

Nothing Required: U.S. Bank Lending after the End of Reserve Requirements

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August 18, 2026

In March 2020, the Federal Reserve eliminated reserve requirements to support lending. Did banks respond by lending more? To answer this question, I use pre-policy differences in reserve burdens to compare banks more and less exposed to the policy change. Banks facing larger pre-policy requirements increased loan balances more after the policy change. Across four specifications, the estimates range from \$0.86 to \$1.02 of additional non-PPP loans over four quarters for each dollar of pre-policy reserve burden. The difference is concentrated in the initial adjustment; I find no evidence of persistently faster loan growth. The estimate survives balance-sheet controls and adjustment for local pandemic conditions.

Keywords: reserve requirements; bank lending; bank balance sheets; monetary policy implementation

JEL Codes: E51; E52; G21

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1. Introduction

On March 15, 2020, the Federal Reserve announced that it would set reserve requirements to zero, saying the change would “help to support lending to households and businesses” ([Board of Governors of the Federal Reserve System 2020b](#)). Did banks respond by lending more? If the policy produced the intended one-time level adjustment, it should appear as a temporary increase in four-quarter loan-balance changes following the policy change rather than persistently faster growth.

The policy applied to every bank, but the burden it removed varied continuously across banks. I use that variation in a continuous-treatment difference-in-differences design. With quarterly Call Reports, I apply the reserve schedule in force immediately before the policy change to each bank’s 2019q4 transaction accounts and scale the resulting gross “reserve burden” by assets. I subtract PPP balances from reported loans to remove the program’s direct contribution. The policy changed in 2020q1, so I treat that quarter as a transition. I then compare four-quarter changes in non-PPP loan balances at banks with larger and smaller pre-policy burdens.

I find that banks facing larger pre-policy reserve requirements did increase loan balances more after the policy change. Across four specifications, the estimates range from \$0.86 to \$1.02 of additional non-PPP loans over four quarters for each dollar of pre-policy reserve burden. Gross burden differs by 1.73 percentage points of assets between the low- and high-exposure terciles, which maps into a roughly 1.5-percentage-point larger initial increase in non-PPP loans relative to 2019q4 assets. The cross-bank difference is concentrated in the initial adjustment; I find no evidence that more-exposed banks sustained faster loan growth.

The event-study coefficient first turns positive in 2020q1 and remains positive through 2020q4. The pre-policy exposure coefficients are jointly indistinguishable from zero ($p = 0.734$), and none is individually significant at 10%. All specifications include bank fixed effects; the estimate remains positive with state-by-quarter fixed effects and quarter-

varying coefficients on 2019q4 balance-sheet controls. The estimate changes little after adding branch-weighted county COVID mortality and unemployment.

This paper provides, to my knowledge, the first positive exposure-based evidence on the 2020 policy. [Koch and Islam \(2026\)](#), the closest published study, conclude that the elimination did not support lending using a common post-policy indicator and a decline in loans relative to deposits. That design estimates a common pandemic-era shift; its ratio outcome also reflects the contemporaneous deposit surge ([Dursun-de Neef and Schandlbauer 2022](#); [Levine et al. 2021](#)). By contrast, I find larger initial non-PPP loan-balance increases at banks with larger predetermined burdens, a relative response compatible with an aggregate decline in loans relative to deposits.

A secondary contribution of this paper is to extend exposure-based evidence on reserve requirements to complete elimination in an abundant-reserves regime. Reserve requirements can affect lending by tying deposits to liquid assets ([Alper et al. 2018](#)). [Curfman and Kandrac \(2022\)](#) exploit a kink in the pre-crisis reserve schedule and find that higher requirements reduced lending; their separate analysis of the 1990 elimination for nontransaction accounts finds that banks reduced liquidity buffers and expanded credit. Other historical changes operated through different margins. [Park and Van Horn \(2015\)](#) find that banks met the 1936–37 increases by changing cash and securities rather than loans. Revisiting that episode, [Calomiris, Mason, and Wheelock \(2023\)](#) find no average lending contraction because most banks were not bound, although loan growth was slower among member banks with lower excess reserves. [Martin, McAndrews, and Skeie \(2016\)](#) show that abundant reserve balances do not mechanically produce more lending.¹

2. Exposure, Timing, and Empirical Design

I use quarterly Call Reports from 2017q1 through 2022q4 ([Federal Financial Institutions Examination Council 2026](#)).² The analysis covers outcomes from 2018q1 through 2022q4

¹Evidence on the independent lending effect of interest on excess reserves is mixed. [Rezende, Temesvary, and Zarutskie \(2019\)](#) find no discernible lending response to IOER changes from 2015 through 2018, whereas [Hogan \(2021\)](#) attributes a majority of the post-2008 decline in loans as a share of bank assets to IOER.

²[Table A1](#) reports summary statistics for the analysis sample, and [Table A2](#) lists the public data sources used in the main estimates and pandemic-demand checks.

because constructing four-quarter changes requires four lagged quarters. For each bank-quarter, I subtract outstanding Paycheck Protection Program (PPP) balances from current and lagged loans, calculate the four-quarter change, and divide by the bank’s fixed 2019q4 assets. The outcome is the net change in the non-PPP loan stock over four quarters rather than loan originations or lending within a single quarter. Subtracting PPP balances removes the program’s direct contribution to reported loans; any indirect effect of PPP on conventional lending remains in the outcome (Granja et al. 2022; Karakaplan 2021). The analysis uses a near-balanced panel to hold bank composition approximately fixed across the event-study horizon. The main sample contains 723 banks and 14,441 bank-quarters, representing 84.8% of 2019q4 assets and 83.1% of deposits.³

To measure the statutory burden eliminated by the policy while keeping bank balance sheets fixed before the change, I apply the reserve-requirement schedule in force immediately before the March 2020 elimination to each bank’s 2019q4 transaction accounts, the last Call Report observed before the policy change. Under that schedule, the first \$16.9 million was exempt, balances between \$16.9 million and \$127.5 million faced a 3% requirement, and balances above \$127.5 million faced a 10% requirement (Board of Governors of the Federal Reserve System 2020a). For bank i , let B_i denote this gross statutory burden and $A_{i,2019q4}$ denote total assets in 2019q4; exposure is $E_i \equiv B_i/A_{i,2019q4}$. Gross burden measures the statutory obligation, not reserve balances held or released. Mean exposure rises from 0.14% of assets in the low-exposure tercile to 1.87% in the high-exposure tercile. Because the outcome and exposure use the same fixed denominator, an exposure coefficient is measured in loan dollars per dollar of gross burden.

Transaction-account intensity is not a nuisance component of exposure to be removed. Net transaction accounts were the reservable base under the pre-policy regime, so banks funded more heavily with those accounts faced larger statutory obligations and experienced a larger regulatory change when the applicable ratios were set to zero. The design asks whether the relationship between this predetermined burden and loan-balance

³The sample requires positive assets, deposits, and net transaction accounts; nonmissing required-reserve and reserve-balance fields; observations in 2019q4, 2020q1, and 2022q4; and observations in at least 18 of the 20 estimating quarters.

changes shifted when the obligation disappeared. The reduced-form estimand captures any burden-related change in banks' ability or willingness to maintain and extend credit. It neither requires banks to have been at a knife-edge reserve constraint nor interprets gross schedule-implied burden as cash mechanically converted into loans.⁴

The corresponding identification concern is not that exposure is correlated with transaction-account intensity—it is by construction—but that this bank characteristic may have predicted differential pandemic-era lending for reasons unrelated to the policy. To control for this concern, I include bank fixed effects, state-by-quarter fixed effects, and quarter-varying coefficients on 2019q4 balance-sheet controls; I also control for uninsured deposits, unused commitments, and small-business lending. Unused commitments enter because they were the strongest cross-bank predictor of the March 2020 C&I drawdowns (Li, Strahan, and Zhang 2020). The identifying assumption is that, conditional on these fixed effects and controls, banks with larger and smaller burdens would otherwise have had parallel four-quarter changes in non-PPP loan balances.

Because the outcome is a four-quarter change constructed from quarter-end loan stocks rather than a flow accumulated uniformly within the quarter, the March 15 announcement could affect the 2020q1 observation despite occurring late in the quarter. The ratios were announced on March 15, became applicable on March 26, and loan balances were measured on March 31 (Board of Governors of the Federal Reserve System 2020b). Banks had 16 days after the official announcement to adjust quarter-end balance sheets. The adjustment need not consist solely of newly originated loans; outstanding balances can respond rapidly through banks' treatment of existing facilities, renewals, repayments, and other credit decisions. Quarterly data cannot determine whether the 2020q1 difference arose before or after March 15, so I estimate that quarter separately rather than classify it as cleanly treated.

⁴Retail sweeps enter the measure insofar as reclassified transaction balances appear in the 2019q4 Call Reports (Anderson and Rasche 2001; Bennett and Peristiani 2002; Styczynski 2026). Table B1 separates gross burden from vault-cash-adjusted burden and reported reserve balances. Table B2 shows that the per-dollar initial coefficient remains positive and similar when exposure is measured net of vault cash or winsorized.

I estimate the timing pattern with a continuous-treatment difference-in-differences specification. Let t index quarters, and let y_{it} denote the four-quarter change in non-PPP loan balances scaled by fixed 2019q4 assets. The indicator D_t^T marks the 2020q1 transition quarter. The labels I , C , and M denote, respectively, the 2020q2–2020q4 initial adjustment, its four-quarter carryover in 2021q1–2021q3, and the mature period in 2021q4–2022q4. For $w \in \{I, C, M\}$, D_t^w indicates that quarter t falls in window w . The most conservative specification is

$$y_{it} = \alpha_i + \delta_{s(i)t} + \gamma E_i D_t^T + \sum_{w \in \{I, C, M\}} \beta_w E_i D_t^w + X_i' \Gamma_t + \varepsilon_{it}. \quad (1)$$

The coefficient γ measures the transition-quarter response; β_w measures the response in window w , with β_I the initial response. The bank fixed effect α_i absorbs permanent bank differences. The function $s(i)$ assigns bank i to its headquarters state, so $\delta_{s(i)t}$ absorbs shocks shared within a state and quarter. The term $X_i' \Gamma_t$ pairs the control vector X_i —standardized 2019q4 bank size, uninsured deposits, unused commitments, and small-business lending, plus missing-value indicators—with the quarter-specific coefficient vector Γ_t . Missing standardized values are set to zero and paired with their indicators. The error ε_{it} contains the remaining bank-quarter determinants of the outcome. Regressions are unweighted, retain zero-burden banks, and cluster standard errors by bank; this specification contains 718 banks and 14,341 observations.

The identifying variation comes from cross-bank differences in burden fixed before the policy. Because the policy applied to every bank, the estimand is relative rather than aggregate: each β_w measures the linear conditional difference in four-quarter non-PPP loan changes associated with one dollar of pre-policy burden ([Callaway, Goodman-Bacon, and Sant’Anna 2024](#)). The dollar unit reports the scale of that conditional cross-bank difference.

TABLE 1. Four-quarter non-PPP loan-balance changes across adjustment windows

	(1)	(2)	(3)	(4)
Transition quarter, 2020q1	0.88*** (0.33)	0.76** (0.34)	0.84** (0.36)	0.70* (0.36)
Initial adjustment, 2020q2–2020q4	0.97** (0.39)	0.93** (0.38)	1.02** (0.45)	0.86** (0.43)
Carryover, 2021q1–2021q3	0.66** (0.33)	0.52 (0.33)	0.55 (0.39)	0.46 (0.37)
Mature period, 2021q4–2022q4	–0.11 (0.66)	–0.27 (0.67)	–0.10 (0.58)	–0.25 (0.60)
Bank fixed effects	✓	✓	✓	✓
Quarter fixed effects	✓	✓		
State × quarter fixed effects			✓	✓
Balance-sheet controls × quarter		✓		✓
Observations	14,441	14,441	14,341	14,341
Banks	723	723	718	718

Note: The outcome is the four-quarter change in non-PPP loan balances divided by fixed 2019q4 assets. Exposure applies the operative 2020 reserve-requirement schedule to 2019q4 transaction accounts and divides the resulting gross burden by the same assets. All specifications include bank fixed effects and estimate the 2020q1 exposure coefficient separately; other fixed effects and controls are shown in the table. Columns (3) and (4) exclude five banks (100 bank-quarter observations) because each is the only sample bank in its headquarters-state-by-quarter cells. Column (4) is the most conservative specification: it combines state-by-quarter fixed effects with quarter-varying coefficients on 2019q4 balance-sheet controls. Standard errors in parentheses are clustered by bank. * $p < .10$, ** $p < .05$, and *** $p < .01$.

3. The Cross-Bank Lending Response Over Time

Banks with greater exposure recorded larger increases in non-PPP loan balances during the initial adjustment. Table 1 reports four initial-adjustment estimates ranging from \$0.86 to \$1.02 of additional non-PPP loan balances over four quarters per dollar of gross schedule-implied burden. The most conservative specification yields an estimate of \$0.86.⁵ This specification absorbs state-by-quarter shocks and allows the effects of 2019q4 balance-sheet controls to vary by quarter.

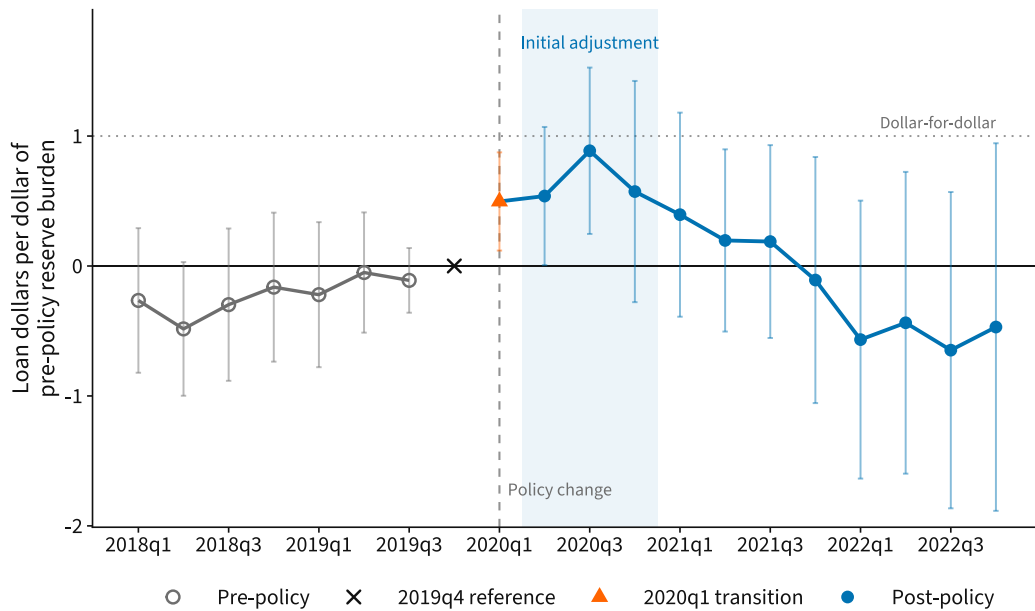
Gross schedule-implied burden differs by 1.73 percentage points of assets between the low- and high-exposure terciles. Using the estimate from column (4), that gap maps into a

⁵Table B3 reports targeted specification and sample checks. On the common 661-bank committed-credit sample, the estimate is \$0.79 (SE 0.45) for non-PPP loans and \$0.79 (SE 0.46) for non-PPP loans plus unused commitments.

roughly 1.5-percentage-point larger increase in non-PPP loan balances relative to 2019q4 assets during the initial adjustment.

The mature-period estimates are centered near zero across all four specifications. For 2021q4–2022q4, they range from $-\$0.10$ to $-\$0.27$, and none is statistically distinguishable from zero. These estimates provide no evidence that more-exposed banks sustained faster loan growth. Because the outcome is a four-quarter change, they do not by themselves establish whether the initial difference in loan levels persisted.

FIGURE 1. The four-quarter lending difference turns positive in the announcement quarter and later returns toward zero



Note: Markers show the exposure coefficients for the four-quarter change in non-PPP loan balances observed in each quarter, measured in loan dollars per dollar of gross operative-schedule burden. Lines connect the quarterly estimates; bars show bank-clustered 90% confidence intervals. The specification includes bank and state-by-quarter fixed effects and quarter interactions with 2019q4 balance-sheet controls (14,341 observations; 718 banks). The x marks the omitted 2019q4 reference quarter, and the triangle marks the 2020q1 transition quarter. The dashed vertical line marks the policy change, the shaded region marks the 2020q2–2020q4 initial adjustment, and the dotted horizontal line marks a dollar-for-dollar response. The joint pre-policy test has $p = 0.734$; no pre-policy coefficient is individually significant at 10%.

Figure 1 traces the column (4) lending difference quarter by quarter. The positive cross-bank difference first appears in 2020q1, the quarter containing the announcement, and remains positive throughout the initial-adjustment and carryover windows. The pre-policy coefficients are jointly indistinguishable from zero ($p = 0.734$), and none is individually

significant at 10%. The point estimate peaks in 2020q3 and then declines. From 2021q1 onward, each quarterly estimate is statistically indistinguishable from zero. A positive four-quarter difference during the initial adjustment and carryover followed by a return toward zero matches the pattern implied by a one-time level adjustment and provides no evidence of persistently faster loan growth.

Table 2 shows that the larger lending increase is unchanged after accounting for two sources of pandemic demand variation. Panel A holds the 700-bank branch-location sample fixed and adds rolling four-quarter county COVID mortality and unemployment, weighted by each bank's 2019 branch deposits. The estimate is \$0.80 without these controls and \$0.81 with them. Panel B combines each bank's 2019 branch footprint with county industry employment shares and national industry employment changes to predict local demand. On that fixed sample, the estimate is \$0.90 without the predicted-demand control and \$0.89 with it.

TABLE 2. Pandemic-demand controls leave the lending estimate unchanged

	Estimate	Obs.	Banks
<i>Panel A. Direct branch-market adjustment</i>			
Common-support baseline	0.80* (0.45)	11,710	700
Add branch-weighted COVID mortality and unemployment	0.81* (0.45)	11,710	700
<i>Panel B. Predicted industry-demand adjustment</i>			
Branch-industry common-sample baseline	0.90** (0.44)	14,222	712
Add predicted industry-demand exposure	0.89** (0.44)	14,222	712

Note: The outcome and exposure match column (4) of Table 1. Each panel holds its sample fixed while adding the named pandemic-demand control. In Panel A, rolling four-quarter county COVID mortality and unemployment are weighted by each bank's 2019 branch deposits. In Panel B, the predicted-demand control combines each bank's June 2019 branch-deposit weights with 2019 county two-digit industry employment shares and national private-industry employment changes from 2019q2 to 2020q2, then allows that exposure to have a separate coefficient each quarter. All specifications include bank and state-by-quarter fixed effects and quarter interactions with 2019q4 balance-sheet controls. Standard errors in parentheses are clustered by bank. * $p < .10$, ** $p < .05$, and *** $p < .01$.

Taken together, these analyses examine observable and preexisting versions of the transaction-account concern: fixed effects, predetermined balance-sheet controls, pre-policy event-study coefficients, and controls for local pandemic conditions and predicted

industry demand. The positive post-policy difference survives the observable alternatives examined here.

4. Interpreting the Initial Cross-Bank Difference

Several features of March 2020 could have raised loan balances, including credit-line drawdowns, heterogeneous borrower demand, and the Federal Reserve's broader policy response ([Li, Strahan, and Zhang 2020](#)). Bank and state-by-quarter fixed effects, predetermined balance-sheet controls, and local-demand adjustments rule out observable versions of these explanations as drivers of the post-policy exposure gradient. They do not distinguish between two policy-linked interpretations of the remaining cross-bank difference.

One interpretation is that eliminating reserve requirements relaxed an institutional constraint on usable liquidity. The Federal Reserve removed a requirement whose pre-policy burden varied across banks, encouraged banks to use capital and liquidity buffers while lending, and stated that zero requirements would support lending ([Board of Governors of the Federal Reserve System 2020b](#)). A 2018 FOMC staff memo notes that required balances did not count toward the liquidity coverage ratio and that large banks typically excluded them from internal stress tests because reserve-maintenance expectations effectively encumbered them ([Logan and Schulhofer-Wohl 2018](#)). The policy could therefore have made liquidity more usable without changing its accounting quantity.

The announcement may also have operated through supervisory permission or moral suasion interacting with the size of the obligation removed. For banks with larger pre-policy burdens, setting the requirement to zero made the official encouragement to use liquidity and balance-sheet capacity more concrete and consequential. This message resembles expansionary moral suasion: official encouragement reinforced by supervisory leverage and bank incentives ([Breton and Wintrobe 1978](#); [Martell 2026](#)). Quarterly Call Reports cannot distinguish these policy-linked interpretations from one another or from

borrower- and customer-composition channels. The empirical result is the initial cross-bank lending difference, not an estimate of a unique mechanism.

5. Conclusion

After the Federal Reserve eliminated reserve requirements, banks facing larger pre-policy burdens recorded larger increases in non-PPP lending. Because the policy applied to every bank, the design identifies this relative cross-bank response rather than the nationwide effect of eliminating reserve requirements. Across four specifications, the estimates range from \$0.86 to \$1.02 of additional loans over four quarters for each dollar of pre-policy burden. The response is concentrated around the announcement and initial adjustment; I find no evidence that more-exposed banks sustained faster loan growth. The estimate is unchanged by controls for local pandemic severity and predicted industry demand. This relative response can coexist with an aggregate decline in loans relative to deposits. Even in an abundant-reserves regime, pre-policy statutory burdens remained informative about which banks expanded lending more when the requirements disappeared.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the author used generative AI tools and services from OpenAI and Anthropic to assist with ideation, code development, manuscript editing, and editorial feedback. After using these tools/services, the author reviewed and edited the content as needed and takes full responsibility for the content of the published article.

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Appendix for “Nothing Required: U.S. Bank Lending after the End of Reserve Requirements”

Appendix A. Data Appendix

Table A1 reports summary statistics for the 723-bank analysis sample. Panel A covers the bank-quarter outcomes from 2018q1 through 2022q4, and Panel B reports the 2019q4 balance-sheet measures used to construct exposure and the balance-sheet controls. The distributions are unweighted, matching the main specifications.

TABLE A1. Summary statistics for the analysis sample

Variable	Obs.	Mean	SD	P25	Median	P75
<i>Panel A. Bank-quarter outcomes, 2018q1–2022q4</i>						
Four-quarter change in non-PPP loans (Percent of 2019q4 assets)	14,441	6.62	13.08	1.21	4.38	8.88
Non-PPP loan stock (Percent of 2019q4 assets)	14,441	74.07	23.84	62.89	73.39	82.79
<i>Panel B. Balance-sheet measures, 2019q4</i>						
Total assets (Billions of dollars)	723	21.9	144.0	0.8	1.8	5.3
Non-PPP loans (Percent of assets)	723	69.7	14.2	64.1	72.9	78.7
Deposits (Percent of assets)	723	81.4	7.1	78.3	82.8	86.5
Net transaction accounts (Percent of assets)	723	16.4	12.4	7.7	11.6	22.8
Gross reserve burden (Percent of assets)	723	0.87	0.94	0.21	0.60	1.18
Reported reserve balances (Percent of assets)	723	3.91	5.07	0.69	2.12	5.25
Uninsured deposits (Percent of assets)	475	26.6	13.2	16.7	25.7	33.9
Unused commitments (Percent of assets)	664	2.4	2.3	0.7	1.9	3.5
Small-business loans (Percent of total loans)	715	2.7	3.6	0.9	2.0	3.3

Note: Panel A uses the 14,441 bank-quarter observations in the main 723-bank panel. Panel B uses one 2019q4 observation per bank. Non-PPP loans subtract reported PPP balances, which equal zero before 2020q2. Loan outcomes and gross burden are divided by fixed 2019q4 assets. Reported reserve balances are balances due from Federal Reserve Banks. The observation counts for uninsured deposits, unused commitments, and small-business loans report banks with nonmissing raw values. The regressions retain banks with missing values: each characteristic is standardized using the observed banks, missing standardized values are set to zero, and a missing-value indicator is interacted with quarter.

Table A2 identifies the public datasets used in the paper. The Call Reports and reserve-requirement schedule construct the main panel and exposure measure. The remaining sources construct the branch-weighted pandemic-demand controls.

TABLE A2. Public data sources

Public data source	Coverage	Use in the paper
FFIEC Call Reports (Federal Financial Institutions Examination Council 2026)	2017q1–2022q4	Bank balance sheets, outcomes, PPP balances, and balance-sheet controls
Federal Reserve reserve-requirement schedule (Board of Governors of the Federal Reserve System 2020a)	Schedule in force before the March 2020 elimination, applied to 2019q4 accounts	Gross and vault-cash-adjusted reserve burdens
FDIC Summary of Deposits (Federal Deposit Insurance Corporation 2019)	June 2019	Branch locations and branch-deposit weights
CDC/NCHS county mortality (National Center for Health Statistics 2022)	2020–2022	Branch-weighted county COVID mortality
BLS Local Area Unemployment Statistics (U.S. Bureau of Labor Statistics 2026a)	2018–2022	Branch-weighted county unemployment
Census County Business Patterns (U.S. Census Bureau 2021)	2019	County industry employment shares
BLS Quarterly Census of Employment and Wages (U.S. Bureau of Labor Statistics 2026b)	2019q2 and 2020q2	National industry employment changes

Note: Call Report field definitions follow the FFIEC reporting instructions. County data are aggregated to banks with June 2019 branch-deposit weights from the Summary of Deposits. The COVID mortality and unemployment measures use rolling four-quarter averages. The predicted industry-demand measure combines 2019 county industry employment shares with national industry employment changes from 2019q2 to 2020q2.

Appendix B. Supporting Evidence

[Table B1](#) distinguishes gross schedule-implied burden from burden net of vault cash and reported reserve balances. Vault-cash-adjusted burden rises from 0.02% of assets in the low-exposure tercile to 1.34% in the high-exposure tercile.

TABLE B1. Pre-policy statutory burden and reported reserve balances

	Low exposure	Middle exposure	High exposure
Banks	241	241	241
Mean transaction accounts (% of assets)	7.39	14.06	27.67
Mean gross burden (% of assets)	0.14	0.60	1.87
Median gross burden (% of assets)	0.13	0.60	1.58
Mean applied vault cash (% of assets)	0.12	0.37	0.53
Mean vault-cash-adjusted burden (% of assets)	0.02	0.23	1.34
Mean reported reserve balances (% of assets)	3.40	3.43	4.89

Note: Balance-sheet values are measured in 2019q4. Except for bank counts, entries are percentages of 2019q4 assets. Gross burden applies the operative 2020 schedule to 2019q4 net transaction accounts. Applied vault cash equals the smaller of reported currency and coin and gross burden; vault-cash-adjusted burden is gross burden minus applied vault cash. Reported reserve balances are balances due from Federal Reserve Banks. None of these accounting measures identifies reserve balances actually released or a reserve shadow price.

Table B2 shows that the per-dollar initial coefficient remains positive under two alternative exposure measures. The estimate is \$0.83 per dollar of burden net of vault cash and \$0.80 when gross burden is winsorized, compared with \$0.86 under the main measure. Because burden net of vault cash differs by 1.32 percentage points of assets between the low- and high-exposure terciles, its implied tercile contrast is about 1.1 percentage points, compared with 1.5 percentage points under gross burden. The operative 2020 thresholds took effect on January 16. Applying the 2019 thresholds to the same 2019q4 accounts yields an initial estimate of \$0.85 (SE 0.43), nearly identical to \$0.86 (SE 0.43) under the 2020 schedule.

TABLE B2. Initial lending estimates under alternative exposure measures

	Estimate	Obs.	Banks
Gross schedule-implied burden	0.86** (0.43)	14,341	718
Burden net of vault cash	0.83* (0.48)	14,341	718
Winsorized gross burden	0.80* (0.45)	14,341	718

Note: The outcome is the four-quarter change in non-PPP loan balances divided by fixed 2019q4 assets. Estimates are loan dollars per dollar of the gross or vault-cash-adjusted burden named by the row. The first row reproduces column (4) of Table 1; the other rows change only the exposure measure. All rows include bank and state-by-quarter fixed effects, a separate 2020q1 exposure coefficient, and quarter interactions with the same 2019q4 balance-sheet controls. Standard errors in parentheses are clustered by bank. * $p < .10$, ** $p < .05$, and *** $p < .01$.

Table B3 collects the targeted specification, sample, and committed-credit checks in one display. The preferred lending estimate remains positive with size-bin-by-quarter fixed effects and after excluding large balance-sheet discontinuities. On a common sample, adding unused commitments to non-PPP loans leaves the estimate essentially unchanged.

TABLE B3. Targeted specification, sample, and committed-credit checks

	Estimate	Obs.	Banks
<i>Panel A. Targeted specification and sample checks</i>			
Preferred specification	0.86** (0.43)	14,341	718
Add size-bin-by-quarter fixed effects	0.87** (0.44)	14,341	718
Exclude large balance-sheet discontinuities	1.14*** (0.35)	13,878	718
<i>Panel B. Credit-line accounting</i>			
Non-PPP loans (common sample)	0.79* (0.45)	13,103	661
Non-PPP loans plus unused commitments	0.79* (0.46)	13,103	661

Note: Panel A uses the four-quarter change in non-PPP loan balances divided by fixed 2019q4 assets. Each row changes only the specification or sample feature named by the row. The discontinuity restriction requires finite four-quarter asset and deposit growth and excludes bank-quarter observations when either absolute growth rate exceeds 50%. Panel B holds the 661-bank sample fixed and compares non-PPP loans with non-PPP loans plus reported unused commitments. A one-for-one conversion of an unused commitment into a funded loan leaves the combined outcome unchanged, although the outcome can also reflect new, canceled, or expired commitments. It does not measure gross loan originations. All specifications use gross burden under the operative 2020 schedule, include bank and state-by-quarter fixed effects, estimate 2020q1 separately, interact the preferred 2019q4 balance-sheet controls with quarter, and cluster standard errors by bank. * $p < .10$, ** $p < .05$, and *** $p < .01$.