktop Underwriter users in Table 1, and a group of matched control lenders. The sample excludes applications for non-conventional loans. Columns 1 and 2 are restricted to originations and Columns 3 and 4 are restricted to applications that end in a denial. Note that processing time is the time to origination for originated loans and the time to denial for denied applications. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Standard errors are clustered by lender × income quartile. Sources: Confidential HMDA; 1990 decennial census; NOAA list of coastal counties; and authors' calculations.

TABLE 5

EFFECT OF THE EARLY LP MARKET SHARE ON HOUSE PRICES AND OCCUPIED HOUSING UNITS

	(1) DEC Value	(2) DEC Value	(3) FHFA Price	(4) Occ. HU
Early LP Market Share (1993)	4.08***	2.14***	3.04***	-1.33
	(0.77)	(0.45)	(0.72)	(0.84)
Early LP Share × Supply Elasticity			-4.47***	5.40***
			(1.71)	(2.03)
Division FE	X	X	X	X
Number of ZIP5	7,294	16,859	7,025	7,025
Number of Counties	719	1,500	634	634
Number of States	51	51	49	49
Number of Observations	7,294	16,859	7,025	7,025

NOTES: Column 1 explores the effect of the early LP market share on the decennial census median house value for owners between 1990-2000. Column 2 compare the effect of increasing the zip sample, which is not feasible with FHFA repeat sales but is possible using the decennial census. Columns 3 and 4 show the interaction with Baum-Snow and Han (2024) supply elasticity measure (space) centered at an elasticity of 1. Column 4 shows the effect on the number of occupied housing units. We condition on the following variables interacted with year dummies, at the zip code level unless otherwise indicated: (county) coastal indicator, number of lenders, large lender market share, thrift market share, commercial bank market share, share of originations sold to either Fannie or Freddie, share of originations sold to Freddie, log median household income. In Columns 3 and 4, we also condition on interactions of supply elasticity with all control variables and with the division fixed effects. Standard errors are clustered by 3-digit zip code. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: HMDA; FHFA HPI; 1990 decennial census and authors' calculations.

TABLE 6

EFFECT OF LOAN PROSPECTOR ADOPTION ON COUNTY HOUSE PRICE AND LENDING CORRELATION

	House Price		Avg Loan to Income		# Home Purchase Loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Integration (LP) × Post	0.16***	0.16**	0.43***	0.43***	0.23**	0.22**
	(0.06)	(0.06)	(0.11)	(0.11)	(0.11)	(0.10)
Integration (DU) × Post	0.08***	0.07***	0.12***	0.13***	0.16***	0.19***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Population corr. (1SD)	1.38***	1.38***	-0.07	-0.07	0.16	0.16
	(0.09)	(0.09)	(0.11)	(0.11)	(0.11)	(0.11)
Per capita income corr. (1SD)	0.68***	0.68***	5.13***	5.12***	0.80***	0.78***
	(0.12)	(0.12)	(0.17)	(0.17)	(0.14)	(0.14)
Loan type integration (1SD)		0.95***		1.94***		6.55***
		(0.24)		(0.46)		(0.44)
County Pair FE	X	X	X	X	X	X
County (1) × CSA (2) × Year FE	X	X	X	X	X	X
Sample	1984-2003	1984-2003	1984-2003	1984-2003	1984-2003	1984-2003
Number of Observations	726,396	726,396	552,527	552,527	553,917	553,917

NOTES: For county i and county j (in Combined Statistical Area $a(j)$) and year t:

$$Corr_{i,j,t} = \alpha_{i,j} + \gamma_{i,a(j),t} + \beta_1 Post_t \Delta H^{LP}_{i,j}{}_{i,j} + \beta_2 Post_t \Delta H^{DU}_{i,j} + Controls_{i,j,t} + \epsilon_{i,j,t}$$

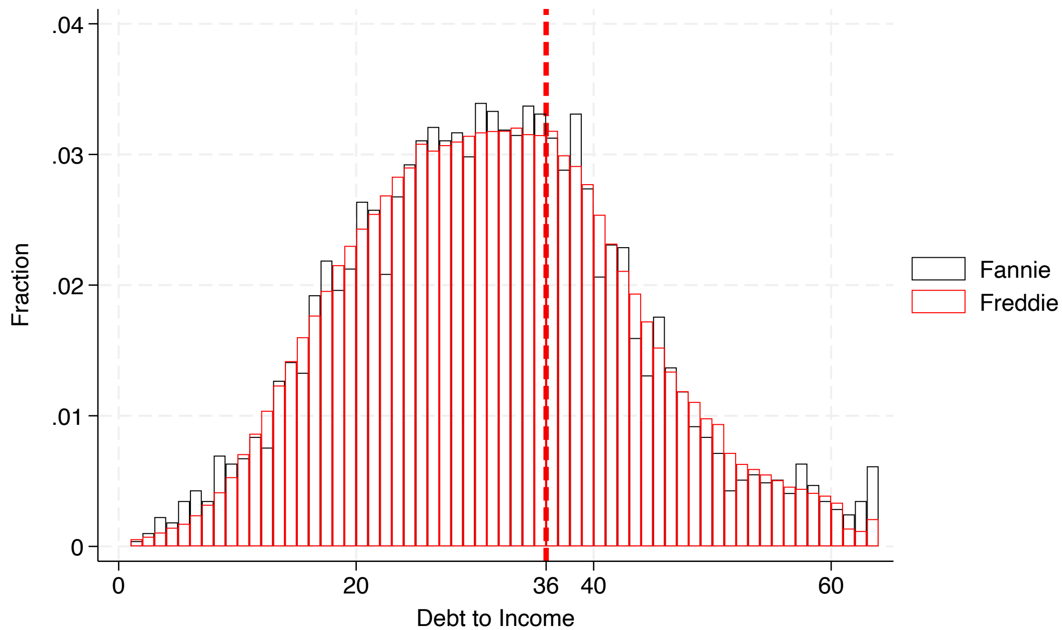
Where $Corr_{i,j,t}$ is the annual growth rate correlation between county i and county j over a 5-year sample period starting in year t, where $t \in \{1984, 1989, 1994, 1999\}$. The post indicator is equal to 0 for $t \in \{1985, 1989\}$ (1989 includes house price growth up until 1993) and 1 for later years. Controls include the annual population growth and per capita income growth correlation computed over the same 5-year period as the dependent variable. In Columns 2, 4 and 6 we also control for an additional measure of lending integration based on the shares of loans sold to Fannie Mae, Freddie Mac and the share of FHA loans. $\Delta H^{LP}_{i,j}{}_{i,j}$ and $\Delta H^{DU}_{i,j}{}_{i,j}$ are trimmed at the 1st and 99th percentile. Standard errors are clustered by Combined Statistical Area pairs. All specifications include fixed effects for time by first county by the CSA of the second county.

Internet Appendix

Automated Underwriting and Housing Market Dynamics

FIGURE A.1

DTI DISTRIBUTION OF LOANS IN PUBLIC GSE DATA ORIGINATED BETWEEN JAN AND JUN 1999

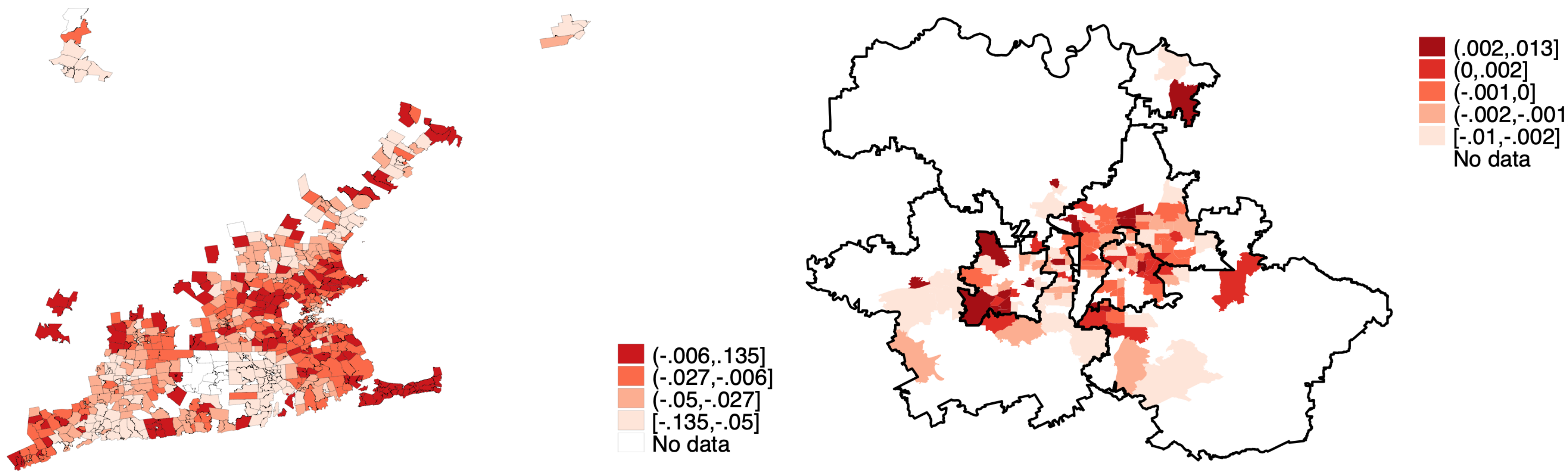


NOTES: Figure A.1 plots the distribution of the back-end debt-to-income ratio using Fannie Mae's Single Family Loan Performance Dataset and Freddie Mac's Loan Level Dataset. We also include loans that were excluded from the originally released datasets to obtain more comprehensive coverage. We also drop loans sold by Norwest due to some unique characteristics of the Norwest distribution (Norwest reached an agreement to exclusively sell to Freddie around this time period in exchange for using an alternative underwriting system instead of LP).

FIGURE A.2

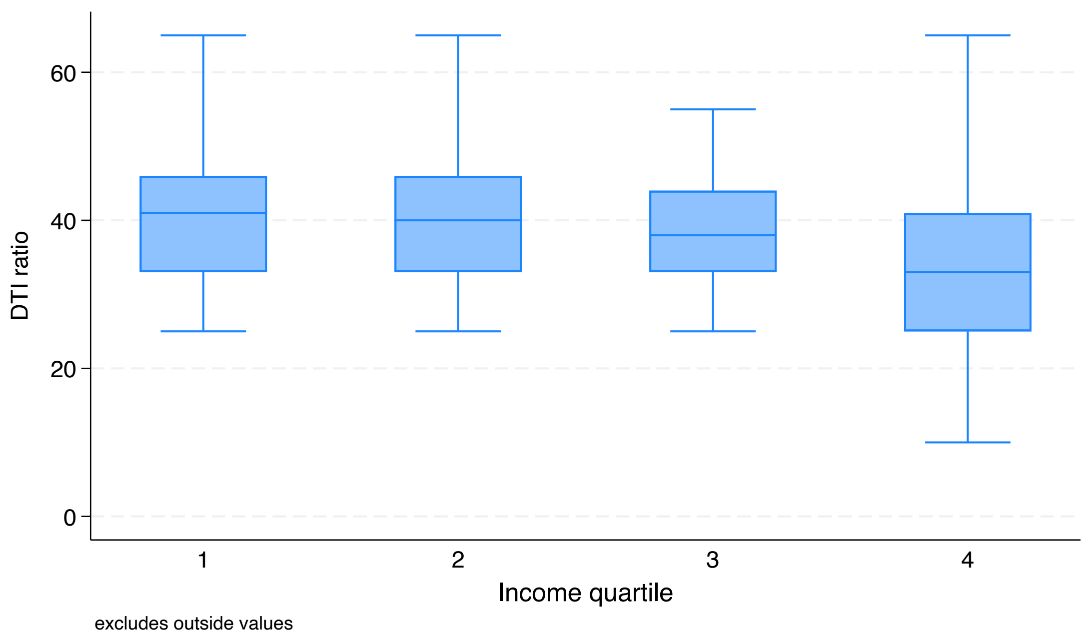
ZIP EXPOSURE VARIATION WITHIN CENSUS DIVISION AND 3-DIGIT ZIP CODES

Panel A. New England census division: CT, ME, MA, NH, RI, VT *Panel B. ZIP3 in Dallas-Fort Worth-Arlington, TX MSA*



NOTES: Variation in residualized exposure across 5-digit zip codes. Black borders in Figure A.2B are 3-digit zip code boundaries. The exposure measure is only plotted for zip codes with a sufficiently long price history to be included in our sample.

FIGURE A.3
DEBT-TO-INCOME DISTRIBUTION BY INCOME QUARTILE

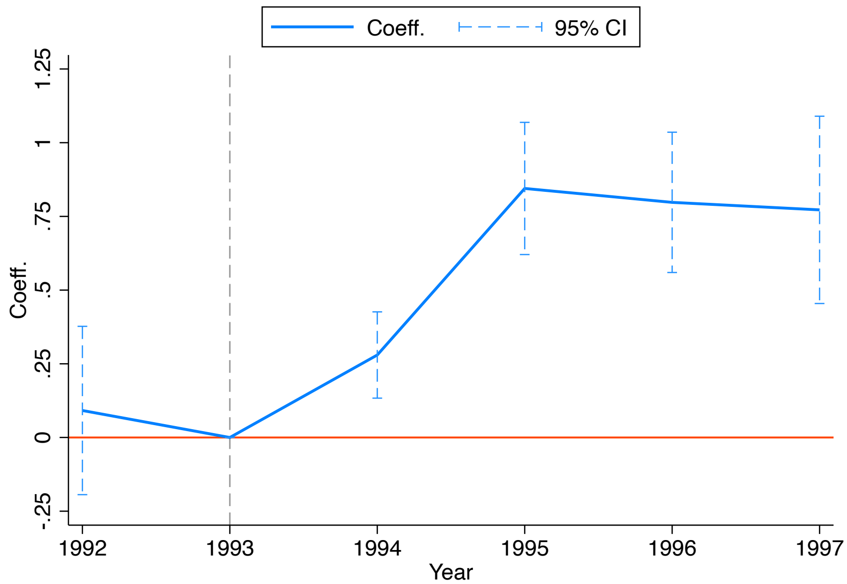


NOTES: This figure is constructed using 2018 HMDA home purchase originations. DTI ratios are top-coded at 60 per cent. Where DTI ratios are binned we assume the DTI is equal to the bin midpoint, and for > 60 we use 65%. DTI is reported by the lender and is the ratio of monthly mortgage payments, property insurance, property taxes, debt payments and certain other financial obligations to gross monthly income. Sources: HMDA.

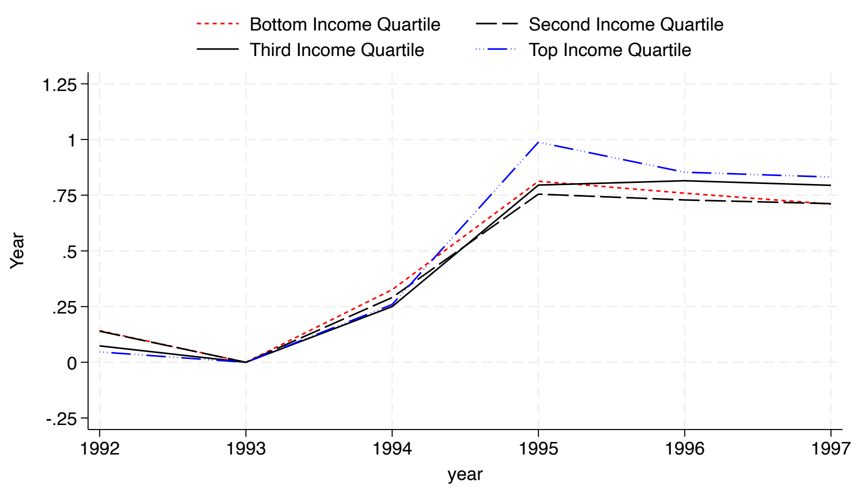
FIGURE A.4

EFFECT OF EARLY LOAN PROSPECTOR ADOPTION ON MORTGAGE ORIGINATION VOLUMES

Panel A. # Originations



Panel B. # Originations by income quartile

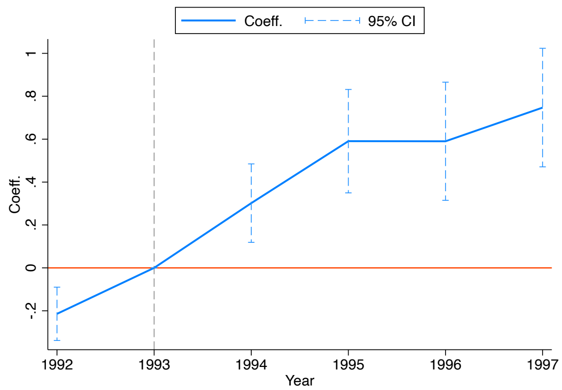


NOTES: Figure A.4A plots estimates of $\{\beta_k\}$ from Equation 4 To construct the dataset we first compute the aggregate number of originations for each lender by income quartile and year. Figure A.4B plots the response separately for each borrower income quartile. Standard errors are clustered by lender $\times$ income quartile.

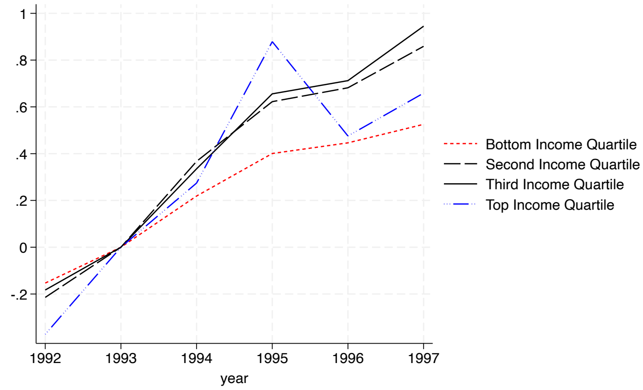
FIGURE A.5

CREDIT RESPONSE WITHOUT LOCATION FIXED EFFECTS

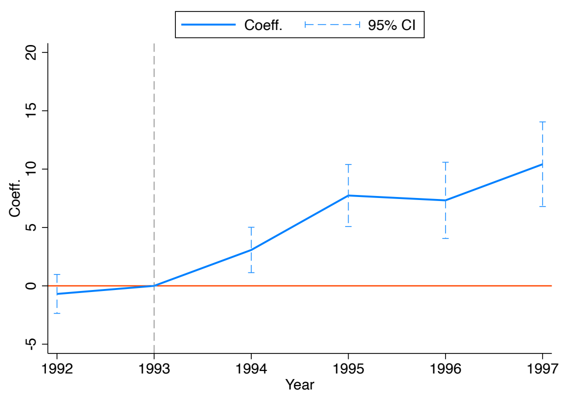
Panel A. Share LTI > 2.5 (Log Change)



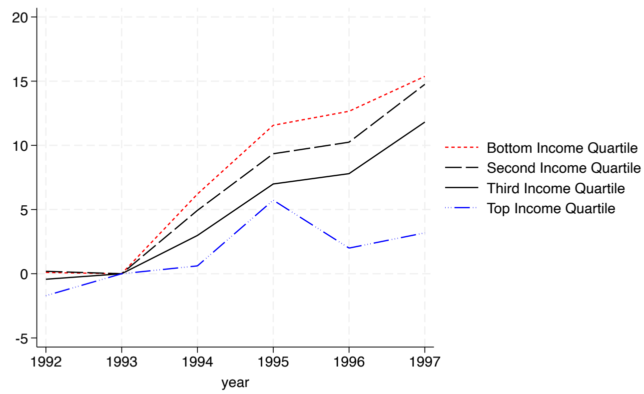
Panel B. *Share LTI > 2.5 by income quartile (Log Change)*



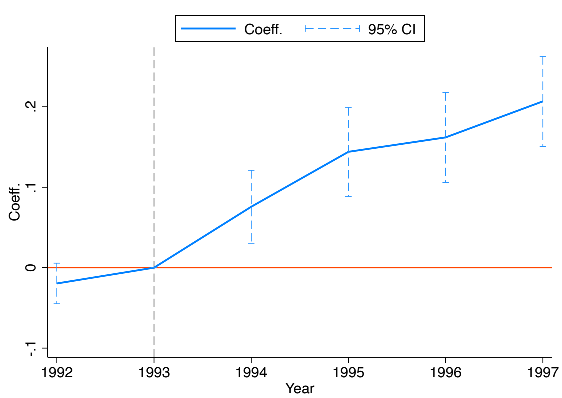
Panel C. Share LTI > 2.5 (pp)



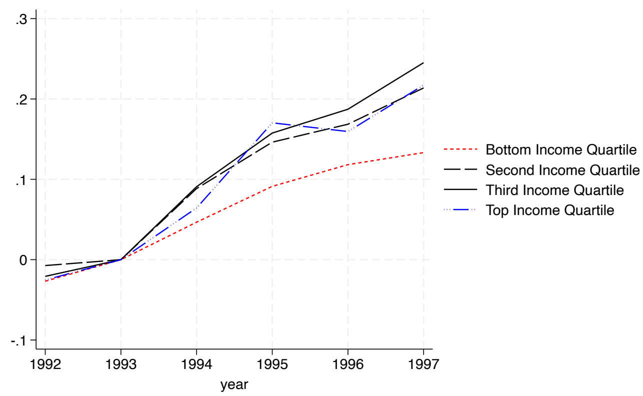
Panel D. Share LTI > 2.5 by income quartile (pp)



Panel E. LTI (Log Change)



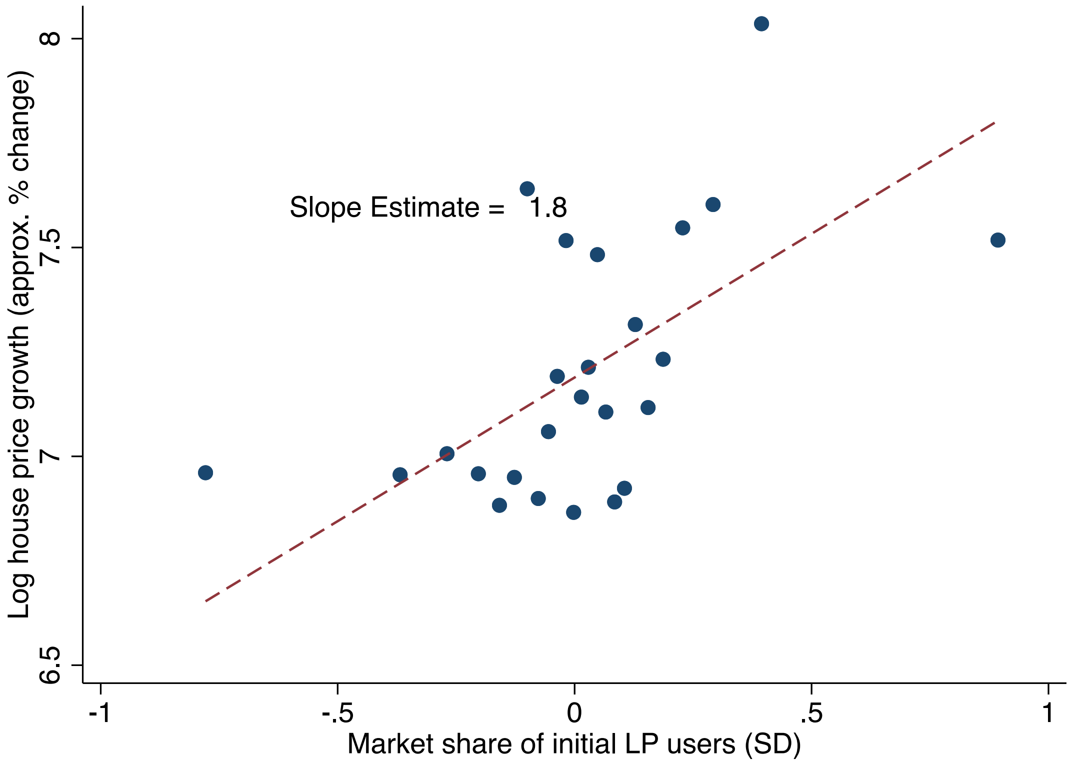
Panel F. LTI by income quartile (Log Change)



NOTES: Refer to the notes to Figure 2. These figures are analogous but do not include location fixed effects (and the sample is therefore expanded to include loans and applications for which the property location was not provided). Sources: HMDA.

FIGURE A.6

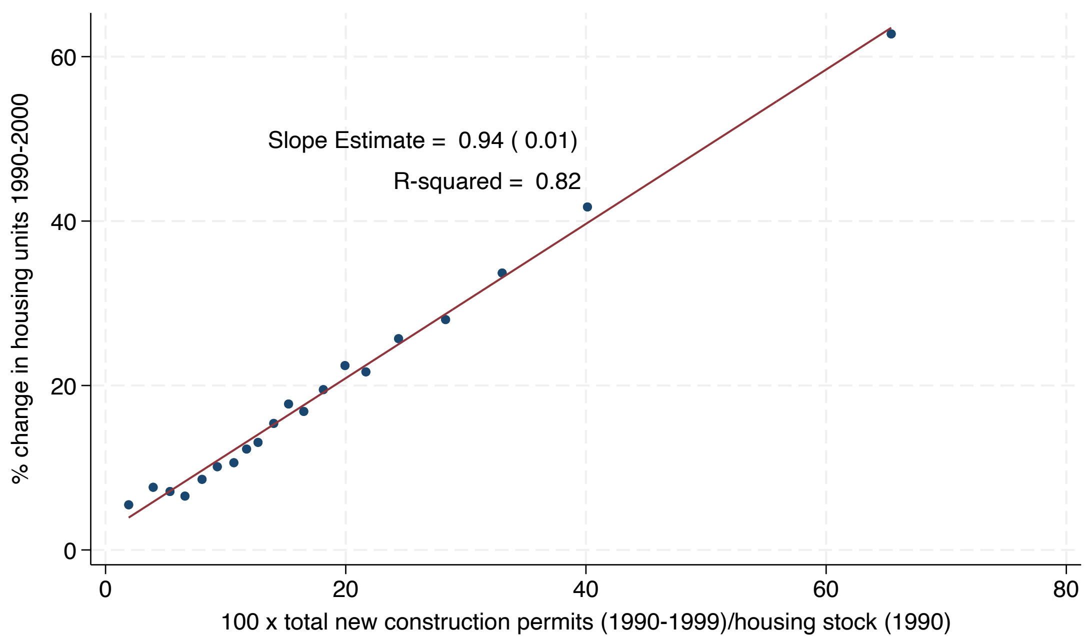
RELATIONSHIP BETWEEN EXPOSURE AND 1993-1996 HOUSE PRICE GROWTH



NOTES: This binned scatter plot shows the zip code log house price change from 1993-1996 by the market share of initial Loan Prospector users. We condition on the following variables interacted with year dummies, at the zip code level unless otherwise indicated: (county) coastal indicator, number of lenders, large lender market share, thrift market share, commercial bank market share, share of originations sold to either Fannie or Freddie, share of originations sold to Freddie, log median household income, Saiz (2010) housing supply elasticity, the combined zip code share of initial DU and LP users $EarlyAUS_c$. The sample includes zip codes in metropolitan counties with with FHFA house price data available continuously from 1990 to 2005. We use census division fixed effects. Sources: FHFA HPI; HMDA; 1990 decennial census; NOAA list of coastal counties.

FIGURE A.7

USING THE BUILDING PERMITS SURVEY TO IMPUTE HOUSING UNITS

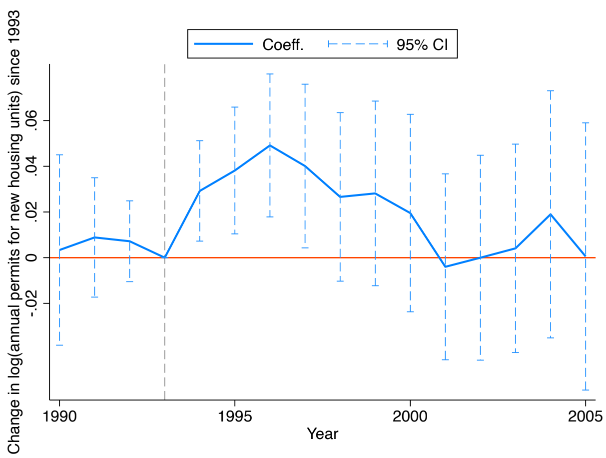


NOTES: Figure A.7 shows the relationship between actual and imputed growth in housing units from the 1990 to 2000 censuses. The vertical axis shows the actual percentage change in the number of housing units in a county. The horizontal axis shows the imputed growth as measured by total new construction permits in the BPS from 1990 to 1999.

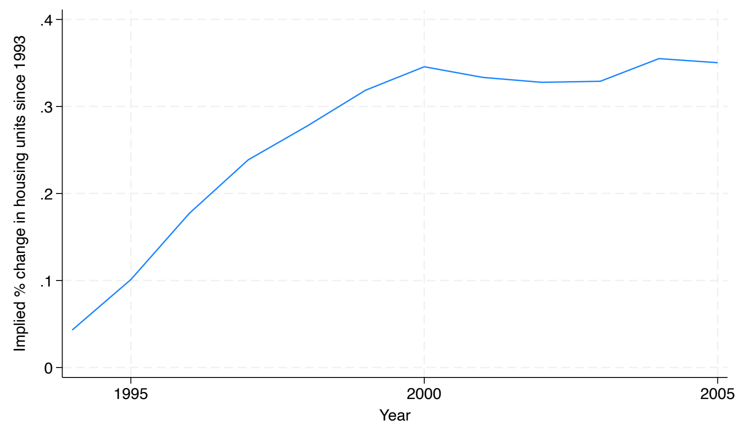
FIGURE A.8

HOUSING SUPPLY RESPONSE MEASURED USING ANNUAL PERMITS ISSUED FOR NEW UNITS

Panel A. Cumulative log change in annual new construction permits



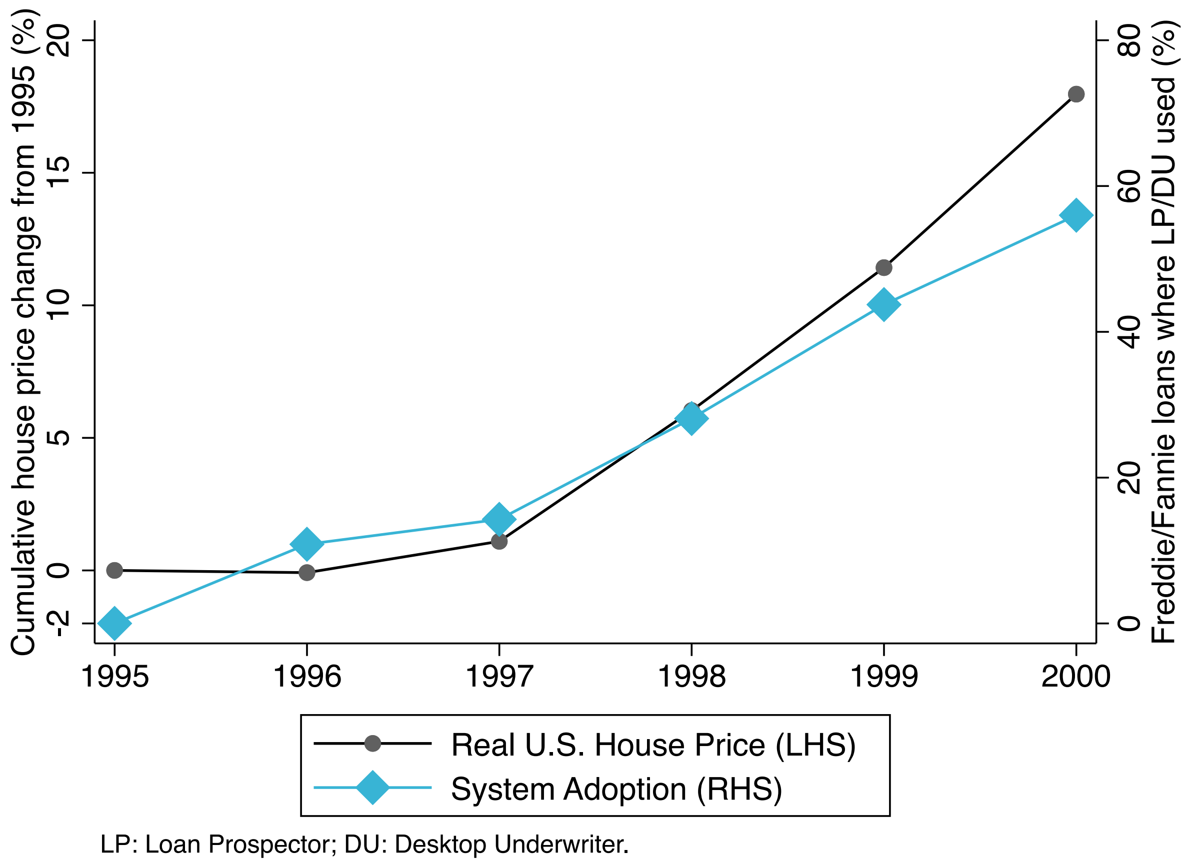
Panel B. Cumulative % change in total housing units



NOTES: Figure A.8 combines the estimates from $\log(Permits_{c,t}) = \delta_c + \gamma_{d,t} + \sum_{\substack{t=1990 \\ k\neq 1993}}^{2005} \mathbb{1}_{t=k}\left(\beta_k \frac{EarlyLP_c}{SD(EarlyLP_c)} + \alpha_k X_c\right) + \epsilon_{c,t}$ with 1990 data on the total county housing stock to plot the cumulative change in housing units.

FIGURE A.9

GRADUAL ADOPTION OF AUTOMATED MORTGAGE UNDERWRITING SYSTEMS



NOTES: Data on systems adoption is from the sources listed in Table A.1. We weight data for Fannie and Freddie's systems using data on Fannie and Freddie's mortgage acquisitions from HUD's GSE public use database. We use linear interpolation to compute values for years where no data is available. Real house price data is from the BIS Residential Property Price database.

TABLE A.1

% OF FANNIE AND FREDDIE PURCHASES PROCESSED USING DESKTOP UNDERWRITER OR LOAN PROSPECTOR

	Fannie Mae		**Freddie Mac**	
	Report	Other	Report	Other
1995				
1996			25	
1997	9		22	54*
1998	22	26*	36	
1999	39		50	>75**
2000	56		56	
2001	59		62	
2002	60		60	
2003			64	
2004			61	

NOTES: This table shows the share of Fannie's purchases processed through DU and the share of Freddie's purchases processed through LP. The table includes numbers from the GSEs' annual reports and numbers reported by Fannie and Freddie representatives to trade journals. The discrepancies these two sources could reflect fluctuations in LP and DU usage within the calendar year, and differences between projected and realized usage. In particular, there is evidence that both Fannie and Freddie projected usage of 80-85% by 1999. These rates were apparently never realized, though during 1999 Freddie stated that over 75% of its purchases were processed through LP. Later annual reports suggest that DU and LP usage stabilized at a lower rate of around 60 per cent because both Fannie and Freddie made agreements with large lenders which allowed them to use alternative systems. Sources: Fannie Mae and Freddie Mac annual reports.
* Wilson, Caroline (1998). Automated Underwriting Goes Mainstream. America's Community Banker, 7(4):36; Gallaher, Douglas (1998). Getting a Payoff from Technology. Mortgage Banking, 58(6): 66–76.
** Murin, Joseph (1999). A Business Transformed by Technology. Mortgage Banking, 60(1): 152.

TABLE A.2

HOW IS SYSTEM CHOICE RELATED TO LENDER CHARACTERISTICS?

Dependent: 1 for initial adopters and 0 for matched control lenders.	LP (1)	DU (2)
Share sold to Fannie or Freddie (1991-1993)	0.17	0.69**
	(0.12)	(0.27)
Average loan-to-income ratio (1991-1993)	0.10	-0.06
	(0.12)	(0.21)
Share LTI > 2.5 (1991-1993)	0.03	0.17
	(0.14)	(0.18)
Portfolio share (1991-1993)	0.08	0.13
	(0.13)	(0.08)
Share bottom quartile income (1991-1993)	0.04	-0.00
	(0.11)	(0.08)
Conventional share of originations (1991-1993)	-0.02	-0.08
	(0.13)	(0.10)
Refinance share of originations (1991-1993)	-0.04	-0.02
	(0.13)	(0.07)
Share of loans purchased (1991-1993)	-0.01	0.03
	(0.09)	(0.06)
Adjusted R-squared	-0.05	0.02
Number of Observations	36	68

NOTES: This table shows estimated coefficients from $Y_l = \alpha + \beta X_l + \epsilon_l$. In Column 1, Y_l is an indicator equal to 1 for initial Loan Prospector users and zero for matched control lenders (who were not initial users of either system). In Column 2, Y_l is an indicator equal to 1 for initial Desktop Underwriter users and zero for matched control lenders. Flagstar Bank is classified as a Loan Prospector user as it reported relying mainly on Loan Prospector during the period we analyze. Portfolio share is the share of loans originated by the institution which were not sold in the the calendar year of origination. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: HMDA.

TABLE A.3

CORRELATION BETWEEN AUS USAGE AND PROCESSING TIME

Dependent variable: Days from application to closing/denial.

	(1) Originated	(2) Originated	(3) Denied	(4) Denied	(5) All	(6) All
AUS Used	-2.910***		11.25***		4.319***	
	(-92.16)		(116.80)		(148.43)	
AUS & Approved		-3.583***		15.04***		4.116***
		(-113.26)		(131.46)		(140.43)
AUS & Issue		6.121***		6.810***		6.521***
		(113.48)		(56.49)		(133.27)
Ln(Loan Amount)	2.237***	1.973***	2.533***	2.722***	2.215***	2.147***
	(144.75)	(127.71)	(46.31)	(49.79)	(141.65)	(136.95)
Ln(Income)	0.0472**	-0.338***	2.723***	2.482***	2.083***	2.040***
	(2.55)	(-18.23)	(43.63)	(39.77)	(114.27)	(111.86)
Number of Observations	5,648,571	5,648,571	871,779	871,779	6,520,350	6,520,350

NOTES: Columns 1, 3 and 5 show estimates of β from: $Time_i = \alpha + \beta AUSused_i + \alpha X_i + \epsilon_i$, where $AUSused_i$ is an indicator equal to one if an automated underwriting system was used for that application and zero otherwise. Columns 2, 4, and 6 show estimates of β from: $Time_i = \alpha + \beta_1 AUSapproved_i + \beta_1 AUSissue_i + \alpha X_i + \epsilon_i$, where $AUSapproved_i$ is an indicator equal to one if an automated underwriting system was used and the AUS gave an approve decision (this does not imply the loan was ultimately originated). $AUSissue_i$ is an indicator equal to one if an automated underwriting system was used and the AUS did not give an approve decision (this does not imply the loan was ultimately denied). The sample in Columns 1 and 2 is 2018-2019 originations, Columns 3 and 4 show estimates for applications that are ultimately denied, and Columns 5 and 6 show the relationship for all denied applications and originated loans. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: Confidential HMDA.

TABLE A.4

LOAN-TO-INCOME RESPONSE BY HOUSING SUPPLY ELASTICITY

	(1) High LTI	(2) High LTI	(3) LTI	(4) LTI
LP User	3.59***	12.85***	10.91***	5.08***
	(1.11)	(4.12)	(2.39)	(1.12)
LP User $\times$ Supply Elasticity	-0.28	0.55	-1.68	-0.65
	(0.92)	(2.96)	(2.14)	(0.95)
Estimation	OLS	PPML	OLS	PPML
ZIP5 $\times$ Yr FE	X	X	X	X
Lender $\times$ Income Quartile FE	X	X	X	X
Income Quartile $\times$ Yr FE	X	X	X	X
Number of ZIP5	8,510	8,510	8,510	8,510
Number of Counties	936	936	936	936
Number of States	49	49	49	49
Number of Observations	830,342	830,342	830,342	830,342

NOTES: This table explores how the response of different LTI measures depends on housing supply elasticity. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: HMDA; FHFA HPI; 1990 decennial census and authors' calculations.

TABLE A.5

SHORT-RUN RESPONSE OF HOUSE PRICE TO CREDIT

	Δ log LTI (1)	Δ log Price (2)	Δ High LTI share (3)	Δ log Price (4)
Δ Log LTI		0.39**		
		(0.17)		
Δ High LTI Share				2.08***
				(0.79)
Early LP	0.03***		0.01***	
	(0.01)		(0.00)	
Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
Division FE	Y	Y	Y	Y
N	6,416	6,416	6,416	6,416
Kleibergen-Paap Wald F stat		9		10

NOTES: This table shows IV regressions of log house price changes on instrumented credit measures. All changes are computed from 1993 to 1995. Columns 1 and 2 show the response of house prices to the average loan-to-income ratio of home purchase originations (the top and bottom 1 per cent of income and loan size distributions each year are dropped before computing LTI). Columns 3 and 4 show the response of house prices to the share of high loan-to-income lending. The dependent variable in Column 3 is the share of home purchase originations with a loan-to-income ratio above 2.5. All specifications include census division fixed effects and census division by year fixed effects and standard errors are clustered by 3-digit zip code. We condition on the following variables, at the zip code level unless otherwise indicated: (county) coastal indicator, number of lenders, large lender market share, thrift market share, commercial bank market share, share of originations sold to either Fannie or Freddie, share of originations sold to Freddie, log median household income, Saiz (2010) housing supply elasticity, the combined zip code share of initial DU and LP users $EarlyAUS_z$. The sample is restricted to zip codes in metropolitan areas with non-missing FHFA house price data. 10%, 5% and 1% significance levels are denoted by *, ** and ***. Sources: HMDA; FHFA HPI; 1990 decennial census; NOAA list of coastal counties; and authors' calculations.