The Real and Social Effects of Online Lending*

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Abstract

Technology-driven innovations in credit markets have real and social implications. Using data from the largest online, peer-to-peer credit market in the United States, I find that borrowing increases when access to traditional credit is restricted, such as after bank mergers and natural disasters. Consequently, online borrowing moderates the diminished growth in business establishments and the heightened crime growth associated with credit scarcity. A percent increase in online lending offsets about 0.25% (0.22%) of the diminished establishment growth and 0.11% (0.18%) of the rise in crime associated with bank mergers (natural disasters). The effects are concentrated among small enterprises and property-related crimes.

Keywords: Banking, crime, entrepreneurship, natural disasters, peer-to-peer credit.

JEL classification: G21, G23, L26, O16.

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1 Introduction

Credit scarcity has negative implications for households and businesses. For instance, imperfections in lending channels impact economic growth by reducing households' consumption (Jensen and Johannesen 2017; Mian, Sufi, and Verner 2017) and curtailing entrepreneurship (Petersen and Rajan 1994; Fisman and Love 2003; Cagetti and De Nardi 2006). Restricting access to credit also diminishes social welfare by spurring crime (Garmaise and Moskowitz 2006). Traditionally, credit supply has been dominated by banks. However, credit markets have been evolving due to innovations in financial technology (FinTech). In particular, online borrowing through peer-to-peer (P2P) platforms is becoming increasingly important. By directly matching borrowers with individuals willing to supply funds, these markets are growing and, at the end of 2016, had originated about 30% of the $100 billion in personal loans in the United States (TransUnion 2017). Despite the increasing adoption of online credit, limited attention has been paid to its real economic and social impacts.

In this study, I investigate the real and social effects of online lending. To do so, I examine several core hypotheses. First, I conjecture that online credit may serve as an alternative source of finance when traditional credit becomes scarce. If online credit serves as a substitute, it may have real and social effects by moderating the negative impacts of credit scarcity. Specifically, I posit that online lending may support small business entrepreneurs, who are an important driver of economic growth.[1] In turn, online borrowing may also impact social welfare by abating the rise in crime growth that is associated with declining economic vitality (Grogger 1998; Gould, Weinberg, and Mustard 2002; Levitt 2001; Garmaise and Moskowitz 2006).

My conjectures are motivated by the observation that developments in the structure of credit markets are likely to impact entrepreneurship. For instance, personal resources (Hanspal 2016), credit cards (Chatterji and Seamans 2012), and bank loans (Berger and Udell 2003; Fracassi, Garmaise, Kogan, and Natividad 2013; Robb and Robinson 2014) are important financing resources for

[1]Since 1995, small businesses have created about 60% of the net new jobs in the United States and employ half of the country's private sector workforce (Haltiwanger, Jarmin, and Miranda 2013; Mills and McCarthy 2016a). Small business growth has also been a focus of policy discussions following the financial crisis. For example, the Federal Reserve held a forum to discuss credit access for small businesses at which Chairman Bernanke stated that: "Making credit accessible to sound small businesses is crucial to our economic recovery and so should be the front and center among our current policy challenges" (Bernanke 2010).

young and small enterprises. But, banks have been reducing lending to small businesses (Cole 2012; Chen, Hanson, and Stein 2017; Cole 2017). While government-sponsored credit programs may help address the declining volume and foster growth (e.g., Hackney (2017)), startup firms still struggle with overcoming weak financial markets (Fisman and Love 2003). Even with targeted lending programs, a gap in access to small dollar loans persists for entrepreneurs (Mills and McCarthy 2016a).[2] Since low principal loans are the primary product of online lenders, this alternative credit channel may have the potential to impact entrepreneurial activity by expanding access to finance.

I empirically test my hypotheses using several data sets. My primary data span from 2007 through 2015 and are from Lending Club, the largest P2P credit platform in the United States. Its micro-level data include detailed loan and credit profiles of its borrowers. The data also include borrowers' locations of residence at the three-digit zip code level.[3] I supplement the P2P data with information on real economic activity from the County Business Pattern (CBP) program conducted by the U.S. Census Bureau. The program maintains annual series of subnational economic data, including the number of business establishments operating in a community, as well as aggregate employment and payroll across establishments. I also rely on crime data from the National Incident Based Reporting System (NIBRS), a reporting system used by law enforcement agencies and under the jurisdiction of the Federal Bureau of Investigation (FBI), to assess the social welfare implications of online credit.

Examining whether individuals turn to online loans when traditional credit is scarce and isolating the subsequent real effects is empirically challenging. For instance, availability of online loans to a community is likely to be an endogenous decision affected by expectations of future economic growth and households' demands for credit. To address this issue, I use a two-step empirical process. First, I identify events which are likely to restrict access to conventional loans. Then, I use a matching and differencing procedure to estimate the economic and social effects of online credit following the reduction in conventional credit.

In the first stage of my empirical approach, I use the occurrence of bank mergers and natural disasters to identify communities which are likely experiencing reduced lending through conventional

[2]Anecdotal evidence further this supports the existence of this gap due to economies of scale in the banking industry. For example, small principal loans cost banks about the same to originate as larger value loans but provide less income.

[3]I, therefore, define communities at this level and aggregate other data to align it with this definition for my analyses.

channels. Specifically, I use non-failing bank mergers (i.e., those which do not require assistance from a regulatory agency or state) as my primary instrument for credit scarcity because a growing literature ties mergers to less competitive credit markets and access to finance.[4] For example, Jagtiani et al. (2018) show that mergers among community banks lead to local small business lending credit gaps that are not filled by the rest of the banking sector. In addition to mergers, I use natural disasters as a supplementary proxy since they are exogenous events that impact lending channels and individuals' credit decisions (Runyan 2006; Morse 2011).[5]

In the second stage of my empirical process, I address the confounding effects of endogenous availability of online credit. To do so, I first match communities with access to online loans to similar communities that do not have access to the market. The matched, non-P2P communities serve as counterfactuals for those which can borrow online. To implement the matching, I exploit the restriction of access to online credit in two states, Iowa and West Virginia. During my sample period, residents of these states could not borrow in the market due to regulatory restrictions. Then, for each community with access to online loans, I select a counterfactual community with similar socio-demographics.

Next, I use a difference-in-difference estimator to identify the real and social effects of online lending. In principle, a difference-in-difference estimation approach provides unbiased effect estimates if, in the absence of the treatment (i.e., access to online credit), the trend over time would have been the same between the treated and counterfactual communities. The matching process is used to address factors which may confound this parallel trend assumption. This combined matching and differencing procedure has been applied across a range of disciplines to identify causal effects, including political science (Liberini, Redoano, and Proto 2017), ecology (Andam, Ferraro, Pfaff, Sanchez-Azofeifa, and Robalino 2008), health policy (Stuart, Huskamp, Duckworth, Simmons, Song, Chernew, and Barry 2014), and financial economics (Morse 2011; Becker and Hvide 2017).

[4]For instance, see Jayaratne and Strahan (1996), Akhavein, Berger, and Humphrey (1997), Prager and Hannan (1998), and Rice and Strahan (2010), and Erel (2011). As a result, mergers have been used to study the effects of credit access on a range of socio-economic outcomes, including small business lending (Berger, Saunders, Scalise, and Udell 1998), loan contracts (Sapienza 2002), urban development, and crime (Garmaise and Moskowitz 2006).

[5]Natural disasters have been used as exogenous shocks in the financial economics literature to examine a variety of topics, including CEO decision-making (Bernile, Bhagwat, and Rau 2017), investors' risk preferences (Bernile, Bhagwat, Kecskes, and Nguyen 2017), and economic growth (Strobl 2011). Disasters have also been used to examine the real effects of another non-traditional credit product, payday loans (Morse 2011).

Consistent with the prior literature, I find that bank mergers reduce credit availability and have important real and social implications. Following a merger, origination of government-sponsored loans aimed at supporting entrepreneurship (e.g., Small Business Association (SBA) loans) declines, by about six percent ($1.69 million). Net growth in business establishments also falls. Specifically, the estimates suggest a decline of about 24 fewer net establishment births within a community in the year after a merger. While the typical community has about 8,479 establishments in operation each year, average net growth is moderate, with about 23 incremental businesses opening annually. The impact is likely to be substantial since reduced access to credit can both inhibit establishment births and potentially spur firm deaths. In line with declining enterprise growth, I also find diminished growth in business payrolls. Moreover, as with Garmaise and Moskowitz (2006), I document a positive relation between crime growth and diminished credit access, with about 660 incremental crimes occurring.

My findings also suggest that individuals turn to online loans when availability of traditional credit is reduced. Specifically, estimates from ordinary least squares (OLS) panel regressions indicate that loan volume increases by about 29.80%, or about $368,626, within a community that experiences a merger. This relation cannot be explained by heterogeneity with respect to communities' socio-demographic profiles or population sizes. Given the interactions between mergers and loan volumes, I examine the real and social implications of online credit.

I find that online lending has real economic and social effects on communities by moderating the impacts of credit scarcity. On the economic front, my estimates suggest that a one percent increase in borrowing offsets about 0.25% of the decline in net business establishment growth. Similarly, a one percent increase in borrowing offsets 0.13% of the decline in net payroll growth. The benefits of expanded access to credit through the peer-to-peer market are concentrated among the smallest enterprises, those with four or fewer employees.

Online credit also counteracts the appreciation in crime, with a one percent increase in loan volume mitigating about 0.11% of the heightened crime growth. The effects are primarily concentrated in property-related crimes, such as thefts and burglaries. The social welfare effects of online credit are not trivial since crime has a large cost to society. The criminology literature (e.g., McCollister, French, and Fang (2010)) suggests that each theft costs a community about $3,532 while a burglary costs about $6,462. Using these estimates, the rise in thefts associated with merger-induced credit scarcity has a cost to the community of approximately $448,564. The rise in burglaries costs a community about $1.85 million. However, a percent increase in online borrowing reduces the total cost of the new thefts (burglaries) to a community by about $358 ($1,665). The reduction in theft (burglary) costs is not trivial, representing about 1.33% (6.18%) of the typical individual's annual income.

Overall, the evidence suggests that online lending has real and social effects by moderating the impacts of credit scarcity following bank mergers. However, a contention could be that mergers among banks may not be random. I, therefore, use the occurrence of natural disasters as supplementary events that are likely to impact credit availability. Consistent with this hypothesis, I find that P2P loan volume rises within a community following a disaster while conventional credit conditions remain tight. Given this evidence, I then re-perform the matching process to align online borrowing communities and non-borrowing communities in the year prior to a disaster and conduct the difference-in-difference tests to examine the real and social implications in this alternative setting.

The estimates suggest that natural disasters hinder economic growth and spur crime. I also find that online lending moderates the aftermath. The magnitudes of the effects are similar to those identified when using bank mergers as scarcity-inducing events. Specifically, a one percent increase in online loan volume offsets about 0.22% of the decline in net establishment growth and 0.18% of the increase in crime growth in the year after a disaster. Overall, the collective evidence suggests that online lending has real and social effects.

To further examine the results, I conduct a series of robustness checks. First, a concern could be that expected crime increases may prompt present-day bank mergers and shape the community's economic vitality and individuals' borrowing decisions. An implication of this reverse causality conjecture is that, if trending crime impacts current decisions, future crime would be a reflection of the current crime trend. That is, current crime growth would spur contemporaneous mergers, online borrowing, and influence economic activity. I find that crime growth is positively correlated with crime growth in the next year. Given this correlation, I should find contemporaneous crime growth to be correlated with mergers, online borrowing, and economic outcomes under this alternative hypothesis. However, I find that crime is not significantly related to concurrent merger activity, P2P borrowing, or economic growth. Collectively, the evidence suggests that reverse causality is not likely to be driving my findings.

Second, it could be possible that online lending impacts the competitive landscape and drives consolidation among banks. However, I find that the probability of a merger is not affected by contemporaneous P2P loan volume or volume in the prior year. Another potential concern may be that the effects are sensitive to the time period used in the analysis. For instance, loan origination was impacted by variation in the regulatory environment in the first few years of the online credit platform. However, excluding data from the early years of the market does not materially impact the findings. Overall, the findings suggest that online credit ameliorates the economic and social effects of frictions in traditional lending channels.

This study contributes to several strands of the financial economics literature, including the literature focusing on technology and financial innovation (Petersen and Rajan 2002; Tufano 2003; Balyuk 2017; Hauswald and Marquez 2003; Butler, Cornaggia, and Gurun 2016; Campbell 2016). Peer-to-peer lending is a key innovation from the recent FinTech revolution. Early studies focused on factors influencing the demand for and supply of funds in the market, such as borrowers' creditworthiness (Iyer, Khwaja, Luttmer, and Shue 2015), trustworthiness (Duarte, Siegel, and Young 2012), and their local economic conditions (Ramcharan and Crowe 2013; Bazley 2017). Availability of traditional credit may also impact online borrowing. For example, recent evidence suggests that online lending is penetrating communities that would benefit from additional credit supply, such as those with concentrated banking markets (Ahmed, Beck, McDaniel, and Schropp 2015; Jagtiani and Lemieux 2017; Jagtiani and Lemieux 2018). Similarly, Tang (2018) suggests that online credit expands in response to a regulatory shock which impacted availability of bank credit. I complement the literature by providing novel evidence that online credit can be a substitute source of finance when traditional credit is scarce. By doing so, online lending has real economic and social effects.

My findings also relate to the literature which documents a positive link between banking, entrepreneurship (Black and Strahan 2002; Kerr and Nanda 2009; Cornaggia, Mao, Tian, and Wolfe 2015), and economic development (Jayaratne and Strahan 1996; Rice and Strahan 2010). Berger, Butler, Hu, and Zekhnini (2017) show that improving financial integration through bank deregulation spurs economic growth. My evidence suggest that online lending contributes to financial integration. By filling a missing rung in the credit ladder, this alternative borrowing channel moderates the effects of short-term but recurring credit market frictions.

More broadly, this study contributes to the literature on household finance (Guiso and Sodini 2013; Zinman 2014). A principal directive of this literature is to identify determinants of individuals' use of new financial products (Campbell 2006). I show that frictions in traditional lending channels influence the adoption of a new loan product. This study also expands the financial economics of crime literature (Becker 1968; Levitt 2001; Lochner 2004; Huck 2015). Crime is costly for households, regulatory agencies, and the economy. For instance, Brushwood, Dhaliwal, Fairhurst, and Serfling (2016) highlight the implications of crime on economic activity through its affects on firms' financing costs. Ultimately, my findings deepen the links between financial integration via expanded credit access, economic growth, and crime by providing novel evidence of the real and social effects of an emerging, technology-driven credit market.

2 Data

My core conjecture is that online credit may be used as an alternative source of finance when availability of traditional credit is reduced. Consequently, online borrowing may mitigate the negative impacts of credit scarcity, particularly diminished growth in business establishments and heightened crime growth. To test these hypotheses, I draw data from a variety of sources. I briefly describe the data sets and provide summary statistics for the key variables in this section.

2.1 Online Lending

Lending Club is the largest P2P credit platform in the United States. To apply for a loan, an individual specifies the amount of funds desired, the loan term (three or five years), the reason for borrowing, and other personal details. Lending Club verifies the information and subsequently assigns an interest rate to the loan based on a proprietary algorithm. Once the loan application is approved, it is listed on the online platform and becomes available for investors to fund.[6] Listed loans typically receive full funding.

Lending Club publicly releases its loan data. The data include borrowers' traditional credit statistics, such as FICO scores, incomes, and debt-to-income ratios, as well as their locations of residence (at the three-digit zip code level). Information on borrowers' loan details, such as amounts,

[6] Lending Club restricts applications to individuals with at least a 640 FICO score and a bank account. The second largest P2P lender, Prosper, relies on these same base requirements.

terms, and interest rates are also included. I use a sample of 197,294 individual loans with a total volume of approximately $3.1 billion issued between 2007 and year-end 2015. The sample of loans is distributed across thirty-two states and 316 three-digit zip code areas.[7] Figure 1 presents the distribution of my sample loans across states. Texas has the largest representation in my sample, with about 19% of the loans being issued to communities in the state.

To examine the real effects of online credit, I construct a three-digit zip code-level measure, *P2P Loan Volume*. Specifically, *P2P Loan Volume* is the natural log of the total loan volume within a community during the year. In Panel A of Table 1, I show that average yearly volume in a community is about $1.24 million and the average loan has an interest rate of about 12.84%. The typical borrower in my sample earns about $64,764, has a FICO credit score of about 707, and a debt-to-income ratio of 16.64.[8] Overall, my sample is consistent with other studies which find that online borrowers tend to be more leveraged than the typical American but have credit scores within the normal range (Morse 2015; Jagtiani and Lemieux 2018).

2.2 Credit Scarcity Events

I use two types of events that are likely to restrict availability of traditional credit. First, I draw from the banking literature (e.g., Prager and Hannan (1998), Berger, Demirgüç-Kunt, Levine, and Haubrich (2004), and Garmaise and Moskowitz (2006)) and use bank mergers as scarcity-inducing events. To implement the empirical tests, I obtain data on mergers from the Federal Reserve. I use mergers between non-failing banks, where one bank's FDIC certificate is surrendered, to identify communities that are likely to face reduced credit access. I define non-failing mergers as those which do not require assistance from the Federal Deposit Insurance Corporation, the National Credit Union Administration, state, or other regulatory agency. I select this inclusion criteria to minimize the possibility that endogenous factors, such as expectations of future neighborhood changes, drive any associations between mergers, online credit use, real economic activity, and crime. Based on these qualifications, I construct an indicator variable, *Bank Merger*, which takes a value of one if a merger occurs within a community during the year, and zero otherwise. Overall, I identify 387 credit scarcity events due to bank mergers during my sample period.

[7] I restrict my sample to communities where crime data are available. Overall, my sample of loans is about 20% of Lending Club's total loan volume during the period but is representative of the full data set.

[8] I include definitions of all variables in Appendix 1.

I also use the occurrence of natural disasters as alternative events that are likely to impact communities' credit availability. I, therefore, obtain community-level disaster data from the Spatial Hazard Events and Losses Database for the United States (SHELDUS). The data set covers natural hazards, such as hurricanes, floods, wildfires, and tornados, across the country and includes information on the dates of the events, affected communities, and direct losses caused by the disasters. Using this data, I identify a credit scarcity event if a disaster that creates at least $250,000 in damage occurs within a community. I employ this selection criteria to address concerns that some disasters may be concentrated in their geographic-breadth or may primarily impact communities through loss of life. Analogous to bank mergers, I construct an indicator variable, *Natural Disaster*, which takes a value of one if a natural disaster occurs within a community during the year, and zero otherwise. Overall, I identify 1,536 disasters which are likely to impact credit availability.

2.3 Small Business Association Loans

In order to examine the availability of conventional credit around mergers and natural disasters, I obtain data from the U.S. Small Business Administration (SBA) on loans provided to small businesses which are supported by the agency. The SBA was created through the Small Business Act of 1953 as an independent agency of the federal government to aid, counsel, and protect the interests of small business concerns. While the SBA does not directly provide credit to individuals, it sets guidelines for loans made by traditional lenders, known as lending partners. Loans that meet the guidelines qualify for SBA guarantees, which reduces the risk for the lending partner and, ultimately, expands access to capital.

Since the predominant lending partners of the SBA are traditional financial institutions, it is likely that mergers among banks and natural disasters will impact the availability of SBA credit. I, therefore, construct a measure, *SBA Loan Volume*, to capture the annual amount of SBA-backed loans originated within each community. To create the measure, I take the natural log of the aggregate loan volume from the SBA's two principal lending programs, the 7(a) and 504 programs.[9] In Panel B of Table 1, I find that about $28.20 million in SBA-backed loans are issued annually

[9] The 7(a) Loan Program is the SBA's primary and most popular program. It supports long-term, fixed rate loans which may be used for a variety of purposes, including funding startup costs, expanding business operations, and refinancing existing debts. The 504 Loan Program specializes in long-term, fixed-rate financing for entrepreneurs to acquire fixed assets and real estate.

within a typical community.

2.4 Business Establishments, Employment, and Payroll

To examine the economic impacts of online lending, I obtain data on real economic activity from the County Business Pattern (CBP) program conducted by the U.S. Census Bureau. This program maintains annual series of subnational economic data, including information on the number of establishments operating in a community, as well as establishments' numbers of employees and payroll expenses.

I aggregate the data to the three-digit zip code-level and develop several measures to examine the real effects of online credit. First, to assess business growth within each community, I create a variable, *Establishment Growth*, which is the yearly change in the total number of establishments. The statistics in Panel B of Table 1 indicate that net growth in business establishments is moderate, with about 23 additional establishments forming within a community during the typical year. Overall, the average community in my sample has about 8,479 establishments operating each year.

I also develop three supplementary measures to further examine the real effects of online credit across several dimensions. Specifically, I construct *Employment Growth*, which is the annual growth in the total number of employees across establishments within a community, to examine employment dynamics. I also assess growth in aggregate first quarter and annual payrolls using *Payroll Growth: First Quarter* and *Payroll Growth: Annual*. During my sample period, I find that, on average, about 775 incremental employees are hired each year while net payroll increases by about $39.46 million. Overall, this collection of measures may provide insights into the relations between P2P credit and important dimensions of economic growth, including entrepreneurship and employment.

2.5 Crime Data

I rely on crime data from the National Incident Based Reporting System (NIBRS) to examine whether online credit impacts social welfare. The system is under the jurisdiction of the Federal Bureau of Investigation (FBI) and is used by law enforcement agencies for collecting and reporting crimes. NIBRS collects data on each single incident and arrest within twenty-two offense categories composed of forty-six specific crimes, termed Group A offenses. Each incident includes information on the type of offense, victim, offender, location of the offense, date, and time.[10]

To assess community-level crime growth, I create a variable, *Crime Growth*, which is the yearly growth in the total number of incidents across all Group A categories. In Panel C of Table 1, I show that there are about 14,633 crimes each year in a typical community and that crime growth is increasing, with about 42 additional crimes per year. I also exploit the richness of the data to separately examine growth across several categories of crime, including thefts, burglaries, assaults, homicides/manslaughters, and weapon law violations. The statistics suggest that the aggregate growth in crime is principally driven by growth in thefts, which includes pocket-picking, purse snatching, and shoplifting.

2.6 Additional Data

I also collect data on factors that are likely to influence demand for finance, entrepreneurial activity, and crime. Specifically, I obtain socio-demographic characteristics for each community from the U.S. Census Bureau, including population size, per capita income, and the proportions of individuals who are male, African American, live in an urban area, and have graduated from college. Wage earning opportunities also play a role in households' entrepreneurial and criminal decisions (Evans and Leighton 1989; Grogger 1998; Gould, Weinberg, and Mustard 2002). To capture these effects in my empirical tests, I use data on yearly unemployment rates from the Bureau of Labor Statistics. I also rely on annual house price data from Federal Housing Finance Agency to account for the impacts of housing market developments on demand for credit (Campbell and Cocco 2007; Bazley 2017), economic activity (Corradin and Popov 2015; Chakraborty, Goldstein, and MacKinlay 2016; Schmalz, Sraer, and Thesmar 2017), and crime (Cui and Walsh 2015).

3 Empirical Methods

My key hypothesis is that online credit may have economic and social effects by serving as an alternative source of finance when traditional credit is scarce. It is inherently difficult to test this hypothesis because it requires identifying communities which are facing reduced access to

[10]In addition to the Group A offenses, the system includes eleven Group B offense categories for which only arrest data are reported. Group B offenses include bad checks, loitering, disorderly conduct, driving under the influence, drunkenness, nonviolent family offenses, liquor law violations, peeping, trespassing, and a miscellaneous category. The estimates are not sensitive to including Group B offenses in the analysis.

conventional credit and a method of isolating the role of online credit on real and social outcomes. To address these challenges, I combine several empirical methods. First, I use bank mergers and natural disasters to instrument for credit scarcity. Then, I match communities with access to online lending (i.e., treated communities) to similar communities which cannot borrow online (i.e., counterfactual communities). I then examine whether online lending has real and social effects around credit scarcity events by comparing economic and social outcomes across the treated and counterfactual communities.

3.1 Bank Mergers, Natural Disasters and Lending

I examine the effects of credit scarcity events on P2P and SBA loan originations using ordinary least squares (OLS) panel regressions. Specifically, I estimate:

$$y_{i,t} = \alpha_0 + \beta_1 CSE_{i,t-1} + \theta X_{i,t} + \psi_i + \rho_t + \epsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is either *P2P Loan Volume* or *SBA Loan Volume* in community i during year t. $CSE_{i,t-1}$ is the primary variable of interest and is an indicator which takes a value of one when a credit scarcity event occurs within community i during year $t-1$, and zero otherwise. Specifically, when using bank mergers as credit scarcity events, *CSE* is equivalent to the *Bank Merger* indicator. When using natural disasters as credit scarcity events, *CSE* is equivalent to the *Natural Disaster* indicator.

In the regression specification, I include various community-level controls, $X_{i,t}$, to account for time-varying determinants of credit demand. Specifically, I control for the age profiles of the resident population, house prices, per capita incomes, unemployment rates, population sizes, and the proportions of the communities that are male, African American, reside in an urban area, and college educated. Additionally, I include year fixed effects, ρ_t, to account for aggregate time trends. The estimation results may still be biased, however, if crime is driven by some unobservable, local characteristics. I, therefore, rely on community fixed effects, ψ_i, to account for time-invariant differences across communities.[11]

[11] In untabulated results, I examine whether the findings are sensitive to the regression specification. I find that the results are robust to using a first-difference specification. To examine the potential impact of outlying data points, I also Winsorize the dependent variables at the top and bottom one percentiles. I find that the results are not materially affected.

3.2 Real and Social Effects of Online Credit

If individuals respond to reductions in conventional credit by using P2P loans, this alternative credit market may impact real economic activity and social welfare. To test this hypothesis, I employ two empirical approaches. First, as a baseline, I conduct OLS panel regressions. Specifically, I estimate:

$$\begin{aligned} y_{i,t} = \alpha_0 &+ \beta_1 CSE_{i,t-1} + \beta_2 P2P\ Loan\ Volume_{i,t} \\ &+ \beta_3 CSE_{i,t-1} \times P2P\ Loan\ Volume_{i,t} \\ &+ \theta X_{i,t} + \psi_i + \rho_t + \epsilon_{i,t} \end{aligned} \tag{2}$$

where $y_{i,t}$ is a measure of economic growth, including *Establishment Growth*, *Employment Growth*, *Payroll Growth: First Quarter*, and *Payroll Growth: Annual*, or *Crime Growth* within community i during year t. As before, $CSE_{i,t-1}$ is an indicator which takes a value of one when a credit scarcity event occurs within community i during year $t-1$, and zero otherwise. *P2P Loan Vol.*$_{i,t}$ is again the natural log of the P2P loan volume within community i during year t. The primary variable of interest is $CSE_{i,t-1}$ × *P2P Loan Vol.*$_{i,t}$ and β_3 captures the effects of online borrowing when traditional credit is scarce. I also include the socio-demographic controls as well as year and community fixed effects in the regressions. When examining crime growth, I include an additional control, *Law Agencies*, which is the number of law agencies within the community that participate in NIBRS. I include this control to address potential concerns that fluctuations in participation in the reporting system may drive reported crime growth.

3.2.1 Matched Difference-in-Difference Methodology

While the preceding OLS estimation of regression (2) can provide insights into the relations between online lending and real and social outcomes, it is subject to endogeneity concerns. For instance, availability of online credit within a community is likely an endogenous decision driven by expectations of economic conditions and households' demands for credit. Ultimately, this inhibits causal interpretation of the estimates. To overcome this econometric challenge and provide causal insights, I use a two-step empirical approach.

First, I match communities with access to online credit (i.e., treated communities) to similar communities that do not have access to the market (i.e., counterfactual communities). This matching procedure was pioneered by Neyman (1923) and Rubin (1974) and the design assesses the effect of a treatment against an estimation of the counterfactual, which would be the outcome had the subject not been exposed to the treatment.

Following the matching, I utilize a difference-in-difference estimator to examine the real and social effects of online credit.[12] The underlying intuition for using the difference-in-difference method is that it provides unbiased effect estimates if, in the absence of the treatment, the trend over time would have been the same between the treated and control groups. However, a potential issue is that the groups may differ in ways related to their trends over time. Matching is used to address this concern and, consequently, is expected to reduce bias from the potential misspecification of the regression model (Ho, Imai, King, and Stuart 2007).

Applying this framework to my setting, the challenge is evaluating the causal effects of online credit on real economic and social outcomes following reduced access to traditional credit. This requires a control community where a credit scarcity event occurs and online credit is unavailable. To implement the process, I exploit the restriction of access to P2P credit in two states, Iowa and West Virginia. Residents of these states could not borrow in the online market during my sample period due to state regulatory restrictions. This plausibly exogenous prohibition of access allows me to construct counterfactuals for a portion of the treated communities.

For each online borrowing community (i.e., treated community), I select a similar non-borrowing community (i.e., control community) using nearest-neighbor matching with replacement. I match communities based on socio-demographic factors in the year prior to the credit scarcity events to remove any economy-wide fluctuations.[13] The characteristics underpinning the process include communities' home values, per capita incomes, gender ratios, the proportions of the residents who are African American, the percentages of residents who have graduated college, the percent residents living in urban areas, unemployment rates, residents' age profiles, and population sizes.

[12]The combined matching and differencing procedure was developed by Heckman, Ichimura, and Todd (1997) and Heckman, Ichimura, Smith, and Todd (1998).

[13]However, I retain year fixed effects to address any residual concerns about time effects.

4 Empirical Evidence

In this section, I report my empirical findings. For ease of presentation, I first report the effects of credit scarcity events due to bank mergers on individuals' borrowing in the online market and on SBA loan volume. I then present evidence related to the real and social effects of online credit. I discuss the implications of natural disaster-driven credit scarcity events in Section 5.

4.1 Impact of Bank Mergers on Credit

Online lending markets may serve as a substitute channel through which individuals borrow when their access to conventional credit is restricted. I test this conjecture by performing OLS panel regressions which estimate the effects of a merger on P2P loan volume in the subsequent year. I also examine whether bank mergers impact access to a traditionally important debt instrument for entrepreneurs, SBA loans. I report the results in Table 2.

The evidence in Panel A indicates that individuals turn to online loans when their local credit market is impacted by a merger. For instance, the estimate from a univariate regression (Column 1) shows that, in the year following a merger, P2P loan volume grows by about 35.40%. Including time-varying economic and demographic controls (Column 2) does not subsume the effect of a merger. Using year fixed effects to rule out aggregate time trends and zip code fixed effects to identify off of within-community variation (Column 3), I find that a merger is associated with a 29.80% increase in loan volume the following year. In economic terms, this is equivalent to an increase of about $368,626, based on an average loan volume of $1.23 million.[14] The impact of a merger is also economically meaningful. For comparison, the estimates suggest that a one standard deviation decline in home values is associated with a 6.30% rise in volume.

Within the same communities, bank mergers are associated with declines in the issuance of SBA-guaranteed loans (Panel B). Accounting for aggregate time effects and identifying off of within-community variation, I find that a merger is associated with a 6.00% reduction in volume the subsequent year (Column 6). This decline is equivalent to about $1.69 million, based on the typical

[14] In Appendix 2, I examine whether P2P borrower quality declines when availability of traditional credit is reduced and if lenders adjust the supply of credit. The evidence suggests that, on average, post-CSE borrowers are not significantly different from individuals who use online credit prior to the event. Moreover, lenders do not significantly adjust the supply of funds.

yearly SBA loan volume. The fall in SBA-backed loans is likely to have real economic implications since these loans promote entrepreneurial activity (Hackney 2017).

Overall, mergers among traditional financial institutions have implications for individuals' borrowing decisions. Specifically, mergers are associated with subsequent increases in the use of P2P loans and diminished originations of loans aimed at supporting small business entrepreneurs.

4.2 Online Credit and Business Establishment Growth

Since P2P loan originations are positively related to merger-induced credit scarcity, I examine whether online lending has real economic effects using two approaches. First, I perform OLS panel regressions using the full data sample to generate baseline insights. To address potential endogeneity concerns, I then employ the matching and difference-in-difference methodology.

In Panel A of Table 3, I report estimates from the full-sample OLS regressions. The evidence suggests that net growth in business establishments is negatively associated with merger-induced credit scarcity. Based on the strictest specification (Column 4), a merger reduces net business growth by about 37 establishments. Conversely, a one percent increase in P2P loan volume is associated with about a 0.07% increase in net growth, based on the average establishment growth rate. Online borrowing also seems to mitigate the decline in the rate of new business formation in the year after a merger. The cumulative effect of a one percent increase in online borrowing moderates about 0.15% of the fall in net establishment growth that is associated with a merger.

The baseline tests suggest a positive correlation between online borrowing and economic growth when availability of traditional credit is restricted. I, therefore, employ the matching procedure to align communities with access to online credit to similar communities that cannot access the market. I match 164 P2P communities to counterfactual, non-P2P communities.[15] Table 4 presents the results of the matching. For each variable, I report differences in the means and $t-$statistics for tests of the hypothesis that differences between areas with access to the online market, in the year prior to a merger, and those that are restricted from borrowing, are zero. Among the eleven variables, none have a difference in means that is significantly different from zero. Additionally, I examine whether the number of banks varies significantly between matched communities. I find that

[15] The treatment communities are distributed across 22 states, including: Colorado, Delaware, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Michigan, Missouri, Montana, North Dakota, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Washington, and Wisconsin.

communities are similar with respect to the presence of local banks (estimate = -2.48; $t-$statistic = -0.80). Overall, the evidence suggests that the matched community pairs are similar on observable dimensions.[16]

In Figure 2, I graphically examine whether the aligned P2P and non-P2P communities are similar with respect to net growth in business establishments. The growth trends appear similar between the matched communities in the pre-merger period. Given these parallel trends, the aligned, non-P2P communities seem appropriate for serving as counterfactuals for the treated communities. The plot also suggests that, after bank mergers, net growth in establishments falls in both P2P and non-P2P communities. However, communities with access to the online market experience a smaller decline.

The results from empirical difference-in-difference tests (Panel B of Table 3) further support the baseline evidence.[17] Specifically, net establishment growth falls in the year after a merger, but online borrowing moderates the decline. Including control variables to increase efficiency and adjust for any residual bias from the matching procedure, the estimate in Column (2) for *Bank Merger* shows that a community which experiences a merger has reduced growth in establishments in the subsequent year. Specifically, the estimate suggests about 24 fewer incremental enterprises.

The estimate for *P2P Loan Volume* is not statistically different from zero, indicating that online lending does not have an independent effect on net establishment growth. However, after a merger, online lending moderates the economic decline. In Column (2), I find that a one percent increase in borrowing offsets about 0.25% of the fall in net growth.

In Columns (3) and (4), I use an alternative measure to assess the impact of online credit. Specifically, I construct an indicator variable, *P2P Area*, which takes a value of one if the community has access to online loans, and zero otherwise. I employ this measure to estimate the average treatment effect of online lending on treated communities (i.e., the average treatment effect on the treated (Athey and Imbens 2006)). Consistent with the prior evidence, I find that establishment growth falls after a bank merger, by about 65 fewer incremental businesses (Column 8). However, communities with access to online credit experience a reduced decline. Particularly, the estimates

[16] I discuss the matching results when using natural disasters as credit scarcity events in Section 5.

[17] To compare a treatment community outcome with its paired control community, I use pair-group fixed effects instead of zip code fixed effects. The pair-group fixed effect facilitates identification off of variation within matched treatment and control community pairs. The results are not materially impacted by omitting this fixed effect.

suggest that, on average, the impact of a merger on net establishment growth is reduced by about 14.93% among P2P communities.

The collective evidence indicates that expanded access to credit, in the form of online loans, has real economic effects. That is, when communities' traditional borrowing channels are impacted by bank mergers, individuals seem to turn to online credit. In turn, access to the new borrowing channel offsets a portion of the decline in the net rate of new business formations that is associated with a merger.

4.2.1 Additional Evidence: Effects Across Establishment Sizes

While the primary evidence suggests that online lending impacts total net establishment growth, the types of debt instruments used across businesses varies. A primary determinant of the choice of instrument is firm size (Berger and Udell 2003). For instance, young and small businesses rely heavily on loans from the owner, his family, and banks (Petersen and Rajan 1994). Building upon this literature, I conjecture that P2P credit is likely to facilitate growth in smaller establishments as larger businesses may have access to other sources of capital, such as trade credit.

To test this hypothesis, I examine the relation between online lending and growth across a spectrum of establishment sizes. I delineate enterprises into four size categories based on the number of employees: (i) 1−4 employees, (ii) 5−9 employees, (iii) 10−19 employees, and (iv) 20+ employees. I then re-perform both the baseline panel regressions and matched difference-in-difference tests using net growth in establishments across size categories as the dependent variables.

I present the results in Table 5. I find that bank mergers principally inhibit net growth in smaller businesses, particularly those with under 20 employees. The estimates also indicate that the benefits of expanded access to credit through the peer-to-peer market are primarily concentrated among the smallest firms. Specifically, those with 1−4 employees. Based on the estimates in Column (2), I find that a one percent increase in online borrowing counteracts about 0.13% of the fall in net growth among the smallest establishments. Overall, the evidence suggests that access to the alternative financing source has real economic effects by facilitating small enterprise growth.

4.3 Validation Test: Employment and Payroll Growth

Since bank mergers and online lending impact establishment growth within a community, employment and payroll growth may be similarly affected. I test this conjecture by re-performing both the baseline panel regressions and the matched difference-in-difference tests using *Employment Growth*, *Payroll Growth: First Quarter*, and *Payroll Growth: Annual* as the dependent variables. I report the estimates from the empirical tests in Table 6.

The evidence is consistent with the findings in the primary analysis, which suggest that bank mergers hinder economic growth. While not statistically significant for employment growth (Panel A), a bank merger significantly reduces growth in establishments' payrolls (Panels B and C).

Online lending does not have an independent impact on growth in employment or payroll. However, after a merger, online credit moderates the reductions in employment and wage growth. For instance, the estimates indicate that a one percent increase in online borrowing offsets about 0.27% of the decline in net employment growth and 0.13% (0.10%) of the fall in first quarter (annual) payroll growth that are associated with mergers. Overall, the findings lend further support that online credit can have positive real effects by moderating the impacts of restricted access to conventional debt.

4.4 Online Credit and Crime Growth

Economic growth has a strong influence on households' social welfare, particularly by abating crime (Grogger 1998; Gould, Weinberg, and Mustard 2002; Levitt 2001). Since online lending impacts communities' economic landscapes after bank mergers, it may also shape social welfare by moderating crime growth. I test this conjecture by re-performing both the baseline panel regressions and the matched difference-in-difference analysis using *Crime Growth* as the dependent variable. I present the results in Table 7.

The estimates from the full-sample regressions in Panel A show that crime growth within a community increases, by about 382 incidents (Column 4), after a merger. Conversely, the estimate for *P2P Loan Volume* is not statistically different from zero, suggesting that online borrowing does not have an independent relation with crime growth. Following a merger, online borrowing is negatively related with crime growth. That is, borrowing in the market mitigates a portion of the rise in crime associated with traditional credit scarcity. Specifically, the estimates in Column (4) indicate that, within a community, a one percent increase in loan volume reduces crime growth by 0.64 incidents. The cumulative effect of a one percent increase in P2P borrowing abates about 0.09% of the spike in crime growth after a merger.

Both the graphical and empirical evidence from the matched difference-in-difference analysis support the findings from the panel regressions. Figure 3 shows that the crime growth trends appear similar between the matched communities in the pre-merger period. Following mergers, growth in crime rises in both control and treatment communities. However, communities with access to the P2P market experience a smaller rise, on average.

The empirical estimates in Panel B (Table 7) show that communities which experience bank mergers also experience heightened crime growth in the subsequent year. In Column (1), the estimate for *P2P Loan Volume* is not statistically significant. Consistent with the panel regressions, the estimate for the interaction between *Bank Merger* and *P2P Loan Volume* is negative and statistically significant, indicating that online borrowing reduces crime growth associated with a merger. Including socio-demographic controls, in Column (2), does not materially impact the estimates. I find that the cumulative effect of a one percent increase in volume mitigates about 0.11% of the crime growth from a merger.

In Columns (3) and (4), I again use the indicator variable, *P2P Area*, to estimate the average treatment effect of online lending on crime within treated communities. In Column (4), I find that crime growth appreciates after a bank merger, by about 649 incremental crimes. However, communities with access to online credit experience a smaller rise. Particularly, the estimates suggest that, on average, the impact of a merger on crime growth is reduced by about 10.91% among P2P communities. Collectively, the evidence suggests that bank mergers have spillover effects on growth in crime. However, the availability of online credit has a moderating influence. That is, peer-to-peer lending affects social welfare by abating a portion of the growth in crime associated with reduced availability of traditional credit.

The social welfare impact of online credit's moderating influence is likely to be non-trivial since crime has a large economic footprint. For instance, in 2007, over 23 million criminal offenses were committed in the United States with an approximate total cost of $194 billion.[18] While the cost of crime varies significantly across the types of crime committed, the aggregate statistics suggest a guideline cost estimate of about $8,434 per offense. Based on this estimate, each percent increase in online borrowing mitigates approximately $6,105 of the aggregate cost of crime to a community due to a merger, which the estimates indicate to be about $5.55 million.

4.4.1 Additional Evidence: Effects Across Crime Types

While the primary analysis focuses on total crime growth, the criminology literature documents heterogeneity in the determinants of crime across crime types. Economic factors, such as employment opportunities, are generally linked to property crimes (e.g., thefts), as opposed to violent crimes (e.g., homicides).[19] Wright and Decker (1994) also note that many burglars identify insufficient cash to meet their current expenses (i.e., insufficient liquidity) as a principal driver of their crimes. In light of this, I examine how property, personal, and societal crimes respond when availability of conventional credit is limited and whether online borrowing has heterogeneous effects across crime types. Specifically, I posit that property crimes will increase following a merger, but that access to online credit will moderate the rise. Conversely, I expect personal and societal crimes to be little influenced by mergers and online lending.

I identify property-related, personal, and societal crimes according to their Uniform Offense Codes (UCR) through NIBRS.[20] To examine property-related crimes, I construct two measures. First, *Theft Growth* is the yearly growth in the number of pocket-picking, purse-snatching, and shoplifting crimes. Second, *Burglary Growth* is the yearly growth in the number of burglary and breaking and entering crimes. I also examine annual growth in severe crimes against people and society. Specifically, I use growth in assaults and homicides/manslaughters as measures of crimes aimed at persons. For societal crimes, I use growth in weapon law violations.

I re-perform both the panel regressions and the matched difference-in-difference tests using the

[18]The U.S. Department of Justice (2008) estimates the economic losses to victims to be about $15 billion and government expenditures on police protection, legal and judicial activities, and corrections to be about $179 billion.

[19]For instance, see Tardiff (1985), Marzuk, Tardiff, and Hirsch (1992), Levitt (2001), and Raphael and Winter-Ebmer (2001).

[20]Specifically, NIBRS classifies crimes into three categories: (i) property, (ii) persons, and (iii) societal. According to NIBRS, the objective of crimes against property is to obtain money, property, or some other benefit. Crimes against persons are those whose victims are always individuals. Crimes against society represent society's prohibition against engaging in certain types of activities.

aforementioned crime growth variables as dependent variables. I report estimates for property crimes in Panel A of Table 8. The estimates show that property-related crime growth increases after a bank merger, but access to online lending moderates the rise. The difference-in-difference estimate for *Bank Merger* in Column (2) shows that about 127 incremental thefts occur in the year after a bank merger. Based on estimates of per-offense expenses (e.g., McCollister, French, and Fang (2010)), this rise in thefts has a cost to the community of approximately $448,564.[21]

The coefficient on the interaction term is negative and statistically significant, suggesting that rising online borrowing counteracts the spike and offsets a portion of the growth. The estimates imply that a one percent increase in online borrowing mitigates about 0.08% of the incremental thefts associated with a merger. Economically speaking, each percent increase in borrowing reduces a community's total cost of new thefts by approximately $358. This reduction is non-trivial and represents about 1.33% of the typical household's annual income.

I find similar effects for mergers and online lending on growth in burglaries. The estimates in Column (4) indicate that about 287 incremental burglaries occur after a merger while a one percent increase in borrowing abates about 0.09% of the additional crimes. The aggregate cost of the spike in burglaries to the community is about $1.85 million and each percent increase in online borrowing offsets about $1,665 of the total cost. This translates to nearly 6.18% of the typical household's annual income.

In Panel B, I report estimates of the effects of mergers and online lending on growth in assaults, homicides and manslaughters, and weapon law violations. Consistent with my hypothesis, I find no significant relations between bank mergers or online credit and severe crimes against people or society. Overall, online borrowing seems to principally reduce growth in crimes related to property, where the objective is to obtain money, property, or some other benefit.

5 Alternative Credit Scarcity Events: Natural Disasters

A potential contention could be that mergers among banks may not be random. To address this concern, I rely on the occurrence of natural disasters as alternative, exogenous events that

[21] McCollister, French, and Fang (2010) estimate per-offense costs for a range of crime types. Where appropriate, the estimates account for both tangible and intangible costs to victims, law enforcement, and the judicial system. The authors estimate the per-incident cost of theft to be $3,532 and the cost of a burglary to be $6,462.

are likely to reduce credit availability (Runyan 2006). To implement the empirical analysis, I match across P2P and non-P2P communities in the years prior to their disasters using the same socio-demographic factors.[22] After aligning the communities, I examine the impact of disasters on originations of online and SBA loans using OLS panel regressions.[23] Importantly, in addition to 7(a) and 504 loans, the SBA issues Disaster Loans to assist businesses with recovering from disasters. I include these loans in *SBA Loan Volume* for the analysis. After examining whether disasters impact credit conditions, I test whether online borrowing has real and social effects following disasters.

5.1 Natural Disasters and Credit Availability

I first examine the dynamics between natural disasters and credit availability. I report the findings in Table 9. In Column (1), I find that P2P loan volume increases in response to a natural disaster. The increase is both statistically and economically significant. That is, the estimate suggests that online loan originations rises by about 51% in the year after a natural disaster.

However, the estimate in Column (2) suggests that availability of conventional credit, as measured by SBA loan volume, after a disaster is not statistically different from availability prior to a disaster. The limited response is surprising given that entrepreneurs lack credit access after a disaster (e.g., Runyan (2006)) and the presence of specialized, SBA-backed disaster loans aimed at supporting economic activity in disaster-hit communities. Overall, the evidence suggests that natural disasters reduce the availability of traditional credit while individuals' demands for online loans increase.

5.2 Real and Social Effects of Online Credit

Since the evidence indicates that online borrowing rises in response to a disaster, I examine whether the access to the market has real economic or social implications. First, I examine whether the matched P2P and non-P2P communities display similar trends in establishment growth in Figure 4. The graphical evidence suggests that the communities have parallel trends in economic growth. The graph also suggests that, after a natural disaster, net growth in business establishments

[22] In untabulated results, I find no significant differences between matched community pairs based on the matching variables.

[23] A potential concern could be that communities experience bank mergers and disasters during the same time period. In untabulated results, I find that less than 5% of communities which experience a disaster also experience a bank merger during my sample period. Excluding these communities does not materially impact the results.

falls in both community groups. However, those with access to the online market experience a smaller decline. In Figure 5, I find that the matched communities also have similar trends in crime growth prior to the natural disasters. Following the disasters, communities which can borrow online have smaller rises in crime relative to those which do not have access to the peer-to-peer credit market.

Given the graphical evidence, I conduct difference-in-difference tests to examine the real and social effects of online credit using the measures of economic activity and crime growth as dependent variables. In each test, I include an additional control variable, *Disaster Damage*, to account for variation in the intensity of the natural disaster. Specifically, the measure is the total dollar amount of damage caused by the disaster.[24]

The estimates in Columns (3) through (7) of Table 9 indicate that natural disasters hinder economic growth. However, online lending moderates the aftermath. Specifically, in Column (3), I find that a one percent increase in P2P loan volume offsets about 0.22% of the decline in net establishment growth in the year after a disaster. In untabulated results, I find that credit scarcity arising from natural disasters impacts net growth in small enterprises but access to online borrowing moderates the diminished growth in the smallest establishments, those with one to four employees. Specifically, the estimates suggest that a one percent increase in online borrowing ameliorates 0.21% (p-value = 0.002) of the reduced growth in the smallest businesses. Net growth in employment and payroll are similarly affected (Columns $3-8$).

After a disaster, social welfare is impacted by heightened crime growth. The estimates in Column (9) indicate that crime growth appreciates but a one percent increase in online lending abates approximately 0.18% of the rise. Using the average cost of a crime to society (e.g., \$8,434), each percent increase in P2P lending mitigates approximately \$4,463 of the aggregate cost of crime due to a disaster, which is about \$2.48 million. Overall, the natural disaster-based evidence further suggests that online lending has real and social implications by counteracting the effects associated with credit scarcity.

[24]The estimates are not materially impacted by omitting this control variable.

6 Robustness Checks

In this section, I perform additional tests to further investigate the primary findings.

6.1 Reverse Causality of Crime

I focus on mergers between non-failing banks to alleviate reverse causality concerns. Nevertheless, a hypothesis could be that expectations of higher crime in the future may prompt present-day mergers, shape the community's economic vitality, and influence borrowing decisions. An implication of this conjecture is that, if trending crime risks influence present-day decisions and outcomes, future crime would have to be a reflection of the current crime trend. In essence, current crime growth would spur contemporaneous mergers, impact P2P borrowing, and influence economic activity.

I find that current crime growth is positively correlated with crime growth in the next year (estimate $= 0.205$; $p-$value < 0.001). In light of this correlation, I should find positive contemporaneous relations between crime growth and mergers, P2P borrowing, and the measures of economic growth under this alternative hypothesis. I test this conjecture by regressing *Bank Merger*, *P2P Loan Vol.*, and the economic growth measures on the concurrent change in crime. I present the results in Table 10. In Columns (1) through (3), crime growth does not significantly predict a merger during the same year. In Columns (4) and (5), crime growth also does not significantly explain concurrent P2P loan volume. Moreover, crime growth is not significantly related to contemporaneous growth in establishments, employment, or payroll (Columns $6 - 13$). Collectively, the evidence suggests that the reverse causation theory is unlikely.

6.2 Online Credit's Influence on Bank Mergers

A potential concern may be that local demand for online credit could influence banks' decisions to merge. For instance, access to the alternative finance source may impact the competitive landscape and drive consolidation among traditional lending institutions. However, this alternative hypothesis is not likely to explain my primary findings since banks can benefit from information spillovers from P2P lending and respond by increasing availability of credit to individuals who have online loans (Balyuk 2017).

I more directly address whether online lending shapes banks' merger decisions by regressing *Bank Merger* on contemporaneous loan volume and volume in the year prior to a merger. I report the estimates in Table A3.1. From probit regressions, I find that neither contemporaneous volume (estimate = -0.007, $z-$statistic = -1.44) nor volume in the preceding year (estimate = -0.003, $z-$statistic = -0.83) significantly explain mergers. The estimates for contemporaneous volume (estimate = -0.004, $t-$statistic = -1.27) and lending in the prior year (estimate = -0.002, $t-$statistic = -0.88) from OLS panel regressions, which include community and year fixed effects, are also not statistically different from zero. Overall, the evidence suggests that P2P borrowing is not a significant driver of bank mergers in my sample.

6.3 Excluding the Early Years of Online Credit

In the primary analysis, I examine the effects of online credit using data spanning the full time period available. However, the P2P market was created in 2007 and individuals' adoption of the online credit platform was not immediate. Consequently, loan volume in the early years was moderate. Moreover, variation in the regulatory environment impacted loan originations during brief periods in 2007 and 2008. To address potential concerns related to these factors, I re-perform the matched difference-in-difference tests using data from 2010 through 2015.

I report the results based on the post-2009 sub-sample in Table 11. The sub-sample estimates are consistent with and similar in magnitude to those in the primary analyses, which uses the full time period. For instance, bank merger-induced credit scarcity (Panel A) reduces net establishment growth (Column 1) in the subsequent year but online borrowing moderates the decline. I find that a one percent increase in loan volume offsets about 0.18% of the fall in net establishment growth. As in the main analysis, bank mergers also spur crime growth but a one percent increase in online borrowing abates about 0.13% of the rise (Column 5).

The effects are also consistent when using natural disasters as credit scarcity events (Panel B). The estimates in Column (1) indicate that a one percent increase in online borrowing ameliorates about 0.17% of the diminished net enterprise growth. In Column (5), I find that a percent increase in online borrowing moderates about 0.19% of the appreciation in crime growth. Overall, the findings suggest that online credit has real effects and influences social welfare.

6.4 Excluding Business Loans

The majority of borrowers state the reason for seeking an online loan is to refinance existing debt. However, a small proportion of applicants, about 1% of my sample, list the loan as a small business loan. Given such, a hypothesis may be that this subset of loans is driving the results. I test this conjecture by excluding business loans from my sample and re-performing the matched difference-in-difference tests.[25] I report the results in Table 12.

The estimates are consistent with and similar in magnitude to those in the primary analyses. For instance, bank merger-induced credit scarcity (Panel A) reduces net establishment growth (Column 1) in the subsequent year but online borrowing moderates the decline. I find that a one percent increase in loan volume offsets about 0.27% of the fall in net establishment growth. Reduced availability of traditional credit also spurs crime growth but a one percent increase in online borrowing abates about 0.13% of the rise (Column 5).

When using natural disasters as credit scarcity events (Panel B), the effects are similar. In Column (1), I find that a one percent increase in online borrowing is associated with a reduction of about 0.17% of the diminished net enterprise growth. In Column (5), the estimates indicate that a percent increase in online borrowing moderates about 0.20% of the appreciation in crime growth. Overall, the findings suggest that the small sample of business loans is not driving the primary findings.

6.5 Per Capita Basis

I account for communities' population sizes in all empirical tests to address potential concerns related the influence of a growing population on an area's economic vitality and crime. By including population as a control, the primary inference from the study should be interpreted as "holding population constant, P2P borrowing supports net establishment growth and moderates crime growth after a merger." Nevertheless, I re-perform the analyses on a per capita basis. Specifically, I adjust my key dependent variables by the community's total population during the year.

I report the estimates in Table A4.1. Overall, the per capita findings are consistent with those from the principal analyses. Specifically, on a per capita basis, bank mergers are associated with

[25] An alternative approach is to exclude communities that had a resident obtain a loan for a business. In untabulated results, I find similar and consistent effects using this method.

reduced economic growth and rising crime. Conversely, online borrowing mitigates the negative effects associated with a merger by serving as an alternative source of finance.

7 Conclusion

Access to credit is a fundamental determinant of the economic landscape. Traditionally, credit supply has been dominated by banks. However, credit markets are evolving due to innovations in technology. One of the most successful FinTech innovations has been the creation of online, peer-to-peer credit platforms. Despite the rapid growth of these markets, limited attention has been paid to their real and social effects.

I examine whether online lending has real economic and social implications. My principal conjecture is that online credit can serve as an alternative source of finance for individuals when conventional credit is scarce. In turn, online lending may moderate the negative economic and social impacts of credit scarcity, such as diminished growth in new businesses and heightened crime growth.

Using bank mergers and natural disasters to identify communities that are facing reduced credit access, I find that online borrowing increases in response to the scarcity of traditional credit. Online borrowing also moderates the diminished growth in business establishments and the heightened crime growth that are associated with the restricted credit access. I find that a percent increase in online borrowing offsets about 0.25% (0.22%) of the diminished establishment growth and 0.11% (0.18%) of the rise in crime associated with bank mergers (natural disasters). The effects of online credit are concentrated among small enterprises and in property-related crimes, such as thefts and burglaries. Overall, the evidence indicates that technology-driven innovations in credit markets have real and social effects. These findings have implications for the ongoing debate about the role of online lending markets in the United States (e.g., Mills and McCarthy (2016b)).

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Figure 1: Loan Distribution Across the States

This figure presents the distribution of my sample of loans across the U.S. states. The y-axis displays the percent of loans originated within communities in the state. The x-axis displays the states in the sample.

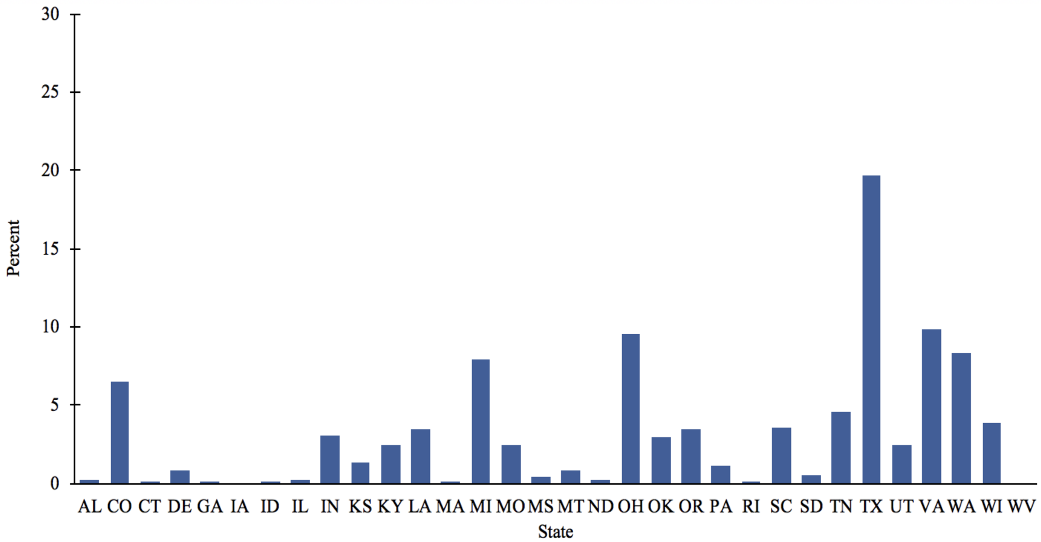

Figure 2: Establishment Growth Around Bank Mergers

This figure presents the average net growth in establishments for treatment and control communities before and after bank mergers. The y-axis displays *Establishment Growth*, the yearly net growth in business establishments. The x-axis displays the time period in relation to when the community experiences a merger.

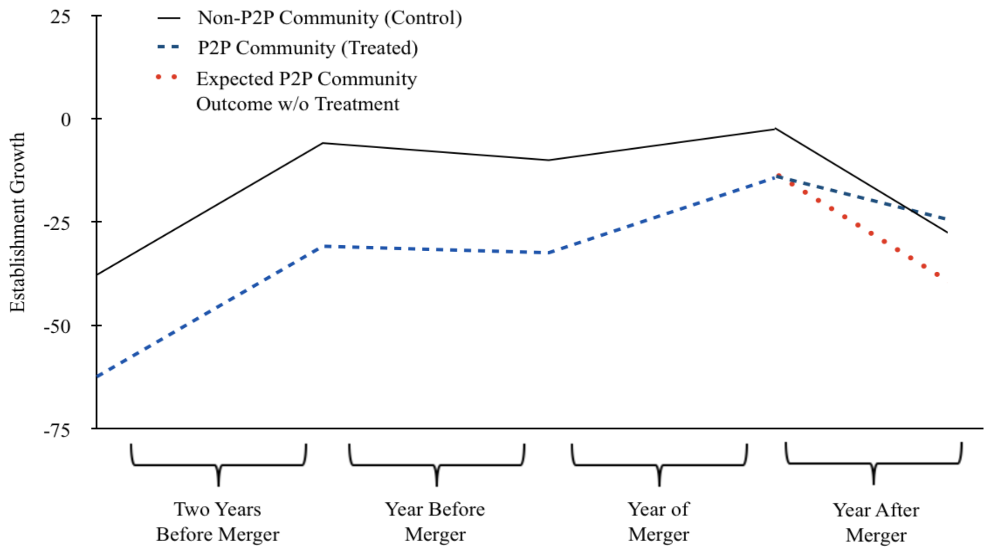

Figure 3: Crime Growth Around Bank Mergers

This figure presents the average growth in crime for treatment and control communities before and after bank mergers. The y-axis displays *Crime Growth*, the yearly growth in total crimes. The x-axis displays the time period in relation to when the community experiences a merger.

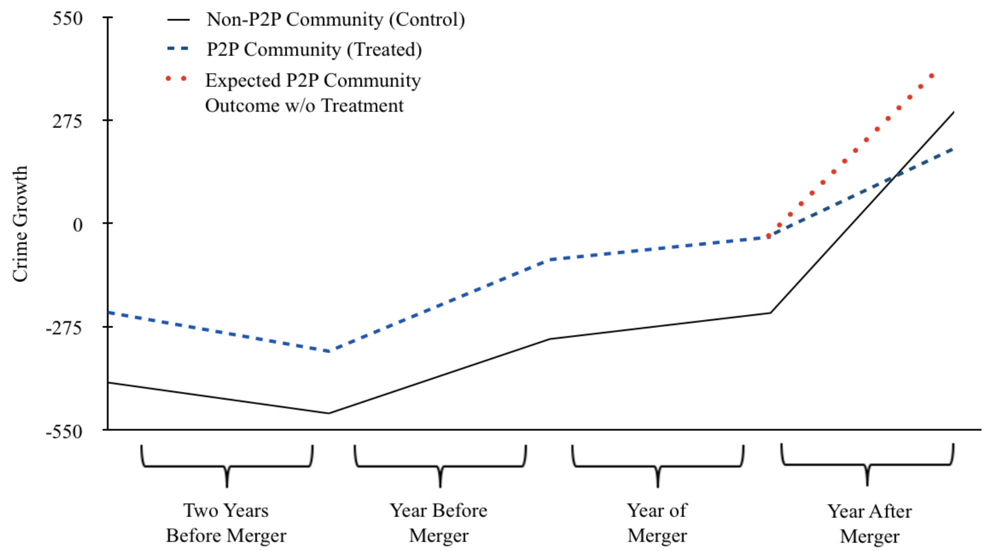

Figure 4: Establishment Growth Around Natural Disasters

This figure presents the average net growth in establishments for treatment and control communities before and after natural disasters. The y-axis displays *Establishment Growth*, the yearly net growth business establishments. The x-axis displays the time period in relation to when the community experiences a disaster.

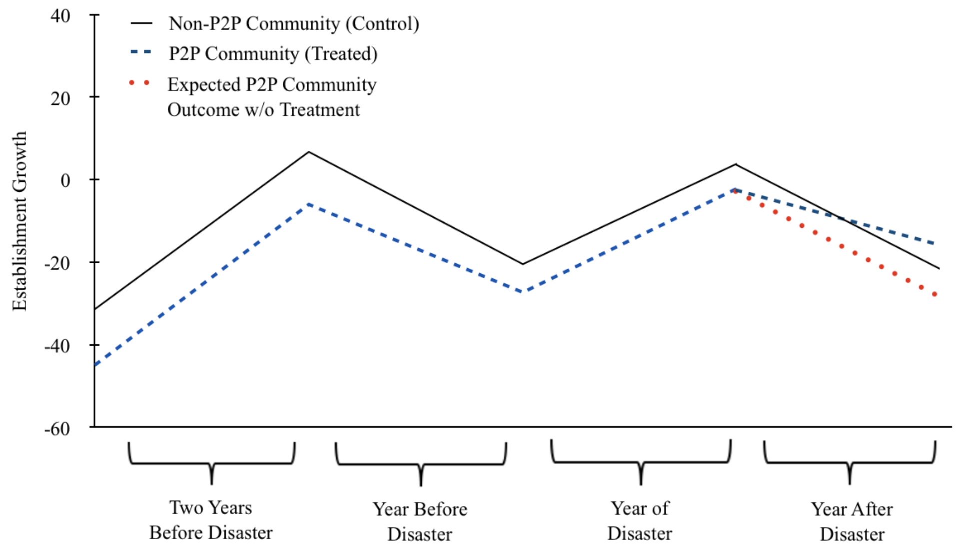

Figure 5: Crime Growth Around Natural Disasters

This figure presents the average growth in crime for treatment and control communities before and after natural disasters. The y-axis displays *Crime Growth*, the yearly growth in total crimes. The x-axis displays the time period in relation to when the community experiences a disaster.

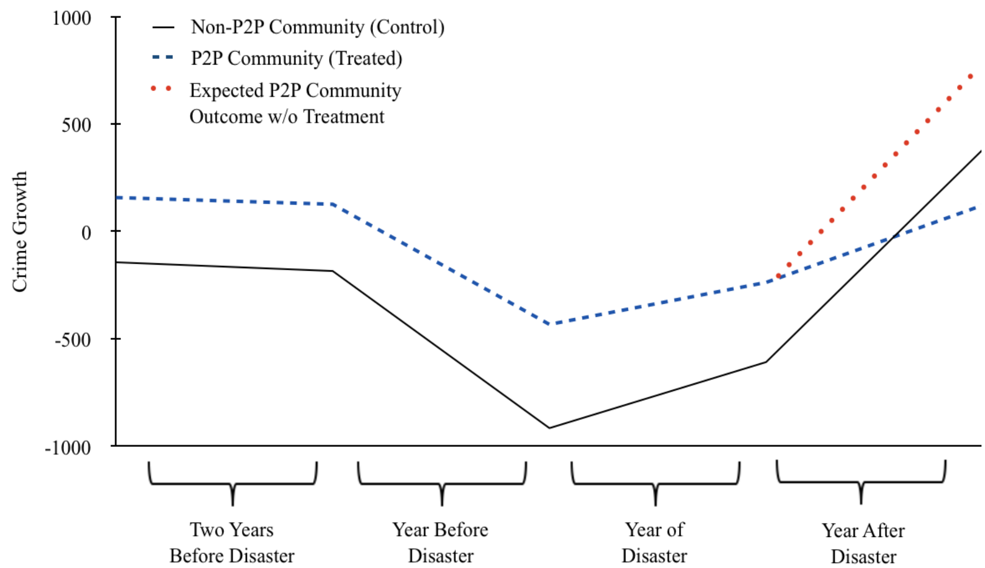

Table 1: Descriptive Statistics

The table presents summary statistics for the primary data sets used in the study. Panel A presents statistics related to P2P loans. *P2P Loan Volume* is the natural log of *Total P2P Loan Volume*, the total annual loan volume within a zip code area. Panel B reports statistics for the economic variables while Panel C presents estimates of crime. Panel D presents statistics describing the demographics characteristics for the three-digit zip code areas. The estimates for *Total P2P Loan Vol.*, *Total SBA Loan Vol.*, *Payroll Growth: First Qtr.*, and *Payroll Growth: Annual* have been scaled by 1,000. Definitions for all variables used in the analysis are included in Appendix I.

Panel A: P2P Credit Statistics				
	Mean	Median	St. Deviation	N
Total P2P Loan Vol.	1,237	57.90	3,542	2,383
P2P Loan Vol.	8.26	10.97	6.36	2,383
Borrower Income	64,764	64,287	19,095	2,383
Debt-to-income	16.64	17.66	4.03	2,383
FICO	707.18	700.44	18.48	2,383
Revolving Credit Util.	51.74	54.81	54.81	2,383
Interest Rate	12.84	12.98	1.73	2,383
Credit Grade	2.67	2.75	0.55	2,383
Funding Percent	99.33	1.00	0.03	2,383
Panel B: Economic Statistics				
	Mean	Median	St. Deviation	N
Bank Merger	0.16	0.00	0.37	2,383
Natural Disaster	0.25	0.00	0.41	2,383
Total SBA Loan Vol.	28,200	7,292	96,500	2,383
SBA Loan Vol.	15.92	15.80	1.40	2,383
SBA Interest Rate	5.88	5.84	0.59	2,383
Total Establishments	8,479	6,093	7,934	2,383
Establishment Growth	23.17	-1.00	229.02	2,383
Est. Growth: 1−4 Employees	8.26	-5.00	133.87	2,383
Est. Growth: 5−9 Employees	-0.50	-1.00	54.67	2,383
Est. Growth: 10−19 Employees	4.43	2.00	43.24	2,383
Est. Growth: 20+ Employees	9.27	5.00	60.16	2,383
Employment Growth	775.52	0.00	7,428	2,383
Payroll Growth: First Qtr.	39,461	11,929	158,907	2,383
Payroll Growth: Annual	159,859	57,240	557,709	2,383
Panel C: Crime Statistics				
	Mean	Median	St. Deviation	N
Total Crimes	14,633	7,355	22,811	2,383
Crime Growth	41.55	-39.00	2,892	2,383
Theft Growth	59.15	13.00	277.85	2,383
Burglary Growth	-18.78	-4.00	439.59	2,383
Assault Growth	2.48	-1.00	589.20	2,383
Homicide/Manslaughter Growth	0.26	0.00	6.43	2,383
Weapons Law Violations Growth	2.38	0.00	43.02	2,383
Law Agencies	179.79	132.00	172.20	2,383

Table 1: Descriptive Statistics – *Continued*

Panel D: Socio-demographic Statistics				
	Mean	Median	St. Deviation	N
African American	8.43	3.87	10.91	2,383
Age 15 - 24	13.64	13.18	2.40	2,383
Age 25 - 44	24.54	24.42	2.83	2,383
Age 45 - 64	27.70	27.17	2.45	2,383
College Educated	28.52	27.37	10.27	2,383
House Prices	130.58	128.65	21.72	2,383
Income	26,930	25,940	5,915	2,383
Male	49.25	49.14	1.05	2,383
Population	179.85	95.43	244.73	2,383
Unemployment Rate	7.32	6.86	2.78	2,383
Urban	65.11	66.10	21.51	2,383

Table 2: Effects of Bank Mergers on Loan Volumes

The table presents estimates from OLS panel regressions of the effects of bank mergers on peer-to-peer and Small Business Association (SBA) loan volumes. In Panel A, the dependent variable in the regressions is *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. In Panel B, the dependent variable in the regressions is *SBA Loan Vol.*, the natural log of the small business loan volume within the zip code during the year. The main explanatory variable is *CSE*, which takes a value of one during the year a bank merger occurs, and zero otherwise. Control variables included in the regressions are: *African American*, *Age 15–24*, *Age 25–44*, *Age 45–64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, and *Urban*. All variables are defined in Appendix I. All regressions include a control to account for an annual time trend. Standard errors are clustered by at the zip code level and $t-$statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	Panel A: P2P Loan Vol.			Panel B: SBA Loan Vol		
	(1)	(2)	(3)	(4)	(5)	(6)
CSE	0.354***	0258***	0.298***	-0.103***	-0.080***	-0.060**
	(3.84)	(4.09)	(4.11)	(-2.97)	(-2.79)	(-2.42)
Controls	N	Y	Y	N	Y	Y
Year FE	N	N	Y	N	N	Y
Zip Code FE	N	N	Y	N	N	Y
N	2,383	2,383	2,383	2,383	2,383	2,383
Adj. R-sq.	0.005	0.675	0.717	0.030	0.219	0.841

Table 3: Effects of Online Credit on Net Establishment Growth

The table presents estimates of the effects of online credit on net establishment growth. The dependent variable in the regressions is *Establishment Growth*, the annual net growth in total establishments within a community. Panel A shows estimates from OLS panel regressions. The main explanatory variable in Columns 1−4 is *CSE* × *P2P Loan Vol.*, which measures the effects of P2P lending within the community during the year following a bank merger. The other independent variables include *CSE*, which takes a value of one during the year a bank merger occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Panel B reports estimates from matched difference-in-difference regressions. The main explanatory variable in Columns 1 and 2 is *CSE* × *P2P Loan Vol.* Columns 3 and 4 use an alternative independent variable, *P2P Indicator*, which is an indicator that equals one if the community has access to the online credit market, and zero otherwise. The main explanatory variable in this specification is *CSE* × *P2P Indicator*, which captures the impact of online credit after a bank merger. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, and *Urban*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code level and *t*-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A: OLS Panel Regressions

	(1)	(2)	(3)	(4)
CSE	-17.565	-21.827	-39.803*	-36.927*
	(-0.72)	(-0.63)	(-1.85)	(-1.74)
P2P Loan Vol.	3.741***	4.256***	2.864***	1.694**
	(4.62)	(6.82)	(3.70)	(2.19)
CSE × P2P Loan Vol.	2.264	1.962	4.439**	3.946**
	(0.75)	(0.60)	(2.42)	(2.18)
Controls	N	Y	Y	Y
Year FE	N	N	Y	Y
Zip Code FE	N	N	N	Y
N	2,383	2,383	2,383	2,383
Adj. R-sq.	0.015	0.106	0.260	0.282

Panel B: Difference-in-Difference

	(1)	(2)	(3)	(4)
CSE	-31.173**	-23.740*	-62.582***	-65.227**
	(-2.70)	(-1.93)	(-3.49)	(-2.27)
P2P Loan Vol.	1.791	2.968		
	(1.06)	(1.55)		
CSE × P2P Loan Vol.	3.116**	2.998**		
	(2.10)	(2.22)		
P2P Area			-21.116	-36.536
			(-1.00)	(-1.63)
CSE × P2P Area			49.217*	46.279*
			(2.05)	(1.83)
Controls	N	Y	N	Y
Year FE	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y
N	656	656	656	656
Adj. R-sq.	0.193	0.213	0.401	0.416

Table 4: Matching Results

The table presents results from the matching exercise where lender communities are matched with non-lender communities (control). Communities are matched in the year prior to a bank merger based on the socio-demographic characteristics. Column (1) reports the average difference in means between matched treatment and control community pairs. Column (2) presents $t-$statistics for difference in means tests between matched treatment and control community pairs. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively. Definitions of all variables are presented in Appendix I.

	(1) Difference	(2) T-statistic	N
African American	4.234	(1.65)	328
Age 15 - 24	-0.934	(-1.16)	328
Age 25 - 44	3.053	(1.44)	328
Age 45 - 64	0.087	(0.17)	328
College Educated	-3.119	(-1.18)	328
House Prices	2.817	(1.01)	328
Income	-30.043	(-0.07)	328
Male	-0.247	(-0.76)	328
Population	5.010	(1.42)	328
Unemployment Rate	1.070	(1.40)	328
Urban	3.073	(0.58)	328

Table 5: Effects of Online Credit Across Establishment Sizes

The table presents regression estimates of the effects of online credit on the growth of establishments based on the number of employees. The dependent variable in Panel A (B, C, D) is the net growth in the number of firms with one to four (five to nine, ten to nineteen, twenty or more) employees. The main explanatory variable is *CSE × P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger. The other independent variables include *CSE*, which takes a value of one during the year a bank merger occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Columns 1, 3, and 5 report estimates from OLS panel regressions. Columns 2, 4, and 6 report estimates from matched difference-in-difference regressions. Control variables included in the regressions are: *African American*, *Age 15–24*, *Age 25–44*, *Age 45–64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, *Urban*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and t-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	Panel A: 1–4 Employees		Panel B: 5–9 Employees		Panel C: 10–19 Employees		Panel D: 20+ Employees	
	(1) OLS	(2) D-i-D	(3) OLS	(4) D-i-D	(5) OLS	(6) D-i-D	(7) OLS	(8) D-i-D
CSE	-28.563**	-25.790*	2.613	-15.693**	-2.218	-12.628*	-5.334	-5.415
	(-2.01)	(-1.69)	(0.40)	(-2.14)	(-0.37)	(-1.75)	(-1.02)	(-0.62)
P2P Loan Vol.	1.290**	0.043	-0.117	0.165	-0.439	-0.804	-0.446	0.589
	(2.04)	(0.03)	(-0.30)	(0.26)	(-1.24)	(-1.36)	(-1.42)	(1.31)
CSE × P2P Loan Vol.	3.074**	2.888**	-0.530	0.544	0.255	0.629	0.228	1.090
	(2.55)	(1.97)	(-0.91)	(0.77)	(0.44)	(0.86)	(0.41)	(1.59)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code/Pair-group FE	Y	Y	Y	Y	Y	Y	Y	Y
N	2,383	656	2,383	656	2,383	656	2,383	656
Adj. R-sq.	0.255	0.283	0.309	0.171	0.226	0.225	0.406	0.163

Table 6: Effects of Online Credit on Net Employment and Payroll Growth

The table presents regression estimates of the effects of online credit on growth in employment and establishment payroll. The dependent variable in Panel A is *Employment Growth*, the annual growth in the number of employees across establishments. The dependent variable in Panel B is *Payroll Growth: First Quarter*, the annual growth in the first quarter payroll across establishments. The dependent variable in Panel C is *Payroll Growth: Annual*, the growth in the annual payroll across establishments. The main explanatory variable is *CSE* × *P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger. The other independent variables include *CSE*, which takes a value of one during the year a bank merger occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Columns 1, 3, and 5 report estimates from OLS panel regressions. Columns 2, 4, and 6 report estimates from matched difference-in-difference regressions. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, and *Urban*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and t-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	Panel A: Employment Growth		Panel B: Payroll Growth: First Qtr.		Panel C: Payroll Growth: Annual	
	(1) OLS	(2) D-i-D	(3) OLS	(4) D-i-D	(5) OLS	(6) D-i-D
CSE	-496.112	-662.894	-13812.022	-45734.011*	-50056.884	-191655.800**
	(-0.64)	(-0.42)	(-0.71)	(-1.82)	(-0.70)	(-2.11)
P2P Loan Vol.	51.179	-141.075	3597.676	-3440.861	13488.129	-12425.761
	(0.82)	(-1.24)	(1.60)	(-1.27)	(1.61)	(-1.30)
CSE × P2P Loan Vol.	108.022*	325.430***	4868.623**	9238.286**	17144.430**	32136.033**
	(1.67)	(2.66)	(2.56)	(2.13)	(2.56)	(2.28)
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Zip Code/Pair-group FE	Y	Y	Y	Y	Y	Y
N	2,383	656	2,383	656	2,383	656
Adj. R-sq.	0.397	0.283	0.291	0.139	0.322	0.144

Table 7: Effects of Online Credit on Crime Growth

The table presents estimates of the effects of online credit on crime growth. The dependent variable in the regressions is *Crime Growth*, the annual growth in total crimes. Panel A shows estimates from OLS panel regressions. The main explanatory variable in Columns 1−4 is *CSE* × *P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger. The other independent variables include *CSE*, which takes a value of one during the year a bank merger occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the online loan volume within the zip code during the year. Panel B reports estimates from matched difference-in-difference regressions. The main explanatory variable in Columns 1 and 2 is *CSE* × *P2P Loan Vol.*. Columns 3 and 4 use an alternative independent variable, *P2P Indicator*, which is an indicator that equals one if the community has access to the online credit market, and zero otherwise. The main explanatory variable in this specification is *CSE* × *P2P Indicator*, which captures the impact of online credit after a bank merger. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, and *Urban*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code level and *t*-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A: OLS Panel Regressions

	(1)	(2)	(3)	(4)
CSE	187.713	324.360*	434.512*	381.983*
	(1.00)	(1.70)	(1.95)	(1.72)
P2P Loan Vol.	25.232**	36.790***	20.042	28.100
	(2.33)	(2.78)	(1.50)	(1.64)
CSE × P2P Loan Vol.	-50.616**	-58.006***	-74.915***	-63.755**
	(-2.46)	(-2.75)	(-2.91)	(-2.50)
Controls	N	Y	Y	Y
Year FE	N	N	Y	Y
Zip Code FE	N	N	N	Y
N	2,383	2,383	2,383	2,383
Adj. R-sq.	0.003	0.007	0.097	0.146

Panel B: Difference-in-Difference

	(1)	(2)	(3)	(4)
CSE	752.743**	659.620*	622.900**	648.551***
	(2.05)	(1.71)	(2.56)	(2.63)
P2P Loan Vol.	34.198	30.217		
	(1.03)	(1.21)		
CSE × P2P Loan Vol.	-109.448***	-100.140***		
	(-2.91)	(-2.78)		
P2P Area			333.641	414.481
			(1.23)	(1.52)
CSE × P2P Area			-503.02**	-485.228*
			(1.85)	(-1.77)
Controls	N	Y	N	Y
Year FE	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y
N	656	656	656	656
Adj. R-sq.	0.352	0.361	0.397	0.430

Table 8: Effects of Online Credit Across Crime Types

The table presents regression estimates of the effects of online credit on growth in crimes across crime types. The dependent variables in the regressions are the growth in crimes by type. The main explanatory variable is *CSE* × *P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger. The other independent variables include *CSE*, which takes a value of one during the year a bank merger occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the online loan volume within the zip code during the year. Panel A reports estimates for property-related crimes, including *Theft Growth* and *Burglary Growth*. Panel B shows estimates for crimes against persons and society, including *Assault Growth*, *Homicide/Manslaughter Growth*, and *Weapons Law Violations Growth*. Columns 1, 3, and 5 report estimates from OLS regressions. Columns 2, 4, and 6 report estimates from matched difference-in-difference regressions. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, *Urban*, and *Law Agencies*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and t−statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	Panel A: Property Crimes				Panel B: Personal and Societal Crimes					
	Theft		Burglary		Assault		Homicide/Manslaughter		Weapons Law Violations	
	(1) OLS	(2) D-i-D	(3) OLS	(4) D-i-D	(5) OLS	(6) D-i-D	(7) OLS	(8) D-i-D	(9) OLS	(10) D-i-D
CSE	79.499**	127.189***	95.097	287.293***	52.454	182.048	-0.213	0.529	1.114	-6.345
	(2.58)	(3.02)	(1.56)	(4.04)	(0.61)	(1.59)	(-0.18)	(0.40)	(0.26)	(-1.55)
P2P Loan Vol.	3.447**	3.033	0.958	1.571	3.740	0.459	0.032	-0.182	0.409	-0.870
	(2.00)	(1.04)	(0.37)	(0.29)	(0.64)	(0.05)	(0.73)	(-1.63)	(1.37)	(-1.55)
CSE × P2P Loan Vol.	-9.439***	-13.038***	-11.503*	-23.839***	-12.662	-12.723	0.054	0.108	-0.034	0.996
	(-3.14)	(-2.83)	(-1.77)	(-3.48)	(-1.29)	(-1.24)	(0.45)	(0.82)	(-0.06)	(1.35)
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code/Pair-group FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	2,383	656	2,383	656	2,383	656	2,383	656	2,383	656
Adj. R-sq.	0.175	0.293	0.173	0.362	0.194	0.262	0.074	-0.017	0.132	0.094

Table 9: Natural Disasters, Credit, and Economic and Social Effects

The table presents estimates of the effects of natural disasters on loan originations as well as the effects of online credit on economic growth and crime growth after a disaster. The dependent variables are: *P2P Loan Volume* (Column 1), *SBA Loan Volume* (Column 2), *Establishment Growth* (Column 3), *Employment Growth* (Column 4), *Payroll Growth: First Quarter* (Column 5), *Payroll Growth: Annual* (Column 6), and *Crime Growth* (Column 7). The main explanatory variable in Columns 1 through 2 is *CSE*, an indicator variable which equals one if a natural disaster occurred within the community during the year, and zero otherwise. The main explanatory variable in Columns 3 through 7 is *CSE* $\times$ *P2P Loan Vol.*, which captures the effect of P2P credit following a disaster. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, *Disaster Damage*, *Urban*, and in Column 7, *Law Agencies*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and $t-$statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	(1) P2P Loan Vol.	(2) SBA Loan Vol.	(3) Est. Growth	(4) Employ. Growth	(5) Payroll Gr.: First Qtr.	(6) Payroll Gr.: Annual	(7) Crime Growth
CSE	0.505**	0.314	-18.011*	-622.933	-26794.018**	-78979.116**	294.604**
	(2.07)	(0.88)	(-1.65)	(-1.21)	(-2.46)	(-2.13)	(2.00)
P2P Loan Vol.			0.382	-58.179	-2081.230	-6666.605	-21.034
			(0.21)	(-0.69)	(-1.21)	(-1.06)	(-0.90)
CSE $\times$ P2P Loan Vol.			3.538***	196.205***	5518.264***	18834.904***	-30.823**
			(3.68)	(3.27)	(2.96)	(2.99)	(-1.98)
Controls	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Zip Code/Pair-group FE	Y	Y	Y	Y	Y	Y	Y
N	864	864	864	864	864	864	864
Adj. R-sq.	0.662	0.736	0.362	0.176	0.278	0.308	0.239

Table 10: Impacts of Contemporaneous Crime Growth

The table presents estimates of the effects of contemporaneous crime growth on bank mergers, P2P loan volume, and economic growth. Columns 1 and 2 (3) in Panel A report marginal effects from probit (OLS) regressions where the dependent variable is *Bank Merger*. *Bank Merger* is equal to one if a bank merger occurred within the zip code area during the year, and zero otherwise. Columns 4 and 5 present estimates from OLS panel regressions where the dependent variable is *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Columns 6 and 7 report estimates from OLS panel regressions where the dependent variable is *Establishment Growth*, the annual growth in the number of establishments within the community. Columns 8 and 9 report estimates from OLS panel regressions where the dependent variable is *Employment Growth*, the annual growth in the number of employees within a community. Columns 10 and 11 present estimates from OLS panel regressions where the dependent variable is *Payroll Growth: First Quarter*, the yearly growth in the total first quarter payroll across establishments within a community. Columns 12 and 13 present estimates from OLS panel regressions where the dependent variable is *Payroll Growth: Annual*, the yearly growth in the total annual payroll across establishments within a community. The primary independent variable in the regressions is *Crime Growth*, the yearly growth in crimes within a zip code. The estimates for *Crime Growth* have been multiplied by 10,000 for in columns 1−7. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, *Urban* and *Law Agencies*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code level and $z-$statistics (Columns 1 and 2) or $t-$statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	Bank Merger			P2P Loan Vol.		Est. Growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Crime Growth	0.007	0.018	-0.001	0.205	0.271	21.340	22.044
	(0.26)	(0.63)	(-0.02)	(0.74)	(1.34)	(1.05)	(1.20)
Controls	N	Y	Y	N	Y	N	Y
Year FE	N	N	Y	N	Y	N	Y
Zip Code FE	N	N	Y	N	Y	N	Y
N	2,383	2,383	2,383	2,383	2,383	2,383	2,383
Pseudo/Adj. R-sq.	0.007	0.036	0.187	0.267	0.680	0.058	0.526

	Employment Growth		Payroll Gr.: First Qtr.		Payroll Gr.: Annual	
	(8)	(9)	(10)	(11)	(12)	(13)
Crime Growth	-0.102	-0.201	-1.925	-3.861	-9.459	-15.978
	(-0.69)	(-1.04)	(-0.88)	(-1.22)	(-1.04)	(-1.23)
Controls	N	Y	N	Y	N	Y
Year FE	N	Y	N	Y	N	Y
Zip Code FE	N	Y	N	Y	N	Y
N	2,383	2,383	2,383	2,383	2,383	2,383
Adj. R-sq.	0.068	0.194	0.034	0.288	0.043	0.344

Table 11: Effects of Online Credit: Post-2009 Sample

The table presents estimates from matched difference-in-difference regressions of the real and social effects of online credit between 2010 and year-end 2015. The dependent variables are *Establishment Growth* (Column 1), *Employment Growth* (Column 2), *Payroll Growth: First Quarter* (Column 3), *Payroll Growth: Annual* (Column 4), and *Crime Growth* (Column 5). The main explanatory variable is *CSE* × *P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger (Panel A) or natural disaster (Panel B). The other independent variables include *CSE*, which takes a value of one during the year a bank merger natural disaster occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Control variables included in the regressions are: *African American*, *Age 15–24*, *Age 25–44*, *Age 45–64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, *Urban*, and in Column 5, *Law Agencies*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and *t*-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A: Credit Scarcity Events: Bank Mergers					
		(2) Employ.	(3) Payroll Gr.:	(4) Payroll Gr.:	(5) Crime
	(1) Est. Growth	Growth	First Qtr.	Annual	Growth
CSE	-54.002*	-2059.173	-133509.200**	-387917.702*	775.745*
	(-1.66)	(-1.17)	(-2.26)	(-1.89)	(1.81)
P2P Loan Vol.	5.112	-13.981	-1331.744	-820.0142	-23.204
	(1.15)	(-0.14)	(-0.65)	(-0.09)	(-0.51)
CSE × P2P Loan Vol.	4.595*	335.520***	11303.501***	34078.422**	-76.256**
	(1.99)	(2.70)	(2.69)	(2.21)	(-2.02)
Controls	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y	Y
N	508	508	508	508	508
Adj. R-sq.	0.322	0.369	0.144	0.166	0.385

Panel B: Credit Scarcity Events: Natural Disasters					
		(2) Employ.	(3) Payroll Gr.:	(4) Payroll Gr.:	(5) Crime
	(1) Est. Growth	Growth	First Qtr.	Annual	Growth
CSE	-34.627**	-1627.286**	-40138.79**	-150032.6**	498.146***
	(-2.21)	(-2.23)	(-2.37)	(-2.49)	(3.51)
P2P Loan Vol.	3.027*	58.650	-479.3893	-1357.55	-47.304
	(2.06)	(0.94)	(-0.31)	(-0.26)	(-0.91)
CSE × P2P Loan Vol.	3.004***	147.356**	5210.866***	17027.620**	-48.277*
	(2.96)	(2.29)	(2.70)	(2.61)	(-2.01)
Controls	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y	Y
N	748	748	748	748	748
Adj. R-sq.	0.294	0.286	0.199	0.207	0.265

Table 12: Excluding Business Loans

The table presents estimates from matched difference-in-difference regressions of the real and social effects of online credit excluding business loans. The dependent variables are *Establishment Growth* (Column 1), *Employment Growth* (Column 2), *Payroll Growth: First Quarter* (Column 3), *Payroll Growth: Annual* (Column 4), and *Crime Growth* (Column 5). The main explanatory variable is *CSE* $\times$ *P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger (Panel A) or natural disaster (Panel B). The other independent variables include *CSE*, which takes a value of one during the year a bank merger natural disaster occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Unemployment Rate*, *Urban*, and in Column 5, *Law Agencies*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and t-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A: Credit Scarcity Events: Bank Mergers

	(1) Est. Growth	(2) Employ. Growth	(3) Payroll Gr.: First Qtr.	(4) Payroll Gr.: Annual	(5) Crime Growth
CSE	-21.497*	-1981.704	-73925.672**	-297744.811***	669.244*
	(-1.79)	(-1.11)	(-2.48)	(-2.76)	(1.71)
P2P Loan Vol.	2.982	-168.427	-3444.155	-12265.701	-10.271
	(-1.54)	(-1.50)	(-1.20)	(-1.25)	(-0.30)
CSE × P2P Loan Vol.	2.849**	342.931**	10438.77**	36233.622**	-75.225**
	(2.21)	(2.58)	(2.47)	(2.62)	(-2.00)
Controls	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y	Y
N	656	656	656	656	656
Adj. R-sq.	0.349	0.296	0.138	0.145	0.318

Panel B: Credit Scarcity Events: Natural Disasters

	(1) Est. Growth	(2) Employ. Growth	(3) Payroll Gr.: First Qtr.	(4) Payroll Gr.: Annual	(5) Crime Growth
CSE	-19.876*	-615.945	-26857.71**	-78510.361**	286.277*
	(-1.72)	(-1.19)	(-2.48)	(-2.12)	(1.95)
P2P Loan Vol.	-0.715	-67.326	-2079.183	-6860.923	-25.823
	(-0.33)	(-0.82)	(-1.24)	(-1.12)	(-1.08)
CSE × P2P Loan Vol.	4.078***	199.851***	5571.136***	18996.320***	-30.290*
	(3.90)	(3.33)	(3.00)	(3.03)	(-1.94)
Controls	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y	Y
N	864	864	864	864	864
Adj. R-sq.	0.378	0.296	0.183	0.31	0.281

Appendix 1: Variable Definitions

This table describes the variables used in the empirical analysis.

Key Explanatory Variables	Definition
Bank Merger	Indicator which equals one if a bank merger occurred within the community during the year, and zero otherwise.
Disaster	Indicator equal to one if a natural disaster occurred within the community during the year, and zero otherwise.
Credit Scarcity Event	Indicator equal to one when a community is impacted by a credit scarcity event (e.g., bank mergers and natural disasters).
P2P Loan Volume	The natural log of the total amount of loans issued within the community during the year.
P2P Area	Indicator variable which equals one if the community has access to P2P credit, and zero otherwise.
P2P Credit Variables	**Definition**
Funding Percent	The average ratio of funds requested to funds received across all loans within a zip code area during the year.
Credit Score	The average Lending Club-assigned credit rating across all borrowers within a zip code area during the year. The scores range from one (the most creditworthy) to seven (the least creditworthy).
FICO	Average FICO score across borrowers within a community each year.
Interest Rate	Average interest rate across loans issued within a community each year.
Borrower Income	The average income of borrowers within a zip code area during the year.
Debt-to-Income	The average debt-to-income ratio across all borrowers within a zip code area during the year.
Revolving Credit Utilization	Average revolving credit utilization rate across borrowers within a community during the year.
Economic Variables	**Definition**
Establishment Growth	Total annual growth in the number of establishments within the three-digit zip code area. Data are from the County Business Patterns series by the U.S. Census Bureau.
Est. Growth: 1−4 Employees	Yearly growth in establishments with one to four employees.
Est. Growth: 5−9 Employees	Yearly growth in establishments with five to nine employees.
Est. Growth: 10−19 Employees	Yearly growth in establishments with ten to nineteen employees.
Est. Growth: 20+ Employees	Yearly growth in establishments with twenty or more employees.
Employment Growth	Annual growth in the County Business Patterns' Mid-March Employees series within a community.
Payroll Growth: First Quarter	Annual growth in the County Business Patterns' First Quarter Payroll series within a zip code area. Numbers are in thousands.
Payroll Growth: Annual	Annual growth in the County Business Patterns' Total Annual Payroll series within a zip code area. Numbers are in thousands.
Total SBA Loan Volume	The total volume of 7(a) and 504 loans supported by the Small Business Administration within a community during the year.
SBA Loan Volume	The natural log of the total volume of 7(a) and 504 loans supported by the Small Business Administration within a community during the year.
SBA Interest Rate	The average interest rate across all SBA loans issued within a community during the year.

Crime Variables	Definition
Total Crimes	The yearly total number of crimes in a community. The data are from the National Incident-based Reporting System.
Crime Growth	The growth in the total number of crimes within the three-digit zip code area during the year. The data are from the National Incident-based Reporting System.
Assault Growth	The annual growth in aggravated assaults, simple assaults, and intimidation crimes within a three-digit zip code area. The UCR codes are 131 132, and 133, respectively.
Burglary Growth	The yearly growth in burglary and breaking and entering crimes. The UCR code is 220.
Homicide/Manslaughter Growth	The yearly increase in murder/non-negligent manslaughter, negligent manslaughter, and justifiable homicides. The UCR codes are 91, 92, and 93, respectively.
Theft Growth	The annual growth in the number of pocket-picking, purse-snatching, and shoplifting crimes. The UCR codes are 231, 232, and 233, respectively.
Weapon Law Violation Growth	The yearly growth in weapon law violations which are violations of laws or ordinances prohibiting the manufacture, sale, purchase, transportation, possession, concealment, or use of firearms, cutting instruments, explosives, incendiary devices, or other deadly weapons. The UCR code is 520.
Law Agencies	The number of law enforcement agencies within each zip code area that participate in NIBRS during the year.

Socio-demographic Variables	Definition
African American	The annual proportion of the three-digit zip code population that is African American. Data are from the U.S. Census Bureau.
Age 15 - 24	The percentage of the three-digit zip code population that is between 15 and 24 years old. Data are from the U.S. Census Bureau.
Age 25 - 44	The percentage of the three-digit zip code population that is between 25 and 44 years old. Data are from the U.S. Census Bureau.
Age 45 - 64	The percentage of the three-digit zip code population that is between 45 and 64 years old. Data are from the U.S. Census Bureau.
College Educated	The annual proportion of the three-digit zip code population that has graduated from college. Data are from the U.S. Census Bureau.
Disaster Damage	The yearly amount of crop and property damage caused by natural disasters within a community.
House Prices	The annual three-digit zip code-level All-transactions House Price Indexes. The data are from the Federal Housing Finance Agency.
Income	Median per capita income among households within the three-digit zip code during the year. Data are from the U.S. Census Bureau.
Male	The annual proportion of the three-digit zip code population that is male. Data are from the U.S. Census Bureau.
Population	The annual, three-digit zip code-level population. The data are from the U.S. Census Bureau and scaled by 1,000.
Unemployment Rate	The annual, three-digit zip code-level unemployment rate. The data are from the Bureau of Labor Statistics.
Urban	The percent of the community's population that is resides in an urban area. Data are from the U.S. Census Bureau.

Appendix 2: Post-CSE Borrower and Market Characteristics

In this section, I examine whether borrower quality in the online market is impacted by reduced access to traditional credit and whether lenders respond. I first examine changes in borrower quality using several measures, including borrowers' reported incomes, debt-to-income ratios, revolving credit utilization, and FICO scores. To examine lenders' responses, I construct measures of loan interest rates and the ratio of funds received to funds requested on the loan application.

In Table A2.1, I report estimates from OLS panel regressions examining borrower characteristics and lending responses surrounding credit scarcity events. Overall, I find no systematic or significant changes in borrower quality around bank mergers (Panel A) or natural disasters (Panel B). Similarly, lenders do not display significant changes in their supply of funds to borrowers.

Table A2.1: Post-CSE Borrower Characteristics and Lender Response

The table presents estimates from OLS panel regressions of the effect of credit scarcity on online borrowers' quality and lenders' responses. Specifically, the dependent variable in Column 1 is *Borrower Income*, the average income of borrowers within a zip code area in a year. The dependent variable in Column 2 is *Debt-to-Income*, the yearly average debt-to-income ratio across borrowers within a zip code. The dependent variable in Column 3 is *Revolving Credit Utilization*, the yearly average revolving credit utilization rate across borrowers within a zip code. The dependent variable in Column 4 is *FICO*, the yearly average FICO score across borrowers within a zip code. The dependent variable in Column 5 is *Interest Rate*, the yearly average loan interest rate across borrowers within a zip code. The dependent variable in Column 6 is *Funding Percent*, the yearly average ratio of the total funds provided by lenders to the funds requested on the loan application across borrowers within a zip code. In Panel A (B), the main explanatory variable is *CSE*, which takes a value of one if the community experiences a bank merger (natural disaster) during the year, and zero otherwise. Control variables included in the regressions are: *African American*, *Age 15–24*, *Age 25–44*, *Age 45–64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Urban*, and *Unemployment Rate*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code level and $t-$statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A: Credit Scarcity Events: Bank Mergers						
	(1) Borrower Income	(2) D-t-I	(3) Revol. Credit Util.	(4) FICO	(5) Interest Rate	(6) Fund. Pct.
CSE	-534.744	0.333	0.088	15.941	0.198	0.021
	(-0.40)	(0.99)	(0.08)	(1.22)	(0.87)	(1.17)
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Zip Code FE	Y	Y	Y	Y	Y	Y
N	2,383	2,383	2,383	2,383	2,383	2,383
R-sq.	0.324	0.698	0.447	0.616	0.677	0.357
Panel B: Credit Scarcity Events: Natural Disasters						
	(1) Borrower Income	(2) D-t-I	(3) Revol. Credit Util.	(4) FICO	(5) Interest Rate	(6) Fund. Pct.
CSE	315.944	-0.048	0.417	-0.491	0.042	-0.000
	(0.39)	(-0.45)	(0.96)	(-0.89)	(0.96)	(-0.05)
Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Zip Code FE	Y	Y	Y	Y	Y	Y
N	2,383	2,383	2,383	2,383	2,383	2,383
R-sq.	0.246	0.672	0.340	0.529	0.594	0.229

Appendix 3: Online Credit's Influence on Bank Mergers

In this section, I examine whether access to the online credit market influences banks' merger activities. This may occur if increasing adoption of the alternative credit market impacts banks' operations and leads to efficiency gains through merging. I test this hypothesis by regressing *Bank Merger* on contemporaneous *P2P Loan Vol.* and *P2P Loan Vol.* in the year prior to a merger.

In Table A3.1, I report estimates from OLS panel regressions and marginal effects from probit regressions. The estimates from the regressions are not significantly different from zero, which suggests that online borrowing is not the primary driver of banks' merger activities in my sample.

Table A3.1: Online Credit's Impact on Bank Merger Activity

The table presents estimates of the effects of P2P lending on banks' merger activity. The dependent variable is *Bank Merger*, which takes a value of one during the year following a bank merger, and zero otherwise. The main explanatory variables in the regressions are *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year, and *P2P Loan Vol.*$_{t-1}$, the natural log of the loan volume in the year prior to a merger. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Population*, *Urban*, and *Unemployment Rate*. Column (1) reports marginal effects from probit regressions. Column (2) reports estimates from OLS panel regressions. All variables are defined in Appendix I. Standard errors are clustered by at the zip code level while $z-$statistics (Column 1) and $t-$statistics (Column 2) are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

	(1) Probit	(2) OLS
P2P Loan Vol.	-0.007	-0.004
	(-1.44)	(-1.27)
P2P Loan Vol.$_{t-1}$	-0.003	-0.002
	(-0.83)	(-0.88)
Controls	Y	Y
Year FE	N	Y
Zip Code FE	N	Y
N	2,383	2,383
R-sq.	0.172	0.188

Appendix 4: Per Capita Estimates

While I control for communities' population sizes in all my empirical tests, I re-perform the analyses on a per capita basis. That is, I adjust my key dependent variables by the community's total population. I report the estimates from matched difference-in-difference tests in Table A4.1. Panel A presents estimates when using bank mergers as credit scarcity events. Panel B reports estimates from natural disaster-based credit scarcity. Overall, the results are consistent with my principal analysis. Specifically, on a per capita basis, reduced availability of traditional credit is associated with diminished economic growth and rising crime. Conversely, online borrowing mitigates the negative effects of scarcity.

Table A4.1: Per Capita Estimates

The table presents per capita estimates from matched difference-in-differences tests of the real and social effects of online lending. The table presents estimates from matched difference-in-difference regressions of the real and social effects of online credit excluding business loans. The dependent variables are *Establishment Growth* (Column 1), *Employment Growth* (Column 2), *Payroll Growth: First Quarter* (Column 3), *Payroll Growth: Annual* (Column 4), and *Crime Growth* (Column 5). The main explanatory variable is *CSE* × *P2P Loan Vol.*, which measures the effects of online lending within the community during the year following a bank merger (Panel A) or natural disaster (Panel B). The other independent variables include *CSE*, which takes a value of one during the year a bank merger natural disaster occurs, and zero otherwise, and *P2P Loan Vol.*, the natural log of the P2P loan volume within the zip code during the year. Control variables included in the regressions are: *African American*, *Age 15−24*, *Age 25−44*, *Age 45−64*, *College Educated*, *House Prices*, *Income*, *Male*, *Unemployment Rate*, *Urban*, and in Column 5, *Law Agencies*. All variables are defined in Appendix I. Standard errors are clustered by at the zip code-level and *t*-statistics are presented in parentheses. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A: Credit Scarcity Events: Bank Mergers					
	(1) Est. Growth	(2) Employ. Growth	(3) Payroll Gr.: First Qtr.	(4) Payroll Gr.: Annual	(5) Crime Growth
CSE	-0.00061***	-0.03689	-1.24622*	-4.51337*	0.00375*
	(-2.66)	(-0.54)	(-1.66)	(-1.74)	(1.93)
P2P Loan Vol.	0.00000	-0.01019	-0.13716*	-0.45800	0.00005
	(0.12)	(-1.64)	(-1.69)	(-1.62)	(0.32)
CSE × P2P Loan Vol.	0.00004**	0.01145**	0.27310**	0.82743**	-0.00058***
	(2.03)	(2.01)	(2.36)	(2.49)	(-2.71)
Controls	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Pair FE	Y	Y	Y	Y	Y
N	656	656	656	656	656
R-sq.	0.207	0.074	0.055	0.069	0.339
Panel B: Credit Scarcity Events: Natural Disasters					
	(1) Est. Growth	(2) Employ. Growth	(3) Payroll Gr.: First Qtr.	(4) Payroll Gr.: Annual	(5) Crime Growth
CSE	-0.00089**	-0.04079	-0.60493*	-2.07678*	0.01246***
	(-2.10)	(-1.35)	(-1.86)	(-1.72)	(3.46)
P2P Loan Vol.	0.000057	-0.00685	-0.08721	-0.29986	0.00064
	(0.90)	(-1.11)	(-1.41)	(-1.27)	(1.26)
CSE × P2P Loan Vol.	0.00009*	0.00592**	0.10511**	0.36698**	-0.00088**
	(1.84)	(2.31)	(2.29)	(2.20)	(-2.27)
Controls	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Pair-group FE	Y	Y	Y	Y	Y
N	864	864	864	864	864
Adj. R-sq.	0.313	0.183	0.343	0.347	0.120