

# Nonbanks, Banks, and Monetary Policy: Real Effects across Three Major U.S. Credit Markets from Granular Data\*

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## Abstract

We show that nonbank financial intermediaries weaken the transmission of monetary policy to corporate investment, consumption, and house prices through a credit channel. For identification, we exploit exhaustive, proprietary loan-level data from the US since the 1990s that identify whether the lender is a bank or nonbank, persistent variation in nonbank lending footprints across industries and locations, and monetary policy shocks. We find that tighter monetary policy shifts the supply of credit from banks to nonbanks. This attenuates the impact of monetary policy on corporate investment and house prices, while largely neutralizing its impact on durable goods consumption via a credit channel. Moreover, regulatory constraints play a crucial role in driving the differential response of nonbanks to monetary policy.

*Keywords:* Nonbank financial intermediaries, Banks, Monetary policy transmission, Household loans, Corporate loans, Credit channel

*JEL:* E44, E51, E52, G21, G23

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# 1. Introduction

Nonbank financial intermediaries play a significant role in many credit markets, both in the US and globally (Financial Stability Board, 2023). However, there is scant evidence on how nonbanks affect the credit channel of monetary policy, including the associated real effects. This contrasts with the huge literature on the bank lending channel of monetary policy. In this paper, we analyze the nonbank credit channel of monetary policy.

The effects of monetary policy on lending by nonbanks are conceptually ambiguous. On the one hand, policymakers have argued that monetary policy “gets in all of the cracks” of the financial system by acting directly on market interest rates (Stein, 2013, then governor of the Federal Reserve Board)—that is, monetary policy could have similar effects on *all* debt-financed financial intermediaries (banks and nonbanks). On the other hand, differences in regulation and funding structures might imply that monetary policy affects banks and nonbanks differently. Banks are subject to stricter regulation, and for example regulatory capital constraints play a key role in the transmission of monetary policy via credit (Kishan and Opiela, 2000, 2006; Jiménez et al., 2012). Banks are also financed with deposits, and tighter monetary policy can lead to a reduction in (non-time) bank deposits (Drechsler et al., 2017; Xiao, 2020; Begeau and Stafford, 2022).

An important obstacle to studying the role of nonbank lenders in monetary transmission is the lack of comprehensive loan-level data that include both banks and nonbanks. For example, credit registers from central banks typically only include loan data from banks. To address this challenge, we exploit confidential loan-level data from three large US credit markets—syndicated corporate loans, residential mortgages, and consumer auto loans—in which we observe, *for each loan, whether the lender is a bank or nonbank*. Banks are defined as deposit-taking institutions. Over our main sample period, nonbanks account for around 38%, 44%, and 55% of lending in syndicated loans, mortgage lending, and auto loans, respectively. We match each loan-level dataset to economic outcomes such as corporate investment, house prices, and auto sales. In each market, the data extend back to the 1990s, allowing us to exploit considerable variation in monetary policy, which we identify using the Gertler and Karadi (2015) measure of monetary policy shocks. In robustness tests, we also use alternative monetary policy measures, notably deviations of monetary policy rates from an estimated natural rate,  $r^*$ . We also control for realized and expected GDP growth, inflation, and financial market volatility.

33 To identify the nonbank credit channel, we exploit persistent variation in nonbank  
34 lending footprints across industries and locations. The rationale for this strategy is that  
35 nonbanks specialize in lending to different types of borrowers (based on their industries  
36 for corporate loans, and based on their locations for household loans), and this cross-  
37 sectional variation is persistent over time. In particular, we first show specialization  
38 in nonbank lending across borrower types. Using the specialization measures of [Blickle  
39 et al. \(forthcoming\)](#) and [Paravisini et al. \(2023\)](#), we find that nonbank lending in certain  
40 industries and counties is materially higher than would be the case under a perfectly  
41 diversified portfolio. Second, we show that in each market, there is significant persistence  
42 in nonbank lending footprints over time. Specifically, past values of the nonbank share  
43 of lending in a given industry (for corporate loans) or in a given county (for mortgages  
44 and auto loans) are strong predictors of future values. These results on specialization  
45 and persistence in nonbank lending to particular industries or locations are consistent  
46 with frictions in the ability of borrowers to substitute between bank and nonbank credit.<sup>5</sup>  
47 Interestingly, we find that the degree of persistence over time is significantly lower for auto  
48 lending than for mortgages and corporate lending, suggesting that substitution between  
49 banks and nonbanks in the cross section over time is easier in the auto loan market than  
50 in the mortgage and corporate lending markets.

51 We next highlight key differences in the effects of monetary policy on total lending at  
52 the industry-level or county-level in the three credit markets. Total industry-level corpo-  
53 rate lending and county-level mortgage lending both fall significantly following a monetary  
54 tightening. In contrast, total county-level auto lending is unaffected by monetary policy.

55 Given the differences in the persistence of nonbank lending shares highlighted above,  
56 we hypothesise that easier substitution between bank and nonbank credit in the auto  
57 loan market could contribute to its weaker response to monetary policy. To test this  
58 hypothesis, we study each of the three markets in more detail. In each case, we first  
59 use loan-level data to estimate the differential impact of monetary policy on bank and  
60 nonbank lending, while using granular fixed effects to control for borrower characteristics  
61 including credit demand. We then aggregate to consider the resulting real effects.

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<sup>5</sup>Existing research has highlighted the importance of: (i) *industry-level* informational advantages in corporate lending ([Giometti and Pietrosanti, 2022](#); [Blickle et al., forthcoming](#)); (ii) *local* information and networks for mortgage lending ([Loutskina and Strahan, 2011](#); [Berrospide et al., 2016](#)); and (iii) *local* auto dealers' and auto lenders' long-term contracts ([Benmelech et al., 2017](#); [Grunewald et al., 2020](#)).

62 We start with syndicated corporate lending. We use the Shared National Credit Pro-  
63 gram (SNC), a confidential supervisory credit register from the Federal Reserve Board,  
64 FDIC, and OCC. SNC records holdings in syndicated loans by both banks and non-  
65 bank investors (such as CLOs, broker-dealers, and hedge funds). Unlike the widely-used  
66 DealScan primary market dataset, the SNC tracks loan holdings over time, and therefore  
67 also accounts for secondary market trading, which is the main way in which nonbanks  
68 acquire syndicated loans. We only analyze new loans, but include immediate sales in the  
69 secondary market, which ensures that we accurately capture participation by nonbanks.

70 A crucial feature of syndicated lending is that it involves multiple lenders participating  
71 in the same loan. This allows us to use loan fixed effects to comprehensively control for  
72 unobserved time-varying borrower characteristics, including credit demand (Khwaja and  
73 Mian, 2008; Chodorow-Reich, 2014). Hence we identify the impact of monetary policy on  
74 credit *supply* by comparing how banks and nonbanks participate in the same loan. We  
75 find that nonbanks increase their credit supply relative to banks when monetary policy  
76 tightens. On average, a 25 basis point tightening leads to an increase in nonbank loan size  
77 of around 4% relative to banks. Moreover, the relative increase in nonbank lending after  
78 a monetary tightening is significantly larger for industries with a larger predetermined  
79 nonbank lending share. This suggests that substitution from bank to nonbank credit is  
80 stronger in industries where nonbanks have a larger established presence.

81 To estimate borrower-level real effects of nonbank credit supply, we match SNC data  
82 to Compustat data on corporate balance sheets, and exploit predetermined variation in  
83 nonbank lending patterns across industries. Consistent with the loan-level results, we find  
84 that, following a monetary tightening, investment, total assets, and total debt decrease by  
85 less for firms in industries with a larger (vs lower) predetermined nonbank presence. These  
86 firms also experience a relative increase in syndicated credit, particularly from nonbanks.

87 The syndicated loan market includes a diverse range of nonbank lenders, which allows  
88 us to shed further light on mechanisms underlying the results. We find that our loan-  
89 level results are driven by weakly-regulated nonbanks—such as CLOs, hedge funds, and  
90 finance companies—rather than mutual funds and insurance companies. This suggests  
91 that regulatory constraints play a key role in driving the differential response of nonbanks  
92 to monetary policy. In contrast, we do not find robust evidence that nonbanks' dependence  
93 on short-term funding affects their response to monetary policy.

94 After examining corporate lending, we study two household credit markets in which  
95 nonbanks also play a large role: residential mortgages (the largest credit market in the US)  
96 and auto lending (which represents over 30% of total consumer credit). For mortgages,  
97 we use loan-level data collected under the Home Mortgage Disclosure Act (HMDA); we  
98 use the confidential version of the dataset, which includes the origination date rather than  
99 only the origination year. For auto lending, we use detailed household-level data from the  
100 New York Fed Consumer Credit Panel (CCP). To control for credit demand conditions,  
101 we use county-time fixed effects as well as a rich set of household-level variables.

102 Our loan-level analysis reveals important differences in the impact of monetary policy  
103 on nonbank lending across these markets. In both household loan markets, nonbanks  
104 increase credit supply relative to banks following a monetary contraction: on average, a  
105 25 basis point tightening is associated with a relative increase in nonbank loan size of  
106 around 1.5% in the mortgage market and around 4.5% in the auto loan market.

107 However, the variation across counties is different. In the mortgage market, the relative  
108 increase in nonbank lending is significantly larger in counties with a larger predetermined  
109 nonbank presence, in line with our results for corporate lending. In contrast, the effect of  
110 monetary policy on nonbank auto lending is unaffected by the degree of predetermined  
111 nonbank presence in the county, suggesting that nonbank credit substitutes better for  
112 bank credit in the auto loan market after a monetary tightening—i.e., in auto loans,  
113 nonbanks expand credit strongly even in counties where banks have a larger presence.

114 Finally, we aggregate to the county level to consider economic effects in the two house-  
115 hold credit markets. Consistent with the loan-level results, we find that when monetary  
116 policy tightens, house prices in counties with a larger predetermined nonbank presence  
117 decrease by less (compared to other counties). In contrast, the impact of monetary policy  
118 on auto sales is unaffected by predetermined nonbank presence in the county. Consistent  
119 with our loan-level results, our county-level analysis suggests that these differences are  
120 explained by differences in the ease of substitution between bank and nonbank credit in  
121 the two markets. In counties with a larger predetermined nonbank presence, total mort-  
122 gage lending falls by less following a monetary tightening, driven by a relative increase  
123 in nonbank lending. Meanwhile, the response of total auto lending to monetary policy is  
124 largely unrelated to the degree of predetermined nonbank presence, consistent with very  
125 strong substitution between bank and nonbank credit across counties.

126 In summary, across all three markets, we find that nonbank lenders weaken the im-  
127 pact of monetary policy on credit supply, thereby affecting corporate investment, durable  
128 goods consumption, and house prices. Specifically, using loan-level data for identifica-  
129 tion, we show that nonbanks raise credit supply relative to banks when monetary policy  
130 tightens. However, the degree of substitution from bank to nonbank credit differs across  
131 markets. For auto loans, the credit substitution is very strong, which largely neutralizes  
132 the impact of monetary policy on auto sales via a credit channel. Meanwhile, in the mort-  
133 gage and corporate loan markets, there is credit substitution but it is incomplete, such  
134 that the impact of monetary policy on house prices and corporate investment is weaker  
135 where nonbanks have a larger presence: for the average predetermined nonbank share,  
136 a 25 basis point monetary tightening reduces investment by 3.5% less and house prices  
137 by 1.4% less due to the nonbank credit channel (relative to industries or counties with  
138 predetermined nonbank share of zero). Finally, we provide evidence from the syndicated  
139 lending market that suggests a key role for regulatory constraints (rather than short-term  
140 funding dependence) in driving the differential response of nonbanks to monetary policy.

141 *Contributions to the literature.* We contribute to the literature on monetary policy  
142 and credit (Bernanke and Blinder, 1992; Adrian and Shin, 2010) by showing empirically  
143 that nonbanks affect the transmission of monetary policy to output (investment and  
144 consumption) and house prices via a credit channel. There is a large literature showing  
145 that banks reduce credit supply when monetary policy tightens: the so-called bank lending  
146 channel of monetary policy (for example, Bernanke and Blinder, 1988; Jiménez et al.,  
147 2012; Drechsler et al., 2017). As highlighted above, however, the empirical evidence on  
148 whether nonbanks affect the transmission of monetary policy via the credit channel—and  
149 particularly the associated real effects—is scant.

150 Our paper is closely related to Xiao (2020), Drechsler et al. (2022), and Buchak et al.  
151 (2024), who also study the relationship between monetary policy and nonbank lending.  
152 We extend this literature in two main ways. First, we estimate real economic effects  
153 associated with the nonbank credit supply channel. Second, we document the impact  
154 of this channel across three major credit markets and several monetary policy cycles.  
155 Importantly, we highlight that the degree of substitution between bank and nonbank credit  
156 differs substantially across markets (with important differences in associated real effects),  
157 which implies that results from one credit market cannot be extrapolated to others. In

158 contrast, [Drechsler et al. \(2022\)](#) compare bank and nonbank lenders in a single market  
159 (the mortgage market) during the 2003–06 and 2014–17 monetary tightening cycles; and  
160 [Buchak et al. \(2024\)](#) compare the impact of quantitative easing on bank and nonbank  
161 mortgage lending using a structural model estimated with post-GFC data. [Xiao \(2020\)](#)  
162 studies money market funds, but not real economy lending by nonbanks.

163 Our key contribution, therefore, is to provide a comprehensive assessment of the role  
164 of nonbanks in the credit channel of monetary policy across markets. We show that  
165 nonbanks weaken the impact of monetary policy on credit supply, and that this in turn  
166 reduces the transmission to corporate investment, durable goods consumption, and house  
167 prices. However, the strength of these effects varies across markets. In particular, the  
168 substitution from bank to nonbank credit is stronger in the auto loan market than in the  
169 mortgage and corporate loan markets, with important implications for the strength of  
170 monetary policy via the credit channel.

171 Our paper is also related to [Chen et al. \(2018\)](#), who analyze the impact of monetary  
172 policy on banks and shadow banks in China. Our paper differs on multiple dimensions.  
173 First, we compare three different credit markets, which reveals important differences in  
174 the strength of the substitution across markets. Second, we use comprehensive loan-  
175 level data, which allows us to better control for borrower fundamentals (including credit  
176 demand) and hence identify credit supply effects, whereas [Chen et al. \(2018\)](#) rely on  
177 lender-level data. Third, we analyze real effects.<sup>6</sup>

178 Finally, we contribute to the broader literature on nonbank financial intermediaries.  
179 Nonbanks have increased lending due to technological advances, liquidity transformation,  
180 and bank regulation ([Moreira and Savov, 2017](#); [Buchak et al., 2018](#); [Ordonez, 2018](#); [Irani  
181 et al., 2021](#)). This may lead to better allocation of risk and credit availability ([Ivashina  
182 and Sun, 2011](#); [Shivdasani and Wang, 2011](#); [Nadauld and Weisbach, 2012](#); [Fuster et al.,  
183 2019](#)). But it may also increase fragility in crisis times ([Irani et al., 2021](#)). We contribute  
184 to this literature by showing that monetary policy affects nonbank credit supply.

185 The paper proceeds as follows. Section 2 summarizes our datasets and discusses our  
186 empirical strategy. Section 3 presents our results on the impact of monetary policy on  
187 bank and nonbank lending in the three markets, and the real effects. Section 4 concludes.

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<sup>6</sup>In contrast to [Cucic and Gorea \(2024\)](#), who study unsecured lending in Denmark, we study multiple credit markets in the largest economy in the world (including significant geographical variation and associated aggregated real effects) and notably mortgages—the key source of household credit.

## 188 2. Datasets and Empirical Strategy

189 In this section, we discuss the datasets that we use for each loan market. We then  
190 discuss our empirical strategy to identify the impact of monetary policy on the supply of  
191 credit from nonbank lenders, and the associated real effects.

### 192 2.1. Datasets

193 A key data obstacle to analyzing the nonbank credit channel is that almost all loan-  
194 level datasets, including central banks' credit registers, only include loans from banks.  
195 We overcome this by studying three major credit markets (corporate loans, mortgages,  
196 and auto loans) where nonbanks play an important role, and where we have access to  
197 confidential loan-level datasets that identify whether the lender is a bank or nonbank.  
198 This allows us to study the effects of monetary policy via nonbanks and banks.<sup>7</sup> Over  
199 our main sample period, nonbanks account for around 38%, 44%, and 55% of lending in  
200 syndicated loans, mortgage lending, and auto loans, respectively.

201 ***Syndicated corporate loans.*** We use data from the Shared National Credit Program  
202 (SNC) to study corporate lending. This is a confidential supervisory credit register of  
203 syndicated loans maintained by the Federal Reserve Board, FDIC, and Office of the  
204 Comptroller of the Currency (OCC). Over our sample period, the SNC collects infor-  
205 mation on all loans of at least \$20 million that are held by three or more regulated  
206 financial institutions (the reporting threshold was raised from \$20 million to \$100 million  
207 in 2018). [Ivashina and Scharfstein \(2010\)](#) report that around 95% of loans in the widely-  
208 used DealScan dataset meet the criteria to be included in the SNC, which suggests that  
209 the SNC's coverage of the syndicated loan market is almost comprehensive. The SNC  
210 dataset provides detailed loan-level information, including the identity of the borrower,  
211 the origination and maturity dates, and the loan type (credit line or term loan).

212 The dataset also identifies the holders of the loan (both banks and nonbank investors)  
213 and the size of their respective investments. Crucially, the dataset tracks these holdings  
214 over time—providing a snapshot of loan holdings at each year-end—and therefore accounts  
215 for secondary market trading, which is the main way in which nonbanks acquire syndicated

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<sup>7</sup>Despite this loan data advantage, we do not have information on nonbank-level balance sheets and funding, and for the auto loan market we do not observe the identity of the lender (we only observe whether it is a bank or nonbank).

216 loans. For our empirical analysis, we focus on the sample of loans originated in year  $t$  (i.e.  
217 new loans); given that syndicated loans are typically distributed in the secondary market  
218 soon after issuance (Lee et al., 2019), the year-end snapshot of loan holdings reflects  
219 sales of these new loans into the secondary market and, therefore, accurately reflects  
220 participation by both banks and nonbanks. We focus on the period from 1999 onward for  
221 comparability with the auto loan sample (discussed below). To study firm-level effects,  
222 we use corporate balance sheet data from Compustat.

223 **Mortgages.** We use mortgage application data collected under the Home Mortgage Dis-  
224 closure Act (HMDA), which records the vast majority of residential mortgages in the US.  
225 We use the confidential version of HMDA, which includes the origination date (the public  
226 version only includes origination year). The loan-level data include detailed information  
227 on the borrower, loan, and property, as well as the name of the lender. We classify lenders  
228 as banks or nonbanks following an algorithm based on that in Buchak et al. (2018); see  
229 Online Appendix B. We focus on the period from 1999:Q1 onward for comparability with  
230 the auto loan dataset. We draw a 20% random sample from HMDA, resulting in a sample  
231 of around 20 million mortgages. For our county-level analysis, we only include counties  
232 that have at least 10 mortgage originations in every quarter prior to 2007, to avoid the  
233 results being driven by very small counties; this captures around 90% of the loans in the  
234 full sample. We also use county-level data on house prices from CoreLogic.

235 **Auto loans.** We use data from the New York Fed Consumer Credit Panel (CCP) to  
236 study the consumer auto loan market. The CCP is based on credit bureau data from  
237 Equifax, and provides anonymized quarterly information on the liabilities of a nationally  
238 representative panel of US households since 1999:Q1. The dataset includes information  
239 on several forms of household debt, including mortgages, credit cards, student loans, and  
240 auto loans. We focus on auto loans, because the dataset separately records auto loan  
241 balances from banks and nonbanks. For each lender type, we identify new auto loans by  
242 an increase in the balance of at least \$500; we then compute the new loan amount as the  
243 difference between the auto loan balance in the current and previous quarter. The CCP  
244 also provides information on the borrower, such as location, birth year, Equifax risk score,  
245 and a bankruptcy indicator. We draw a 10% random sample from the CCP, which yields  
246 a panel of around 1.6 million households. For more information on the CCP, see Lee and  
247 van der Klaauw (2010). We also use county-level auto sales data from Polk.

248 *2.2. Specialization and persistence in nonbank lending*

249 If there are frictions in the ability of borrowers to switch between types of lenders—  
250 nonbanks vs banks—then differences in the response of banks and nonbanks to monetary  
251 policy could lead to differences in borrowers’ access to credit. In addition to the long liter-  
252 ature on borrower-lender relationships, new research—notably [Blickle et al. \(forthcoming\)](#)  
253 and [Paravisini et al. \(2023\)](#)—has documented that lenders specialize in lending to groups  
254 of borrowers, rather than to a particular borrower (e.g., borrowers in real estate, or in  
255 trade, rather than a single borrower in this particular industry). In this subsection, follow-  
256 ing these papers, we analyze similar specialization measures in our three credit markets,  
257 as well as the persistence of this specialization over time.<sup>8</sup>

258 To test the idea that frictions could affect the ability of borrowers to switch between  
259 bank and nonbank lenders in our three markets, we first estimate measures of lender  
260 specialization adapted from [Blickle et al. \(forthcoming\)](#) and [Paravisini et al. \(2023\)](#). These  
261 show that nonbank lending to certain industries (for corporate loans) and counties (for  
262 mortgages and auto loans) is materially higher than would be the case under a perfectly  
263 diversified portfolio.<sup>9</sup>

264 We first compute the share of loans to industry or county  $s$  in total new lending by  
265 lender type  $j$  (bank or nonbank) in period  $t$ :

$$\text{Lending share}_{j,s,t} = \frac{\text{Loan amount}_{j,s,t}}{\sum_s \text{Loan amount}_{j,s,t}}.$$

266 We then compute the share of loans to industry or county  $s$  in total new lending across

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<sup>8</sup>Previous research has shown that in the syndicated lending market, lenders specialize in lending to particular industries due to informational advantages ([Bao, 2022](#); [Giometti and Pietrosanti, 2022](#); [Jiang and Li, 2022](#); [Blickle et al., forthcoming](#)). Corporate borrowers’ access to credit from particular lenders is therefore likely to depend on their industry. Similar arguments apply to household loans, where local expertise matters. In the mortgage market, local knowledge improves lenders’ ability to assess borrower risk ([Loutskina and Strahan, 2011](#); [Ergungor and Moulton, 2014](#)). Local networks also continue to play an important role in banks’ decisions about where to lend ([Berrospide et al., 2016](#); [Gilje et al., 2016](#)) and borrowers’ decisions about where to borrow ([Alexandrov and Koulayev, 2017](#)). Geography also affects access to credit in the auto loan market. Local auto dealers typically maintain long-term relationships with a small number of auto lenders (banks and nonbank finance companies); when a dealer sells a car on finance, it negotiates the terms of the loan on behalf of one of these lenders ([Grunewald et al., 2020](#)). These long-term relationships limit the choice of lenders available to consumers at any given dealer, and hence generate geographical variation in access to credit from particular lenders ([Benmelech et al., 2017](#)).

<sup>9</sup>[Blickle et al. \(forthcoming\)](#) compute these measures at the level of individual lenders. We compute them at the level of lender types (i.e. summing over banks or nonbanks), since the focus of our paper is nonbank versus bank credit supply, and we do not observe the identity of individual lenders in all markets (see Section 2.1).

267 both lender types (i.e. summing over both banks and nonbanks) in period  $t$ :

$$\text{Lending share}_{s,t} = \frac{\text{Loan amount}_{s,t}}{\sum_s \text{Loan amount}_{s,t}}.$$

268 By comparing the first ratio to the second, we can obtain measures of the degree to which  
269 lender type  $j$ 's lending to industry or county  $s$  differs from the share implied by perfect  
270 diversification across industries or counties. Following [Blickle et al. \(forthcoming\)](#), we  
271 compute two such measures:

$$\text{Relative specialization}_{j,s,t} = \frac{\text{Lending share}_{j,s,t}}{\text{Lending share}_{s,t}} \quad (1)$$

272 and

$$\text{Excess specialization}_{j,s,t} = \text{Lending share}_{j,s,t} - \text{Lending share}_{s,t}. \quad (2)$$

273 We summarize these measures of specialization for nonbank lenders in Panel A of Table  
274 2, where we present the average values of each of the specialization measures over our  
275 main sample period (1999–2012), both for the “top” industry or county (i.e. the industry  
276 or county in which the specialization measure is highest at time  $t$ ) and for all other  
277 industries or counties.<sup>10</sup> For mortgages and auto lending, the “relative specialization”  
278 measure indicates that nonbanks are over-invested in the “top” county by a factor of  
279 around 2; that is, the top county accounts for around double the share of nonbank lending  
280 that would be consistent with a perfectly diversified portfolio. The degree of relative  
281 specialization is even stronger for corporate lending, where nonbanks are over-invested in  
282 the “top” industry by a factor of 4.25. The “excess specialization measure” also indicates  
283 that nonbank lending to certain counties or industries significantly exceeds what would  
284 be predicted by perfect diversification.<sup>11</sup>

285 In summary, patterns of nonbank lending across industries and counties suggest a  
286 significant degree of specialization in nonbank lending, consistent with the idea that credit  
287 market frictions affect borrowers' ability to access nonbank credit. To provide further  
288 evidence for such frictions, we test for persistence over time in the nonbank share of

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<sup>10</sup>Throughout the paper, industry is defined as three-digit NAICS.

<sup>11</sup>Our results on industry-level specialization are comparable to those in [Blickle et al. \(forthcoming\)](#).

289 lending across industries and counties. We run regressions of the form:

$$\text{Nonbank share}_{s,t} = \delta_t + \beta \text{Nonbank share}_{s,t-1} + \varepsilon_{s,t}, \quad (3)$$

290 where  $\text{Nonbank share}_{s,t}$  is the nonbank share of lending in industry  $s$  and year  $t$  (for  
291 corporate loans) or county  $s$  and quarter  $t$  (for mortgages and auto loans), and  $\delta_t$  is a  
292 time fixed effect. The sample period is 1999–2012.

293 Results from this regression are presented in Panel B of Table 2. In each market,  
294 we observe highly statistically significant persistence over time in the nonbank share of  
295 lending, consistent with frictions in the ability of borrowers to switch between bank and  
296 nonbank lenders. However, we also observe important differences across markets: our  
297 estimate of  $\beta$  is above 0.9 for corporate lending and mortgages, but only around 0.3 for  
298 auto lending, suggesting that substitution between bank and nonbank lending is easier in  
299 the auto loan market than in the other markets.

300 Taken together, using specialization measures from the literature, these results sug-  
301 gest significant specialization and persistence in nonbank lending across industries (for  
302 corporate loans) and across locations (for household loans). This, in turn, potentially  
303 limits borrowers’ ability to substitute between bank and nonbank credit after monetary  
304 policy changes, a question that we analyze in the rest of the paper.

### 305 *2.3. Monetary policy*

306 In this subsection we discuss how we measure monetary policy, the effects of monetary  
307 policy on total lending across the three different credit markets, and finally how monetary  
308 policy may affect banks and nonbanks differently.

309 ***Identifying monetary policy.*** In this paper we analyze the level of new loan issuance  
310 (at the loan level or aggregated), hence we use the level of monetary policy rates. For  
311 identification, we use the series of monetary policy shocks constructed by [Gertler and](#)  
312 [Karadi \(2015\)](#). This measure is based on high-frequency changes in three-months-ahead  
313 Fed Funds futures prices around FOMC policy announcements, and is available for the  
314 period 1990–2012. To convert this shock series into a level series, we take the cumulative  
315 sum, in line with recent literature ([Romer and Romer, 2004](#); [Coibion, 2012](#); [Cloyne and](#)  
316 [Hürtgen, 2016](#); [Ramey, 2016](#); [Bu et al., 2021](#); [Döttling and Ratnovski, 2023](#)). We explain

317 this transformation in more detail in Online [Appendix C](#).<sup>12</sup>

318 To validate that the Gertler-Karadi cumulative sum provides a good measure of the  
319 level of the monetary policy stance, we compare it to an estimate of  $(r - r^*)$ , i.e. the gap  
320 between the short-term real interest rate  $r$  and the neutral rate (or natural rate)  $r^*$ , which  
321 is a common measure of the stance of monetary policy (e.g., [Powell, 2023](#)). Using the  
322 New York Fed’s update of the [Laubach and Williams \(2003\)](#) measure of  $r^*$ , we estimate  
323 that over our main regression sample period (1999–2012), the correlation between the  
324 Gertler-Karadi cumulative sum and  $(r - r^*)$  is around 0.72 (see [Figure A1](#)).

325 We use several other measures of monetary policy in robustness tests: the Fed Funds  
326 rate; the shadow rate of [Wu and Xia \(2016\)](#), which reflects unconventional monetary policy  
327 tools when the Fed Funds rate is at its lower bound; the cumulative sum of [Nakamura and](#)  
328 [Steinsson \(2018\)](#) monetary policy shocks; and the [Laubach and Williams \(2003\)](#) estimate  
329 of  $(r - r^*)$  itself. We also consider an instrumental variables approach, where we use the  
330 Gertler-Karadi cumulative sum as an instrument for the estimate of  $(r - r^*)$ .

331 We also include a range of macroeconomic control variables in all our regressions:  
332 annual real GDP growth (from FRED); the average one-quarter-ahead forecast for real  
333 GDP growth from the Philadelphia Fed’s Survey of Professional Forecasters; annual PCE  
334 inflation (from FRED); and the VIX (a measure of financial market volatility).

335 ***Monetary policy and industry-level and county-level lending.*** Before turning to  
336 our loan-level analysis, we estimate the relationship between monetary policy and total  
337 lending at the industry-level or county-level in our three credit markets, via regressions  
338 of the form:

$$\text{Log}(\text{Total lending})_{s,t} = \alpha_s + \beta \text{MP}_{t-1} + \gamma \text{Controls}_{s,t-1} + \varepsilon_{s,t}, \quad (4)$$

339 where  $\text{Log}(\text{Total lending})_{s,t}$  is the log of total new lending to industry  $s$  in year  $t$  (for  
340 corporate loans) or county  $s$  in quarter  $t$  (for mortgages and auto loans), and  $\alpha_s$  is an  
341 industry or county fixed effect.  $\text{MP}_{t-1}$  is our baseline measure of monetary policy, i.e. the  
342 lagged cumulative sum of Gertler-Karadi monetary policy shocks (see above). We include  
343 a vector of lagged macroeconomic control variables (real GDP growth, forecasted GDP

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<sup>12</sup>While our baseline regressions are estimated in levels, in Online [Appendix D](#) we also estimate more aggregated regressions in differences.

344 growth, PCE inflation, and the VIX), as well as time-varying industry-level or county-level  
345 controls. The sample period is 1999–2012.

346 Results from this regression are summarised in Table 3. We find that total industry-  
347 level corporate lending and county-level mortgage lending both fall significantly following  
348 a monetary tightening (columns 1–4). In contrast, total county-level auto lending is not  
349 significantly affected by monetary policy (columns 5 and 6).

350 Given the large differences in the persistence of nonbank lending shares in corporate  
351 loans and mortgages vs auto loans (Section 2.2), we hypothesise that easier substitution  
352 between bank and nonbank credit in the auto loan market could contribute to this market’s  
353 weaker response to monetary policy. We explore this idea in the remainder of the paper.

354 *Monetary policy and nonbanks versus banks.* The large literature on the bank  
355 lending channel of monetary policy has identified a range of mechanisms that can cause  
356 bank credit supply to fall in response to tighter monetary policy. These include mech-  
357 anisms that appeal to specific features of the banking sector, such as capital regulation  
358 and deposit funding. Since nonbank lenders are typically less affected by regulation (in-  
359 cluding capital regulation) and do not have access to deposit funding, these mechanisms  
360 may apply differently to banks and nonbanks.

361 For example, tighter monetary policy may reduce the value of long-term bank securities  
362 and loans (by weakening the financial position of bank borrowers). This may tighten bank  
363 capital constraints (Disyatat, 2011), and hence lead to a fall in bank lending, particularly  
364 for banks that are already more capital constrained (Bernanke, 2007; Jiménez et al.,  
365 2012). By imposing tighter capital requirements, bank regulation potentially strengthens  
366 this channel (Kishan and Opiela, 2000, 2006; Van den Heuvel, 2002; Gambacorta, 2005).  
367 In contrast, the nonbanks that we study are generally subject to much weaker regulation,  
368 making them less likely to be affected by this channel.

369 Differences in short-term funding could also drive differences in bank and nonbank  
370 lending. Following regulators and the literature, we define banks as deposit-taking insti-  
371 tutions; by definition, therefore, the nonbanks that we study do not have access to deposit  
372 funding. This implies that any impact of monetary policy on deposit markets is likely  
373 to affect banks and nonbanks differently. In particular, under the “deposits channel” of  
374 Drechsler et al. (2017), when monetary policy tightens, banks use their deposit market  
375 power to raise deposit rates by less than the Fed Funds rate, in order to increase net

376 interest margins. In response, households move some of their deposits to higher-yielding  
377 instruments such as money market funds (MMFs). Since wholesale funding is more ex-  
378 pensive than deposit funding, this reduction in bank deposits may lead to a reduction in  
379 bank credit supply. Nonbanks do not use deposit funding and are therefore not subject  
380 to this channel. Indeed, if some of these deposits are recycled by MMFs into funding  
381 instruments that are used by nonbanks (such as commercial paper and repo), then this  
382 channel could lead to an increase in the funding available for nonbanks, and hence sup-  
383 port nonbank lending (Xiao, 2020). Begenau and Stafford (2022) have questioned the  
384 deposits channel, and moreover MMFs could lend back to banks.<sup>13</sup> Nevertheless, what is  
385 important for our paper is that monetary policy may affect deposits, which could impact  
386 banks and nonbanks differently, since banks use deposits while nonbanks do not.

### 387 3. Main Results

388 In this section, we study the impact of monetary policy on nonbank lending in each  
389 of our three markets in more depth. In each case, we first use loan-level data to estimate  
390 the differential impact of monetary policy on bank and nonbank lending, while controlling  
391 for granular borrower characteristics including credit demand. We then aggregate to the  
392 firm or county level, which allows us to assess the degree of substitution between banks  
393 and nonbanks, and the resulting effects on activity in the real economy. Given the high  
394 degree of specialization and persistence in nonbank lending established in Section 2.2, we  
395 particularly focus on how these effects vary with predetermined (1999) nonbank lending  
396 footprints across industries and counties.

#### 397 3.1. Syndicated corporate lending

398 A syndicated loan is a loan extended by multiple lenders to a single borrower. The  
399 syndicated loan market is a key source of funding for US corporates, with issuance of  
400 around \$2.4 trillion in 2019. Typically, a borrower takes out a “package” that includes  
401 several individual loan “facilities.” The two main types of facility are credit lines and  
402 term loans. Most lenders in the primary market are banks, although certain nonbanks

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<sup>13</sup>Aggregate evidence from the 2022–23 US tightening cycle appears consistent with bank deposits in part flowing to MMFs. As the Fed Funds rate rose, banks raised deposit rates by less than the Fed Funds rate, resulting in falling deposit balances (Kang-Landsberg et al., 2023). Meanwhile, MMF yields moved in line with the Fed Funds rate, resulting in flows into MMFs (Afonso et al., 2023).

403 (investment banks and finance companies) are also important. However, originators often  
 404 sell portions of the loan immediately in a secondary market, where a range of nonbank  
 405 investors play a much larger role, notably collateralized loan obligations (CLOs), but also  
 406 hedge funds, mutual funds, pensions funds, and other nonbank lenders. The SNC dataset  
 407 that we use tracks holdings of syndicated loans over time (by both banks and nonbanks),  
 408 which allows us to account for secondary market trading and hence accurately measure  
 409 nonbank participation.

410 The structure of the syndicated loan market allows us to cleanly identify credit supply  
 411 effects, for two reasons. First, syndicated loans involve multiple lenders participating in  
 412 the same loan. This allows us to compare how different lenders lend to the same firm  
 413 at the same time. Specifically, we use loan fixed effects, which control for unobserved  
 414 time-varying borrower characteristics, including credit demand (Khwaja and Mian, 2008;  
 415 Chodorow-Reich, 2014).<sup>14</sup> Second, while the borrower chooses the lead arranger for the  
 416 loan at the time of origination, the other members of the original syndicate are selected  
 417 in a book-building process run by the lead arranger, and are therefore not chosen by the  
 418 borrower (Bruche et al., 2020). Moreover, secondary market trades are entirely outside of  
 419 the borrower’s control. This ensures that the composition of lenders participating in the  
 420 loan is supply-driven.

421 ***Loan-level analysis.*** To estimate the impact of monetary policy on nonbank lending  
 422 relative to banks, we run regressions of the form:

$$\text{Log(Loan amount)}_{i,l,t} = \alpha_i + \delta_l + \beta (\text{Nonbank}_l \times \text{MP}_{t-1}) + \gamma \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \quad (5)$$

423 where  $\text{Log(Loan amount)}_{i,l,t}$  is the log of the amount of new loan  $i$  held by lender  $l$  in  
 424 year  $t$ .<sup>15</sup>  $\text{Nonbank}_l$  is an indicator variable equal to one for nonbank lenders and zero  
 425 for banks. We interact this indicator with  $\text{MP}_{t-1}$ , the lagged cumulative sum of Gertler-  
 426 Karadi monetary policy shocks. The coefficient  $\beta$  therefore provides an estimate of how  
 427 nonbank lending changes relative to bank lending when monetary policy tightens.

428 Since syndicated loans involve multiple lenders participating in the same loan, we

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<sup>14</sup>Since we focus on new loans, these loan fixed effects are equivalent to loan-time fixed effects, because each loan only appears in the sample on one date.

<sup>15</sup>As discussed in Section 2.1, we only include loans originated in year  $t$  (new loans), but include immediate sales of these loans into the secondary market.

429 can include loan fixed effects  $\alpha_i$ . These fixed effects imply that our results are driven  
430 by variation between bank and nonbank participation in the same loan, and therefore  
431 comprehensively control for borrower and loan characteristics, including credit demand.  
432 Note that, since we only include new loans (loans originated in year  $t$ ), the loan fixed effects  
433 absorb time fixed effects and hence control for the uninteracted level of all macroeconomic  
434 variables, including monetary policy,  $MP_{t-1}$ . We also include lender fixed effects  $\delta_l$  to  
435 control for time-invariant lender characteristics. Finally, we include interactions between  
436  $Nonbank_t$  and our standard set of macroeconomic control variables (real GDP growth,  
437 forecasted GDP growth, PCE inflation, and the VIX). The sample period is 1999–2012.

438 Results for equation (5) are summarised in Table 4. Column 1 includes all new loans.  
439 We find that nonbanks increase their syndicated loan holdings *relative* to banks when  
440 monetary policy tightens: a 25 basis point monetary tightening leads to a relative increase  
441 in nonbank loan size of around 4%.<sup>16</sup> The relative expansion holds for both credit lines  
442 (column 2) and term loans (column 3), with a larger effect for term loans, in which most  
443 nonbank lenders are more active.

444 Given the specialization and persistence in industry-level nonbank lending documented  
445 in Section 2.2, in columns 4–6 we add a triple interaction between the nonbank lender indi-  
446 cator, the monetary policy measure, and the industry-level predetermined nonbank share,  
447 measured as the share of outstanding term loans held by nonbanks as of 1999 (industry  
448 is measured as three-digit NAICS). We find that the relative increase in nonbank lending  
449 following a monetary tightening is significantly larger for industries where nonbanks have  
450 historically had a larger presence: a one-standard-deviation increase in predetermined  
451 nonbank share (0.14) increases the impact of a 25 basis point tightening on nonbank vs  
452 bank lending by around 1.3 percentage points. In columns 7–9, we do a similar exercise,  
453 but in this case we estimate the impact of monetary policy on nonbank vs bank lending  
454 separately for industries in each quartile of predetermined nonbank share. We again find  
455 that the relative increase in nonbank lending following a monetary tightening is larger for  
456 industries with a larger predetermined nonbank presence, with insignificant results (and  
457 a much lower estimated coefficient) for the lowest quartile. In sum, these results suggest  
458 that, after a monetary tightening, nonbanks expand credit relatively more to firms in

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<sup>16</sup>Note that Table 3 shows that, after a monetary policy tightening, total lending falls. Our descriptions of results in Tables 4–9 are in relative terms, i.e., after a monetary tightening, lending by nonbanks falls by less than lending by banks, which we describe as an increase in nonbank lending relative to banks.

459 industries in which nonbanks have a larger (vs lower) established presence, and hence the  
460 ease with which firms can substitute from bank to nonbank lending following a monetary  
461 tightening crucially depends on their industry.

462 **Further robustness tests.** We present several further robustness tests in the Online  
463 Appendix. In Table F1, we re-estimate our main specification using several alternative  
464 measures of monetary policy: the Fed Funds rate; the shadow rate of Wu and Xia (2016);  
465 the cumulative sum of Nakamura and Steinsson (2018) monetary policy shocks; and an  
466 estimate of  $(r - r^*)$ . Relatedly, in Table F2, we run instrumental variable regressions where  
467 we measure monetary policy using  $(r - r^*)$  instrumented by the Gertler-Karadi cumulative  
468 sum; this approach can be motivated on the basis that  $(r - r^*)$  is an appropriate measure  
469 of the monetary policy stance but is measured with error, since  $r^*$  is unobservable (and  
470 is sometimes estimated with future information). In each case, we again find that tighter  
471 monetary policy is associated with higher nonbank (vs bank) lending. Our results are  
472 also similar if we measure predetermined nonbank share as of 2001 (rather than 1999)  
473 and start the regression sample in 2002 (Table F3).

474 In Table F4, we include an interaction between the nonbank lender indicator, the  
475 monetary policy measure, and an indicator variable equal to one if lender  $l$  has lent  
476 to borrower  $b$  in a previous loan; this should capture informational advantages at the  
477 borrower level, rather than the industry level.<sup>17</sup> For term loans, we find that the relative  
478 increase in nonbank lending is larger if the lender has previously lent to the borrower.  
479 For credit lines, the estimated coefficient is positive but insignificant.<sup>18</sup> Finally, in Table  
480 F5, the dependent variable is the proportion of loan  $i$  held by lender  $l$  in year  $t$ . We find  
481 that the share of the loan held by nonbanks increases when monetary policy tightens.

482 **Firm-level analysis.** To explore firm-level real effects, we use Compustat to construct  
483 a panel dataset of balance sheet and income statement variables for listed US firms, and  
484 merge this with the industry-level predetermined nonbank share of lending from SNC. We

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<sup>17</sup>For example, Hachem (2011) shows that relationship lenders smooth out the transmission of monetary policy shocks in environments with asymmetric information.

<sup>18</sup>Table F4 also shows that a monetary tightening leads to higher lending from nonbanks (relative to banks) *in general*, including nonbanks that have not previously lent to the borrower. The increase in lending from nonbanks that have a previous relationship with the borrower is *additional* to this more general nonbank effect.

485 then run regressions of the form:

$$\text{Outcome}_{i,j,t} = \alpha_i + \delta_t + \beta (\text{Nonbank share}_j \times \text{MP}_{t-1}) + \gamma \text{Controls}_{i,j,t-1} + \varepsilon_{i,t}, \quad (6)$$

486 for firm  $i$  in industry  $j$  in year  $t$ , where “nonbank share” is the nonbank share of out-  
487 standing term loans measured at the three-digit industry level as of 1999. We include firm  
488 fixed effects  $\alpha_i$ , year fixed effects  $\delta_t$ , lagged time-varying firm-level controls (return-on-  
489 assets and log of total assets), and lagged macro controls interacted with predetermined  
490 nonbank share. Note that the year fixed effects control for the uninteracted level of all  
491 macroeconomic variables, including monetary policy,  $\text{MP}_{t-1}$ . We also include interactions  
492 between the monetary policy measure and two industry-level control variables (return-on-  
493 assets measured at the industry level, and log of total industry assets); these control for  
494 industry-level variation in the impact of monetary policy via characteristics other than  
495 access to nonbank credit.

496 The results are summarised in Table 5. Column 1 shows that, following a monetary  
497 tightening, firms in industries with a higher predetermined nonbank share of credit in-  
498 crease CAPEX relative to other firms. For a firm in an industry with the average value of  
499 predetermined nonbank share (0.27), a 25 basis point monetary tightening is associated  
500 with an increase in CAPEX of around 3.5%, relative to firms in industries where prede-  
501 termined nonbank share is zero. These firms also experience a relative increase in fixed  
502 assets (column 2) and total assets (column 3). Consistent with these real effects, we find  
503 that for a firm in an industry with average predetermined nonbank share, a 25 basis point  
504 tightening is associated with an increase in total debt of around 1.7%, relative to firms in  
505 industries with predetermined nonbank share of zero (column 4). Columns 5 and 6 (which  
506 are based on new syndicated term loans) show that this increase in total debt includes  
507 increased syndicated lending (column 5), particularly from nonbanks (column 6).<sup>19</sup>

508 The results in Table 5 estimate the impact of nonbank credit on monetary transmission  
509 to firm-level economic outcomes in a “reduced form” manner. In Online Appendix E, we  
510 show more explicitly that this impact comes via credit markets, by running two-stage least  
511 squares regressions where the first-stage estimates the impact of nonbanks on monetary

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<sup>19</sup>The sample size in columns 5 and 6 is much smaller than in columns 1–4. This is because columns 5 and 6 only include observations where the firm obtains a new syndicated loan, whereas columns 1–4 include all observations where the firm reports balance sheet data.

512 transmission to firm-level credit, and the second-stage regresses firm-level outcomes on the  
513 predicted values for credit from the first-stage. The results using this two-stage approach  
514 are fully consistent with the reduced form results in Table 5.

515 Taken together, the results in this section suggest that: (i) nonbanks weaken the overall  
516 transmission of monetary policy via credit, as nonbanks in part substitute for reductions  
517 in bank lending after a monetary tightening; and (ii) the imperfect substitution between  
518 banks and nonbanks is not compensated with other sources of finance, leading to real  
519 effects at the firm level through the nonbank credit channel of monetary policy.

520 ***Variation across nonbank lenders.*** The syndicated lending market includes a diverse  
521 range of nonbank lenders. This allows us to shed light on the mechanisms potentially  
522 underlying the differential response of banks and nonbanks to monetary policy.

523 In Table 6, we estimate our loan-level equation (5) with separate coefficients for dif-  
524 ferent types of nonbank lender. The results for each lender type are summarised in  
525 column 6, which shows that the increase in nonbank lending relative to banks is primarily  
526 driven by four types of nonbanks: collateralized loan obligations (CLOs), hedge funds,  
527 broker-dealers, and finance companies. The estimated coefficient for pension funds is also  
528 relatively large but is not statistically significant at conventional levels. Motivated by  
529 these results, we group nonbank lenders into different categories to test two potential  
530 mechanisms.

531 We first consider a role for regulatory constraints. Nonbanks are typically more weakly  
532 regulated than banks, which could be why nonbanks increase lending relative to banks  
533 after a monetary tightening. Under this mechanism, we should find that the increase in  
534 credit supply is larger for the least regulated nonbanks. To test this idea, we compare  
535 different types of investment funds that are each major investors in syndicated loans  
536 but which face very different regulatory regimes: mutual funds (which are subject to  
537 a range of regulatory requirements under the Investment Company Act of 1940) versus  
538 hedge funds and CLOs (which are much more lightly regulated). In column 1 of Table  
539 6, we separately compare these “high regulation” (mutual funds) and “low regulation”  
540 (hedge funds and CLOs) investment funds to banks (we drop all other nonbanks from the  
541 sample). In response to tighter monetary policy, we observe a large increase in syndicated  
542 loan holdings by low regulation nonbanks relative both to banks and to high regulation  
543 nonbanks. In column 2, we expand these categories to include nonbanks with a wider range

544 of business models: here the high regulation group consists of mutual funds, insurance  
545 companies, and pension funds, while the low regulation group consists of hedge funds,  
546 CLOs, and finance companies. We again find that the increase in nonbank relative to  
547 bank lending following a monetary tightening is driven by the less-regulated nonbanks.

548 We next consider a role for differences in funding structure. By acting directly on  
549 short-term funding costs, monetary policy could affect financial intermediaries differently  
550 depending on their exposure to short-term funding markets. We therefore categorise non-  
551 banks into two groups: a “short-term funding” group consisting of broker-dealers, hedge  
552 funds, and finance companies (which rely heavily on short-term money market instru-  
553 ments such as repo and commercial paper); and a “long-term funding” group consisting  
554 of insurance companies and pension funds (which have longer-term funding structures).<sup>20</sup>  
555 In column 3, we compare each of these two groups to banks (again, we drop all other  
556 nonbanks from the sample), and find no difference in their response to monetary policy.

557 Finally, in columns 4 and 5, we estimate regressions that simultaneously test for both  
558 regulatory and funding mechanisms (we use our first measure of regulatory stringency  
559 in column 4 and our second measure in column 5). In both cases, we again find that  
560 the relative increase in nonbank lending is driven by “low-regulation” nonbanks.<sup>21</sup> In  
561 summary, these results suggest that differences in regulatory constraints play a key role  
562 in driving the differential response of nonbanks to monetary policy, while there is no clear,  
563 robust evidence of a mechanism involving differences in short-term funding structures.

### 564 *3.2. Household lending: Mortgages and auto loans*

565 In this subsection, we present results for both mortgages and auto loans, first at the  
566 loan level, and second aggregated at the county level to analyze overall economic effects.

567 ***Residential mortgage lending.*** With around \$10 trillion outstanding, the household  
568 mortgage market is the largest credit market in the US. Nonbanks have played a significant  
569 role in the market since the 1990s, and account for around 44% of loan originations over  
570 our main sample period (1999–2012). After origination, lenders can either hold mortgages  
571 on their balance sheets or sell them in the secondary market. Nonbanks typically focus

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<sup>20</sup>These groups reflect the fragile versus stable liability categories in [Irani et al. \(2021\)](#).

<sup>21</sup>In column 5, we find evidence that nonbanks with short-term funding structures reduce lending relative to banks following a monetary tightening, although comparing columns 3–5 shows that this result is not robust across specifications.

572 on loans that can be sold to the GSEs or Ginnie Mae, and play a much smaller role in  
 573 the market for “jumbo” loans. Lenders must fund mortgages at origination, even if they  
 574 sell them later. Nonbank lenders rely heavily on short-term wholesale funding, notably  
 575 warehouse lines of credit—a form of short-term credit extended mostly by commercial and  
 576 investment banks (Kim et al., 2018). The lines are paid off with the proceeds of mortgage  
 577 sales and securitization.

578 To estimate the differential response of bank and nonbank mortgage lending to mon-  
 579 etary policy, we estimate loan-level regressions of the form:

$$\text{Log(Loan amount)}_{i,l,c,t} = \alpha_{c,t} + \delta_{c,l} + \beta (\text{Nonbank}_{l,t} \times \text{MP}_{t-1}) + \gamma \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \quad (7)$$

580 where  $\text{Log(Loan amount)}_{i,l,c,t}$  is the log of the size of mortgage  $i$  originated by lender  $l$   
 581 in county  $c$  and quarter  $t$ .  $\text{Nonbank}_{l,t}$  is an indicator variable equal to one for nonbank  
 582 lenders and zero for banks (the time subscript accounts for the fact that some lenders  
 583 switch charters during our sample period).  $\text{MP}_{t-1}$  is our measure of monetary policy.

584 An important advantage of using loan-level data is that we can include county-time  
 585 fixed effects  $\alpha_{c,t}$ . These control for time-varying local economic conditions that are likely to  
 586 be correlated with credit demand (such as local housing market developments), and imply  
 587 that our results are driven by variation between bank and nonbank lending within the same  
 588 county in the same time period (year:quarter). Lender-county fixed effects  $\delta_{c,l}$  control for  
 589 persistent differences in lender-level lending across counties. As explained in Section 2,  
 590 we also include borrower-level characteristics (income, gender, race), and macroeconomic  
 591 control variables interacted with the nonbank lender indicator. For consistency with the  
 592 auto loan analysis below, our baseline sample period is 1999–2012.

593 We present estimated results for equation (7) in Table 7. The results mirror our  
 594 loan-level results for corporate loans. When monetary policy tightens, nonbanks extend  
 595 significantly larger mortgages than banks, with a 25 basis point monetary tightening  
 596 leading to a relative increase in nonbank loan size of around 1.5% (columns 1 and 2).  
 597 In columns 3 and 4, we add a triple interaction between the nonbank lender indicator,  
 598 the monetary policy measure, and the county-level nonbank share of mortgage lending  
 599 in 1999:Q1.<sup>22</sup> We find that the relative increase in nonbank lending is larger in counties

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<sup>22</sup>We show the cross-country distribution of nonbank mortgage lending in Figure A2 in the Appendix.

600 with a larger predetermined nonbank share: a one-standard-deviation increase in nonbank  
601 share (0.09) increases the impact of a 25 basis point monetary tightening on nonbank vs  
602 bank lending by around 1 percentage point. We obtain a similar result when we estimate  
603 the nonbank vs bank lending response separately for counties in each quartile of prede-  
604 termined nonbank share (columns 5 and 6): the relative expansion in nonbank lending  
605 following a monetary tightening increases monotonically for higher quartiles. In sum,  
606 these results suggest that, after a monetary tightening, nonbanks expand credit relatively  
607 more to households in counties in which nonbanks have historically been more specialized,  
608 and hence the ease with which households can substitute from bank to nonbank lending  
609 following a monetary tightening crucially depends on their location.

610 As for corporate loans, we obtain similar results for a range of monetary policy mea-  
611 sures (Table F6), including when we instrument  $(r - r^*)$  with the Gertler-Karadi cumu-  
612 lative sum (Table F2). Results are also similar if we measure predetermined nonbank  
613 share as of 2001:Q1 rather than 1999:Q1 (Table F3). In Table F4, we include a triple  
614 interaction between the nonbank lender indicator, the monetary policy measure, and an  
615 indicator variable equal to one if lender  $l$  has originated a mortgage in county  $c$  within the  
616 previous two years; this provides a more granular measure of local market knowledge. We  
617 find that the relative increase in nonbank lending is larger in counties where the lender  
618 has recent lending experience.<sup>23</sup>

619 Overall, our loan-level results suggest that nonbanks relatively increase mortgage  
620 credit supply after a monetary tightening, but that the substitution between bank and  
621 nonbank mortgage credit in response to monetary policy is significantly affected by the  
622 degree to which nonbanks have an established local presence.

623 **Consumer auto loans.** Most new cars in the US are bought on credit or leased. At  
624 its peak in 2006, outstanding auto credit was \$785 billion, accounting for over 30% of  
625 consumer debt. Nonbank finance companies—including the captive financing arms of car  
626 manufacturers (e.g. Ford Motor Credit)—have long played an important role in the auto  
627 loan market (Ludvigson, 1998). During our sample period, nonbanks typically account for  
628 more than half of lending. Most nonbank lenders in the auto loan market rely on short-

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<sup>23</sup>As for corporate loans, we also find that a monetary tightening leads to more credit supply from nonbanks (relative to banks) *in general*, including from nonbanks that have not recently lent to the county.

629 term wholesale funding, such as asset-backed commercial paper (ABCP). [Benmelech et al.](#)  
630 [\(2017\)](#) describe the history of nonbank credit in this market.

631 To estimate the relative response of bank and nonbank credit to monetary policy, we  
632 estimate the regression:

$$\text{Log(Loan amount)}_{i,l,c,t} = \alpha_{c,t} + \delta_{c,l} + \beta (\text{Nonbank}_l \times \text{MP}_{t-1}) + \gamma \text{Controls}_{i,l,t} + \varepsilon_{i,l,t}, \quad (8)$$

633 where  $\text{Log(Loan amount)}_{i,l,c,t}$  is the log of the size of the new loan to individual  $i$  from  
634 lender type  $l$  (bank or nonbank) in county  $c$  and quarter  $t$ .  $\text{Nonbank}_l$  is an indicator  
635 variable equal to one for loans from nonbanks and zero for loans from banks.  $\text{MP}_{t-1}$  is  
636 the monetary policy measure.

637 As explained in [Section 2](#), we exploit the granularity of our dataset by including a rich  
638 set of lagged household-level variables to control for borrower risk and determinants of  
639 credit demand: Equifax risk score, income, birth-year fixed effects, an indicator variable  
640 for bankruptcy, and outstanding balances in other types of credit (mortgage, credit card,  
641 and other consumer loans). We include county  $\times$  time fixed effects  $\alpha_{c,t}$  to control for local  
642 demand conditions, and county  $\times$  lender-type fixed effects  $\delta_{c,l}$  to control for persistent  
643 differences in nonbank vs bank lending across counties.<sup>24</sup> We also include interactions  
644 between the nonbank loan indicator and our standard set of macroeconomic controls.  
645 The sample period is 1999–2012.

646 We present results for equation (8) in [Table 8](#). Columns 1 and 2 show that when  
647 monetary policy tightens, nonbanks extend significantly larger auto loans than banks: a  
648 25 basis point monetary tightening is associated with an increase in nonbank loan size of  
649 around 4.5% compared to banks.

650 In columns 3 and 4 we add a triple interaction between the nonbank loan indicator,  
651 the monetary policy measure, and the county-level predetermined nonbank share of out-  
652 standing auto credit as of 1999:Q1 (the start of the sample).<sup>25</sup> In contrast to the other  
653 two credit markets, we find that the relative increase in nonbank lending following a  
654 monetary tightening does not vary significantly across counties in relation to the prede-

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<sup>24</sup>Unlike the other two markets, in this market we do not observe the identity of the lender, but only whether it is a bank or nonbank. Hence we cannot include lender fixed effects, so instead include lender type (interacted with county) fixed effects.

<sup>25</sup>We show the cross-country distribution of nonbank auto lending in [Figure A3](#) in the Online Appendix.

655 terminated nonbank share. Similarly, in columns 5 and 6, when we estimate the relative  
656 response of nonbank credit to monetary policy separately for counties in each quartile of  
657 predetermined nonbank share, we again find that the relative increase in nonbank lending  
658 is similar across counties.<sup>26</sup>

659 These results therefore suggest that—unlike the corporate loan and mortgage markets—  
660 the degree of substitutability between bank and nonbank auto credit is largely unaffected  
661 by predetermined nonbank presence, suggesting that frictions in the ability of borrowers  
662 to switch between bank and nonbank credit are much weaker in this market. This is  
663 consistent with the lower persistence over time of nonbank auto credit documented in  
664 Table 2, and is also consistent with the weaker response of total auto credit to monetary  
665 policy shown in Table 3.<sup>27</sup> We explore this further in the county-level analysis below.

666 *County-level analysis for mortgages and auto loans.* We now test whether the  
667 differences in credit supply highlighted above (estimated using loan-level data) translate  
668 into different effects on real economic activity associated with our two household credit  
669 markets. To do so, we estimate county-level regressions of the form:

$$\text{Outcome}_{c,t} = \alpha_c + \delta_t + \beta (\text{Nonbank share}_c \times \text{MP}_{t-1}) + \gamma \text{Controls}_{c,t} + \varepsilon_{c,t}, \quad (9)$$

670 for county  $c$  in quarter  $t$ , where “nonbank share” is the county-level predetermined non-  
671 bank share of mortgage lending (or outstanding auto credit) in 1999:Q1. We include  
672 county fixed effects  $\alpha_c$ , time fixed effects  $\delta_t$ , lagged time-varying county-level controls,  
673 and lagged macro controls interacted with predetermined nonbank share. We also include  
674 the interaction between the monetary policy measure and lagged county-level income to  
675 control for other county-level factors that might impact monetary transmission.

676 Estimated results for equation (9) are shown in Table 9. We find that when monetary  
677 policy tightens, house prices in counties with a larger predetermined nonbank presence  
678 increase relative to house prices elsewhere (column 1). For a county with the average  
679 value of predetermined nonbank share (0.51), a 25 basis point monetary tightening is

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<sup>26</sup>Results are similar for a range of monetary policy measures (Tables F7 and F2), and if we measure predetermined nonbank share as of 2001:Q1 rather than 1999:Q1 (Table F3).

<sup>27</sup>As monetary policy tightens, banks cut lending. In the auto loan market, nonbanks increase lending strongly even in local areas where nonbanks have a lower presence (and hence banks have a larger presence). Therefore, substitution between bank and nonbank credit is much easier than in the other two credit markets, which reduces the effects of monetary policy on total lending.

680 associated with an increase in house prices of around 1.4%, relative to a county with  
681 nonbank share of zero. In contrast, the impact of monetary policy on auto sales is not  
682 significantly affected by predetermined nonbank presence in the county (column 4).

683 Consistent with our loan-level results, our county-level analysis suggest that these  
684 differences are explained by differences in substitution between bank and nonbank credit  
685 in the two markets. In counties with a larger predetermined nonbank presence, total  
686 mortgage lending falls by less following a monetary tightening (column 2), driven by an  
687 increase in nonbank lending relative to banks (column 3). Meanwhile, the response of  
688 total auto lending to monetary policy is largely unrelated to the degree of predetermined  
689 nonbank presence, consistent with very strong substitution between bank and nonbank  
690 credit across counties (column 5).<sup>28</sup>

691 The results in Table 9 suggest that the strong substitution from bank to nonbank credit  
692 following a monetary tightening largely neutralizes any impact of monetary policy on  
693 auto sales via credit supply.<sup>29</sup> Differently, for mortgages, we find that nonbanks attenuate  
694 (but do not fully neutralize) the impact of monetary policy on total mortgage lending  
695 volumes and house prices via a credit supply channel. These results are consistent with  
696 the differences between the auto loan and mortgage markets in Tables 2, 3, 7, and 8.

#### 697 4. Conclusion

698 In this paper, we show that nonbank lenders weaken the transmission of monetary  
699 policy to investment, consumption, and house prices via a credit supply channel. Across  
700 three major credit markets, we find that tighter monetary policy shifts credit supply

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<sup>28</sup>In Table F8, we show that the *number* of loans from nonbanks after a monetary tightening increases with predetermined nonbank share in both markets, but with a larger effect for mortgages (column 1) than auto loans (column 3), suggesting that the total mortgage and auto credit effects in Table 9 reflect the extensive margin, in addition to the intensive margin results shown in Tables 7 and 8. In Table F9, we estimate separate responses of total bank and nonbank lending at the firm-level or county-level in each of our three markets. Total nonbank lending (incorporating both the intensive and extensive margins) increases more for industries and counties with a larger predetermined nonbank presence. In the auto loan market, we also observe strong retrenchment by banks in counties with higher nonbank share, suggesting that banks respond by cutting lending to counties where they have a smaller relative presence themselves, and concentrating more on counties where they have a larger relative presence.

<sup>29</sup>We also observe limits to this substitution. In column 1 of Table F10 in the Online Appendix, we estimate equation (9) for total auto credit, but replace predetermined nonbank share with an indicator variable equal to one for counties whose nonbank share was in the lowest quartile in 1999:Q1. We find that when monetary policy tightens, total auto credit in the lowest-quartile counties falls relative to other counties. Column 2 shows that these effects are driven by each of the other quartiles. Column 3 shows that the lowest-quartile counties also experience a fall in auto sales.

701 from banks to nonbanks, thereby attenuating its impact on corporate investment and  
702 house prices, while largely neutralizing its impact on durable goods consumption via  
703 credit. Interestingly, evidence from the syndicated lending market suggests a key role for  
704 regulatory constraints in driving the differential response of nonbanks to monetary policy.

705 This paper exploits exhaustive, confidential US loan-level data that identify whether  
706 the lender is a bank or nonbank. However, we are limited in our ability to study par-  
707 ticular borrower-lender pairs over time, as in the auto loan market, we do not observe  
708 the identities of individual lenders, and households typically take out very few mortgages  
709 over their lifetimes. Therefore, an interesting question for future research is whether long-  
710 term relationships between specific borrowers and lenders—which can be analyzed in the  
711 corporate credit market—are different for banks vs nonbanks with respect to firms, and  
712 what the implications for monetary policy transmission are.

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Table 1: Summary statistics

| Variable  | Observations | Mean  | Std Dev | Q1    | Median | Q3    |
|---|--------------|-------|---------|-------|--------|-------|
| <i>Monetary policy measures</i>                       |              |       |         |       |        |       |
| MP (demeaned cumulative sum of Gertler-Karadi shocks) | 54           | 0.00  | 0.46    | -0.47 | -0.02  | 0.09  |
| Federal Funds rate                                    | 56           | 2.50  | 2.17    | 0.19  | 1.85   | 4.74  |
| Wu-Xia shadow rate                                    | 56           | 2.14  | 2.48    | -0.03 | 1.52   | 4.67  |
| $(r - r^*)$   | 56           | -0.59 | 1.77    | -2.17 | -1.53  | 1.35  |
| <i>Loan-level corporate lending variables</i>         |              |       |         |       |        |       |
| Log(Loan amount)                                      | 743,621      | 15.07 | 1.72    | 13.86 | 14.93  | 16.34 |
| Nonbank lender indicator                              | 743,621      | 0.60  | 0.49    | 0     | 1      | 1     |
| 1999 nonbank share                                    | 743,621      | 0.26  | 0.14    | 0.15  | 0.26   | 0.37  |
| <i>Firm-level variables</i>                           |              |       |         |       |        |       |
| Log(CAPEX)  | 61,085       | 15.61 | 2.98    | 13.60 | 15.81  | 17.75 |
| Log(Fixed assets)                                     | 62,861       | 17.12 | 3.22    | 15.00 | 17.27  | 19.44 |
| Log(Total assets)                                     | 66,158       | 18.94 | 2.76    | 17.23 | 19.13  | 20.88 |
| Log(Total debt)                                       | 66,069       | 17.14 | 2.87    | 14.20 | 16.69  | 19.62 |
| Log(Total new term loans)                             | 7,393        | 18.67 | 1.43    | 17.73 | 18.68  | 19.63 |
| Nonbank share of new term loans                       | 7,393        | 0.35  | 0.36    | 0     | 0.21   | 0.75  |
| 1999 nonbank share                                    | 64,050       | 0.27  | 0.12    | 0.19  | 0.29   | 0.37  |
| <i>Industry-level variables</i>                       |              |       |         |       |        |       |
| Log(Total new syndicated credit)                      | 1,014        | 22.11 | 1.57    | 21.11 | 22.22  | 23.26 |
| Nonbank share of lending                              | 934          | 0.44  | 0.26    | 0.22  | 0.47   | 0.65  |
| <i>Loan-level mortgage variables</i>                  |              |       |         |       |        |       |
| Log(Loan amount)                                      | 19,869,381   | 11.83 | 0.80    | 11.39 | 11.89  | 12.37 |
| Nonbank lender indicator                              | 19,869,381   | 0.37  | 0.48    | 0     | 0      | 1     |
| 1999 nonbank share                                    | 19,869,381   | 0.40  | 0.09    | 0.34  | 0.39   | 0.46  |
| <i>County-level mortgage variables</i>                |              |       |         |       |        |       |
| Log(House price index)                                | 42,294       | 4.86  | 0.22    | 4.70  | 4.82   | 4.99  |
| Log(Total lending)                                    | 42,294       | 19.04 | 1.38    | 18.04 | 18.93  | 19.95 |
| Nonbank share of lending                              | 42,294       | 0.34  | 0.12    | 0.25  | 0.35   | 0.43  |
| 1999 nonbank share                                    | 42,294       | 0.51  | 0.11    | 0.44  | 0.50   | 0.58  |
| <i>Loan-level auto loan variables</i>                 |              |       |         |       |        |       |
| Log(Loan amount)                                      | 1,014,831    | 9.44  | 0.82    | 9.09  | 9.59   | 9.97  |
| Nonbank lender indicator                              | 1,014,831    | 0.53  | 0.50    | 0     | 1      | 1     |
| 1999 nonbank share                                    | 1,014,831    | 0.55  | 0.17    | 0.47  | 0.58   | 0.67  |
| <i>County-level auto loan variables</i>               |              |       |         |       |        |       |
| Log(Auto sales)                                       | 122,987      | 5.70  | 1.52    | 4.63  | 5.50   | 6.56  |
| Log(Total lending)                                    | 158,459      | 8.30  | 4.76    | 7.92  | 10.31  | 11.38 |
| Nonbank share of lending                              | 121,103      | 0.50  | 0.36    | 0.16  | 0.51   | 0.81  |
| 1999 nonbank share                                    | 158,461      | 0.53  | 0.28    | 0.35  | 0.55   | 0.72  |

*Notes:* The table shows summary statistics for variables used in the regressions. Auto sales refers to the number of car sales. The sample period is 1999–2012 (2002–2012 for auto sales).

Table 2: Specialization and persistence in nonbank lending

| <i>Panel A: Specialization</i>  |                 |        |           |        |            |        |
|---------------------------------|-----------------|--------|-----------|--------|------------|--------|
| Market:                         | Corporate loans |        | Mortgages |        | Auto loans |        |
| County / industry:              | Top             | Others | Top       | Others | Top        | Others |
|                                 | (1)             | (2)    | (3)       | (4)    | (5)        | (6)    |
| Relative specialization         | 4.25            | 1.18   | 2.42      | 1.00   | 1.89       | 1.14   |
| Excess specialization (percent) | 4.95            | -0.06  | 0.65      | 0.00   | 0.81       | 0.00   |

| <i>Panel B: Persistence</i> |                          |          |           |          |            |          |
|-----------------------------|--------------------------|----------|-----------|----------|------------|----------|
| Market:                     | Corporate loans          |          | Mortgages |          | Auto loans |          |
| Dependent variable:         | Nonbank share of lending |          |           |          |            |          |
|                             | (1)                      | (2)      | (3)       | (4)      | (5)        | (6)      |
| Lagged nonbank share        | 0.950***                 | 0.942*** | 0.917***  | 0.939*** | 0.280***   | 0.285*** |
|                             | (0.021)                  | (0.020)  | (0.021)   | (0.020)  | (0.019)    | (0.021)  |
| Time fixed effects          | No                       | Yes      | No        | Yes      | No         | Yes      |
| Observations                | 934                      | 934      | 42,294    | 42,294   | 121,300    | 121,300  |
| $R^2$                       | 0.638                    | 0.645    | 0.845     | 0.908    | 0.082      | 0.140    |

*Notes:* Panel A summarizes measures of nonbank lending specialization based on the measures in [Blickle et al. \(forthcoming\)](#). These are computed at the industry level (for corporate loans) or the county level (for mortgages and auto loans) over the sample period 1999–2012. “Relative specialization” and “excess specialization” are defined in equations (1) and (2), respectively. The table shows the average value of each of the measures, both for the “top” industry or county (i.e. the industry or county in which the specialization measure is highest at time  $t$ ) and for all other industries or counties. Panel B shows regression results for equation (3) estimated at the industry-year level (for corporate loans) or the county-quarter level (for mortgages and auto loans). The sample period is 2000–2012 (columns 1 and 2) or 1999–2012 (columns 3–6). The dependent variable is the nonbank share of total lending in the industry or county; this is regressed on its first lag and time fixed effects. Observations are weighted by lagged industry-level total lending (columns 1 and 2) or lagged county-level income (columns 3–6). Standard errors are clustered by industry (columns 1 and 2) or county and state-quarter (columns 3–6) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 3: Monetary policy and industry-level and county-level lending

| Market:                | Corporate loans      |                      | Mortgages            |                      | Auto loans       |                  |
|------------------------|----------------------|----------------------|----------------------|----------------------|------------------|------------------|
| Dependent variable:    | (1)                  | (2)                  | Total lending        |                      | (5)              | (6)              |
|                        | (1)                  | (2)                  | (3)                  | (4)                  | (5)              | (6)              |
| MP                     | -0.457***<br>(0.099) | -0.436***<br>(0.067) | -0.132***<br>(0.040) | -0.159***<br>(0.039) | 0.130<br>(0.081) | 0.005<br>(0.046) |
| Industry fixed effects | No                   | Yes                  | -                    | -                    | -                | -                |
| Industry controls      | No                   | Yes                  | -                    | -                    | -                | -                |
| County fixed effects   | -                    | -                    | No                   | Yes                  | No               | Yes              |
| County controls        | -                    | -                    | No                   | Yes                  | No               | Yes              |
| Macro controls         | No                   | Yes                  | No                   | Yes                  | No               | Yes              |
| Observations           | 910                  | 889                  | 42,294               | 42,294               | 158,463          | 158,463          |
| $R^2$                  | 0.021                | 0.825                | 0.002                | 0.895                | 0.000            | 0.494            |

*Notes:* The table shows regression results for equation (4) estimated at the industry-year level (for corporate loans) or the county-quarter level (for mortgages and auto loans). The sample period is 1999–2012. The dependent variables are the log of industry-level total syndicated lending (columns 1 and 2), the log of total county-level mortgage lending (columns 3 and 4), and the log of total county-level new auto lending (columns 5 and 6). ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Industry controls are lags of log(total assets) and return-on-assets measured at the industry level. County controls are lagged county-level income (column 4), or lagged county-level income and lagged average Equifax risk score (column 6). Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by industry (columns 1 and 2) or county and state-quarter (columns 3–6) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 4: Impact of monetary policy on loan-level corporate lending

| Dependent variable:<br>Sample:                 | Loan amount         |                     |                     |                     |                     |                     |                     |                     |                     |
|--|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|  | All loans<br>(1)    | Credit lines<br>(2) | Term loans<br>(3)   | All loans<br>(4)    | Credit lines<br>(5) | Term loans<br>(6)   | All loans<br>(7)    | Credit lines<br>(8) | Term loans<br>(9)   |
| Nonbank $\times$ MP                            | 0.159***<br>(0.039) | 0.105***<br>(0.032) | 0.160***<br>(0.043) | 0.154***<br>(0.039) | 0.105***<br>(0.032) | 0.151***<br>(0.043) |                     |                     |                     |
| Nonbank $\times$ Nonbank share $\times$ MP     |                     |                     |                     | 0.364**<br>(0.152)  | 0.275*<br>(0.162)   | 0.381**<br>(0.181)  |                     |                     |                     |
| Nonbank $\times$ Q1(Nonbank share) $\times$ MP |                     |                     |                     |                     |                     |                     | 0.095<br>(0.080)    | 0.092<br>(0.065)    | 0.081<br>(0.113)    |
| Nonbank $\times$ Q2(Nonbank share) $\times$ MP |                     |                     |                     |                     |                     |                     | 0.213***<br>(0.073) | 0.155**<br>(0.069)  | 0.225***<br>(0.086) |
| Nonbank $\times$ Q3(Nonbank share) $\times$ MP |                     |                     |                     |                     |                     |                     | 0.260***<br>(0.052) | 0.139***<br>(0.050) | 0.286***<br>(0.062) |
| Nonbank $\times$ Q4(Nonbank share) $\times$ MP |                     |                     |                     |                     |                     |                     | 0.195***<br>(0.061) | 0.143**<br>(0.061)  | 0.199***<br>(0.068) |
| Loan fixed effects                             | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lender fixed effects                           | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Macro control interactions                     | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lower-order terms                              | -                   | -                   | -                   | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Observations                                   | 743,621             | 235,719             | 504,837             | 743,621             | 235,719             | 504,837             | 743,621             | 235,719             | 504,837             |
| $R^2$  | 0.828               | 0.899               | 0.703               | 0.828               | 0.899               | 0.703               | 0.828               | 0.899               | 0.703               |

*Notes:* The table shows regression results for equation (5) estimated at the loan-lender level. The sample period is 1999–2012, and the sample consists of loans originated in year  $t$ . The dependent variable is the log of the lender’s holding of the loan. ‘Nonbank’ is an indicator variable for nonbank lenders. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘Nonbank share’ is the nonbank share of total outstanding syndicated term loans, based on SNC data and measured at the industry level (three-digit NAICS) as of 1999. ‘Q1(Nonbank share)’ is an indicator variable for industries in the first quartile of 1999 nonbank share. Similarly for Q2, Q3, and Q4. For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by loan and lender-year and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 5: Impact of monetary policy on firm-level outcomes

| Dependent variable:                       | Compustat variables |                     |                     |                   | New syndicated credit  |                      |
|---|---------------------|---------------------|---------------------|-------------------|------------------------|----------------------|
|   | CAPEX<br>(1)        | Fixed assets<br>(2) | Total assets<br>(3) | Total debt<br>(4) | Total new loans<br>(5) | Nonbank share<br>(6) |
| Nonbank share $\times$ MP                 | 0.524**<br>(0.203)  | 0.484***<br>(0.173) | 0.215**<br>(0.084)  | 0.247*<br>(0.142) | 1.040*<br>(0.537)      | 0.215**<br>(0.092)   |
| Firm fixed effects                        | Yes                 | Yes                 | Yes                 | Yes               | No                     | No                   |
| Industry fixed effects                    | -                   | -                   | -                   | -                 | Yes                    | Yes                  |
| Time fixed effects                        | Yes                 | Yes                 | Yes                 | Yes               | Yes                    | Yes                  |
| Firm controls                             | Yes                 | Yes                 | Yes                 | Yes               | No                     | No                   |
| Industry controls $\times$ MP             | Yes                 | Yes                 | Yes                 | Yes               | Yes                    | Yes                  |
| Industry controls $\times$ Macro controls | Yes                 | Yes                 | Yes                 | Yes               | Yes                    | Yes                  |
| Nonbank share $\times$ Macro controls     | Yes                 | Yes                 | Yes                 | Yes               | Yes                    | Yes                  |
| Observations                              | 61,485              | 63,296              | 66,264              | 66,174            | 7,393                  | 7,393                |
| $R^2$                                     | 0.923               | 0.964               | 0.968               | 0.905             | 0.166                  | 0.155                |

*Notes:* The table shows regression results for equation (6) estimated at the firm-year level. The sample consists of listed US firms from 2000 to 2012, excluding financial services and public sector firms. In columns 1–4, the dependent variables are from Compustat: log of CAPEX (column 1), log of property, plant, and equipment (column 2), log of total assets (column 3), and log of total debt (column 4). In columns 5 and 6, the dependent variables are based on new syndicated term loans from SNC: log of total new term loans (column 5), and nonbank share of new term loans (column 6). ‘Nonbank share’ is the nonbank share of total outstanding syndicated term loans, based on SNC data and measured at the industry level (three-digit NAICS) as of 1999. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Firm controls are lags of log(total assets) and return-on-assets. Industry controls are lags of log(total assets) and return-on-assets measured at the industry level. Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by industry-year and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 6: Impact of monetary policy on loan-level corporate lending, by lender type

| Dependent variable:                    | Loan amount         |                     |                   |                     |                      |                     |
|--|---------------------|---------------------|-------------------|---------------------|----------------------|---------------------|
|  | (1)                 | (2)                 | (3)               | (4)                 | (5)                  | (6)                 |
| Low regulation $\times$ MP             | 0.548***<br>(0.071) |                     |                   | 0.475***<br>(0.059) |                      |                     |
| High regulation $\times$ MP            | -0.001<br>(0.090)   |                     |                   | -0.056<br>(0.084)   |                      |                     |
| Low regulation (extended) $\times$ MP  |                     | 0.354***<br>(0.061) |                   |                     | 0.443***<br>(0.060)  |                     |
| High regulation (extended) $\times$ MP |                     | -0.060<br>(0.075)   |                   |                     | -0.076<br>(0.084)    |                     |
| Short-term funding $\times$ MP         |                     |                     | 0.066<br>(0.051)  | 0.031<br>(0.039)    | -0.248***<br>(0.043) |                     |
| Long-term funding $\times$ MP          |                     |                     | -0.056<br>(0.092) | 0.008<br>(0.082)    | 0.065<br>(0.089)     |                     |
| CLO $\times$ MP                        |                     |                     |                   |                     |                      | 0.532***<br>(0.061) |
| Hedge fund $\times$ MP                 |                     |                     |                   |                     |                      | 0.277***<br>(0.081) |
| Broker-dealer $\times$ MP              |                     |                     |                   |                     |                      | 0.275***<br>(0.078) |
| Finance company $\times$ MP            |                     |                     |                   |                     |                      | 0.088*<br>(0.053)   |
| Pension fund $\times$ MP               |                     |                     |                   |                     |                      | 0.191<br>(0.146)    |
| Insurance company $\times$ MP          |                     |                     |                   |                     |                      | -0.030<br>(0.086)   |
| Mutual fund $\times$ MP                |                     |                     |                   |                     |                      | -0.039<br>(0.083)   |
| Other nonbank $\times$ MP              |                     |                     |                   |                     |                      | 0.089<br>(0.056)    |
| Loan fixed effects                     | Yes                 | Yes                 | Yes               | Yes                 | Yes                  | Yes                 |
| Lender fixed effects                   | Yes                 | Yes                 | Yes               | Yes                 | Yes                  | Yes                 |
| Lender-type $\times$ Macro controls    | Yes                 | Yes                 | Yes               | Yes                 | Yes                  | Yes                 |
| Observations                           | 627,372             | 681,784             | 387,551           | 687,270             | 687,270              | 744,784             |
| $R^2$                                  | 0.836               | 0.834               | 0.850             | 0.833               | 0.833                | 0.829               |

*Notes:* The table shows regression results for equation (5) estimated at the loan-lender level, with separate coefficients for different types of nonbank lender. The sample period is 1999–2012, and the sample consists of loans originated in year  $t$ . The dependent variable is the log of the lender’s holding of the loan. ‘Low regulation’ is an indicator variable for CLOs and hedge funds. ‘High regulation’ is an indicator variable for mutual funds. ‘Low regulation (extended)’ is an indicator variable for CLOs, hedge funds, and finance companies. ‘High regulation (extended)’ is an indicator variable for mutual funds, insurance companies, and pension funds. ‘Short-term funding’ is an indicator variable for broker-dealers, hedge funds, and finance companies. ‘Long-term funding’ is an indicator variable for insurance companies and pension funds. We drop from the regression sample nonbanks that are classified as neither low nor high regulation (columns 1 and 2), nonbanks that are classified as neither short-term nor long-term funding (column 3), or nonbanks that are not classified in any of these categories (columns 4 and 5). ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by loan and lender-year and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 7: Impact of monetary policy on loan-level mortgage lending

| Dependent variable:                            | Loan amount         |                     |                     |                     |                     |                     |
|--|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|  | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 |
| Nonbank $\times$ MP                            | 0.067***<br>(0.008) | 0.060***<br>(0.007) | 0.064***<br>(0.007) | 0.056***<br>(0.007) |                     |                     |
| Nonbank $\times$ Nonbank share $\times$ MP     |                     |                     | 0.450***<br>(0.059) | 0.331***<br>(0.054) |                     |                     |
| Nonbank $\times$ Q1(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.017**<br>(0.008)  | 0.020**<br>(0.008)  |
| Nonbank $\times$ Q2(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.027***<br>(0.009) | 0.029***<br>(0.009) |
| Nonbank $\times$ Q3(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.074***<br>(0.012) | 0.058***<br>(0.009) |
| Nonbank $\times$ Q4(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.109***<br>(0.011) | 0.096***<br>(0.011) |
| Time fixed effects                             | Yes                 | -                   | Yes                 | -                   | Yes                 | -                   |
| County $\times$ Time fixed effects             | No                  | Yes                 | No                  | Yes                 | No                  | Yes                 |
| County $\times$ Lender fixed effects           | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Loan-level controls                            | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Macro control interactions                     | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lower-order terms                              | -                   | -                   | Yes                 | Yes                 | Yes                 | Yes                 |
| Observations                                   | 19,869,381          | 19,869,381          | 19,869,381          | 19,869,381          | 19,869,381          | 19,869,381          |
| $R^2$  | 0.465               | 0.472               | 0.465               | 0.472               | 0.465               | 0.472               |

*Notes:* The table shows regression results for equation (7) estimated at the loan level. The sample period is 1999–2012. The dependent variable is the log of the size of the mortgage. ‘Nonbank’ is an indicator variable for nonbank lenders. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘Nonbank share’ is the county-level nonbank share of mortgage lending in 1999:Q1. ‘Q1(Nonbank share)’ is an indicator variable for counties in the first quartile of 1999 nonbank share. Similarly for Q2, Q3, and Q4. Loan-level controls are log applicant income, and indicator variables for female borrowers and black borrowers. For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by county and quarter and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 8: Impact of monetary policy on loan-level auto lending

| Dependent variable:                            | Loan amount         |                     |                     |                     |                     |                     |
|--|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|  | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 |
| Nonbank $\times$ MP                            | 0.180***<br>(0.028) | 0.186***<br>(0.028) | 0.184***<br>(0.028) | 0.186***<br>(0.028) |                     |                     |
| Nonbank $\times$ Nonbank share $\times$ MP     |                     |                     | -0.041<br>(0.040)   | -0.033<br>(0.043)   |                     |                     |
| Nonbank $\times$ Q1(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.193***<br>(0.030) | 0.187***<br>(0.032) |
| Nonbank $\times$ Q2(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.199***<br>(0.029) | 0.201***<br>(0.028) |
| Nonbank $\times$ Q3(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.177***<br>(0.030) | 0.179***<br>(0.029) |
| Nonbank $\times$ Q4(Nonbank share) $\times$ MP |                     |                     |                     |                     | 0.150***<br>(0.031) | 0.159***<br>(0.038) |
| Time fixed effects                             | Yes                 | -                   | Yes                 | -                   | Yes                 | -                   |
| County $\times$ Time fixed effects             | No                  | Yes                 | No                  | Yes                 | No                  | Yes                 |
| County $\times$ Lender-type fixed effects      | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Loan-level controls                            | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Macro control interactions                     | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lower-order terms                              | -                   | -                   | Yes                 | -                   | Yes                 | -                   |
| Observations                                   | 1,014,831           | 984,324             | 1,014,831           | 984,324             | 1,014,831           | 984,324             |
| $R^2$  | 0.089               | 0.176               | 0.089               | 0.176               | 0.089               | 0.177               |

*Notes:* The table shows regression results for equation (8) estimated at the loan level. The sample period is 1999–2012. The dependent variable is the log of new auto lending. ‘Nonbank’ is an indicator variable for loans from nonbanks. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘Nonbank share’ is the county-level nonbank share of outstanding auto loans in 1999:Q1. ‘Q1(Nonbank share)’ is an indicator variable for counties in the first quartile of 1999 nonbank share. Similarly for Q2, Q3, and Q4. Loan-level controls are lags of Equifax risk score, log income, log mortgage balance, log credit card balance, log other consumer loan balance, an indicator variable for bankruptcy, and birth-year fixed effects. For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by county and quarter and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table 9: Impact of monetary policy on county-level mortgages and auto lending

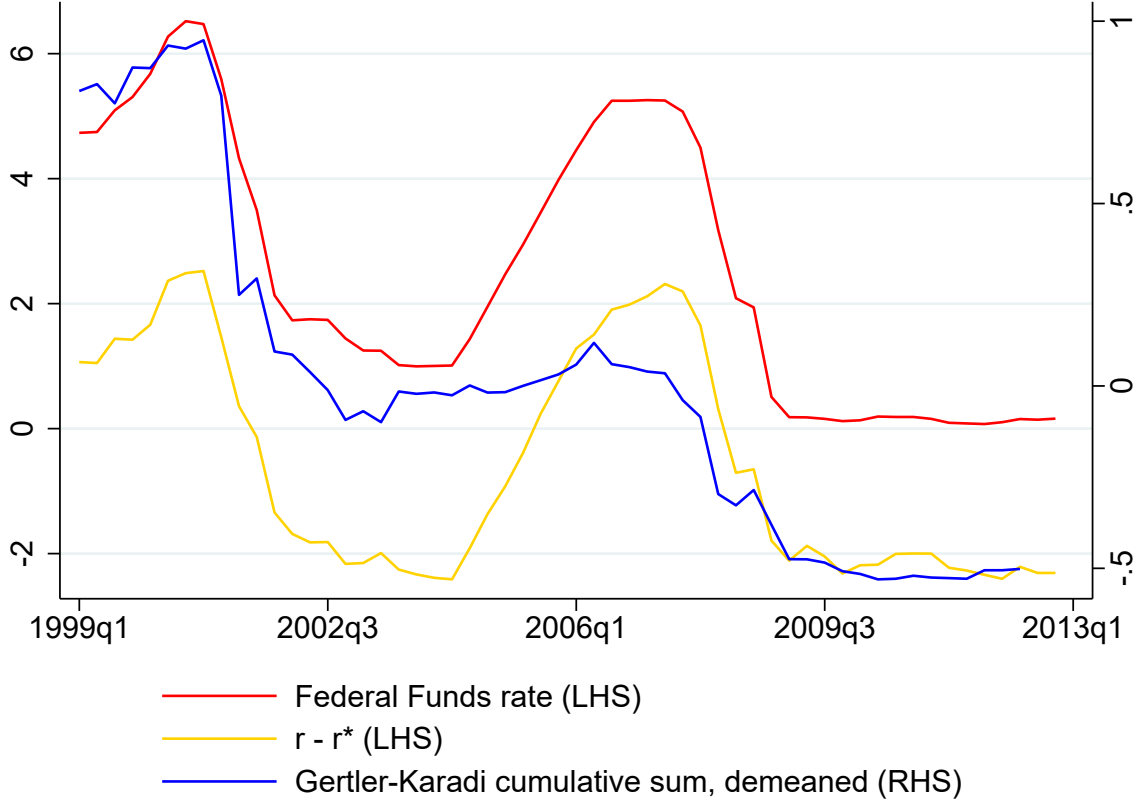
| Market:                               | Mortgages          |                   |                     | Auto loans       |                  |                     |
|---------------------------------------|--------------------|-------------------|---------------------|------------------|------------------|---------------------|
| Dependent variable:                   | House prices       | Total lending     | Nonbank share       | Auto sales       | Total lending    | Nonbank share       |
|                                       | (1)                | (2)               | (3)                 | (4)              | (5)              | (6)                 |
| Nonbank share $\times$ MP             | 0.110**<br>(0.047) | 0.272+<br>(0.168) | 0.155***<br>(0.034) | 0.037<br>(0.023) | 0.100<br>(0.119) | 0.086***<br>(0.019) |
| County fixed effects                  | Yes                | Yes               | Yes                 | Yes              | Yes              | Yes                 |
| Time fixed effects                    | Yes                | Yes               | Yes                 | Yes              | Yes              | Yes                 |
| County controls                       | Yes                | Yes               | Yes                 | Yes              | Yes              | Yes                 |
| County income $\times$ MP             | Yes                | Yes               | Yes                 | Yes              | Yes              | Yes                 |
| County income $\times$ Macro controls | Yes                | Yes               | Yes                 | Yes              | Yes              | Yes                 |
| Nonbank share $\times$ Macro controls | Yes                | Yes               | Yes                 | Yes              | Yes              | Yes                 |
| Observations                          | 42,294             | 42,294            | 42,294              | 122,987          | 158,459          | 121,103             |
| $R^2$                                 | 0.802              | 0.970             | 0.849               | 0.989            | 0.502            | 0.148               |

*Notes:* The table shows regression results for equation (9) estimated at the county-quarter level. The sample period is 1999–2012 (2002–2012 for column 4). Columns 1–3 show results for the mortgage market, and the dependent variables are: log of the house price index (column 1), log of total mortgage lending (column 2), and nonbank share of mortgage lending (column 3). Columns 4–6 show results for the auto loan market, and the dependent variables are: log of the number of auto sales (column 4), log of total new auto lending (column 5), and nonbank share of new auto lending (column 6). ‘Nonbank share’ is the county-level nonbank share of mortgage lending in 1999:Q1 (columns 1–3), or the county-level nonbank share of outstanding auto credit as of 1999:Q1 (columns 4–6). ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. County controls are lagged county-level income (columns 1–3), or lagged county-level income and lagged average Equifax risk score (columns 4–6). Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by county and quarter and shown in parentheses. +, \*, \*\*, and \*\*\* indicate significance at 12%, 10%, 5%, and 1%.

**ONLINE APPENDIX**

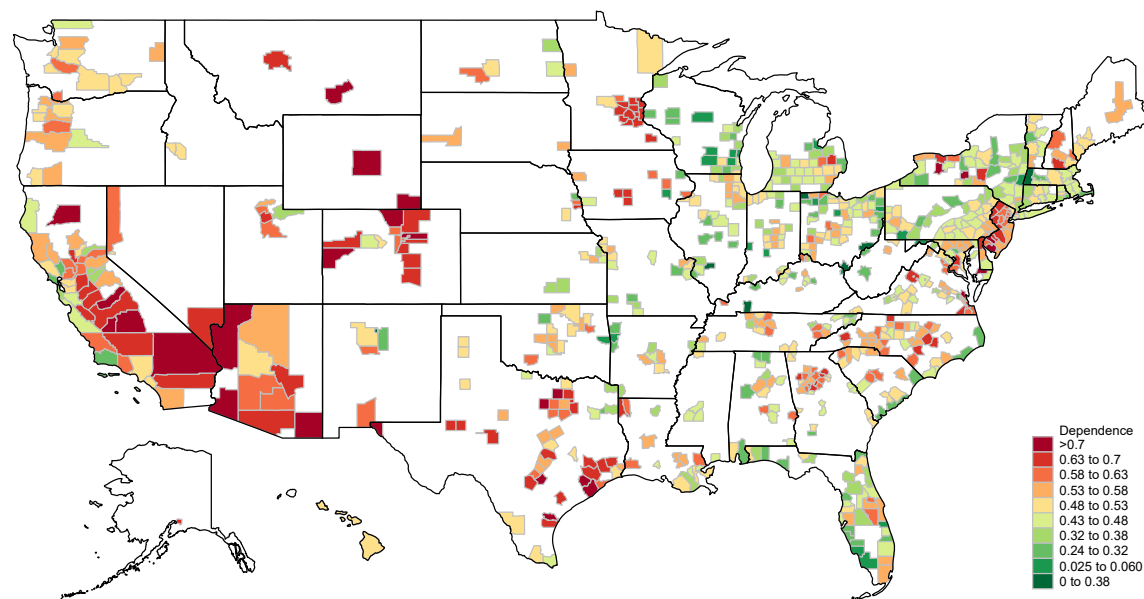
Appendix A. Figures

Figure A1: Measures of monetary policy



Notes: The figure shows the Federal Funds rate, the cumulative sum of Gertler and Karadi (2015) monetary policy shocks, and an estimate of  $(r - r^*)$  based on the New York Fed's update of the Laubach and Williams (2003) measure of  $r^*$ .

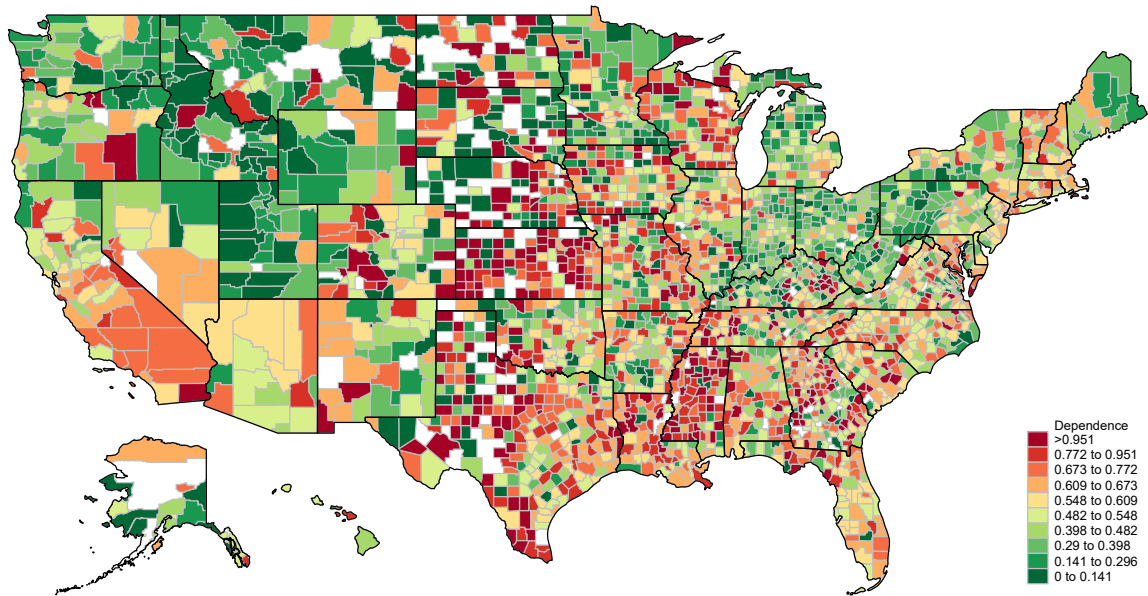
Figure A2: Distribution of nonbank mortgage lending



Source: HMDA

*Notes:* The figure shows the county-level nonbank share of mortgage lending in 1999:Q1, based on data from HMDA. We only include counties that have at least 10 mortgage originations in every quarter prior to 2007.

Figure A3: Distribution of nonbank auto lending



Source: Federal Reserve Bank of Chicago / Equifax

*Notes:* The figure shows the county-level share of outstanding auto loans financed by nonbanks in 1999:Q1, based on data from the New York Fed Consumer Credit Panel.

## Appendix B. Classification of nonbank lenders in HMDA dataset

We classify lenders in the HMDA dataset as banks or nonbanks following an algorithm based on that in [Buchak et al. \(2018\)](#). A lender is classified as a bank if it meets one of the four criteria below; otherwise it is classified as a nonbank.

1. All lenders regulated by the following agencies are classified as banks: OCC, FDIC, OTS, NCUA, CFPB.
2. Lenders regulated by the Federal Reserve System with the following (non-case-sensitive) strings in their name are classified as banks: “BANK”, “BK”, “BANCO”, “BANC”, “B&T”, “BNK”.
3. Lenders identified by HMDA’s “other lender code” as “Bank, Savings Association, or Credit Union” or “Mortgage Banking Subsidiary of a Community Bank” are classified as banks.
4. Following [Buchak et al. \(2018\)](#), we manually reclassify five lenders differently to the classification typically associated with their regulator.

## Appendix C. Construction of monetary policy shocks

As discussed in Section 2.3, for our main measure of monetary policy, we use the series of monetary policy shocks constructed by Gertler and Karadi (2015). This series is measured in terms of *changes*, specifically changes in the three-months-ahead Fed Funds futures price in the 30 minutes around FOMC policy announcements. However, we require a measure of the *level* of the monetary policy stance. This is because the dependent variable in our loan-level regressions is based on the level of new loan issuance, which cannot easily be converted into changes because individual households and firms take out loans infrequently.

For example, an individual will typically take out a new mortgage only a few times during her lifetime, for instance when she moves home or when her existing mortgage deal comes to an end. And at that point, the size and terms of the mortgage that she can obtain are likely to be based on credit conditions at the time, rather than recent *changes* in credit conditions. Similarly, individuals take out new auto loans infrequently (for example when they buy a car), at which point the loan terms that they can obtain are determined by the level of credit conditions at the time, rather than recent changes in credit conditions.

Following recent literature, we therefore convert the shock series into a level series by taking the cumulative sum (Romer and Romer, 2004; Coibion, 2012; Cloyne and Hürtgen, 2016; Ramey, 2016; Bu et al., 2021; Döttling and Ratnovski, 2023). Specifically, if  $\epsilon_h$  is the Gertler-Karadi shock in quarter  $h$ , then we measure the level of the monetary policy stance in quarter  $t$  as:

$$\text{MP}_t = \sum_{h=1}^t \epsilon_h,$$

where  $h = 1$  denotes the start of the Gertler-Karadi shock series (1990:Q1).

To validate that the Gertler-Karadi cumulative sum provides a good measure of the level of the monetary policy stance, we compare it to an estimate of  $(r - r^*)$ , i.e. the gap between the short-term real interest rate and the neutral rate. We construct our estimate of  $(r - r^*)$  as:

$$(\text{Fed Funds rate} - \text{E}[\pi]) - r^*,$$

where we use the New York Fed's update of the Laubach and Williams (2003) measure of  $r^*$ , and the estimate of short-term inflation expectations used by the New York Fed to

compute that estimate of  $r^*$ .<sup>30</sup> We estimate that over our main regression sample period (1999–2012), the correlation between the Gertler-Karadi cumulative sum and  $(r - r^*)$  is around 0.72.

In Figure A1, we plot the cumulative sum of Gertler-Karadi shocks against the Fed Funds rate and our estimate of  $(r - r^*)$ . The Gertler-Karadi sum reflects the broad contours of both the Fed Funds rate and  $(r - r^*)$ , with deviations that reflect the fact that the Gertler-Karadi series is based only on the surprise component of monetary policy.

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<sup>30</sup><https://www.newyorkfed.org/research/policy/rstar>

## Appendix D. Industry-level and county-level regressions in differences

As discussed in Section 2.3 and Appendix C, the dependent variable in our loan-level regressions is based on the level of new loan issuance. This cannot easily be converted into changes because individual households and firms take out loans infrequently. These regressions therefore require a measure of the level of monetary policy. For our baseline regressions, we use the monetary policy shocks of Gertler and Karadi (2015), which we convert into a level series by taking the cumulative sum.

However, when we aggregate to the industry level (for corporate loans) or county level (for mortgages and auto loans), the panel structure of the dataset means that it is also possible to run regressions in differences. In this appendix, we therefore replicate key industry-level and county-level regressions with the dependent variable in differences and using non-cumulated monetary policy shocks.

We first replicate results from Table 3. Specifically, we run industry-level and county-level regressions of the form:

$$y_{s,t+1} - y_{s,t-1} = \beta \text{MP shock}_{t-1} + \gamma \text{Controls}_{s,t-1} + \varepsilon_{s,t}, \quad (\text{D1})$$

where  $y_{s,t}$  is the log of total new lending to industry  $s$  in year  $t$  (for corporate loans) or county  $s$  in quarter  $t$  (for mortgages and auto loans). MP shock $_{t-1}$  is the (non-cumulated) monetary policy shock series of either Gertler and Karadi (2015), Bauer and Swanson (2022), or Nakamura and Steinsson (2018). The vector of controls includes lagged macroeconomic variables (real GDP growth, forecasted GDP growth, PCE inflation, and the VIX), and time-varying industry-level or county-level controls.

The results for regression (D1) are summarised in Table D1. As in Table 3, we find that total industry-level corporate lending and county-level mortgage lending fall following a contractionary monetary policy shock (columns 1–6), although for corporate lending, this effect is only statistically significant when using the Bauer-Swanson shocks. Meanwhile, we observe no significant impact of monetary policy shocks on total county-level auto lending (columns 7–9), also consistent with Table 3.

We next replicate results from Table 9. We estimate county-level regressions of the form:

$$y_{c,t+4} - y_{c,t-1} = \delta_t + \beta (\text{Nonbank share}_c \times \text{MP shock}_{t-1}) + \gamma \text{Controls}_{c,t-1} + \varepsilon_{c,t}, \quad (\text{D2})$$

where  $y_{c,t}$  is the nonbank share of either mortgage lending or auto lending in county  $c$  in quarter  $t$ , and  $\text{Nonbank share}_c$  is the county-level predetermined nonbank share of mortgage lending (or outstanding auto credit) in 1999:Q1. We include the same set of control variable as in Table 9 in the main paper, except that we do not include county fixed effects since our dependent variable is in differences. For equation (D2) we include longer differences ( $t - 1$  to  $t + 4$ ) because we find, empirically, that significant differences in the nonbank share of lending across counties take somewhat longer to emerge, likely reflecting lags in the monetary transmission mechanism.

Estimated results for equation (D2) are shown in Table D2. For both markets, we find that, following a contractionary monetary policy shock, the nonbank share of lending increases in counties with a larger predetermined nonbank presence (relative to other counties), consistent with our results in Table 9.

Table D1: Monetary policy and industry-level and county-level lending in differences

| Market:             | Corporate loans        |                      |                   | Mortgages           |                      |                      | Auto loans       |                  |                  |
|---------------------|------------------------|----------------------|-------------------|---------------------|----------------------|----------------------|------------------|------------------|------------------|
| Dependent variable: | $\Delta$ Total lending |                      |                   |                     |                      |                      |                  |                  |                  |
|                     | (1)                    | (2)                  | (3)               | (4)                 | (5)                  | (6)                  | (7)              | (8)              | (9)              |
| Gertler-Karadi      | -0.156<br>(0.169)      |                      |                   | -0.077**<br>(0.039) |                      |                      | 0.052<br>(0.108) |                  |                  |
| Bauer-Swanson       |                        | -1.200***<br>(0.314) |                   |                     | -0.255***<br>(0.069) |                      |                  | 0.040<br>(0.242) |                  |
| Nakamura-Steinsson  |                        |                      | -0.095<br>(0.211) |                     |                      | -0.291***<br>(0.046) |                  |                  | 0.118<br>(0.136) |
| Industry controls   | Yes                    | Yes                  | Yes               | -                   | -                    | -                    | -                | -                | -                |
| County controls     | -                      | -                    | -                 | Yes                 | Yes                  | Yes                  | Yes              | Yes              | Yes              |
| Macro controls      | Yes                    | Yes                  | Yes               | Yes                 | Yes                  | Yes                  | Yes              | Yes              | Yes              |
| Observations        | 821                    | 821                  | 821               | 41,496              | 41,496               | 41,496               | 120,312          | 120,312          | 120,312          |
| $R^2$               | 0.233                  | 0.245                | 0.232             | 0.143               | 0.147                | 0.152                | 0.001            | 0.001            | 0.001            |

*Notes:* The table shows regression results for equation (D1) estimated at the industry-year level (for corporate loans) or the county-quarter level (for mortgages and auto loans). The sample period is 1999–2012. The dependent variable is the change from  $t - 1$  to  $t + 1$  in the log of industry-level total syndicated lending (columns 1–3), total county-level mortgage lending (columns 4–6), or total county-level new auto lending (columns 7–9). The main explanatory variable is the lagged monetary policy shock series of [Gertler and Karadi \(2015\)](#), [Bauer and Swanson \(2022\)](#), or [Nakamura and Steinsson \(2018\)](#). Industry controls are lags of log(total assets) and return-on-assets measured at the industry level. County controls are lagged county-level income (column 4–6), or lagged county-level income and lagged average Equifax risk score (column 7–9). Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by industry (columns 1–3) or county and state-quarter (columns 4–9) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table D2: Impact of monetary policy on county-level nonbank share in differences

| Market:                                   | Mortgages              |                    |                    | Auto loans |         |         |
|---|------------------------|--------------------|--------------------|------------|---------|---------|
| Dependent variable:                       | $\Delta$ Nonbank share |                    |                    |            |         |         |
|   | (1)                    | (2)                | (3)                | (4)        | (5)     | (6)     |
| Nonbank share $\times$ Gertler-Karadi     | 0.089*                 |                    |                    | 0.118**    |         |         |
|   | (0.049)                |                    |                    | (0.048)    |         |         |
| Nonbank share $\times$ Bauer-Swanson      |                        | 0.142 <sup>+</sup> |                    |            | 0.227** |         |
|   |                        | (0.090)            |                    |            | (0.101) |         |
| Nonbank share $\times$ Nakamura-Steinsson |                        |                    | 0.102 <sup>+</sup> |            |         | 0.164** |
|   |                        |                    | (0.067)            |            |         | (0.071) |
| Time fixed effects                        | Yes                    | Yes                | Yes                | Yes        | Yes     | Yes     |
| County controls                           | Yes                    | Yes                | Yes                | Yes        | Yes     | Yes     |
| County income $\times$ MP shock           | Yes                    | Yes                | Yes                | Yes        | Yes     | Yes     |
| County income $\times$ Macro controls     | Yes                    | Yes                | Yes                | Yes        | Yes     | Yes     |
| Nonbank share $\times$ Macro controls     | Yes                    | Yes                | Yes                | Yes        | Yes     | Yes     |
| Observations                              | 39,102                 | 39,102             | 39,102             | 73,780     | 73,780  | 73,780  |
| $R^2$                                     | 0.575                  | 0.576              | 0.575              | 0.030      | 0.030   | 0.030   |

*Notes:* The table shows regression results for equation (D2) estimated at the county-quarter level. The sample period is 1999–2012. The dependent variable is the change from  $t - 1$  to  $t + 4$  in the nonbank share of mortgage lending (columns 1–3) or new auto lending (columns 4–6). ‘Nonbank share’ is the county-level nonbank share of mortgage lending in 1999:Q1 (columns 1–3), or the county-level nonbank share of outstanding auto credit as of 1999:Q1 (columns 4–6). This is interacted with the lagged monetary policy shocks of [Gertler and Karadi \(2015\)](#), [Bauer and Swanson \(2022\)](#), or [Nakamura and Steinsson \(2018\)](#). County controls are lagged county-level income (columns 1–3), or lagged county-level income and lagged average Equifax risk score (columns 4–6). Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by county and quarter and shown in parentheses. <sup>+</sup>, \*, \*\*, and \*\*\* indicate significance at 13%, 10%, 5%, and 1%.

## Appendix E. Two-stage firm-level and county-level regressions

Our results in Tables 5 and 9 show that exposure to nonbank credit weakens the impact of monetary policy on both credit (at the firm-level and county-level for mortgages) and economic outcomes (such as firm-level investment and county-level house prices). That is, we estimate the impact of nonbank credit on monetary transmission to economic outcomes in a “reduced form” manner.

To clarify that the impact on economic outcomes comes via credit markets, we run two-stage least squares (2SLS) regressions where the first-stage estimates the impact of nonbanks on monetary transmission to credit, and the second-stage regresses economic outcomes on the predicted values for credit from the first-stage.

More specifically, the first-stage regressions are summarised in Table 5, column 4 for firm-level regressions, and Table 9, column 2 for county-level mortgage regressions. The second-stage regressions are then summarised in Table E1, where the dependent variables are CAPEX, fixed assets, total assets (firm-level), and the house price index (county-level). Since we may suffer from a weak instruments problem, we show [Anderson and Rubin \(1949\)](#) standard errors for the second-stage regressions (see [Keane and Neal, 2024](#)).

The results in Table E1 show that higher credit is associated with stronger economic outcomes, both when we estimate the impact of credit on economic outcomes via OLS (columns 1, 3, 5, 7) and when we instrument credit using the interaction between monetary policy and predetermined exposure to nonbank credit (columns 2, 4, 6, 8).

Table E1: Two-stage firm-level and county-level regressions

| Dependent variable:<br>Model:      | Compustat variables |                    |                     |                     |                     |                    | Mortgages           |                    |
|------------------------------------|---------------------|--------------------|---------------------|---------------------|---------------------|--------------------|---------------------|--------------------|
|                                    | CAPEX               |                    | Fixed assets        |                     | Total assets        |                    | House prices        |                    |
|                                    | OLS<br>(1)          | 2SLS<br>(2)        | OLS<br>(3)          | 2SLS<br>(4)         | OLS<br>(5)          | 2SLS<br>(6)        | OLS<br>(7)          | 2SLS<br>(8)        |
| Total debt                         | 0.090***<br>(0.007) | 2.122**<br>(0.822) | 0.119***<br>(0.006) | 1.957***<br>(0.698) | 0.092***<br>(0.004) | 0.869**<br>(0.339) |                     |                    |
| Total mortgage lending             |                     |                    |                     |                     |                     |                    | 0.233***<br>(0.013) | 0.403**<br>(0.187) |
| Firm fixed effects                 | Yes                 | Yes                | Yes                 | Yes                 | Yes                 | Yes                | -                   | -                  |
| Firm controls                      | Yes                 | Yes                | Yes                 | Yes                 | Yes                 | Yes                | -                   | -                  |
| Industry controls × MP             | Yes                 | Yes                | Yes                 | Yes                 | Yes                 | Yes                | -                   | -                  |
| Industry controls × Macro controls | Yes                 | Yes                | Yes                 | Yes                 | Yes                 | Yes                | -                   | -                  |
| County fixed effects               | -                   | -                  | -                   | -                   | -                   | -                  | Yes                 | Yes                |
| County controls                    | -                   | -                  | -                   | -                   | -                   | -                  | Yes                 | Yes                |
| County income × MP                 | -                   | -                  | -                   | -                   | -                   | -                  | Yes                 | Yes                |
| County income × Macro controls     | -                   | -                  | -                   | -                   | -                   | -                  | Yes                 | Yes                |
| Time fixed effects                 | Yes                 | Yes                | Yes                 | Yes                 | Yes                 | Yes                | Yes                 | Yes                |
| Nonbank share × Macro controls     | Yes                 | Yes                | Yes                 | Yes                 | Yes                 | Yes                | Yes                 | Yes                |
| Observations                       | 61,264              | 61,485             | 63,079              | 63,296              | 66,038              | 66,264             | 42,294              | 42,294             |
| $R^2$                              | 0.924               | -                  | 0.965               | -                   | 0.969               | -                  | 0.867               | -                  |

*Notes:* Columns 1–6 show regression results based on equation (6) estimated at the firm-year level. The sample consists of listed US firms from 2000 to 2012, excluding financial services and public sector firms. The dependent variables are: log of CAPEX (columns 1 and 2), log of property, plant, and equipment (columns 3 and 4), and log of total assets (columns 5 and 6). ‘Total debt’ is the log of total balance sheet debt (from Compustat). Columns 1, 3, and 5 are estimated by OLS. Columns 2, 4, and 6 are estimated by two-stage least squares, where the first-stage regression is shown in Table 5, column 4. That is, ‘total debt’ is instrumented by the interaction between the industry-level nonbank share of total outstanding syndicated term loans (based on SNC data as of 1999) and the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Columns 7–8 show regression results based on equation (9) estimated at the county-quarter level. The sample period is 1999–2012. The dependent variable is the log of the house price index. ‘Total mortgage lending’ is the log of total county-level mortgage lending. Column 7 is estimated by OLS. Column 8 is estimated by two-stage least squares, where the first-stage regression is shown in Table 9, column 2. That is, ‘total mortgage lending’ is instrumented by the interaction between the county-level nonbank share of mortgage lending in 1999:Q1 and the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Control variables are described in Tables 5 and 9. Standard errors are clustered by industry-year (columns 1–6) or county and quarter (columns 7 and 8) and shown in parentheses. All 2SLS columns report [Anderson and Rubin \(1949\)](#) standard errors. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

## Appendix F. Further robustness tests

Table F1: Impact of monetary policy on loan-level corporate lending (alternative policy measures)

| Dependent variable:                 | Loan amount         |                     |                    |                  |
|-------------------------------------|---------------------|---------------------|--------------------|------------------|
|                                     | (1)                 | (2)                 | (3)                | (4)              |
| Nonbank $\times$ Fed Funds          | 0.027***<br>(0.008) |                     |                    |                  |
| Nonbank $\times$ Wu-Xia             |                     | 0.028***<br>(0.008) |                    |                  |
| Nonbank $\times$ ( $r - r^*$ )      |                     |                     | 0.019**<br>(0.009) |                  |
| Nonbank $\times$ Nakamura-Steinsson |                     |                     |                    | 0.034<br>(0.064) |
| Loan fixed effects                  | Yes                 | Yes                 | Yes                | Yes              |
| Lender fixed effects                | Yes                 | Yes                 | Yes                | Yes              |
| Nonbank $\times$ Macro controls     | Yes                 | Yes                 | Yes                | Yes              |
| Observations                        | 744,784             | 744,784             | 744,784            | 744,784          |
| $R^2$                               | 0.829               | 0.829               | 0.829              | 0.828            |

*Notes:* The table shows regression results for equation (5) estimated at the loan-lender level. The sample period is 1999–2012, and the sample consists of loans originated in year  $t$ . The dependent variable is the log of the lender’s holding of the loan. ‘Nonbank’ is an indicator variable for nonbank lenders. ‘Fed Funds’ is the lagged Fed Funds rate. ‘Wu-Xia’ is the lagged shadow rate of [Wu and Xia \(2016\)](#). ( $r - r^*$ ) is the lag of the estimated gap between the short-term real interest rate and the neutral rate, based on the New York Fed’s update of the [Laubach and Williams \(2003\)](#) measure of  $r^*$ . ‘Nakamura-Steinsson’ is the lagged cumulative sum of [Nakamura and Steinsson \(2018\)](#) monetary policy shocks. Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by loan and lender-year and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F2: Impact of monetary policy on lending (instrumental variables)

| Market:   | Corporate loans     |                     |                     | Mortgages           |                     |                     | Auto loans          |                     |
|---|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| Dependent variable:                                   | Loan amount         |                     |                     |                     |                     |                     |                     |                     |
| Sample:   | All loans           | Credit lines        | Term loans          | All loans           | All loans           | All loans           | All loans           | All loans           |
|   | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 | (7)                 | (8)                 |
| Nonbank $\times (r - r^*)$                            | 0.067***<br>(0.017) | 0.041***<br>(0.013) | 0.070***<br>(0.019) |                     | 0.016***<br>(0.002) |                     | 0.056***<br>(0.010) |                     |
| Nonbank $\times$ Q1(Nonbank share) $\times (r - r^*)$ |                     |                     |                     | 0.024<br>(0.023)    |                     | 0.005**<br>(0.002)  |                     | 0.056***<br>(0.011) |
| Nonbank $\times$ Q2(Nonbank share) $\times (r - r^*)$ |                     |                     |                     | 0.061***<br>(0.022) |                     | 0.007***<br>(0.003) |                     | 0.060***<br>(0.010) |
| Nonbank $\times$ Q3(Nonbank share) $\times (r - r^*)$ |                     |                     |                     | 0.076***<br>(0.016) |                     | 0.015***<br>(0.003) |                     | 0.053***<br>(0.011) |
| Nonbank $\times$ Q4(Nonbank share) $\times (r - r^*)$ |                     |                     |                     | 0.055***<br>(0.018) |                     | 0.026***<br>(0.004) |                     | 0.048***<br>(0.012) |
| Loan fixed effects                                    | Yes                 | Yes                 | Yes                 | Yes                 | No                  | No                  | No                  | No                  |
| Lender fixed effects                                  | Yes                 | Yes                 | Yes                 | Yes                 | -                   | -                   | No                  | No                  |
| County $\times$ Time fixed effects                    | -                   | -                   | -                   | -                   | Yes                 | Yes                 | Yes                 | Yes                 |
| County $\times$ Lender fixed effects                  | -                   | -                   | -                   | -                   | Yes                 | Yes                 | No                  | No                  |
| County $\times$ Lender-type fixed effects             | -                   | -                   | -                   | -                   | -                   | -                   | Yes                 | Yes                 |
| Loan-level controls                                   | -                   | -                   | -                   | -                   | Yes                 | Yes                 | Yes                 | Yes                 |
| Macro control interactions                            | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lower-order terms                                     | -                   | -                   | -                   | Yes                 | -                   | Yes                 | -                   | -                   |
| Observations  | 743,621             | 235,719             | 504,837             | 743,621             | 19,869,381          | 19,869,381          | 984,324             | 984,324             |

*Notes:* Columns 1–4 show instrumental variable regression results for equation (5) estimated at the loan-lender level. Columns 5 and 6 show instrumental variable regression results for equation (7) estimated at the loan level. Columns 7 and 8 show instrumental variable regression results for equation (8) estimated at the loan level. The sample period is 1999–2012. For columns 1–4, the sample consists of loans originated in year  $t$ . The dependent variable is the log of the lender’s holding of the loan (columns 1–4), the log of the size of the mortgage (columns 5 and 6), or the log of new auto lending (columns 7 and 8). ‘Nonbank’ is an indicator variable for nonbank lenders (columns 1–6) or loans from nonbanks (columns 7 and 8).  $(r - r^*)$  is the lag of the estimated gap between the short-term real interest rate and the neutral rate, based on the New York Fed’s update of the Laubach and Williams (2003) measure of  $r^*$ . All variables involving  $(r - r^*)$  are instrumented by corresponding variables involving the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘Nonbank share’ is the nonbank share of total outstanding syndicated term loans, based on SNC data and measured at the industry level (three-digit NAICS) as of 1999 (columns 1–4); the county-level nonbank share of mortgage lending in 1999:Q1 (columns 5 and 6); or the county-level nonbank share of outstanding auto loans in 1999:Q1 (columns 7 and 8). ‘Q1(Nonbank share)’ is an indicator variable for industries or counties in the first quartile of 1999 nonbank share. Similarly for Q2, Q3, and Q4. Loan-level controls are log applicant income, and indicator variables for female borrowers and black borrowers (columns 5 and 6); or lags of Equifax risk score, log income, log mortgage balance, log credit card balance, log other consumer loan balance, an indicator variable for bankruptcy, and birth-year fixed effects (columns 7 and 8). For all interactions involving  $(r - r^*)$ , we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by loan and lender-year (columns 1–4) or county and quarter (columns 5–8) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F3: Impact of monetary policy on lending (2001 nonbank share)

| Market:   | Corporate loans     |                     |                     | Mortgages           |                     | Auto loans          |                     |
|---|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| Dependent variable:                             | Loan amount         |                     |                     |                     |                     |                     |                     |
| Sample:   | All loans           | Credit lines        | Term loans          | All loans           | All loans           | All loans           | All loans           |
|   | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 | (7)                 |
| Nonbank $\times$ MP                             | 0.201***<br>(0.053) | 0.137***<br>(0.052) | 0.192***<br>(0.056) | 0.048***<br>(0.010) | 0.053***<br>(0.010) | 0.209***<br>(0.042) | 0.214***<br>(0.041) |
| Nonbank $\times$ 2001 nonbank share $\times$ MP | 0.630**<br>(0.313)  | -0.050<br>(0.444)   | 0.714**<br>(0.356)  | 0.615***<br>(0.087) | 0.461***<br>(0.074) | 0.004<br>(0.070)    | 0.022<br>(0.067)    |
| Loan fixed effects                              | Yes                 | Yes                 | Yes                 | No                  | No                  | No                  | No                  |
| Lender fixed effects                            | Yes                 | Yes                 | Yes                 | -                   | -                   | No                  | No                  |
| Time fixed effects                              | -                   | -                   | -                   | Yes                 | -                   | Yes                 | -                   |
| County $\times$ Time fixed effects              | -                   | -                   | -                   | No                  | Yes                 | No                  | Yes                 |
| County $\times$ Lender fixed effects            | -                   | -                   | -                   | Yes                 | Yes                 | No                  | No                  |
| County $\times$ Lender-type fixed effects       | -                   | -                   | -                   | -                   | -                   | Yes                 | Yes                 |
| Loan-level controls                             | -                   | -                   | -                   | Yes                 | Yes                 | Yes                 | Yes                 |
| Macro control interactions                      | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lower-order terms                               | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | -                   |
| Observations                                    | 684,444             | 199,917             | 481,541             | 17,902,219          | 17,902,219          | 855,352             | 828,588             |
| $R^2$   | 0.825               | 0.903               | 0.700               | 0.452               | 0.458               | 0.089               | 0.176               |

*Notes:* Columns 1–3 show regression results for equation (5) estimated at the loan-lender level. Columns 4 and 5 show regression results for equation (7) estimated at the loan level. Columns 6 and 7 show regression results for equation (8) estimated at the loan level. For columns 1–3, the sample period is 2002–2012, and the sample consists of loans originated in year  $t$ . For columns 4–7, the sample period is 2001–2012. The dependent variable is the log of the lender’s holding of the loan (columns 1–3), the log of the size of the mortgage (columns 4 and 5), or the log of new auto lending (columns 6 and 7). ‘Nonbank’ is an indicator variable for nonbank lenders (columns 1–5) or loans from nonbanks (columns 6 and 7). ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘2001 nonbank share’ is the nonbank share of total outstanding syndicated term loans, based on SNC data and measured at the industry level (three-digit NAICS) as of 2001 (columns 1–3); the county-level nonbank share of mortgage lending in 2001:Q1 (columns 4 and 5); or the county-level nonbank share of new auto loans in 2001:Q1 (columns 6 and 7). Loan-level controls are log applicant income, and indicator variables for female borrowers and black borrowers (columns 4 and 5); or lags of Equifax risk score, log income, log mortgage balance, log credit card balance, log other consumer loan balance, an indicator variable for bankruptcy, and birth-year fixed effects (columns 6 and 7). For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by loan and lender-year (columns 1–3) or county and quarter (columns 4–7) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F4: Past lender-level relationships

| Market:                                | Corporate loans     |                  |                    | Mortgages           |                     |
|--|---------------------|------------------|--------------------|---------------------|---------------------|
|  | Loan amount         |                  |                    |                     |                     |
| Dependent variable:                    |                     |                  |                    |                     |                     |
| Sample:                                | All loans           | Credit lines     | Term loans         | All loans           | All loans           |
|  | (1)                 | (2)              | (3)                | (4)                 | (5)                 |
| Nonbank $\times$ MP                    | 0.118***<br>(0.038) | 0.043<br>(0.040) | 0.095**<br>(0.043) | 0.053***<br>(0.015) | 0.047***<br>(0.013) |
| Nonbank $\times$ MP $\times$ Past loan | 0.046<br>(0.032)    | 0.059<br>(0.040) | 0.107**<br>(0.043) | 0.023*<br>(0.014)   | 0.028**<br>(0.013)  |
| Loan fixed effects                     | Yes                 | Yes              | Yes                | No                  | No                  |
| County fixed effects                   | -                   | -                | -                  | Yes                 | -                   |
| Time fixed effects                     | -                   | -                | -                  | Yes                 | -                   |
| County $\times$ Time fixed effects     | -                   | -                | -                  | No                  | Yes                 |
| Lender fixed effects                   | Yes                 | Yes              | Yes                | Yes                 | Yes                 |
| Loan-level controls                    | -                   | -                | -                  | Yes                 | Yes                 |
| Macro control interactions             | Yes                 | Yes              | Yes                | Yes                 | Yes                 |
| Lower-order terms                      | Yes                 | Yes              | Yes                | Yes                 | Yes                 |
| Observations                           | 744,784             | 236,505          | 505,212            | 20,194,895          | 20,194,895          |
| $R^2$                                  | 0.829               | 0.899            | 0.703              | 0.438               | 0.448               |

*Notes:* Columns 1–3 show regression results for equation (5) estimated at the loan-lender level. Columns 4 and 5 show regression results for equation (7) estimated at the loan level. The sample period is 1999–2012. The dependent variable is the log of the lender’s holding of the loan (columns 1–3), or the log of the size of the mortgage (columns 4 and 5). ‘Nonbank’ is an indicator variable for nonbank lenders. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘Past loan’ is an indicator variable equal to one if the lender has lent to the borrower in a previous syndicated loan (columns 1–3), or an indicator variable equal to one if the lender has originated a mortgage in the county within the last two years (columns 4 and 5). Loan-level controls (columns 4 and 5) are log applicant income, and indicator variables for female borrowers and black borrowers. For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by loan and lender-year (columns 1–3) or by county and quarter (columns 4 and 5) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F5: Impact of monetary policy on corporate lending shares

| Dependent variable:<br>Sample:  | Lender-level share of loan |                     |                     |
|---------------------------------|----------------------------|---------------------|---------------------|
|                                 | All loans<br>(1)           | Credit lines<br>(2) | Term loans<br>(3)   |
| Nonbank $\times$ MP             | 0.010***<br>(0.002)        | 0.010***<br>(0.004) | 0.008***<br>(0.002) |
| Loan fixed effects              | Yes                        | Yes                 | Yes                 |
| Lender fixed effects            | Yes                        | Yes                 | Yes                 |
| Nonbank $\times$ Macro controls | Yes                        | Yes                 | Yes                 |
| Observations                    | 749,673                    | 238,790             | 507,807             |
| $R^2$                           | 0.776                      | 0.735               | 0.739               |

*Notes:* The table shows regression results for equation (5) estimated at the loan-lender level. The sample period is 1999–2012, and the sample consists of loans originated in year  $t$ . The dependent variable is the proportion of loan  $i$  held by lender  $l$ . ‘Nonbank’ is an indicator variable for nonbank lenders. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by loan and lender-year and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F6: Impact of monetary policy on loan-level mortgage lending (alternative policy measures)

| Dependent variable:                  | Loan amount         |                     |                     |                     |                     |                     |                     |                     |
|--------------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
|                                      | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 | (7)                 | (8)                 |
| Nonbank $\times$ Fed Funds           | 0.012***<br>(0.002) | 0.010***<br>(0.002) |                     |                     |                     |                     |                     |                     |
| Nonbank $\times$ Wu-Xia              |                     |                     | 0.009***<br>(0.002) | 0.008***<br>(0.002) |                     |                     |                     |                     |
| Nonbank $\times$ ( $r - r^*$ )       |                     |                     |                     |                     | 0.012***<br>(0.002) | 0.010***<br>(0.002) |                     |                     |
| Nonbank $\times$ Nakamura-Steinsson  |                     |                     |                     |                     |                     |                     | 0.074***<br>(0.018) | 0.057***<br>(0.017) |
| Time fixed effects                   | Yes                 | -                   | Yes                 | -                   | Yes                 | -                   | Yes                 | -                   |
| County $\times$ Time fixed effects   | No                  | Yes                 | No                  | Yes                 | No                  | Yes                 | No                  | Yes                 |
| County $\times$ Lender fixed effects | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Loan-level controls                  | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Nonbank $\times$ Macro controls      | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Observations                         | 20,043,023          | 20,043,023          | 20,043,023          | 20,043,023          | 20,043,023          | 20,043,023          | 20,043,023          | 20,043,023          |
| $R^2$                                | 0.464               | 0.471               | 0.464               | 0.471               | 0.464               | 0.471               | 0.464               | 0.471               |

*Notes:* The table shows regression results for equation (7) estimated at the loan level. The sample period is 1999–2012. The dependent variable is the log of the size of the mortgage. ‘Nonbank’ is an indicator variable for nonbank lenders. ‘Fed Funds’ is the lagged Fed Funds rate. ‘Wu-Xia’ is the lagged shadow rate of [Wu and Xia \(2016\)](#). ( $r - r^*$ ) is the lag of the estimated gap between the short-term real interest rate and the neutral rate, based on the New York Fed’s update of the [Laubach and Williams \(2003\)](#) measure of  $r^*$ . ‘Nakamura-Steinsson’ is the lagged cumulative sum of [Nakamura and Steinsson \(2018\)](#) monetary policy shocks. Loan-level controls are log applicant income, and indicator variables for female borrowers and black borrowers. Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by county and quarter and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F7: Impact of monetary policy on loan-level auto lending (alternative policy measures)

| Dependent variable:                       | Loan amount         |                     |                     |                     |                     |                     |                    |                    |
|---|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|--------------------|--------------------|
|   | (1)                 | (2)                 | (3)                 | (4)                 | (5)                 | (6)                 | (7)                | (8)                |
| Nonbank $\times$ Fed Funds                | 0.030***<br>(0.006) | 0.031***<br>(0.006) |                     |                     |                     |                     |                    |                    |
| Nonbank $\times$ Wu-Xia                   |                     |                     | 0.023***<br>(0.005) | 0.023***<br>(0.005) |                     |                     |                    |                    |
| Nonbank $\times$ ( $r - r^*$ )            |                     |                     |                     |                     | 0.029***<br>(0.008) | 0.030***<br>(0.008) |                    |                    |
| Nonbank $\times$ Nakamura-Steinsson       |                     |                     |                     |                     |                     |                     | 0.211**<br>(0.082) | 0.212**<br>(0.085) |
| Time fixed effects                        | Yes                 | -                   | Yes                 | -                   | Yes                 | -                   | Yes                | -                  |
| County $\times$ Time fixed effects        | No                  | Yes                 | No                  | Yes                 | No                  | Yes                 | No                 | Yes                |
| County $\times$ Lender-type fixed effects | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                | Yes                |
| Loan-level controls                       | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                | Yes                |
| Nonbank $\times$ Macro controls           | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                 | Yes                | Yes                |
| Observations                              | 1,017,766           | 985,434             | 1,017,766           | 985,434             | 1,017,766           | 985,434             | 1,017,766          | 985,434            |
| $R^2$                                     | 0.089               | 0.176               | 0.089               | 0.176               | 0.089               | 0.176               | 0.088              | 0.176              |

*Notes:* The table shows regression results for equation (8) estimated at the loan level. The sample period is 1999–2012. The dependent variable is the log of new auto lending. ‘Nonbank’ is an indicator variable for loans from nonbanks. ‘Fed Funds’ is the lagged Fed Funds rate. ‘Wu-Xia’ is the lagged shadow rate of [Wu and Xia \(2016\)](#). ( $r - r^*$ ) is the lag of the estimated gap between the short-term real interest rate and the neutral rate, based on the New York Fed’s update of the [Laubach and Williams \(2003\)](#) measure of  $r^*$ . ‘Nakamura-Steinsson’ is the lagged cumulative sum of [Nakamura and Steinsson \(2018\)](#) monetary policy shocks. Loan-level controls are lags of Equifax risk score, log income, log mortgage balance, log credit card balance, log other consumer loan balance, an indicator variable for bankruptcy, and birth-year fixed effects. Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by county and quarter and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F8: Extensive margin of nonbank mortgage and auto credit

| Market:                               | Mortgages           |                  | Auto loans          |                  |
|---------------------------------------|---------------------|------------------|---------------------|------------------|
|                                       | Nonbank             | Total            | Nonbank             | Total            |
| Dependent variable:                   | (1)                 | (2)              | (3)                 | (4)              |
| Nonbank share $\times$ MP             | 0.416***<br>(0.113) | 0.193<br>(0.146) | 0.066***<br>(0.017) | 0.000<br>(0.018) |
| County fixed effects                  | Yes                 | Yes              | Yes                 | Yes              |
| Time fixed effects                    | Yes                 | Yes              | Yes                 | Yes              |
| County controls                       | Yes                 | Yes              | Yes                 | Yes              |
| Nonbank share $\times$ Macro controls | Yes                 | Yes              | Yes                 | Yes              |
| Observations                          | 42,294              | 42,294           | 158,459             | 158,459          |
| $R^2$                                 | 0.964               | 0.970            | 0.788               | 0.826            |

*Notes:* The table shows regression results for equation (9) estimated at the county-quarter level. The sample period is 1999–2012. Columns 1 and 2 show results for the mortgage market, and columns 3 and 4 show results for the auto loan market. The dependent variables are the log of the number of new loans from nonbanks (columns 1 and 3), and the log of the number of total new loans (columns 2 and 4). ‘Nonbank share’ is the county-level nonbank share of mortgage lending in 1999:Q1 (columns 1 and 2), or the county-level nonbank share of outstanding auto credit as of 1999:Q1 (columns 3 and 4). ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. County controls are lagged county-level income (columns 1 and 2), or lagged county-level income and lagged average Equifax risk score (columns 3 and 4). Macro controls are lags of real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX. Standard errors are clustered by industry-year and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F9: Impact of monetary policy on firm-level and county-level nonbank and bank lending

| Market:                       | Corporate loans |         | Mortgages |         | Auto loans |           |
|-------------------------------|-----------------|---------|-----------|---------|------------|-----------|
| Dependent variable:           | Nonbank         | Bank    | Nonbank   | Bank    | Nonbank    | Bank      |
|                               | (1)             | (2)     | (3)       | (4)     | (5)        | (6)       |
| Nonbank share $\times$ MP     | 4.776**         | 0.400   | 0.568***  | -0.008  | 0.482***   | -0.501*** |
|                               | (2.194)         | (0.532) | (0.124)   | (0.133) | (0.128)    | (0.106)   |
| Industry fixed effects        | Yes             | Yes     | -         | -       | -          | -         |
| Industry controls $\times$ MP | Yes             | Yes     | -         | -       | -          | -         |
| County fixed effects          | -               | -       | Yes       | Yes     | Yes        | Yes       |
| County controls               | -               | -       | Yes       | Yes     | Yes        | Yes       |
| County income $\times$ MP     | -               | -       | Yes       | Yes     | Yes        | Yes       |
| Time fixed effects            | Yes             | Yes     | Yes       | Yes     | Yes        | Yes       |
| Macro control interactions    | Yes             | Yes     | Yes       | Yes     | Yes        | Yes       |
| Observations                  | 7,393           | 7,393   | 42,294    | 42,294  | 158,459    | 158,459   |
| $R^2$                         | 0.136           | 0.084   | 0.963     | 0.969   | 0.489      | 0.490     |

*Notes:* Columns 1 and 2 show regression results for equation (6) estimated at the firm-year level for corporate loans. Columns 3–6 show regression results for equation (9) estimated at the county-quarter level for mortgages (columns 3 and 4) and auto loans (columns 5 and 6). The sample period is 2000–2012 (columns 1 and 2) or 1999–2012 (columns 3–6). The dependent variables are: the log of total new syndicated term lending from nonbanks (column 1) and banks (column 2); the log of total mortgage lending from nonbanks (column 3) and banks (column 4); and the log of total new auto lending from nonbanks (column 5) and banks (column 6). ‘Nonbank share’ is the nonbank share of total outstanding syndicated term loans, based on SNC data and measured at the industry level (three-digit NAICS) as of 1999 (columns 1 and 2); the county-level nonbank share of mortgage lending in 1999:Q1 (columns 3 and 4); or the county-level nonbank share of outstanding auto credit as of 1999:Q1 (columns 5 and 6). ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. Industry controls are lags of log(total assets) and return-on-assets measured at the industry level. County controls are lagged county-level income (columns 3 and 4), or lagged county-level income and lagged average Equifax risk score (columns 5 and 6). For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Standard errors are clustered by industry-year (columns 1 and 2) or county and quarter (columns 3–6) and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F10: Impact of monetary policy on county-level auto lending (by quartile)

| Dependent variable:           | Total lending |         | Auto sales |
|-------------------------------|---------------|---------|------------|
|                               | (1)           | (2)     | (3)        |
| Q1(Nonbank share) $\times$ MP | -0.110*       |         | -0.035**   |
|                               | (0.056)       |         | (0.016)    |
| Q2(Nonbank share) $\times$ MP |               | 0.113*  |            |
|                               |               | (0.054) |            |
| Q3(Nonbank share) $\times$ MP |               | 0.132*  |            |
|                               |               | (0.063) |            |
| Q4(Nonbank share) $\times$ MP |               | 0.090   |            |
|                               |               | (0.090) |            |
| County fixed effects          | Yes           | Yes     | Yes        |
| Time fixed effects            | Yes           | Yes     | Yes        |
| County controls               | Yes           | Yes     | Yes        |
| County income $\times$ MP     | Yes           | Yes     | Yes        |
| Macro control interactions    | Yes           | Yes     | Yes        |
| Observations                  | 158,459       | 158,459 | 122,987    |
| $R^2$                         | 0.502         | 0.502   | 0.989      |

*Notes:* The table shows regression results for equation (9) estimated at the county-quarter level. The sample period is 1999–2012 (2002–2012 for column 3). The dependent variables are log of total new auto lending (columns 1 and 2), and log of the number of auto sales (column 3). ‘Nonbank share’ is the county-level nonbank share of outstanding auto credit as of 1999:Q1. ‘Q1(Nonbank share)’ is an indicator variable for counties in the first quartile of 1999 nonbank share. Similarly for Q2, Q3, and Q4. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. County controls are lagged county-level income and lagged average Equifax risk score. For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Standard errors are clustered by county and quarter and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.

Table F11: Impact of monetary policy on loan-level mortgage lending (variation over time)

| Dependent variable:                        | Loan amount         |                     |                     |                     |
|--|---------------------|---------------------|---------------------|---------------------|
|  | (1)                 | (2)                 | (3)                 | (4)                 |
| Nonbank $\times$ MP                        | 0.050***<br>(0.011) | 0.035***<br>(0.010) | 0.047***<br>(0.011) | 0.032***<br>(0.010) |
| Nonbank $\times$ MP $\times$ Trend         | 0.001***<br>(0.000) | 0.001***<br>(0.000) | 0.001***<br>(0.000) | 0.001***<br>(0.000) |
| Nonbank $\times$ Nonbank share $\times$ MP |                     |                     | 0.455***<br>(0.058) | 0.339***<br>(0.053) |
| Time fixed effects                         | Yes                 | -                   | Yes                 | -                   |
| County $\times$ Time fixed effects         | No                  | Yes                 | No                  | Yes                 |
| County $\times$ Lender fixed effects       | Yes                 | Yes                 | Yes                 | Yes                 |
| Loan-level controls                        | Yes                 | Yes                 | Yes                 | Yes                 |
| Macro control interactions                 | Yes                 | Yes                 | Yes                 | Yes                 |
| Lower-order terms                          | Yes                 | Yes                 | Yes                 | Yes                 |
| Observations                               | 19,869,381          | 19,869,381          | 19,869,381          | 19,869,381          |
| $R^2$                                      | 0.465               | 0.472               | 0.465               | 0.472               |

*Notes:* The table shows regression results for equation (7) estimated at the loan level. The sample period is 1999–2012. The dependent variable is the log of the size of the mortgage. ‘Nonbank’ is an indicator variable for nonbank lenders. ‘MP’ is the lagged cumulative sum of Gertler-Karadi monetary policy shocks. ‘Nonbank share’ is the county-level nonbank share of mortgage lending in 1999:Q1. ‘Trend’ is a linear quarterly time trend. Loan-level controls are log applicant income, and indicator variables for female borrowers and black borrowers. For all interactions involving MP, we also include corresponding interactions involving a vector of lagged macro controls (real GDP growth, forecasted real GDP growth, PCE inflation, and the VIX). Lower-level interactions and level effects are estimated unless spanned by the fixed effects. Standard errors are clustered by county and quarter and shown in parentheses. \*, \*\*, and \*\*\* indicate significance at 10%, 5%, and 1%.