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Did the Paycheck Protection Program Help Small Businesses? Evidence from Commercial Mortgage-Backed Securities†

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In this study, we examine the broader economic effects of the US federal government's Paycheck Protection Program by focusing on the performance of securitized commercial mortgages. We provide novel evidence for spillover effects of government interventions in the face of economic crises. We find that the PPP reduced mortgage delinquencies by approximately $36 billion in 2020. The strongest effects occur when PPP funds targeted businesses in areas most affected by COVID-19, where banks overperformed in providing PPP loans, and among mortgages on properties in retail and lodging. Thus, PPP relief to small businesses eased economic distress beyond the labor market. (*JEL* G21, G28, I12, I18, L25, R33)

When the coronavirus disease (COVID-19) became a global pandemic in early 2020, governments responded with a series of measures designed to curtail the spread of the disease, including international travel bans, quarantine directives, and business shutdowns. These measures quickly accelerated into an economic crisis and global recession, with evidence suggesting that many businesses experienced immediate financial distress.[1] In response, governments introduced a variety of policies designed to provide direct support to businesses and individuals, with a growing body of literature documenting their efficacy (Gourinchas

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[1] Given the scale of the economic crisis resulting from the pandemic, it is not surprising that numerous studies were quickly published documenting the pandemic's negative effects on asset prices (Ling, Wang, and Zhou 2020; Alfaro et al. 2020; van Dijk, Kinsella Thompson, and Geltner 2020; Ramelli and Wagner 2020; Hassan et al. 2020; Altig et al. 2020; Milcheva 2022), consumer spending (Chetty et al. 2020; Horvath, Kay, and Wix 2020), and employment (Chetty et al. 2020; Dingel and Neiman 2020; Borjas and Cassidy 2020; Koren and Pető 2020).

–et al. 2021; Chetty et al. 2020; Granja et al. 2022; Capponi, Jia, and Rios 2021; Beland, Brodeur, and Wright 2020; Dave et al. 2021; Bartik et al. 2020b; Hubbard and Strain 2020; Autor et al. 2022). This literature generally explores the direct impact of government policies on targeted entities, paying little attention to the indirect or second-order impact of these policies. In this study, we expand the literature by providing novel evidence of spillover effects to the broader economy from policies designed to mitigate the economic consequences of government restrictions on small businesses.

Our focus is on assessing whether the Paycheck Protection Program (PPP)—designed to aid small businesses and included in the $2 trillion Coronavirus Aid, Relief, and Economic Security (CARES) Act enacted March 27, 2020—indirectly supported commercial property owners who lease space to small businesses and thereby reduced the incidence of commercial mortgage default.[2] The Small Business Administration (SBA) implemented the PPP in two rounds that lasted from April 2020 to August 2020. The SBA announced a third round in January 2021, with the new legislation approved in March 2021. We focus on PPP loans approved during the first two rounds, which together provided more than $525 billion in federal loans in 2020.

Although the PPP explicitly prohibited passive investors from participation, there were indeed channels by which PPP funding could indirectly benefit commercial real estate landlords and investors. For example, Figure 1 depicts a "tenant channel" whereby tenants continue making their rent payments after receiving PPP loans.[3] We test this spillover hypothesis by examining the default propensity of nonresidential commercial mortgage-backed securities (CMBS) loans between January 2019 and December 2020 using an event study framework. A CMBS is a financial vehicle that pools mortgages collateralized by commercial properties and originated from many lenders into a single trust that is sold to multiple investors. We use an administrative dataset that allows us to observe the detailed monthly loan performance records for nearly the entire nonresidential CMBS market in the United States. The CMBS market provides an important source of liquidity for lenders (An, Deng, and Gabriel 2011), accounting for 12 percent of the $2.83 trillion in US nonresidential commercial mortgage debt (see Board of Governors of the Federal Reserve System 2018:IV).

Evaluating the PPP's impact on the CMBS market requires estimating a counterfactual outcome in the absence of the program, which is challenging because all US counties that had properties serving as collateral for mortgages contained in CMBS received PPP loans (see Figure A.1 in the online Appendix). Moreover, several confounding events occurred around the passage of the PPP. For example, the federal government issued foreclosure and eviction moratoria

[2] At the onset of the pandemic in the United States, many real estate market commentators predicted significant losses for commercial property investors and lenders as a result of tenants' inability to pay rent. For example, a March 24, 2020, article in the *Wall Street Journal* reported that one industry participant predicted that commercial real estate debt could see loss rates of 2.5 percent over the next five years in comparison to the 0.1 percent loss rate in 2019 (Grant 2020).

[3] While the loans were generally advertised as available to small businesses for the purpose of covering their payrolls, the funds could also be used to pay rent (see §1102 in US Congress 2020b).

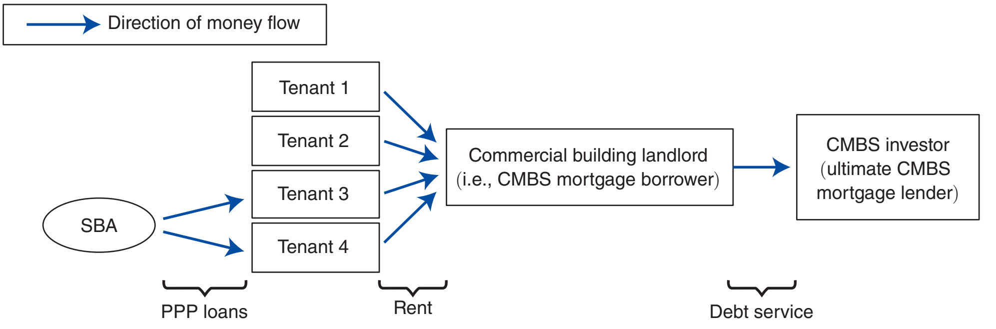

FIGURE 1. PPP FUND FLOW IN CMBS MORTGAGES

Notes: This figure illustrates the tenant channel whereby PPP loan funds reach CMBS mortgages via PPP borrowers. PPP borrowers are tenants in commercial properties backing CMBS mortgages and use a portion of the funds to pay for rent expenses to operate in those commercial buildings.

on residential properties that gave Fannie Mae and Freddie Mac the authority to enter into forbearance agreements with the owners of multifamily properties and residential properties more generally (Ambrose, An, and Lopez 2022; An, Gabriel, and Tzur-Ilan 2022). Thus, to achieve identification, we rely on the heterogeneity in loan performance across private nonresidential mortgages that have variation in the timing of initial exposure and size of the exposure to PPP funds, which is driven by the actions of the tenants of the underlying collateral.

Our identification strategy rests on two observations. First, empirical evidence demonstrates that the change in the ratio of a property's net operating income (NOI) to debt service (DS), which is commonly known as the debt service coverage ratio (DSCR), is inversely correlated with a borrower's incentive to default (Vandell 1984; Goldberg and Capone 2002; Ciochetti et al. 2003).[4] Second, PPP funds may substitute for an income shock to the NOI of commercial property. Thus, the PPP loans have the potential to impact the default propensity.

Figure 2 provides visual evidence to support our identification strategy by showing the differences in the 30+ days delinquency rate across groups of commercial mortgages over the period from January 2019 to December 2020. We clearly see that mortgage delinquencies spike at the onset of government shutdown orders (i.e., the following March–April 2020).[5] However, panel B, which focuses on mortgages identified as having at least one tenant that received a PPP loan, shows that properties with the greatest exposure to PPP funds per dollar of debt service experienced the least economic stress. Thus, a relationship appears to exist between the relative exposure to PPP funds and the continuity of commercial mortgage debt payments during the pandemic. However, a formal analysis of the PPP effects on mortgage performance must account for loan heterogeneity and staggered treatment adoption.

[4] Commercial real estate mortgages are generally nonrecourse, making default an explicit function of the ability of the property cash flow (NOI) to support the debt service payment.

[5] The first statewide stay at home order was issued on March 19, 2020, by California. Other states issued shutdown orders during the first week of April (Mervosh, Lu, and Swales 2020).

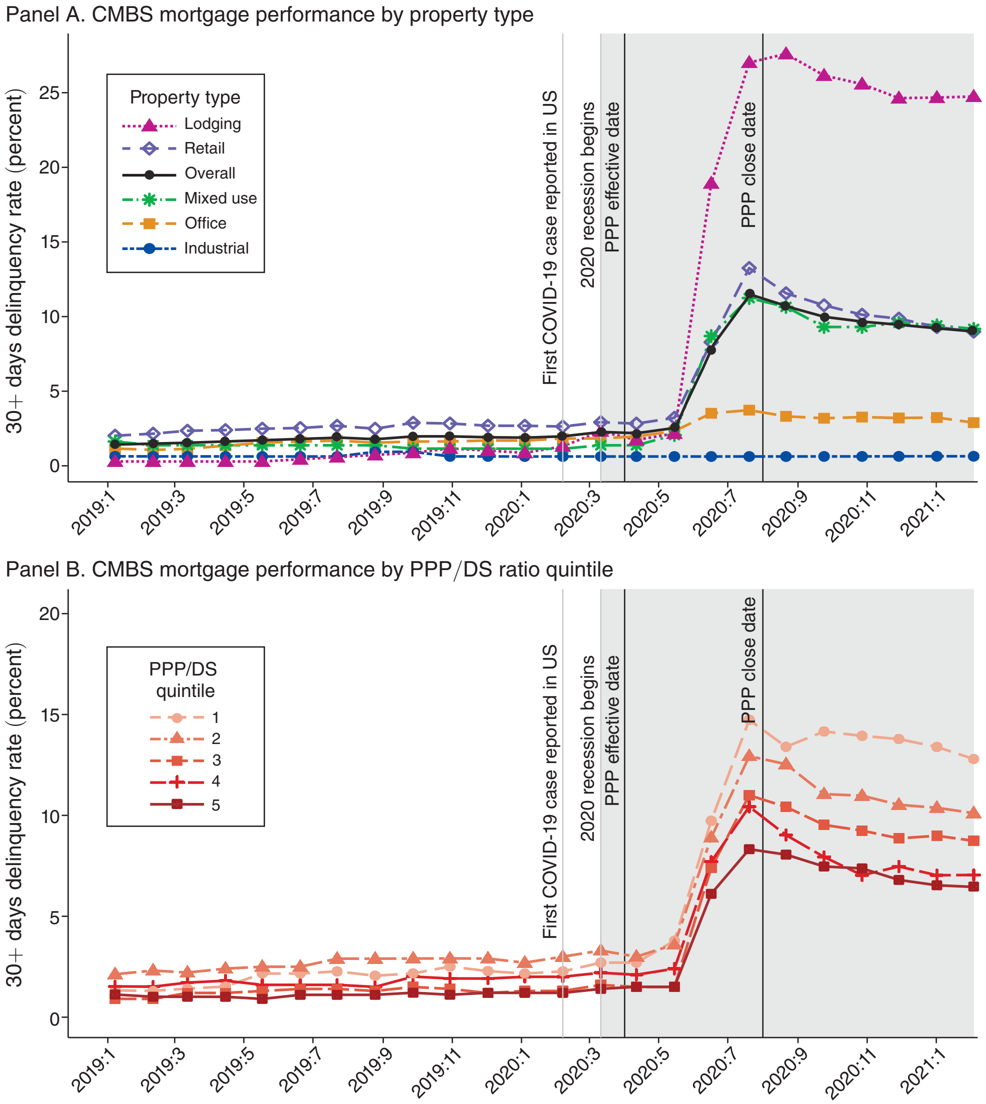

Figure 2. CMBS Mortgage Performance during the COVID-19 Pandemic

Notes: This figure displays the 30+ days delinquency rate of CMBS mortgages during the COVID-19 pandemic from January 2019 to December 2020. In panel A, mortgages in the full CMBS sample are divided by property type. In panel B, mortgages in the PPP-CMBS matched sample are divided into static quintiles (within property type) based on PPP/DS ratio. PPP/DS quintile 1 represents the CMBS mortgages with the lowest PPP/DS ratio, whereas PPP/DS quintile 5 represents CMBS mortgages with the highest PPP/DS ratio.

Our empirical strategy is based on a two-way fixed effects (TWFE) specification with continuous treatment that exploits the within-loan variation in monthly loan performance records to formally compare the mortgage delinquency rate of CMBS mortgages before and after passage of the PPP across mortgages with varying degrees of treatment intensity. In our context, treatment intensity is measured as the ratio of PPP loan exposure to the mortgage payment amount. The model includes a rich set of loan and state-time fixed effects that allows us to estimate the average treatment response (ATR) of the 30+ days delinquency likelihood from a marginal change in the PPP treatment intensity. Since our continuous treatment TWFE framework rests on the strong parallel trends assumption (Callaway, Goodman-Bacon, and Sant'Anna 2021), we confirm our causal interpretation by estimating a variety of specifications that relax this assumption. We also employ a dynamic TWFE model that captures the time-varying effects of the ATR, and we estimate the interacted-weighted (IW) estimator proposed by Sun and Abraham (2021) to control for staggered treatment adoption. Furthermore, we explore variations on the Sun and Abraham (2021) model using cohort-specific profiles to account for potential violations in the parallel trends assumption and heterogeneous treatment effects arising from loan-specific or geographic-specific trends (see Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021). Finally, using data on institution-grade commercial property investments from the National Council of Real Estate Investment Fiduciaries (NCREIF), we conduct an analysis to show the external validity of our hypothesis that the PPP attenuated pandemic-related income shocks.

Our primary result suggests that the PPP attenuated increases in the delinquency rate of commercial mortgages during the pandemic by about 1 to 1.5 percentage points at the average treatment intensity level with 95 percent confidence. The ATR may vary depending on the size of the mortgage and exposure to program funds. For example, the default likelihood is 5.59 percentage points lower for a mortgage at the ninety-fifth intensity percentile compared to a mortgage at the fifth percentile after exposure.[6] As a benchmark, the average delinquency rate of commercial mortgages, which was about 2 percent before the pandemic hit, increased to a peak of 11.3 percent in June 2020. Hence, the risk of default was cut in half for mortgages that had a high exposure to PPP funds compared to those hardly exposed. In terms of economic impact, the confidence interval suggests that the PPP averted between \$29.7 and \$41.9 billion in potential pandemic-related commercial mortgage defaults, assuming our matched PPP-CMBS sample is representative of the broader \$2.83 trillion commercial real estate debt market. In terms of program efficiency, we note that approximately 15 percent of PPP funds extended to small businesses were used to pay rent, suggesting that about \$19.7 billion in PPP funds averted an average of \$36 billion in mortgage defaults. To put this in perspective, we estimate that the PPP averted an amount of losses equal to about 59 percent of the highest annual conduit CMBS default losses seen in the aftermath of the Great Financial Crisis (GFC). It is important to remember that these estimates are indirect or spillover benefits that are in addition to the direct employment benefits targeted under the PPP.

We also explore whether the PPP loans had differential effects by property type to identify how various industries responded to the program. We find that the PPP had a stronger effect on reducing default rates on mortgages collateralized by retail properties where the typical tenant roll includes small businesses (such as restaurants, cafés, and shops). These businesses tend to rely on foot traffic and face-to-face

[6] We use the terms "default" and "delinquency" interchangeably.

interactions that were restricted during the state- and county-wide shutdowns of nonessential businesses. We find strong effects for mortgages collateralized by lodging properties too, but not for office or industrial properties where distress was low. Along the loan-size dimension, although both landlords with small or large mortgages (within each property type) benefited from tenants obtaining PPP loans, landlords with particularly large mortgages and debt service payments were more sensitive to PPP funds than less leveraged landlords.

However, our results likely understate the intended impact of the PPP since the initial wave of PPP loans did not necessarily reach small businesses in the hardest-hit locations (Granja et al. 2022; Chetty et al. 2020). When we analyze the impact of the PPP over time, we find that the largest effect occurred one month after initial treatment, which coincides with the distribution of additional PPP funds, with the delinquency rate for mortgages exposed to PPP funds declining by 1.5 percentage points at the average PPP/DS ratio. As Bartik et al. (2020b) and Granja et al. (2022) point out that banking relationships played a significant role in determining whether a business was approved for a PPP loan, we provide further evidence suggesting that the first-round funding attenuated the delinquency rate of mortgages at a greater level in locations that were more exposed to banks that had higher PPP participation rates relative to non-PPP lending to small businesses. Furthermore, we observe stronger responses to PPP funds in locations where small businesses are more likely to participate, such as urban areas. Thus, our results suggest that the effects of the program on mortgage delinquencies could have been greater if the PPP loans had been targeted at the hardest-hit locations during the first funding round. From a policy perspective, our results provide evidence that policymakers should attempt to ensure that future PPP loans target those areas and small businesses most impacted by COVID-19.

Our study contributes to three streams in the literature. First, we expand the literature exploring the economic effects associated with the COVID-19 pandemic (Alfaro et al. 2020; Borjas and Cassidy 2020; Koren and Pető 2020; Horvath, Kay, and Wix 2020; Hassan et al. 2020; Gourinchas et al. 2021; Altig et al. 2020; van Dijk, Kinsella Thompson, and Geltner 2020; Ramelli and Wagner 2020). For example, our study complements the early analysis of Ling, Wang, and Zhou (2020) who found negative short-term market reactions to firms holding portfolios of commercial real estate property that dissipated over time. Our analysis provides evidence that the PPP was the causal mechanism underlying these short-term market reactions.

Second, we expand the literature that focuses on impact analyses of programs designed to counter the economic effects of the pandemic (Beland, Brodeur, and Wright 2020; Calem, Covas, and Freedman 2020; Capponi, Jia, and Rios 2021; Chetty et al. 2020; Dave et al. 2021; Faulkender, Jackman, and Miran 2020; Gourinchas et al. 2021; Granja et al. 2022; Li and Strahan 2021; Liu and Volker 2020; Bartik et al. 2020b; Hubbard and Strain 2020; Autor et al. 2022). Our paper is most closely related to those focusing on the PPP. For example, Granja et al. (2022) focus on microlevel employment patterns during the PPP, concluding that firms used funds to meet nonpayroll commitments. Their analysis suggests that the PPP would have had medium-term impacts. We confirm this conjecture by demonstrating that the PPP effects were felt primarily during the second round of funding. Barrios et al. (2020) focus on how the allocation of PPP funds impacted business payrolls. On the theoretical front, Joaquim and Netto (2021) and Elenev, Landvoigt, and Van Nieuwerburgh (2022) provide models examining the optimal targets of PPP funding. In contrast, our analyses reveal the indirect effects of the PPP on entities not directly targeted by policymakers. Furthermore, our analyses feature objective evidence of the economic impact via an observable outcome—mortgage defaults. Thus, we provide novel empirical evidence linking direct policy interventions to broader economic outcomes. In particular, our findings suggest that policymakers should broaden the scope of traditional cost/benefit analysis when considering the merits of future market interventions.

Third, we contribute to the literature that focuses on the more general role of government policy interventions in response to economic crises. For example, in response to the mortgage crisis that began in 2007, the federal government created the Home Affordable Modification Program (HAMP) and the Home Affordable Refinance Program (HARP) to provide relief to struggling homeowners hardest hit by the recession. Agarwal et al. (2017) find that HAMP prevented more than 600,000 residential foreclosures. Meanwhile, Agarwal et al. (2015) find that HARP increased the refinance rate by 1.5 percent, which led to an aggregate increase in US consumption by about \$20 billion. While the majority of these programs were aimed at households (Mian and Sufi 2012; Parker et al. 2013; Kaplan and Violante 2014; Agarwal et al. 2015, 2017; Baker et al. 2020), we explore a critical intervention targeting small businesses to demonstrate a spillover effect to a nontargeted economic sector.

The remainder of the paper is organized as follows. Section I introduces the PPP, Section II describes the data and main variables, Section III illustrates the empirical methods, Section IV reports the estimation results, Section V provides a set of robustness checks, Section VI offers external validity of the findings, Section VII discusses the economic magnitude of the estimates, and Section VIII concludes.

I. The Paycheck Protection Program

The CARES Act made the SBA responsible for administering the PPP, which involves overseeing loans originated under the program by local banking institutions (US Congress 2020b). According to the SBA (2020), PPP loans may have maturities between two and five years with annual interest rates of 1 percent, loan payments may be deferred for ten months and require no collateral or personal guarantees; and the maximum loan amount is the lesser of \$10 million or 2.5 times a business's average monthly payroll costs in 2019. In addition, PPP loans (principal amount and accrued interest) are forgivable under three conditions (§1106 US Congress 2020b). First, the small business must maintain or rehire employees at their pre-pandemic compensation levels. Second, the business may only use the PPP funds for eligible expenses. Third, a maximum of 40 percent of PPP funds are allowed for nonpayroll expenses, such as rent or mortgage interest expenses. Initially, only 25 percent of the funds used for nonpayroll purposes qualified for forgiveness (US Small Business Administration 2020a, 13 C.F.R. §120). However, the passage of the PPP Flexibility

Act on June 5, 2020, revised public law to allow for up to 40 percent of the funds from a PPP loan that were used for nonpayroll expenses to be completely forgiven (US Congress 2020d).

The SBA began to accept PPP loan applications in early April 2020. In general, businesses that qualified for a PPP loan had to have fewer than 500 employees and could apply for only one PPP loan (SBA 2020). By April 16, 2020, about $350 billion in PPP loans had been issued, exhausting the initial funds earmarked for the small business relief program (Li and Strahan 2021). A second round of funding began on April 27, 2020, after the passage of the Paycheck Protection Program and Health Care Enhancement Act (US Congress 2020c), which earmarked an additional $320 billion for the PPP. According to Li and Strahan (2021), the second round of PPP funds was distributed mostly in late April and early May 2020. The program ended on August 8, 2020 (US Congress 2020a). Over the course of the two rounds, approximately $525 billion in PPP funds were distributed to more than five million borrowers (Hubbard and Strain 2020). New legislation in 2021 reopened the program with additional funding, modified eligibility rules, and offered an extended application deadline (June 30, 2021) (US Congress 2021). Over the following six months, more than $277 billion in additional loans were approved for new and existing PPP borrowers (SBA 2021). As of February 2022, about 87 percent of the PPP funds disbursed had been forgiven (SBA 2022). We focus on the first two rounds of funding that were distributed in 2020, which coincide with the initial economic effects related to the pandemic.

A rich academic debate exists on whether the PPP was successful in encouraging employee retention. For example, Chetty et al. (2020) find that after program implementation, employment at firms just below the 500-employee eligibility cutoff was about 2 percent higher than that at ineligible firms just above the cutoff, implying that during its first two rounds the PPP saved approximately 1.29 million jobs at a cost of about $377,000 per job. Autor et al. (2022); Granja et al. (2022); and Hubbard and Strain (2020) similarly estimate that the PPP had a modest impact on employment. However, Faulkender, Jackman, and Miran (2020) note that focusing on firms with roughly 500 employees each to determine the PPP effect on employment likely understates the impact of the PPP because 95 percent of PPP loans were distributed to firms with 38 or fewer employees. Faulkender, Jackman, and Miran (2020) estimate that unemployment insurance claims were 1 to 2 percentage points lower in counties where PPP loan coverage was 10 percentage points higher. As a result, they extrapolate that the PPP saved approximately 18.6 million jobs. Likewise, Li and Strahan (2021) demonstrate that the PPP is associated with a lower incidence of unemployment insurance claims after accounting for the supply of PPP loans with the local banking structure. Based on survey data, Bartik et al. (2020b) find that firms that obtained a PPP loan reported retaining two to three more jobs on their payrolls than firms that did not obtain a PPP loan after the first round of PPP funding. Given more than five million PPP borrowers from April to August 2020 totaling to $525 billion in PPP funds, the point estimates from Bartik et al. (2020b) imply that the PPP saved between 10 and 15 million jobs at a cost of less than $53,000 per job.

Regardless of the PPP's direct impact on employment, firms that obtained a PPP loan have been found to have had a better chance of remaining open during the COVID-19 pandemic (Bartik et al. 2020b; Hubbard and Strain 2020; Granja et al. 2022). PPP loans may have helped firms avoid closure since the CARES Act allowed firms to use a portion of their PPP loans for nonpayroll purposes, including mortgage interest payments, rent or lease payments, and utility payments (see §1102 and §1106 in US Congress 2020b). Moreover, even if firms used PPP loans only to cover payroll expenses, firms may have used their reserves—which are typically equivalent to one to two months of cash holdings—to pay other operating expenses on which they could have defaulted in the absence of the PPP (Bartik et al. 2020a). According to Granja et al. (2022), who examine responses to the Census Small Business Pulse Survey administered from April to June 2020, a 10 percentage point increase in the share of a state's firms receiving a PPP loan is associated with a 4.9 percentage point decline in missed scheduled payments, including mortgage, rent, and utility payments.

Thus, based on the findings that the PPP did stabilize small businesses, we conjecture that the PPP created financial stability for the commercial mortgage market, particularly for those landlords who rely on timely rent payments from small businesses. Although we do not observe whether a tenant defaults on rent, we do observe whether a lender defaults on mortgage payments, which is a function of the revenue the collateral property generates from tenants paying rent. As Hubbard and Strain (2020) note, it is more effective to focus on revenue outcomes than payroll costs when evaluating the PPP. Thus, we focus on default decisions on commercial mortgages, which depend on the revenue generated from business tenants and the amount of the debt service.

II. Data

Using the Trepp database, we collect monthly performance records on privately securitized commercial mortgages that were outstanding between January 2019 and December 2020. The data cover about 12 months before and 12 months after the World Health Organization first received reports of the novel coronavirus on December 31, 2019 (World Health Organization 2020). As one of the largest providers of information on securitized commercial mortgages, Trepp tracks more than 1,500 CMBS deals comprising more than 200,000 commercial mortgages. The Trepp database contains extensive information about commercial real estate debt, including CMBS mortgage performance, underwriting criteria, and property collateral.

We merge the monthly Trepp CMBS mortgage data with COVID-19 records from the Johns Hopkins University Coronavirus Resource Center to obtain the county-month COVID-19 exposure metric.[7] We obtain county-month unemployment rates from the Bureau of Labor Statistics. Finally, we use the Real Estate Investment Trust aggregate property type equity indices from the National Association of Real Estate Investment Trusts to proxy for changes in commercial property prices.

[7] For more details on Johns Hopkins University's COVID-19 data, see Dong, Du, and Gardner (2020). Figure A.2 in the online Appendix illustrates the geographic variation over time in the cumulative COVID-19 cases per 100 population in US counties that appear in the Trepp CMBS sample.

We restrict our analysis to mortgages that were securitized and performing as of January 2019 since the borrowers could not have optimized their financing choices in anticipation of the unexpected pandemic shock. Moreover, we limit our analysis to mortgages collateralized by industrial, lodging (hotel), office, retail, and mixed-use properties in the United States.[8] We exclude mortgages with collateral in multiple counties or missing location data.[9] Finally, we remove mortgages with missing information about the loan amount, property value, current loan-to-value (LTV) ratio, contract rate, county unemployment rate, county COVID-19 rate, and county 30+ days delinquency rate, leaving 16,443 unique mortgages with collateral located in more than 1,100 counties in all 50 states and the District of Columbia, representing about 92 percent of the relevant mortgages in the Trepp universe. In the subsequent analysis, we refer to this dataset as the full CMBS sample.

The total volume of CMBS mortgages in the full sample amounts to approximately \$330.34 billion, which represents 97 percent of the nonresidential/nonfarm commercial mortgage debt held in asset-backed securities as of 2018:IV (see Board of Governors of the Federal Reserve System 2018:IV). Thus, the full sample is representative of the entire nonresidential CMBS market with about 42 percent of the mortgages collateralized by retail property, 21 percent by office property, 16 percent by lodging property, 14 percent by industrial property, and 7 percent by mixed-use property.

Table 1, panel A provides summary statistics of key variables as of January 2019. Since we observe monthly performance records for each mortgage, the full sample contains more than 362,000 loan-month observations. We note that the average property had an LTV ratio of 51.7 percent with an average of 68 months remaining to maturity. We also report the average late payment status, 30+ days delinquency status, and special servicing status in Table 1. The late payment variable flags CMBS mortgages for which a payment has not been received on time even if the borrower is still in a grace period. The delinquency status variable flags CMBS mortgages with payments that are 30+ days late. Special servicing refers to the transfer of loans by the CMBS deal master servicer to the special servicer when a CMBS mortgage is at risk of default or fails to perform as expected (Ambrose, Sanders, and Yavas 2016). The late and delinquency status variables reflect the borrower's actions leading to mortgage default (i.e., failure to make the required payments), while transfer to special servicing reflects the active monitoring of the master servicer.

Panel A of Figure 2 shows the mortgage performance and indicates that in 2019 the 30+ days delinquency rate is flat and declining across the five property types, with the retail loans having the highest rates. In 2020:II, the delinquency rates of all five loan categories increase sharply but not proportionately. While the average delinquency rate across all loans peaks at 11.3 percent in June 2020, the delinquency rate of retail

[8] We exclude mortgages in the healthcare and multifamily sectors that may have benefited from non-PPP funds or other policies such as the federal foreclosure and eviction moratoria. Ambrose, An, and Lopez (2022) point out that a significant proportion of multifamily mortgages are insured by government-sponsored enterprises that may share rental income shocks with multifamily borrowers to mitigate eviction risk. For similar reasons, we also exclude commercial mortgages used to finance single-family rentals.

[9] We match zip codes to counties using the HUD-USPS ZIP Code Crosswalk (US Department of Housing and Urban Development, Office of Policy Development and Research 2020).

Table 1—Summary Statistics for Mortgages

Panel A. Full CMBS sample				Loan property type				
Variables	*N*	Mean	SD	Ind	Lod	Mix	Off	Ret
Securitization loan balance ($million)	16,443	20.091	49.709	8.234	19.79	24.186	34.016	16.390
Securitization property value ($million)	16,443	69.284	259.695	14.169	50.213	124.784	137.795	50.448
Monthly debt service ($thousand)	16,251	82.499	104.095	42.107	92.071	92.945	124.781	69.656
Current LTV (%)	16,443	51.715	20.535	39.501	55.541	54.688	48.405	55.449
Contract spread (%)	16,427	2.324	0.816	2.289	2.376	2.211	2.217	2.388
Origination year	16,443	2014	4.067	2014	2015	2014	2014	2013
Origination term (months)	16,443	125.276	40.286	126.545	115.718	130.762	118.192	130.991
Remaining term (months)	16,328	67.896	33.138	71.829	70.726	79.937	66.658	64.113
Interest only	16,443	0.188	0.391	0.138	0.076	0.268	0.288	0.182
Recourse	16,443	0.005	0.073	0.003	0.004	0.028	0.006	0.003
Late payment	16,443	0.067	0.250	0.023	0.053	0.060	0.074	0.083
Delinquent 30+ days	16,443	0.045	0.208	0.012	0.031	0.028	0.053	0.061
Delinquent 60+ days	16,443	0.044	0.204	0.011	0.029	0.027	0.051	0.059
Special servicing	16,443	0.045	0.206	0.011	0.031	0.026	0.056	0.058

Panel B. PPP-CMBS matched sample				PPP/debt service quintile				
Variables	*N*	Mean	SD	Q1	Q2	Q3	Q4	Q5
PPP/debt service	4,924	1.269	2.482	0.064	0.266	0.612	1.275	4.047
Securitization loan balance ($million)	4,924	23.401	52.566	41.372	25.866	21.987	14.343	14.769
Securitization property value ($million)	4,924	90.538	332.053	146.054	105.316	99.107	51.828	54.465
Monthly debt service ($thousand)	4,924	95.874	114.296	153.867	104.609	93.914	68.242	63.025
Current LTV (%)	4,924	54.21	19.287	50.044	54.596	55.461	56.092	54.543
Contract spread (%)	4,924	2.147	0.638	2.117	2.141	2.123	2.191	2.163
Origination year	4,924	2014	2.693	2014	2014	2014	2015	2015
Origination term (months)	4,924	120.015	22.91	119.505	120.039	121.069	119.193	120.232
Remaining terms (months)	4,924	72.502	26.941	70.63	72.346	72.848	72.153	74.398
Interest only	4,924	0.172	0.378	0.216	0.175	0.158	0.144	0.171
Recourse	4,924	0.004	0.062	0.002	0.002	0.005	0.004	0.006
Late payment	4,924	0.035	0.184	0.037	0.047	0.034	0.032	0.025
Delinquent 30+ days	4,924	0.014	0.117	0.013	0.021	0.009	0.015	0.011
Delinquent 60+ days	4,924	0.012	0.111	0.011	0.019	0.009	0.013	0.01
Special servicing	4,924	0.015	0.121	0.019	0.017	0.014	0.013	0.011

Notes: Panel A reports nonmissing variables for unique CMBS mortgages in the full CMBS sample and by property type subsamples (industrial, lodging, mixed use, office, and retail). Panel B reports nonmissing variables for unique CMBS mortgages in the PPP-CMBS matched sample, which consists of CMBS mortgages matched to at least one PPP loan from the SBA dataset on PPP borrowers, and by PPP/debt service quintile subsamples. PPP/debt service is the total PPP funds matched to a property divided by the debt service of the same property. The values of the variables are reported as observed on January 2019 in panel A and the first observable loan record in panel B. Q stands for quintile.

property increased from 5.5 percent to 11.9 percent year-over-year in June 2020, and for lodging property it increased from 3 percent to 27 percent. In contrast, we observe almost no change in the delinquency rates for industrial and office properties, remaining around 1 percent and 4 percent, respectively.

In order to assess whether the CMBS mortgages are representative of the broader market, we compare and contrast key mortgage characteristics by property type for mortgages that are securitized (Table A.1, panel A, in the online Appendix) and mortgages that are held in portfolio (Table A.1, panel B) to understand the similarities between the two forms of commercial real estate debt. The statistics on the sample of portfolio-held mortgages come from data collected by Glancy et al. (2021). We observe many similarities between the securitized mortgages and the portfolio-held mortgages. For example, property values, mortgage loan amounts, LTV ratios, and rate spreads are similar across the CMBS and portfolio loan groups, giving us confidence that our results can be generalized to the wider commercial real estate market. We do note one distinction among mortgage groups: most portfolio-held mortgages are recourse whereas securitized mortgages are nonrecourse.[10] Regardless of the differences between portfolio and CMBS mortgages, we note that our analyses focus on the 30+ days delinquency status, which is a common early signal of distress that is sensitive to income shocks in the commercial mortgage market. Thus, we argue that our results can be generalized to the broader commercial mortgage market since the pandemic was an unanticipated random shock and our sample is restricted to mortgages originated before the pandemic. As a result, we expect that a landlord's choice of financing was conditionally independent of anticipation about potential exposure to the federal program.

To provide further external validity that the experience of properties financed with CMBS mortgages can be generalized to the wider market, in Table A.2 in the online Appendix we compare the mean county-level market value of the collateral property underlying CMBS mortgages from Trepp to that of commercial property privately owned by members of NCREIF.[11] Although Trepp contains twice as many nonresidential commercial properties as NCREIF and offers much wider geographic coverage, a comparison between the two groups of properties provides some insight into the external validity of the Trepp data because most NCREIF properties are not leveraged. Panel A of online Appendix Table A.2 shows that the mean value of Trepp properties is significantly lower than that of NCREIF properties in each property-type sector, whereas panel B of Table A.2 shows that the mean differences become statistically insignificant or less economically meaningful once the sample is restricted to properties in overlapping counties. Hence, the Trepp data include properties that are similar to commercial real estate that is generally not leveraged.

We next merge the monthly Trepp data with PPP loan data from the SBA in order to observe the intensive exposure to the PPP relative to the mortgage debt service. The PPP-CMBS matching process requires standardizing addresses in both the SBA PPP loan and Trepp CMBS mortgage datasets. There are approximately 4.5 billion

[10] It is an empirical question whether securitized mortgages and portfolio-held mortgages produce sizable differences in the overall losses during a pandemic. On the one hand, Downs and Xu (2015) found that compared to securitized mortgages, portfolio-held mortgages were less likely to be restructured and more likely to be foreclosed during the GFC. On the other hand, portfolio lenders may have a higher recovery rate than securitized loans. Furthermore, such a lender may have a comparative advantage when renegotiating debt on the type of property collateralizing mortgages held in portfolio (Black, Krainer, and Nichols 2017).

[11] We aggregate the mean value of 6,413 nonresidential commercial properties from NCREIF as of 2019:IV to the county level by property type. Similarly, we aggregate the estimated market value of more than 15,000 nonresidential commercial properties from Trepp as of December 2019 to the county level by property type.

PPP loan records that provide complete address information. We focus on the first draw of PPP loans approved by August 8, 2020, and restrict CMBS mortgages to those with a single asset as collateral. We identify 24,015 PPP loan records that report the same address as properties in the CMBS sample. The match rate reflects the fact that many PPP recipients may operate in a commercial property that does not have mortgage debt or that is leveraged by a non-CMBS mortgage.[12] To avoid confusion, we refer to this dataset as the PPP-CMBS matched sample. Panel B of Table 1 reports the descriptive statistics for this sample.

Table A.3, panel A, in the online Appendix compares the PPP loan characteristics by whether the PPP loans were matched to CMBS mortgages. The matched PPP loans have an average size of \$184,208, which is more than that of the unmatched PPP loans. The average number of jobs reported by the PPP borrowers in the matched sample is about 19, or about 6 more jobs than reported by the PPP borrowers in the unmatched sample. However, Cohen's d-statistic indicates that the mean differences in PPP loan size and jobs reported between the matched and unmatched PPP loans are less than 20% of their pooled standard deviations, implying that the differences are small and not economically meaningful despite being statistically significant according to two-way t-tests.[13] For a subset of PPP loans, the SBA reports the allocation of loan proceeds (i.e., payroll, rent, or other expenses). The PPP loan recipients in the matched (unmatched) sample report using 16 percent (15 percent) of the funds to pay rent, which suggests that the allocation of program funds to support rent payments is consistent across participants. We use this estimate in Section VII to better understand the efficacy of the PPP funds used for rent.

We observe that approximately 31.6 percent of the properties in our sample have at least one tenant who obtained a PPP loan. Table A.4 in the online Appendix compares the mean differences for key underwriting factors between CMBS mortgages that were matched to at least one PPP loan and CMBS mortgages that were not matched to a PPP loan. Although the differences for several characteristics are statistically significant, the reported d-statistics indicate that the mean differences are small and not economically meaningful. Still, the misclassification of CMBS mortgages might not be random and may introduce bias into our analysis in an unpredictable way. We address this concern in the next section.

III. Empirical Method

Our empirical framework relies on the established income shock trigger theory of mortgage default (Vandell 1984). Under this theory, default may occur when NOI (defined as rental income (Y) less operating expenses (E)) falls below the mortgage's debt service (DS), which is a function of the loan's interest rate and loan value. Consistent with the program's rules, we further assume that PPP funds (denoted as PPP) may have offset pandemic-induced income shocks. As a result, the indirect

[12] To better understand the overlap between the PPP data and CMBS data, in Figure A.3 in the online Appendix we plot the counties that have at least one PPP loan that is matched to a CMBS mortgage using borrower and property addresses. The county-level coverage is constrained to locations where we observe at least one CMBS property.

[13] Cohen's d-statistic is the mean difference divided by the pooled standard deviation. A d-statistic with an absolute magnitude of 0.2 or less is considered small and one of 0.8 or more is considered large.

effect of the PPP on property owners comes from the potential for small business tenants who qualify for PPP funds to use a portion of those funds to pay rent. Hence, during the pandemic, default on mortgage payments occurs when

$$\Pr(Default = 1) = \Pr(Y + PPP - E < DS)$$

or

$$\Pr(Default = 1) = \Pr(Y/DS + PPP/DS - E/DS < 1),$$

which implies that $\partial \Pr(Default)/\partial(PPP/DS) < 0$; as PPP funds per dollar of debt service increase, the default likelihood of the subject property decreases. Hence, we hypothesize that the introduction of the PPP had an effect on properties that is commensurate with treatment intensity, which is the PPP to debt service (PPP/DS) ratio.

We compute the PPP treatment intensity as the aggregated dollar amount of all PPP loans matched to a given property divided by the mortgage debt service. The PPP loan dollar amount is measured as the PPP borrower's initial amount approved by the SBA. The debt service is measured as the scheduled monthly principal and interest payments in January 2019 multiplied by 12. If the PPP/DS ratio exceeds one, then the amount of PPP funds available at a property could cover an entire year of mortgage payments if used entirely for rent. Figure 3 shows the distribution of the PPP/DS ratio by mortgage loan-size quintile (within each property type) in panel A and collateral property type in panel B. We observe that the average PPP/DS ratio (captured by the red dots) decreases as the mortgage-size quintile increases. We also find that the average PPP/DS ratio is greatest among mortgages collateralized by office and industrial properties. However, the PPP/DS ratio varies widely across the various forms of grouping.

To isolate the effects of the PPP tenant channel along the intensive margin on commercial mortgage defaults, we estimate the following TWFE model:

$$Y_{i,t} = \delta_0 D_{i,t}^{l \geq 0} + \delta_1 \left(D_{i,t}^{l \geq 0} \times PPP/DS_i\right) + \tau_{s,t} + \kappa_i + \varepsilon_{i,t}, \tag{1}$$

where $Y_{i,t}$ is an indicator of whether mortgage i is 30+ days behind on payments at time t and $\varepsilon_{i,t}$ is an error term. $D_{i,t}^{l \geq 0}$ takes a value of one if at least one tenant in the property underlying mortgage i obtains a PPP loan before time t and zero otherwise. The l superscript stands for the relative time in months from treatment. By construction, $D_{i,t}^{l \geq 0}$ varies across mortgages and over time with a lag reflecting that mortgages are paid in arrears and that a treatment that falls between two mortgage pay periods should affect the latter. Put differently, the event-time dummies are defined by the date relative to the loan-specific treatment date. Since mortgage payments are due in arrears at the first of the next month, we assume that treatment occurs on the month following exposure to the PPP. For example, if the property of mortgage i is matched to a PPP loan in April 2020, then treatment occurs in May because the April mortgage payment is due May 1. Hence, for this mortgage, $D_{i,t}^{l \geq 0}$ is one in May and thereafter (where $l \geq 0$) and zero for earlier months (where $l < 0$).

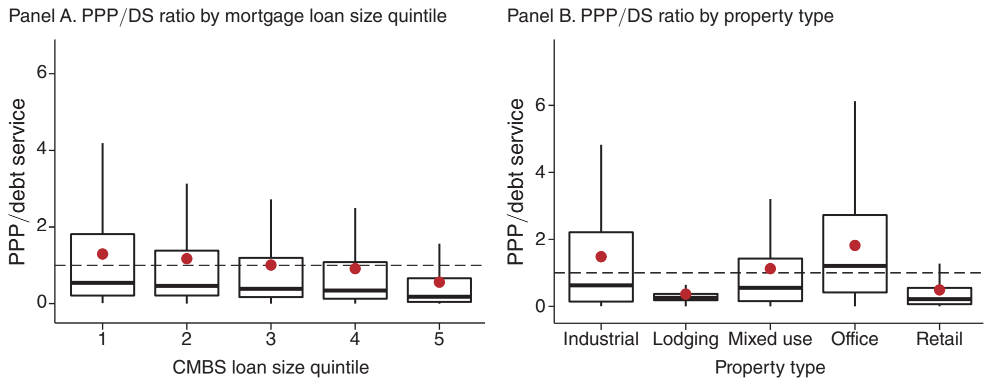

Figure 3. Descriptive Statistics of PPP Treatment Intensity

Notes: This figure illustrates the variation in the total PPP dollar amount to debt service ratio across the PPP-CMBS matched sample by loan-size quintile (panel A) and property type (panel B). *PPP/DS* is constructed as the ratio of total 2020 PPP funds in dollars to scheduled annual mortgage debt services (using January 2019 scheduled debt services multiplied by 12). The total PPP dollar amount for a CMBS mortgage is determined by calculating the sum of the total PPP funding to all PPP borrowers who report the same address as the underlying collateral property. The red dots represent the mean, and the box plots represent the twenty-fifth, fiftieth, and seventy-fifth percentiles of the *PPP/DS* ratio. The dashed line marks one.

About 84 percent of the mortgages are first treated in May 2020, 13 percent in June 2020, 2 percent in July 2020, and 1 percent in August or September 2020. PPP/DS_i is the static continuous treatment variable. δ_0 and δ_1 are the unknown parameters to be estimated. Whereas δ_0 represents the average effect of the PPP on mortgages with a strictly positive *PPP/DS* ratio close to zero, δ_1 is the ATR of default from a marginal change in the *PPP/DS* ratio among mortgages exposed to PPP funds.

We include state-year-month fixed effects $(\tau_{s,t})$ and loan fixed effects (κ_i) to account for time- and loan-specific heterogeneity, respectively. The state-time fixed effects rule out common trends from the pandemic or economic environment (e.g., eviction moratoria) that may influence mortgage performance before and after introduction of the PPP in a way that may be unique to specific states. The loan fixed effects control for local tendencies or regulations in a specific location (e.g., legal deterrents for missing mortgage payments) as well as time-invariant mortgage heterogeneity (e.g., size at securitization; mortgage-specific credit enhancements; and borrower, lender, underwriter, or investor characteristics that may play a role in mortgage default decisions). In particular, the loan fixed effects allow us to exploit the within-loan variation to identify the PPP effect on mortgage performance.

Causal analysis in the continuous treatment TWFE framework relies on the strong parallel trends assumption, which in our context requires that the average evolution of delinquencies across all mortgages from the pre- to posttreatment period, if the mortgages had been assigned another treatment intensity level during the pandemic, be the same as mortgages that experienced that treatment intensity level (see Callaway, Goodman-Bacon, and Sant'Anna 2021). We relax this assumption implicitly by including state-specific time fixed effects in equation (1) such that the parallel trends assumption only needs to hold conditionally on the evolution of state-specific time trends. We also explore settings with time-varying covariates, such as the local unemployment rate or COVID-19 rate, to further condition when the parallel trends assumption may hold but at the cost of assuming that those controls are unaffected by the PPP. Moreover, since the parallel trends assumption only needs to hold among mortgages with varying treatment intensities (Callaway, Goodman-Bacon, and Sant'Anna 2021), we focus on using the PPP-CMBS matched sample to estimate our models.

Another recent concern in the difference-in-difference literature is that "negative weights" may occur in a setting with staggered treatment. The potential for negative weights poses a threat to the standard assumptions for causal inference because they may conflate estimators in TWFE models if treatment effects are heterogeneous with respect to time or other dimensions (e.g., de Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Borusyak, Jaravel, and Spiess 2024; Sun and Abraham, 2021). For example, the program rules changed the forgivable amount of PPP loans that could be used for nonpayroll expenses from 25 percent in April to 40 percent in June 2020, which may have influenced treatment timing for when a firm used PPP funds to pay rent. In addition, policymakers revised the eligibility criteria when many critics of the program pointed out that the initial distribution of PPP funds did not reach the hardest-hit businesses or locations (Bartik et al. 2020b, 2020d; Chetty et al. 2020; Granja et al. 2022; Humphries, Neilson, and Ulyssea 2020). To investigate this concern, we explore heterogeneous treatment effects in multiple ways.

First, we test for time-varying effects using a dynamic TWFE model. To do so, we modify equation (1) to include a fully saturated series of interaction terms that capture the ATR for each period before and after treatment:

$$Y_{i,t} = \sum_{l \in L} \left[\delta_0^l D_{i,t}^l + \delta_1^l \left(D_{i,t}^l \times PPP/DS_i \right) \right] + \tau_{s,t} + \kappa_i + \varepsilon_{i,t}, \tag{2}$$

where $D_{i,t}^l$ takes a value of one for loan i at time l months from treatment such that $l \in L \equiv \{\leq -5\} \cup \{-4, -3, -2, 0, 1, 2, \ldots, 7\}$. The treatment date for loan i occurs at time t_i^*. Hence, $D_{i,t}^l$ is zero whenever $l \neq t - t_i^*$. Following standard practice (e.g., Freyaldenhoven et al. 2021), we leave out the indicator for the month immediately preceding the treatment date $(t - t_i^* = -1)$. For example, the reference pretreatment period where $t = t_i^* - 1$ is April 2020 for mortgages that are treated in May 2020. The model combines the event-time dummies for months five or more before treatment to address issues of multicollinearity (Sun and Abraham 2021). By interacting the relative event-time indicators with the PPP/DS ratio, we obtain estimates of the treatment response dynamics across time. For simultaneous multiple hypotheses testing of the event-time path, we calculate the sup-t critical value following Montiel Olea and Plagborg-Møller (2019) as suggested by Freyaldenhoven et al. (2021).[14]

[14] List, Shaikh, and Xu (2019) stress the importance of controlling for the probability of a false rejection when simultaneously testing the null hypothesis of many parameters in a model. The sup-t critical value accounts for the increased likelihood of falsely rejecting a null hypothesis of no effect when including many parameters in a model.

Second, we employ the IW estimator from Sun and Abraham (2021) to address concerns that cohort-specific trends could contaminate the leads and lags of a dynamic TWFE model. To do so, we identify CMBS cohorts by the timing of first treatment and estimate the following model:

$$(3)\quad Y_{i,t} = \sum_{l \in L} \sum_{c \in C} \mathbf{1}\{C_i = c\} \cdot \left[\delta_0^{l,c} D_{i,t}^{l,c} + \delta_1^{l,c}\left(D_{i,t}^{l,c} \times PPP/DS_i\right)\right] + \tau_{s,t} + \kappa_i + \varepsilon_{i,t},$$

where $\mathbf{1}\{C_i = c\}$ is an indicator function for whether loan i belongs to cohort $c \in C \equiv \{\text{May 2020, June 2020, July 2020}\}$. We consider mortgages first treated in May 2020 as cohort one, June 2020 as cohort two, July 2020 as cohort three, August 2020 as cohort four, and September 2020 as cohort five. Since all mortgages in our sample are treated, we limit the sample time horizon to July 2020, which shortens the set of relative event-time indicators ($L \equiv \{\leq -5\} \cup \{-4, -3, -2, 0, 1, 2\}$) and thus mechanically excludes the posttreatment indicators for the last treated cohorts (cohorts four and five) to avoid contamination from Goodman-Bacon's (2021) "forbidden comparison." As a result, equation (3) produces an $L \times C$ matrix of time-varying cohort-specific ATRs ($\hat{\delta}_1^{l,c}$). We recover the IW estimator of the ATR at relative time l as

$$(4)\quad ATR^l = \sum_{c \in C} w^{l,c} \cdot \hat{\delta}_1^{l,c},$$

where $w^{l,c}$ is the weight of cohort c at event time l. Following Sun and Abraham (2021), we determine the weight for each $\hat{\delta}_1^{l,c}$ as the sample share of each cohort in C in the relative time period.[15] We bootstrap the procedure 1,000 times with replacement to obtain standard errors for appropriate statistical inference and infer the sup-t critical value from equation (3).[16]

Third, to account for possible violations of the parallel trends assumption along other dimensions beyond treatment timing, we recompute variations of the Sun and Abraham (2021) estimator using cohort-specific profiles. For instance, if landlords with larger mortgages systematically have greater difficulty avoiding delinquency from a pandemic-related revenue shock than landlords with a smaller mortgage, the counterfactual evolution of the mortgage delinquency rate could vary depending on debt service, complicating the interpretation of ATR estimates. Moreover, the economic and social effects of the PPP could have varied with many other factors, such as density (urban versus rural locations) or political orientation. Hence, we partition the cohorts C in equation (3) by property

[15] We retrieve the weights by iteratively regressing the event-time indicators in equation (3) on cohort indicators as follows:

$$\mathbf{1}\{C_i = c\} = \sum_{l \in L} w^{l,c} \cdot D_{i,t}^{l,c},$$

using the same sample used to estimate equation (3) but excluding observations that correspond to the base period $t = t_i^* - 1$ and observations that correspond to the "not yet treated" group. This ensures that the weights across the cohorts sum to one for each relative time l.

[16] Section B in the online Appendix describes the bootstrapping procedure.

type or property-type specific loan size to recover more precise weighted averages of the ATRs. To reduce the dimension of comparisons, we exclude observations from loans that were first treated in June 2020 or July 2020. Alternatively, we use county-level characteristics to profile cohorts, including the population density reported in the US Census's 2014–2018 American Community Survey five-year estimates (US Census Bureau 2022), the share of voters who voted Republican during the 2016 US presidential election (MIT Election Data and Science Lab 2018), and a measure of exposure to bank performance in PPP lending constructed by Granja et al. (2022).

We construct a single parameter to compare the results across the various model specifications. Specifically, we compute

$$\overline{ATR} = \frac{1}{N} \cdot \sum_{l \geq 0} ATR^{l}, \tag{5}$$

where N is the number of posttreatment ATR estimators. Likewise, we summarize the pre-trend ATR effects. These simple procedures are proposed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). However, we provide bootstrapped-based t-statistics for statistical inference when applicable.

IV. Results: PPP Impact on Mortgage Delinquencies

To better understand the influence of PPP funds on mortgage default, we visually inspect the loan performance across PPP/DS ratio quintiles (within each property type) in panel B of Figure 2 using the PPP-CMBS matched sample. We observe that prior to the implementation of the program, the loan performance as measured by the PPP/DS ratio across the various quintiles is parallel. Furthermore, we observe that the delinquencies increase to a lower level for mortgages with a high treatment intensity (i.e., those in higher PPP/DS quintiles) than for those with a lower treatment intensity. These figures are consistent with the hypothesis that mortgages with a high treatment intensity were shielded from the distress during the pandemic much more than mortgages with a low treatment intensity.

A. *Standard TWFE Estimates*

In column 1 of Table 2, we report the OLS coefficient estimate of the interaction term $D_{i,t}^{l \geq 0} \times PPP/DS_i$ from equation (1) using the PPP-CMBS matched sample. This sample and specification allow us to test the PPP treatment intensity based on a direct link between a given PPP loan and mortgaged property. The estimated coefficient $(\hat{\delta}_1)$ is -0.01 and is statistically significant at the 1 percent level, suggesting that the default likelihood is much lower for mortgages with a high treatment intensity relative to those with a low treatment intensity. For example, the default likelihood of mortgages with a PPP/DS ratio at the ninety-fifth percentile is 5.59 percentage points $(= -0.010 \times (5.611 - 0.017))$ lower than that of mortgages at the fifth percentile during the time following initial exposure to PPP funds. The ATR indicates a decline in the mortgage default likelihood of 1.27 percentage points

TABLE 2—AVERAGE TREATMENT RESPONSE OF THE 30+ DAYS DELINQUENCY LIKELIHOOD ON PPP INTENSITY

Event time ($\hat{\delta}_1^l$)	Single (1)	Dynamic (2)	Time (3)	Type (4)	Size (5)	Density (6)	Politics (7)	PPPE (8)
$l = -5+$	·	0.001	−0.004	0.002	0.001	0.001	0.001	0.001
	·	(2.418)	(−1.106)	(2.107)	(2.578)	(2.615)	(2.584)	(1.961)
−4	·	0.001	−0.003	0.001	0.001	0.001	0.000	0.000
	·	(1.685)	(−1.004)	(1.171)	(1.297)	(1.657)	(1.473)	(0.963)
−3	·	0.000	−0.004	0.000	0.000	0.000	−0.000	−0.000
	·	(0.412)	(−1.148)	(0.549)	(0.131)	(0.080)	(−0.056)	(−0.454)
−2	·	0.000	−0.004	0.001	0.000	0.000	0.0000	0.000
	·	(0.987)	(−1.368)	(1.646)	(0.691)	(1.302)	(0.962)	(0.452)
0	·	−0.006	−0.011	−0.0120	−0.008	−0.006	−0.006	−0.006
	·	(−7.837)	(−3.415)	(−3.103)	(−5.029)	(−6.739)	(−6.751)	(−6.953)
1	·	−0.012	−0.010	−0.018	−0.018	−0.012	−0.012	−0.012
	·	(−10.210)	(−7.780)	(−3.820)	(−8.407)	(−8.809)	(−8.729)	(−9.122)
2	·	−0.011	−0.011	−0.0120	−0.016	−0.012	−0.011	−0.011
	·	(−10.813)	(−10.486)	(−2.553)	(−8.004)	(−9.047)	(−9.042)	(−9.867)
3	·	−0.010	·	·	·	·	·	·
	·	(−10.628)	·	·	·	·	·	·
4	·	−0.009	·	·	·	·	·	·
	·	(−9.780)	·	·	·	·	·	·
5	·	−0.010	·	·	·	·	·	·
	·	(−9.592)	·	·	·	·	·	·
6	·	−0.009	·	·	·	·	·	·
	·	(−9.778)	·	·	·	·	·	·
7	·	−0.009	·	·	·	·	·	·
	·	(−9.308)	·	·	·	·	·	·
$\overline{ATR}$	−0.010	−0.010	−0.011	−0.014	−0.014	−0.010	−0.010	−0.010
	(11.594)	·	(−7.455)	(−3.972)	(−7.855)	(−9.403)	(−9.279)	(−9.919)
Pre-trends	·	0.000	−0.004	0.001	0.001	0.000	0.000	0.000
	·	·	(−1.171)	(1.723)	(1.358)	(1.721)	(1.560)	(0.911)
Sup-*t*	·	2.844	2.975	3.247	3.211	3.159	3.150	3.179

Notes: This table displays estimates of the ATR of the 30+ days delinquency likelihood to a marginal change in the PPP/debt service ratio at various event times relative to the first month before the treatment date (i.e., $l = -1$, where l indicates the number of months before or after treatment). More specifically, column 1 reports an OLS estimate of $\hat{\delta}_1$ (or $\overline{ATR}$) from the TWFE model (equation (1)) and column (2) reports OLS estimates of the $\hat{\delta}_1^l$ (or ATR^l) parameters from the dynamic TWFE model (equation (2)) using PPP-CMBS matched mortgage performance records from January 2019 to December 2020. Columns 3–8 report ATR^l estimates from equation (4) using different cohort classifications and PPP-CMBS matched mortgage performance records from January 2019 to July 2020. Cohorts are defined by treatment timing in column 3, property type in column 4, loan-size quintiles in column 5, county population density quintiles in column 6, county Republican voter-share quintiles in column 7, and county PPP bank performance (PPPE) quintiles in column 8. Columns 4–8 exclude mortgages that were treated in June 2020 and July 2020. The t-statistics are in parentheses when available and calculated using robust standard errors clustered by loan in columns 1 and 2; they are calculated using bootstrapped standard errors in columns 3–8. The sup-t statistic is the 95 percent critical value for simultaneous multiple hypotheses testing. $\overline{ATR}$ is the arithmetic average of the posttreatment effects (see equation (5)), while pre-trends is the arithmetic average of the pretreatment ATR effects.

($= -0.010 \times 1.269$) at the mean level of treatment intensity. The ATR at the mean intensity is the relative difference in the posttreatment mortgage delinquency rate between mortgages with an average treatment intensity—measured by PPP/DS—and mortgages with a treatment intensity that is close to zero (i.e., $PPP/DS_i \approx 0$). Hence, as we hypothesize, an inverse relationship appears to exist between the amount of PPP funds distributed to tenants and a landlord's incentive to default on mortgage debt.

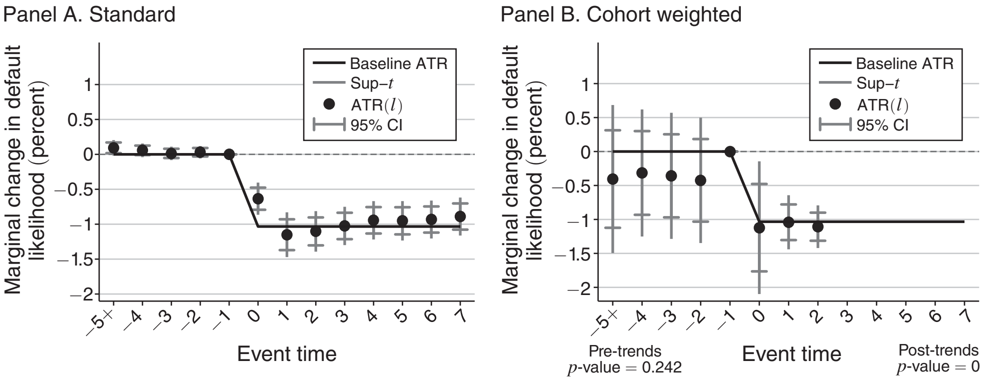

Figure 4. Average Treatment Response

Notes: This figure displays the event path of the ATR of the 30+ days delinquency rate on the *PPP*/*DS* ratio at each time period before or after treatment relative to the pre-trend period ($l = -1$). The baseline OLS estimate of the ATR from equation (1) is denoted in bold black. The 95 percent confidence intervals (CI) are in brackets. The outer lines of the 95 percent confidence intervals confine the uniform sup-*t* confidence band. Panel A reports statistics from the dynamic TWFE model in equation (2), whereas panel B reports statistics from the cohort-weighted TWFE model in equation (4).

B. *Dynamic TWFE Estimates*

Column 2 of Table 2 reports the coefficient estimate of the δ_1^l parameter for each relative event time (l) in equation (2) using the PPP-CMBS matched sample. Each value represents the average response of the 30+ days mortgage delinquency likelihood to the PPP treatment intensity at l months before or after treatment compared to the mortgage performance in the pretreatment month ($l = -1$). We report *t*-statistics based on standard errors clustered by loan in parentheses and the 95 percent sup-*t* critical value at the bottom of the column. In panel A of Figure 4, we plot the event-time point estimates from equation (2) with a vertical line through each point denoting the sup-*t* significance range with horizontal marks denoting the standard 95 percent statistical confidence interval. As a reference, the figure also overlays the point estimate of the ATR from equation (1) as a black line in bold.

As a falsification test of the parallel trends assumption, we examine whether the *PPP*/*DS* ratio negatively affects the 30+ days delinquency likelihood during the period before the pre-trend date. The first two pre-trend $\hat{\delta}_1^l$ parameter estimates (where $l = -2$ and $l = -3$) are virtually zero and statistically insignificant at the traditional levels. The ATRs for $l = -4$ and $l = -5+$ are positive but close to zero and have sup-*t* confidence bounds that cross the *y*-axis at zero, indicating that we cannot reject the null hypothesis that they are different from zero. Overall, considering the entire event-time path, the falsification test suggests that the mortgage default rate among loans with varying degrees of PPP treatment intensity did not systematically decrease prior to the landlords' exposure to the PPP funds linked to their tenants.[17]

In contrast, we observe that the posttreatment estimates of the ATR where $l \geq 0$ are negative, are statistically significant at the conventional 1 percent level, and have sup-t confidence intervals that do not include zero. For instance, the ATR at $l = 0$ is about 0.6 percent. A Wald test of coefficient equality indicates that the ATR at $l = 0$ is statistically different from the ATR at $l = -2$, with an F-statistic of 60.53 and a p-value of 0.000, showing clear evidence of a discontinuity around the treatment date. However, the policy's effect did not fully materialize until one month after initial treatment, which coincides with the date the SBA distributed additional PPP funding for most (85 percent) of the mortgages in the sample. During the first month after initial treatment, the likelihood of delinquency for a mortgage exposed to PPP funds was attenuated by about 1.52 percentage points $(= 1.2\% \times 1.269)$ at the average PPP/DS ratio. Indeed, the Wald test indicates that the ATR at $l = 0$ is statistically different from the ATR at $l = 1$ (with an F-statistic of 34.7 and a p-value of 0.000). Thereafter, the average response of the 30+ days delinquency likelihood to PPP funds slightly diminished. A Wald test of the difference between the ATR at $l = 1$ and $l = 7$ produces an F-statistic of 8.5 and a p-value of 0.004.

C. *Cohort-Weighted Dynamic TWFE Estimates*

Although the post-trend ATRs using the dynamic TWFE model precisely average to the baseline estimate of -1% in column 2, we find evidence of time-varying effects that may conflate both pre-trend and post-trend ATR estimates. Furthermore, mortgage delinquency across PPP treatment intensity levels could evolve differently over time depending on initial treatment timing, violating the parallel trends assumption. Thus, we turn to the IW estimator from Sun and Abraham (2021) to calibrate ATR estimates for staggered treatment adoption. Column 3 of Table 2 reports calculations of the event-time ATR^l estimates based on equation (4) using OLS estimates of equation (3). We report bootstrapped t-statistics in parentheses and a corresponding sup-t critical value of 2.975. We find that the effect of the PPP/DS ratio on the delinquency rate at each posttreatment time is similar to prior estimates. The mean ATR is -1.1 percent and has a bootstrapped t-statistic that is higher than the sup-t critical value (with a p-value of 0.000), whereas the average pre-trend effects are statistically indistinguishable from zero (with a p-value of 0.242), providing comfort to our assumptions.

[17] The power test proposed by Roth (2022) indicates that under a linear pre-trend of 0.02 percent, there is an 80 percent chance that our pretest would detect a significant pre-trend, which could in turn bias the magnitude of the posttreatment ATR estimates by up to 18 percent. Although the sup-t confidence intervals capture this potential bias and the level of the pretreatment ATR estimates are not economically meaningful, we explore other confidence intervals that incorporate assumptions about violations to the parallel trends assumption. Following Rambachan and Roth (2023), we compute confidence intervals that allow for a linear trend that is $\bar{M}$ times the maximum linear trend observed from the pre-trend ATR coefficients, which is approximately 0.04 percent. Figure A.5 in the online Appendix shows the hypothetical confidence intervals for ATR at $l = 0$ (panel A) and ATR at $l = 1$ (panel B) across different $\bar{M}$. For both panels, we observe that the violation could be twice the maximum observed difference in the pre-trend estimates and remain statistically significant.

Panel B of Figure 4 plots the event-time ATR^l estimates from the IW estimator along with bootstrapped 95 percent confidence intervals and sup-*t* confidence bands. We find that the event-time path extrapolated from the baseline ATR (equation (1)) falls within the bands of the entire event-time path implied by the IW estimator. We stress that the purpose of the IW estimator is to reduce concerns about "negative weights" in the presence of heterogeneous treatment effects from staggered treatment adoption, and we note that the findings remain consistent with the baseline estimates. One caveat is that this finding pertains to capturing the initial effects of the program since the IW estimation approach requires that we limit the sample to observations through July 2020. However, the majority of PPP funds were distributed during this period, which attenuates this concern.

D. *Heterogeneous Weighting*

To formally recognize the heterogeneous PPP responses across various dimensions, we calibrate the ATR estimates by property type, loan size, population density, political orientation, and bank performance in PPP lending. To do so, first we redefine the cohort definition for mortgages treated in May 2020. Second, we estimate equation (3) with the redefined cohort definition using CMBS mortgage records from January 2019 to July 2020 for mortgages that were treated in May, August, or September. As before, the latter two groups serve as the "not-yet-treated" baseline. Finally, we calculate the IW estimator of the ATR from equation (4) for each event time using the $(\hat{\delta}_1^{l,c})$ parameter estimates from the previous step and the sample share of each group in C in the relative time period.

Property Type.—We examine the effect across property types as the pandemic affected businesses in various industries differently. Chetty et al. (2020), for example, note that consumer spending on in-person services decreased by 65.1 percent from January to April 2020, while spending on remote services (e.g., utilities, education, and finance and insurance) fell by only 15 percent. As evident in Figure 2, we also see greater change in the nationwide delinquency rate across property types that rely on in-person versus remote services. Hence, the average response to treatment intensity may vary over time with the income shock experienced by different industries during the pandemic.

In column 4 of Table 2, we report the event-time path of the ATR of mortgages weighted by the property type of the underlying collateral. Our sample contains five property type groups such that $C \equiv$ (Industrial, Lodging, Mixed Use, Office, and Retail) in equations (3) and (4). Panel A of Figure 5 illustrates the corresponding results. As before, we report bootstrapped *t*-statistics and the sup-*t* critical value. We also report the average ATR across the posttreatment event times at the bottom of column 4 in Table 2. The average effect of the PPP treatment intensity on the likelihood of mortgage delinquency is negative and statistically significant at the sup-*t* critical level. In fact, the magnitude of the average posttreatment ATR is stronger than the prior estimates. In contrast, the average ATR across the pretreatment event times is virtually zero and not statistically significant at the 95 percent sup-*t* critical level. Hence, we again find compelling

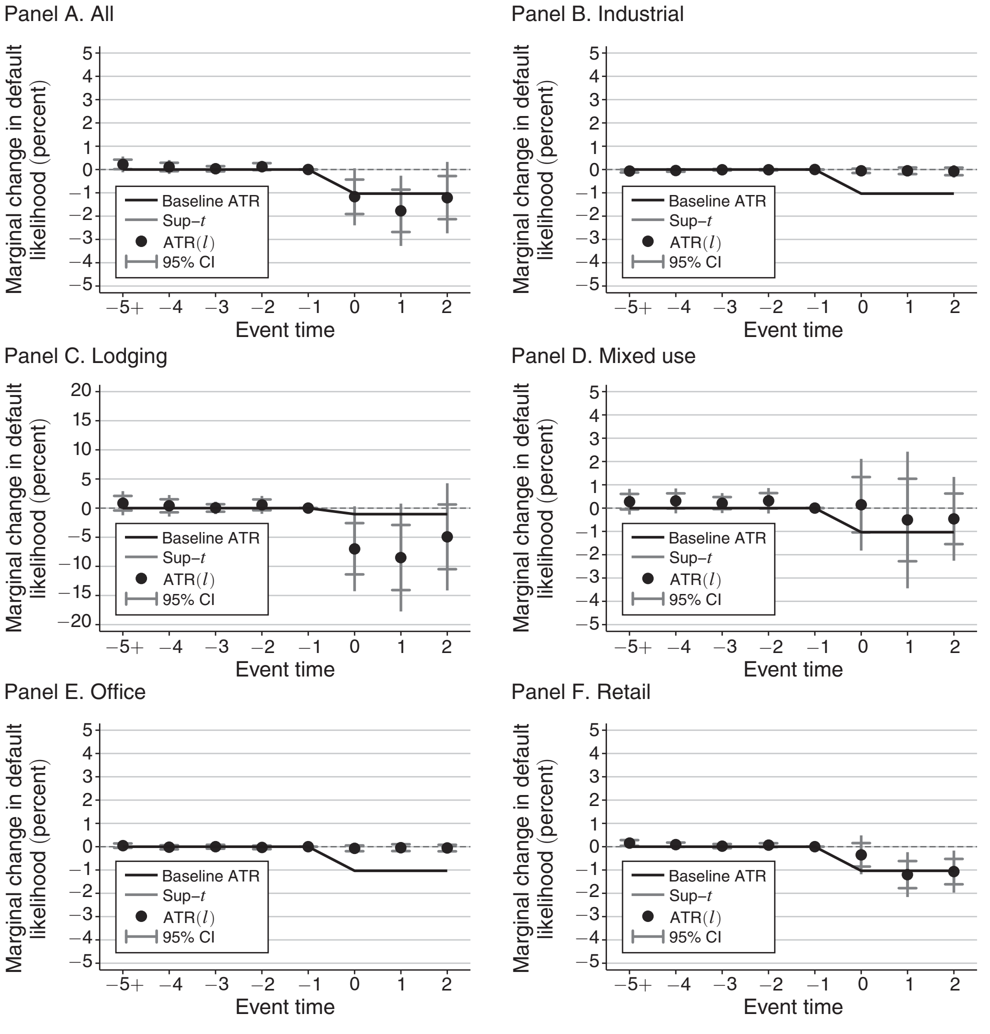

Figure 5. Average Treatment Response by Property Type

Notes: Panel A displays the event path of the property type weighted ATR of the 30+ days delinquency rate on the PPP/DS ratio at each time period before or after treatment relative to the pre-trend period ($l = -1$) for the cohort of mortgages first treated in May from equation (4). Panels B–F report the ATR statistics ($\delta_1^{l,c}$) from equation (3) for each mortgage collateral property type. In each panel, the baseline OLS estimate of the ATR from equation (1) is denoted in bold black. The 95 percent confidence intervals are in brackets. The outer lines of the 95 percent confidence intervals confine the uniform sup-t confidence band.

evidence that the treatment intensity is negatively associated with the likelihood of default.

Panels B–F report the event-time path components ($\delta_1^{l,c}$) from equation (3) for each property type. The highest treatment response is observed for mortgages on lodging properties, followed by those on retail and mixed-use properties. In the lodging industry, mortgages with the average PPP/DS of 0.361 experienced a delinquency rate that is about 2.5 percentage points ($= 0.0698 \times 0.361$) lower than that of lodging mortgages with virtually no exposure to PPP funds during the first month following exposure to the program's funds.[18] In contrast, the delinquency rates of mortgages for industrial and office properties do not vary with the PPP/DS ratio even though the average treatment intensity for industrial and office mortgages is large. Thus, Figure 5 confirms that the sensitivity of a landlord's incentive to default during the pandemic depends not only on the amount of PPP funds that their tenants receive but also on whether the landlord's property caters to businesses that rely on face-to-face interactions (i.e., businesses in the hospitality, tourism, and entertainment sectors).

Overall, the results confirm that the indirect exposure to tenants with PPP loans helped the landlord weather the economic uncertainty during the pandemic. Furthermore, the results show that greater exposure to PPP funds reduces financial distress for property owners, even if the PPP exposure is indirect. Finally, our findings imply that providing additional PPP support to lodging or retail tenants as opposed to those in the industrial or office sector could have further reduced commercial mortgage delinquencies. This suggests that positive spillover effects from the PPP depend on the PPP's targeting success.

Loan Size.—Another threat to identification of the ATR is that the counterfactual mortgage performance could vary depending on loan size. In addition, the mortgage size could correlate with the payroll of underlying tenants and in turn affect treatment intensity. To calibrate the ATR estimates by loan size, we group the PPP-CMBS matched sample as of January 2019 into five static-size quintiles (within each property type) and redefine cohorts in the IW estimation procedure such that $C \equiv \{Q1, Q2, Q3, Q4, Q5\}$ where Q1 stands for the smallest and Q5 stands for the largest mortgages. Column 5 of Table 2 reports the event-time path of the ATR of mortgages weighted by the loan-size quintiles, while panel A of Figure 6 illustrates the results. Panels B to F of Figure 6 report the results by loan-size quintile. We again see clear evidence of a discontinuity in the event-time path of the average effect of the PPP intensity on mortgage delinquency in each loan-size quintile. We observe that the marginal effects are greater for mortgages in the upper quintile of their respective property type. Furthermore, the ATRs, which are now calibrated for heterogeneous effects from differences in the time path of mortgages varying in size, remain consistent with prior baseline estimates.

Population Density and Political Orientation.—Alternatively, we use county-level quintiles of the population density and political orientation to define cohorts.[19] These variables provide various measures that proxy for urban areas. Columns 6 and 7 display the overall IW ATR statistics for the density and political orientation cohort framework, respectively. Figures 7 and 8 report the

[18] A Wald test suggests that the posttreatment ATRs at $l = 0$, 1, and 2 are jointly significant with an F-statistic of 4.64 and a p-value of 0.003. Steiner and Tchistyi (2021) similarly find that airport hotels with exposure to PPP funds were better able to weather lost revenue than competing hotels with no exposure to PPP funds were.

[19] At the county-level we construct quintiles based on the population density or the share of voters that voted Republican and then assign mortgages to the corresponding quintile based on the location of the underlying collateral.

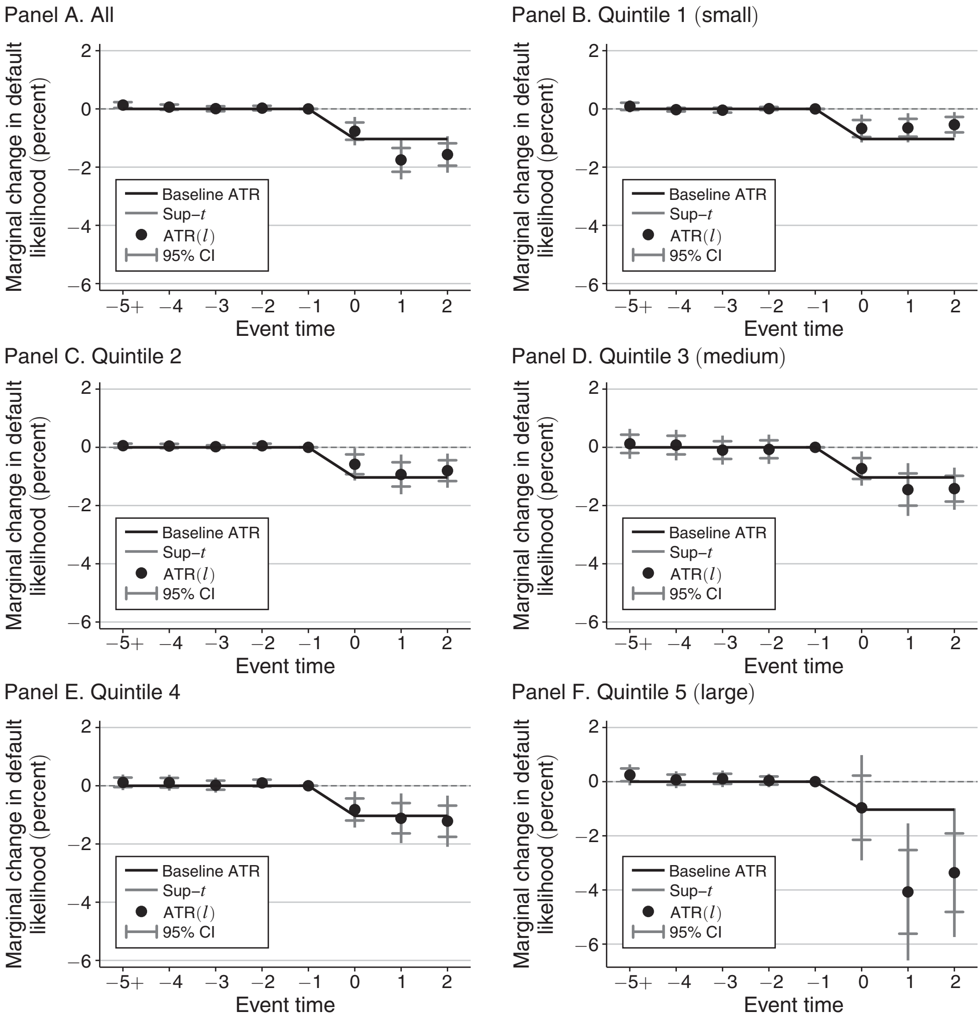

FIGURE 6. AVERAGE TREATMENT RESPONSE BY LOAN-SIZE QUINTILE

Notes: Panel A displays the event path of the loan-size quintile weighted ATR of the 30+ days delinquency rate on the PPP/DS ratio at each time period before or after treatment relative to the pre-trend period ($l = -1$) for the cohort of mortgages first treated in May from equation (4). Panels B–F report the ATR statistics ($\delta_1^{l,c}$) from equation (3) for each mortgage loan-size quintile where Q1 denotes the smallest loans within each property type. In each panel, the baseline OLS estimate of the ATR from equation (1) is denoted in bold black. The 95 percent confidence intervals are in brackets. The outer lines of the 95 percent confidence intervals confine the uniform sup-t confidence band.

heterogeneity-robust results and their $\delta_1^{l,c}$ components. The falsification tests of the pre-trend periods indicate that the ATR effect did not materialize before treatment regardless of whether the property is located in high-density, low-density, Democratic, Republican, or politically mixed areas. We also observe significant ATR effects posttreatment upon weighing the effects across geographic cohorts, whether defined by population density or political orientation. Although the results are consistent with our prior estimates, we observe that the confidence intervals

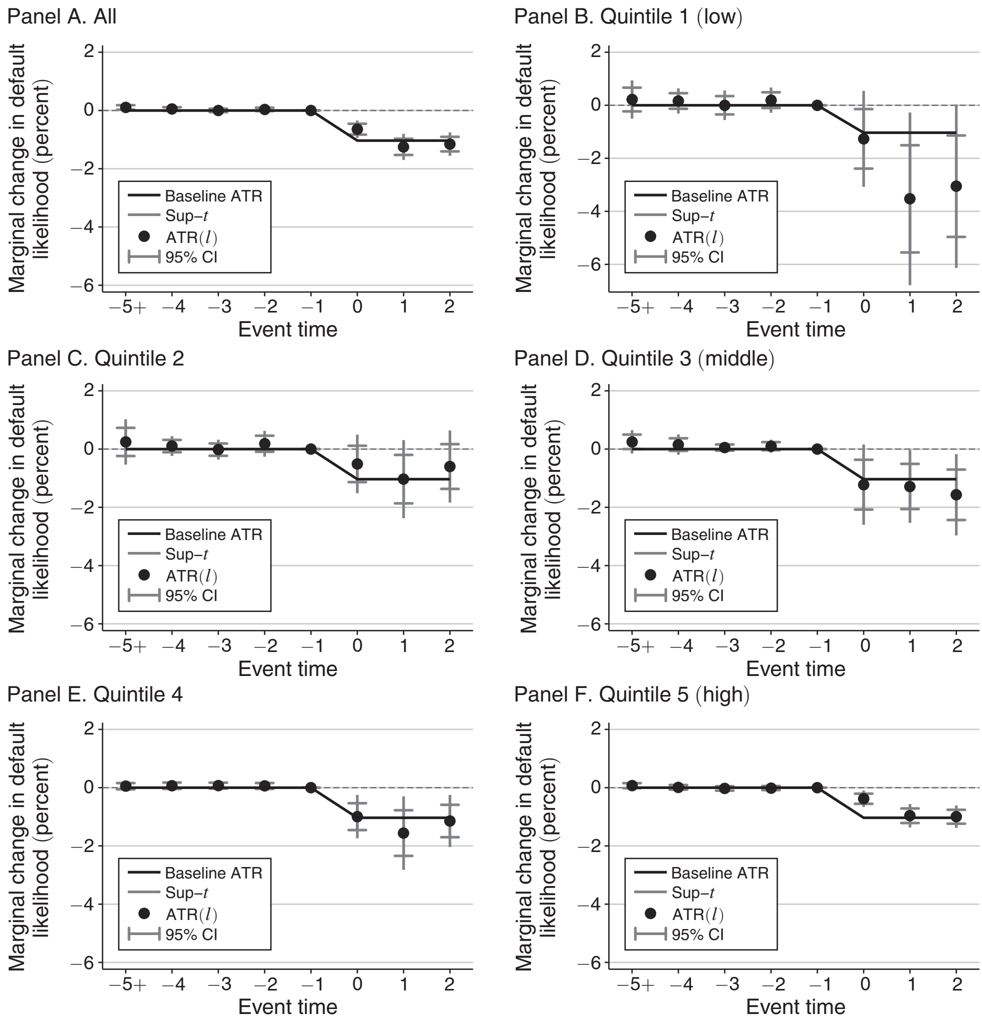

Figure 7. Average Treatment Response by Population Density Quintile

Notes: Panel A displays the event path of the population density quintile weighted ATR of the 30+ days delinquency rate on the PPP/DS ratio at each time period before or after treatment relative to the pre-trend period $(l = -1)$ for the cohort of mortgages first treated in May from equation (4). Panels B–F report the ATR statistics $(\delta_1^{l,c})$ from equation (3) for each population density quintile where Q1 denotes the least dense counties. In each panel, the baseline OLS estimate of the ATR from equation (1) is denoted in bold black. The 95 percent confidence intervals are in brackets. The outer lines of the 95 percent confidence intervals confine the uniform sup-t confidence band.

on the ATR estimates are tighter in urban locations, characterized as having high density and being politically Democratic leaning, than in rural locations. The findings suggest that the PPP intensity effects are driven mostly by urban locations.

PPP Bank Performance.—In this section, we test whether mortgage delinquencies evolve differently across geographic cohorts depending on the PPP lending activity of local banks. If bank lending relationships play a role in whether a local

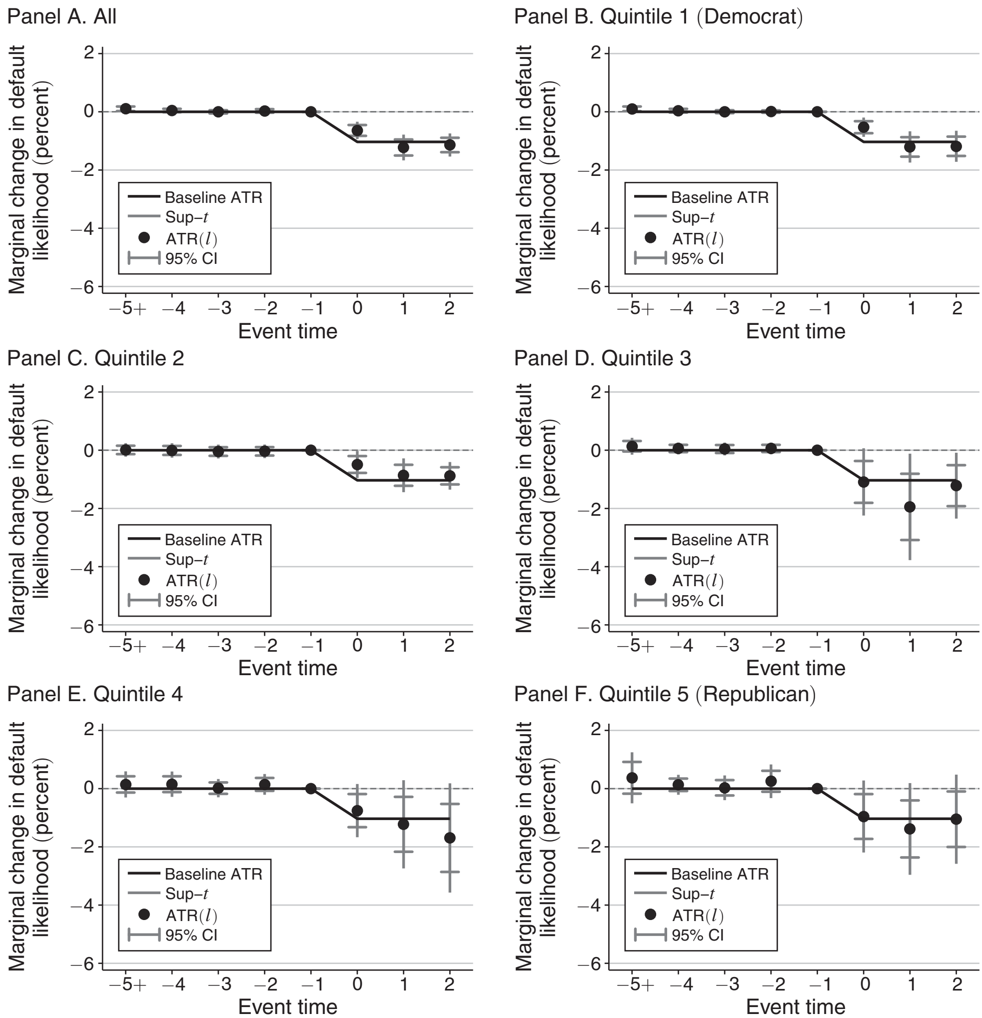

Figure 8. Average Treatment Response by Republican-Share Quintile

Notes: Panel A displays the event path of the Republican voter-share quintile weighted ATR of the 30+ days delinquency rate on the PPP/DS ratio at each time period before or after treatment relative to the pre-trend period ($l = -1$) for the cohort of mortgages first treated in May from equation (4). Panels B–F report the ATR statistics ($\delta_1^{l,c}$) from equation (3) for each Republican voter-share quintile where Q1 denotes the counties with the lowest Republican voter shares. In each panel, the baseline OLS estimate of the ATR from equation (1) is denoted in bold black. The 95 percent confidence intervals are in brackets. The outer lines of the 95 percent confidence intervals confine the uniform sup-t confidence band.

small business obtained a PPP loan, the distribution of PPP loan originations may be skewed, which could influence the indirect exposure of mortgages to the program. We use a county-level measure of exposure to bank performance in PPP lending constructed by Granja et al. (2022) to examine possible geographic variation in treatment intensity or treatment effects. This measure is a Bartik-type instrument based on the national share of PPP loans issued relative to the national share of other small business lending activity. Specifically, Granja et al. (2022) measure the performance of each bank in a given location as:

$$PPPE_b = \frac{1}{2} \times \frac{PPP\%_b - SBL\%_b}{PPP\%_b + SBL\%_b},$$

where $PPP\%_b$ is the national share of PPP loans originated by bank b during the first round (April 3–April 16) and $SBL\%_b$ is the national share of that same bank's non-PPP loans extended to small businesses as of 2019:IV. The exposure of a CMBS mortgage to bank performance in PPP lending in a given location ($PPPE_c$) is measured as the average $PPPE_b$ in county c weighted by each bank's respective share of deposits in the same county:

$$PPPE_c = \sum_b \left(\omega_b \times PPPE_b\right),$$

where ω_b is the weight of bank b in county c.[20] A high value for $PPPE_c$ implies that the chance of a local borrower being approved for a PPP loan is high, whereas a low $PPPE_c$ level implies the opposite.

We redefine cohorts in the IW estimation procedure to reflect geography quintiles of the $PPPE_c$ measure such that $C \equiv \{Q1, Q2, Q3, Q4, Q5\}$ where Q1 stands for the lowest $PPPE_c$ quintile and Q5 stands for the highest $PPPE_c$ quintile at the county level. The results are reported in column 8 of Table 2 and panel A of Figure 9. Panels B–F illustrate the event-time path the $\delta_1^{l,c}$ parameter estimates from the $PPPE_c$ quintile-interacted cohort regression. We observe a higher treatment response for mortgages in locations that had greater exposure to PPP bank performance during the first round. Illustrating the impact of PPP treatment intensity across PPP bank performance levels, the difference in the ATR in the top $PPPE_c$ quintile is about 90 percent greater than counties in the bottom $PPPE_c$ quintile. The relationship appears to be nonlinear. In counties in the fourth quintile (panel E), the ATR at treatment is about -2 percent, whereas it is about -0.4 percent in counties in the first quintile (panel B). In other words, the mortgage delinquency rate is attenuated more effectively in counties where local small businesses were more likely to obtain PPP financing during the first round than in counties with the opposite scenario. The differences are less obvious as time passes and as Congress allocated additional funds to the PPP. This result is consistent with findings by Granja et al. (2022) that the share of businesses reporting that they missed a scheduled payment is low in locations where $PPPE_c$ is high. Although these findings provide evidence of heterogeneous treatment effects, the ATR weighted by $PPPE_c$ is identical to the baseline ATR estimate.

V. Robustness Checks

In the online Appendix, we examine alternative specifications using different layers of controls to understand the identifying variation and how static and dynamic

[20] The average $PPPE_c$ is 0.19 and ranges from -0.5 to 0.5 with a standard deviation of 0.169 at the county level.

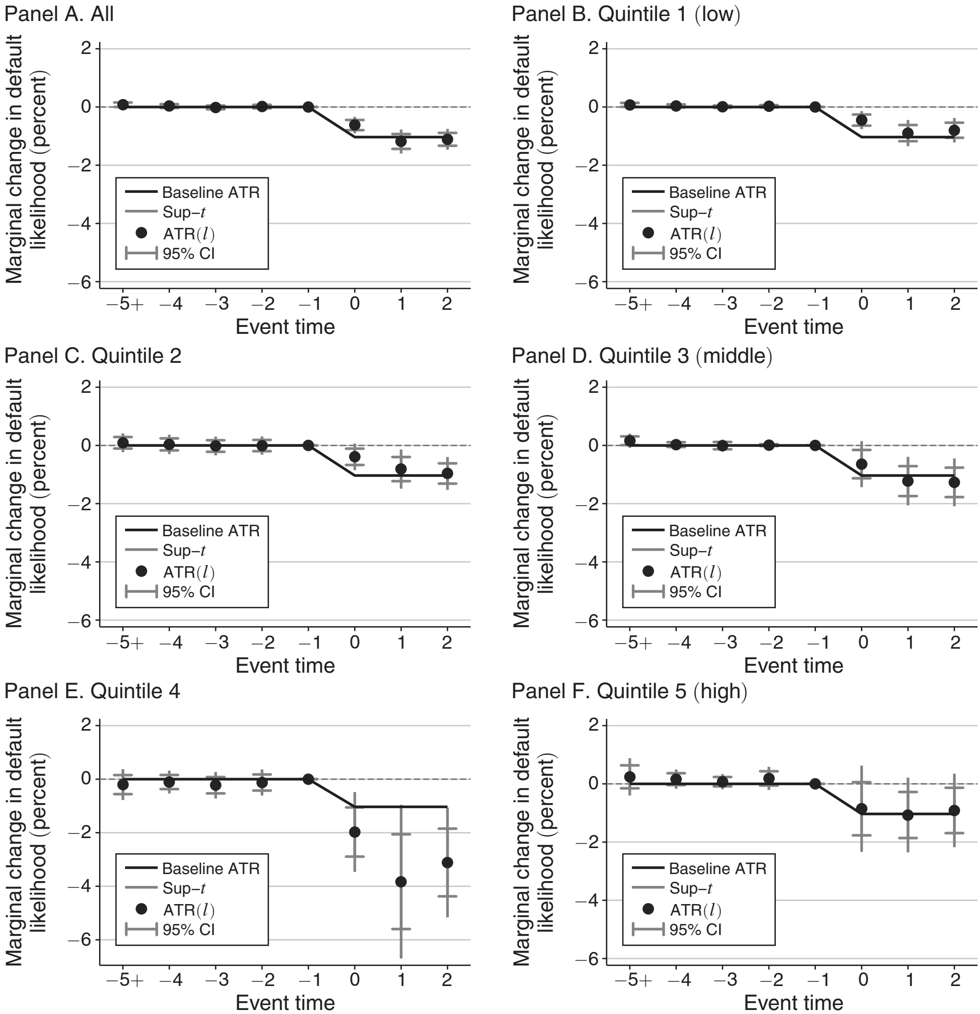

FIGURE 9. AVERAGE TREATMENT RESPONSE BY PPPE QUINTILE

Notes: Panel A displays the event path of the PPPE quintile weighted ATR of the 30+ days delinquency rate on the PPP/DS ratio at each time period before or after treatment relative to the pre-trend period ($l = -1$) for the cohort of mortgages first treated in May from equation (4). Panels B–F report the ATR statistics ($\delta_1^{l,c}$) from equation (3) for each PPPE quintile where Q1 denotes the counties with the lowest PPPE levels. In each panel, the baseline OLS estimate of the ATR from equation (1) is denoted in bold black. The 95 percent confidence intervals are in brackets. The outer lines of the 95 percent confidence intervals confine the uniform sup-t confidence band.

estimates of the ATR differ as we control for sources of variation.[21] The inclusion of additional control variables relaxes the parallel trends assumption since it only needs to hold conditionally on the model specification. However, a tradeoff is that time-varying characteristics, like the current LTV or local delinquency rate, could be affected by

[21] Section C in the online Appendix provides a detailed description of the control variables.

treatment and bias the point estimates of the ATR effects (see Angrist and Pischke 2008; Borusyak, Jaravel, and Spiess 2024; Callaway and Santa'Anna 2021).

In online Appendix Table A.5, we observe that the relation between mortgage default and treatment intensity is generally negative at a similar magnitude of the baseline estimates and statistically significant at the 1 percent level even as the amount of variation explained by the model increases with the additional regressors. Columns 1–3 of online Appendix Table A.6 confirm that the ATR of mortgage delinquency to the PPP/DS ratio is significant in the posttreatment periods and not the pre-trend periods at the sup-t critical value, regardless of whether we condition the model on nationwide, state, or county-level time trends. In column 4 of online Appendix Table A.6, we limit the sample to mortgage records from January 2020 to July 2020 for the dynamic TWFE model and find similar results, which provides further confirmation of a causal connection under a weaker parallel trends assumption.

We also consider alternative measures of mortgage default (online Appendix Table A.7) and a more precise measure of the posttreatment date that uses the first mortgage payment date as the proxy day for when mortgage payments are due to account for mortgage payment grace periods (online Appendix Table A.8). We note that the results are consistent with the baseline findings. The findings are robust to the functional form too. For instance, we test our main specification using a Cox proportional hazard model and arrive at similar results (online Appendix Table A.9). Moreover, we find that the magnitude of the average PPP treatment intensity effect appears to be greater when modeled nonlinearly (e.g., using a quadratic or cubic function) rather than linearly, suggesting that our main results are conservative (see Table A.10 and Figure A.6 in the online Appendix).

Callaway, Goodman-Bacon, and Sant'Anna (2021) raise the question of whether selection bias affects estimates of the treatment effect in a TWFE model with a continuous treatment variable unless a stronger form of the standard parallel trends assumption holds. However, if the counterfactual evolution of the mortgage performance varies across mortgages assigned to different loan-size quintiles or PPP/DS ratios in the absence of the PPP policy, then the point estimates for the average response to treatment intensity could be biased in a nonobvious direction (Callaway, Goodman-Bacon, and Sant'Anna 2021). To assuage this concern, we interact the county-level COVID-19 rate with static PPP/DS deciles in a continuous treatment specification to control for variation in loan performance throughout the COVID-19 pandemic. The interactions control for the possibility that the spread of COVID-19 affected mortgages of various PPP/DS levels differently. In the online Appendix, Table A.11 reports the results for this specification with and without control variables. The estimates of the average PPP treatment intensity effects on the mortgage delinquency likelihood are similar to prior estimates. Hence, our results are robust to the criticism that in the absence of the PPP policy, the pandemic may have had a differential effect on mortgages with different PPP treatment intensity levels.

Another concern related to sample selection bias is that a landlord could have had direct exposure to PPP funds. Although landlords generally were ineligible to obtain PPP funds because they are categorized as passive investors rather than small businesses, it is possible that a commercial property owner obtained a PPP loan directly from the SBA and used the funds to make debt service payments. To ensure that estimates of PPP treatment intensity result from landlords' indirect exposure to the program, we exclude the mortgages that were matched to a PPP loan whose borrower has the two-digit real estate sector North American Industry Classification System (NAICS) code and reestimate the continuous treatment model (equation (1)). This analysis supports the implicit assumption that PPP treatment intensity is exogenous because the landlord has no control over whether tenants apply for PPP funds. Our conclusions about the relation between mortgage defaults and treatment intensity remain unchanged (see Table A.12 in the online Appendix).[22]

VI. External Validity

The key channel through which the PPP affects mortgage default is the substitution of an income stream that would allow a tenant to continue making rental payments and bolster the property's NOI. Our results imply that the NOI fell during the pandemic, but it did not fall by as much for commercial properties exposed to the PPP as it did for properties that were not linked to PPP funds. Thus, to provide external validity to the previous analysis and to identify the initial impact of the program on the revenue of commercial properties, we focus on the quarterly operating performance of more than 8,000 office, industrial, retail, and lodging properties from 2019:I to 2020:IV using property-level data from the NCREIF. Online Appendix Table A.13 provides summary statistics on the NCREIF commercial properties by property type.[23]

We examine the correlation between the quarterly change in NOI and the bank lending activity of PPP loans using the following model:

$$NOI_{i,t} = \delta_1\left(D_t^{l\geq 0} \times PPPE_i\right) + \mathbf{X}_{i,t}\beta + \tau_t + \rho_i + \varepsilon_{i,t}, \tag{6}$$

where $NOI_{i,t}$ is the NOI/SQFT for property i at time t, $D_t^{l\geq 0}$ is one if the time is on or after 2020:II and zero otherwise, $PPPE_i$ is the county-level PPP exposure variable for property i, and $\varepsilon_{i,t}$ is an error term. $\mathbf{X}_{i,t}$ is a matrix of controls that includes the capital expenditures per square foot (CAPEX/SQFT) of property i, the county unemployment rate as of the end of the first month of each quarter, and the county COVID-19 rate (defined as the number of cases over the past 30 days since the first day of the quarter divided by the county population). We include year-quarter fixed effects (τ_t) and property-specific fixed effects (ρ_i) to rule out common trends and idiosyncratic elements that could affect the NOI. We estimate equation (6) by property type subsamples.

[22] We observe 22 instances in which the PPP borrower name is the same as the CMBS borrower name; excluding these observations does not materially affect our results.

[23] We exclude observations with missing variables (2.8 percent of the NCREIF properties) and winsorize the NOI per square foot (NOI/SQFT) at the 1 percent tails.

Table 3—NOI Analysis

Dep. var.: NOI/SQFT Property type	Retail (1)	Lodging (2)	Industrial (3)	Office (4)
$D^{l \geq 0} \times PPPE$	1.26	3.63	0.00	0.52
	(1.77)	(1.20)	(0.05)	(1.18)
CAPEX/SQFT	−0.00	−0.08	−0.00	−0.01
	(−0.50)	(−5.72)	(−0.80)	(−3.65)
Unemployment rate (%)	−0.08	0.09	0.00	−0.01
	(−2.48)	(1.26)	(1.40)	(−0.58)
COVID-19 rate (%)	0.13	−0.63	0.02	−0.15
	(0.77)	(−1.32)	(0.70)	(−1.87)
Observations	9,896	554	29,877	11,879
Adjusted R^2	0.76	0.60	0.84	0.81
Constant	✓	✓	✓	✓
Year × quarter FE	✓	✓	✓	✓
Property FE	✓	✓	✓	✓

Notes: This table reports OLS estimates of equation (6) of the NOI/SQFT on PPPE. The sample in each column consists of quarterly data on the nonresidential commercial properties from 2019:I to 2020:IV that were held by the NCREIF and classified as retail, lodging, industrial, or office. $D^{l \geq 0}$ is one if the time is on or after 2020:II and zero otherwise. The unemployment rate and COVID-19 rate are at the county level. *t*-statistics based on robust standard errors clustered by property are reported in parentheses.

Table 3 shows that the interaction term for retail properties is positive and statistically significant at the 10 percent level. The average quarterly NOI for retail is approximately \$0.52 per square foot (or about \$122,000) more in locations where PPP lending activity is high (i.e., nintieth percentile) compared to where it is low (i.e., tenth percentile) from 2020:II to 2020:IV.[24] The results provide evidence that the program helped small businesses that rely on face-to-face interactions pay their rent during the pandemic, which is the mechanism through which the PPP may have led to a reduction in mortgage delinquencies. For example, \$122,000 in additional funds spilling over from the PPP loans issued to retail tenants would provide a landlord with enough cash flow to completely cover the average debt service of \$70,000 for a retail property for slightly under two months (see Table 1) and thus help the landlord avoid entering the 30+ days delinquency status. The effect is also positive and economically large but statistically insignificant for lodging properties. The economic effect of the PPPE for lodging is $\$214{,}754\ (= \$3.63 \times [0.38 - (-0.03)] \times 144{,}295$ square feet), which is more than twice the average lodging monthly debt service of \$92,071. The lack of statistical significance could be due to a low number of observations in the lodging subsample. In contrast, the PPP lending activity appears to have no economically meaningful or statistically significant effects among industrial and office type properties, which is consistent with our main findings.

[24] $\$0.52 = \$1.26 \times [0.38 - (-0.03)]$; $\$122{,}000 \approx 0.52 \times 234{,}420$ square feet.

VII. Discussion and Policy Implications

In Section IV we find that following the distribution of the PPP loan funds, the delinquency rate is 1.27 percentage points lower for a PPP-CMBS matched mortgage with the average PPP/DS ratio of 1.269 than for a mortgage with zero PPP exposure.[25] The 95 percent confidence interval of the average degradation in the delinquency rate is 1.05 percentage points to 1.48 percentage points. Thus, assuming that the PPP-CMBS sample is representative of the overall \$2.83 trillion commercial real estate debt market, as discussed in Section II, the confidence interval implies that the PPP averted between \$29.7 billion and \$41.9 billion in potential pandemic-related mortgage defaults.

To understand the cost effectiveness of preventing an average of about \$36 billion in mortgage defaults, we estimate the amount of PPP funds used to pay rent to landlords using information from the loans in the PPP-CMBS sample.[26] We note that of the \$525 billion in 2020 PPP funds that were distributed, approximately 15 percent (or \$78.8 billion) was used to pay rent according to data from the SBA that details the uses of PPP loans. We further note that 25 percent of commercial real estate properties in the US market have outstanding mortgages (see Ghent, Torous, and Valkanov 2019). Thus, approximately \$19.7 billion in PPP funds were used to pay rent to leveraged landlords, which in turn averted \$36 billion in commercial real estate mortgage defaults.

As a benchmark, we track the aggregate realized losses of industrial, lodging, mixed-use, office, and retail commercial loans in private CMBS deals that were liquidated from 2001 to 2020 (see online Appendix Figure A.7). For example, in the aftermath of the GFC, the loans liquidated in 2013 resulted in a realized loss of \$5.32 billion. To put this in perspective, we note that in 2019:I, CMBS accounted for about 12 percent of the \$2.83 trillion in US nonresidential commercial mortgage debt (see Table L.220, Board of Governors of the Federal Reserve System (2018:IV)). Additionally, Wong (2018) reports a loss rate of 50 percent for 9,272 commercial loans in private CMBS deals that were liquidated with losses from 2003 to 2012. Hence, the PPP averted about \$2.2 billion in CMBS losses, which is approximately 58.9 percent of the most severe annual conduit CMBS losses seen following the GFC.[27]

One consequence of substantial mortgage losses in the private CMBS market during the GFC is that the CMBS market shut down, suggesting a similar response during the pandemic "but for" the intervention of the PPP. As Figure 10 shows, the average number of CMBS deals issued during the pandemic dips slightly below that of the pre-pandemic/post-GFC period. In contrast, the average number of CMBS deals issued following the GFC from 2007 to 2009 dropped drastically relative to the pre-GFC period. Since private banks rely on the ability to liquidate large loans through the securitization process, our results call for future research on whether the

[25] $0.0127 = 0.01 \times 1.269$.

[26] $\$36 \text{ billion} = 0.0127 \times \$2{,}832 \text{ billion}$.

[27] Note that the CMBS market in 2013:I was approximately 1.45 times greater than in 2019:I and thus we scaled the 2019 loss estimate by this amount. $58.9\% = (50\% \times \$36 \text{ billion} \times 0.12 \times 1.45)/\$5.32 \text{ billion}$.

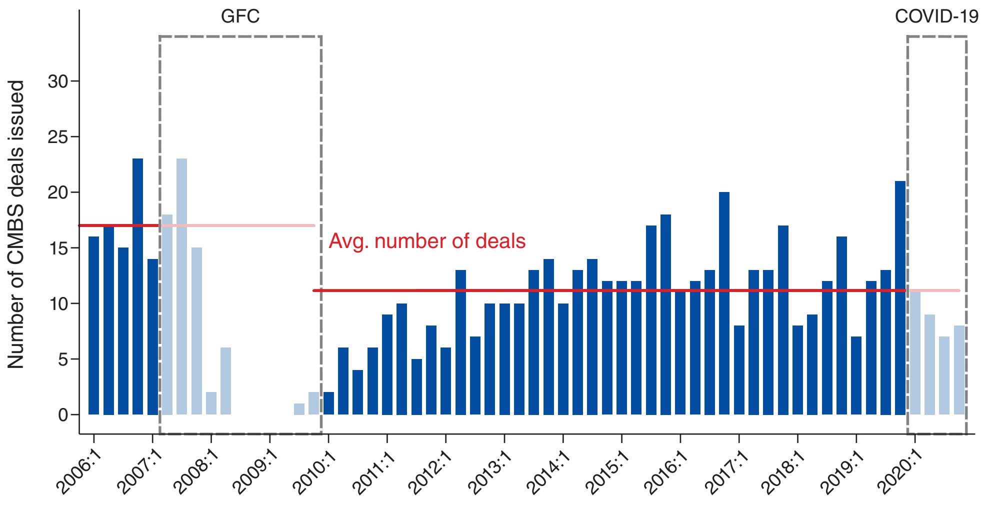

Figure 10. Private CMBS Deal Issuance from January 2006 to December 2020

Notes: This figure displays the CMBS deal issuance from January 2006 to December 2020, constructed using Trepp data. The number of CMBS deal issuance is counted for CMBS deals issued for conduit CMBS mortgages backed by commercial buildings in similar property types as those of our study sample in each quarter. The shaded area represents the GFC (2007:II–2009:IV) and COVID-19 periods (2020:I–2020:IV). The red lines illustrate the average quarterly deal number of CMBS deals issued prior to the GFC (2006:I–2007:I) and prior to COVID-19 (2010:I–2019:IV), respectively.

PPP curtailed financial contagion by reducing the incidence of commercial mortgage defaults.

VIII. Conclusion

In this paper, we provide new evidence on the effectiveness of policies designed to mitigate the impact of the COVID-19 pandemic on the US economy by focusing on the commercial real estate mortgage market. Using a continuous treatment intensity measure, we demonstrate that the federal government's PPP reduced delinquencies on securitized commercial mortgages. The results indicate that mortgages with greater exposure to PPP funds relative to their debt service payments had a lower likelihood of default during the pandemic than those with less exposure. These findings substantiate the argument that small businesses used relief from PPP loans to make rent payments, thereby allowing landlords to remain current on their mortgage payments. Thus, our analysis suggests that the PPP helped to ease economic distress beyond the labor market, ultimately providing support to commercial property owners via the funding of small business tenants.

From a policy perspective, our results imply that the PPP created financial stability in the securitized commercial mortgage market, which is a significant source of liquidity for the banking industry. Because landlords received rental payments from forgivable PPP loans issued to tenants, they were better able to weather income shocks and avoid defaulting on debt obligations. For example, the issuance of CMBS deals virtually shut down in 2009 during the GFC, straining the ability of banks to offload large commercial mortgages. By contrast, the CMBS market did not shut down in 2020 during the COVID-19 pandemic.

However, our analysis also points to room for improvement. For example, in examining the PPP's rollout, we find the PPP had a smaller impact on commercial mortgage delinquencies during the program's initial phase when PPP loans were not targeted at the areas or firms hardest hit by the pandemic. Furthermore, by exploiting geographic variation in exposure to banking relationships, we note that the impact of PPP funds was greatest in locations where funds from the first phase were distributed to businesses that were likely not the target recipients. Finally, we observe that PPP effects were absent in the office and industrial sectors where distress was low. As a result, our analysis reveals that PPP efficiency could have been enhanced by greater targeting of businesses in areas most affected by COVID-19 as well as those most impacted by government shutdown orders.

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