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Digital Multisided Platforms and Women's Health: An Empirical Analysis of Peer-to-Peer Lending and Abortion Rates

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Abstract. Digital multisided platforms and peer-to-peer marketplaces are increasingly attracting attention from scholars, with a significant amount of work examining their societal implications. Despite such focus, limited work has investigated the social implications of the democratization of capital by peer-to-peer (P2P) lending platforms, particularly within the healthcare sector, where financial constraints can be daunting. In this study, we investigate the effects of P2P lending platforms on local abortion rates, a medical procedure characterized by significant financial barriers and social stigma. Leveraging the entry of the P2P platform LendingClub into different states at different times, and a differences-in-differences approach, we find that the entry of LendingClub is associated with an increase in the rate at which women choose not to carry to term. Further, results indicate the effects vary based on the characteristics of the local area, with stronger effects manifesting in more religious areas and areas with lower levels of education, suggesting that social frictions in the form of stigma and knowledge asymmetries can influence financial need. Theoretical and practical implications are discussed within.

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Keywords: multisided digital platforms • peer-to-peer lending • medical financing • stigma • medical loans • abortion • causal inference

An abortion ban only hurts women who lack the means to go someplace else.

–Ruth Bader Ginsburg[1]

Introduction

The societal implications of digital multisided platforms have received increased attention by information systems (IS) scholars in recent years, with emerging work investigating a variety of social outcomes ranging from the prevalence of sexually transmitted diseases to drunk driving, changes in the labor force, and charitable giving (Chan and Ghose 2014, Chan et al. 2016, Greenwood and Agarwal 2016, Greenwood and Wattal 2017, Burtch and Chan 2019, Wang and Overby 2021). Within this body of research, scholars have noted that digital platforms both enhance communication and reduce transaction costs, leading to striking shifts in behavior. In the case of drunk driving, ridesharing platforms increase the accessibility of transportation and address a significant social need. In the short-term dating context, matching platforms reduce the cost to discover partners (Bapna et al. 2016). And in the case of peer-to-peer (P2P) lending, platforms reduce barriers that limit access to capital (Wang and Overby 2021). In this paper, we build on this stream by considering how the entry of P2P lending platforms that increase liquidity influences social outcomes by increasing access to medical care (Freedman and Jin 2008). The introduction of P2P lending has in many ways democratized financial markets, thereby increasing access to capital for would-be borrowers (Lin et al. 2013, Wang and Overby 2021). We consider the impact of P2P lending on a social outcome that has serious policy implications and is subject to significant social stigma: women's access to medical care in the form of abortion services.

A woman's decision to terminate a pregnancy remains a hot-button social and political issue in the United States, with state and federal governments implementing statutory rules that either increase, or limit, access to such procedures (Matthews et al. 1997). The decision-making process that underlies accessing care is complex, and prior work identifies several factors that affect both how a woman may choose and how aggregate policies influence those decisions (Harper et al. 2005). These range from individual-level factors like socioeconomic status, religious norms, privacy and security, and the presence of family and support systems; to macroeconomic characteristics like the availability of clinics, regulatory burden on physicians and nurses, and which services are offered (Jones and Zolna 2008, Jones and Jerman 2014, 2017).

Yet, there is another factor that is often overlooked—*financial costs*, which are often nontrivial (Medoff 2008, Jones et al. 2013, Roberts et al. 2014). Whereas abortion services come in several forms (ranging from pharmacological options like mifepristone to surgical intervention), the typical cost of a first-trimester procedure varies between $490 and $1,320, without accounting for travel, lost wages, and postprocedure recovery (Jones and Kooistra 2011, Jones et al. 2013). These financial costs, and the factors that influence them, are a common legal tool used by policymakers to deter women from seeking care (Ginsburg 1985; an example is the Abortion Providers Loan Elimination Act [HR 7792][2]) and can include the cost of the procedure itself, travel expenses, lost wages from missed work, and postoperative care (Beauchamp 2016). Each of these costs can increase rapidly based on clinical availability and local regulation (Jerman et al. 2016, Bearak et al. 2017). Indeed, there is considerable anecdotal evidence of women driving hundreds of miles and planning to sleep in their cars to secure out-of-state care[3] and paying tens of thousands of dollars to secure the procedure when local statute prevents them from accessing care,[4] as well as evidence of women being put in significant financial straits when attempting to obtain an abortion.[5] Popular press outlets from NPR,[6] to *Glamour*,[7] to even *Teen Vogue*,[8] have run serious exposés that highlight the economic costs of aborting a pregnancy. It should therefore come as no surprise that women have sought out many forms of funding (including acquiring debt; Figure 1) when either terminating a pregnancy or seeking treatment for broader reproductive care.[9]

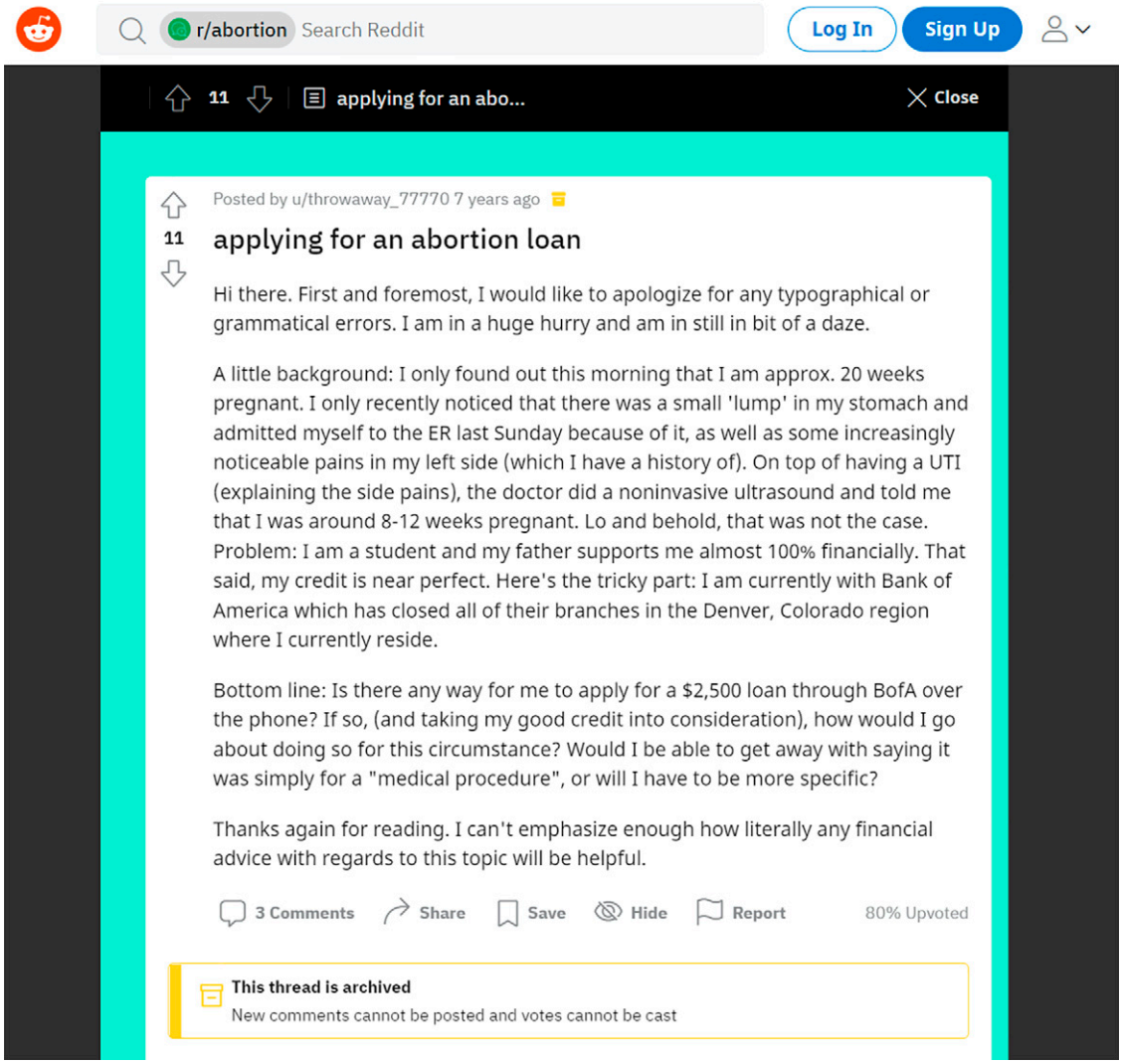
The presence of these barriers is compounded by the fact that the procedure is rarely covered by private insurance (Jones et al. 2013), and the Hyde Amendment (18 U.S.C. §3006A) prevents federal dollars from being used (Jones et al. 2010, 2013). This is of particular concern for women of lower socioeconomic status (i.e., those on Medicaid), who are disproportionately likely to seek out such care. Thus, immediate short-term access to capital, that is, during the time that the procedure is feasible (before viability at 24–28 weeks), is critical. Still, the implications of the difficulty of accessing capital remains understudied in academic research. In addressing this gap, we hypothesize that P2P platforms may reduce financial barriers by offering private and flexible access to capital, thereby allowing women to circumvent the social and financial barriers to care.

We test this central thesis by exploiting the rollout of a leading P2P lending platform, LendingClub. Building on the rationale that access to capital will reduce the financial barriers that women face when accessing abortion services (Matthews et al. 1997, Jerman and Jones 2014, Wang and Overby 2021), our expectation is a positive relationship between the platform's entry and the number of women who elect not to carry to term. Leveraging a difference-in-differences approach, we identify the observed effect by casting locations where the platform has entered as treated, and casting locations where the platform has yet to arrive as control. This identification strategy is based on the fact that the residents of a state can only access capital from these platforms once the platform is legally allowed to operate by local regulators. This identification strategy is plausible for two reasons. First, intuitively, it is unlikely that LendingClub is sampling on the abortion rate in local areas when making the decision to enter a market (as evidenced by the platform attempting to enter all 50 states simultaneously). Second, and more importantly, state regulators were the parties responsible for approving LendingClub's entry, and did so conditional upon adherence to state-level bank chartering rules (Sinha and Snow 2017). The control group is thus comprised of states where the platform was not permitted to enter due to incompatibility with established banking statute.

To gain insight into the relationship between the platform and local procedure rates, we conduct our analysis at the level of the *county*, rather than the state (results are consistent at the state level). Results show a significant relationship between the entry of the LendingClub platform and the per-capita abortion rate, despite the overall downward trend in the number of procedures conducted over the last two decades (Jones and Jerman 2017). In terms of effect size, our results indicate that entry is associated with roughly 675 additional procedures annually. This underscores the degree to which access to capital may have prevented women from accessing care. Our findings are robust to multiple falsification tests, including examining pretreatment trends, placebos, and alternate specifications.

Owing to the inherent sensitivity of the procedure, and the stigma associated with it, we explore heterogeneity in the estimated treatment effect mentioned above. Given that a quarter of American women will make the decision to terminate a pregnancy before they are 45 (Jones and Jerman 2017) and that 57% of women who consider terminating a pregnancy experience some kind of harassment (Jones and Kooistra 2011), it is unsurprising that social barriers (e.g., social sigma or emotional backlash from friends, families, and society at large) may exacerbate the financial costs faced by women. We explore two regional factors that may influence such social costs: differences in education (including sex education) and local religiosity. Findings indicate that the effects of platform entry are lower in counties with higher-than-median education levels. Interestingly, in states where sex education is

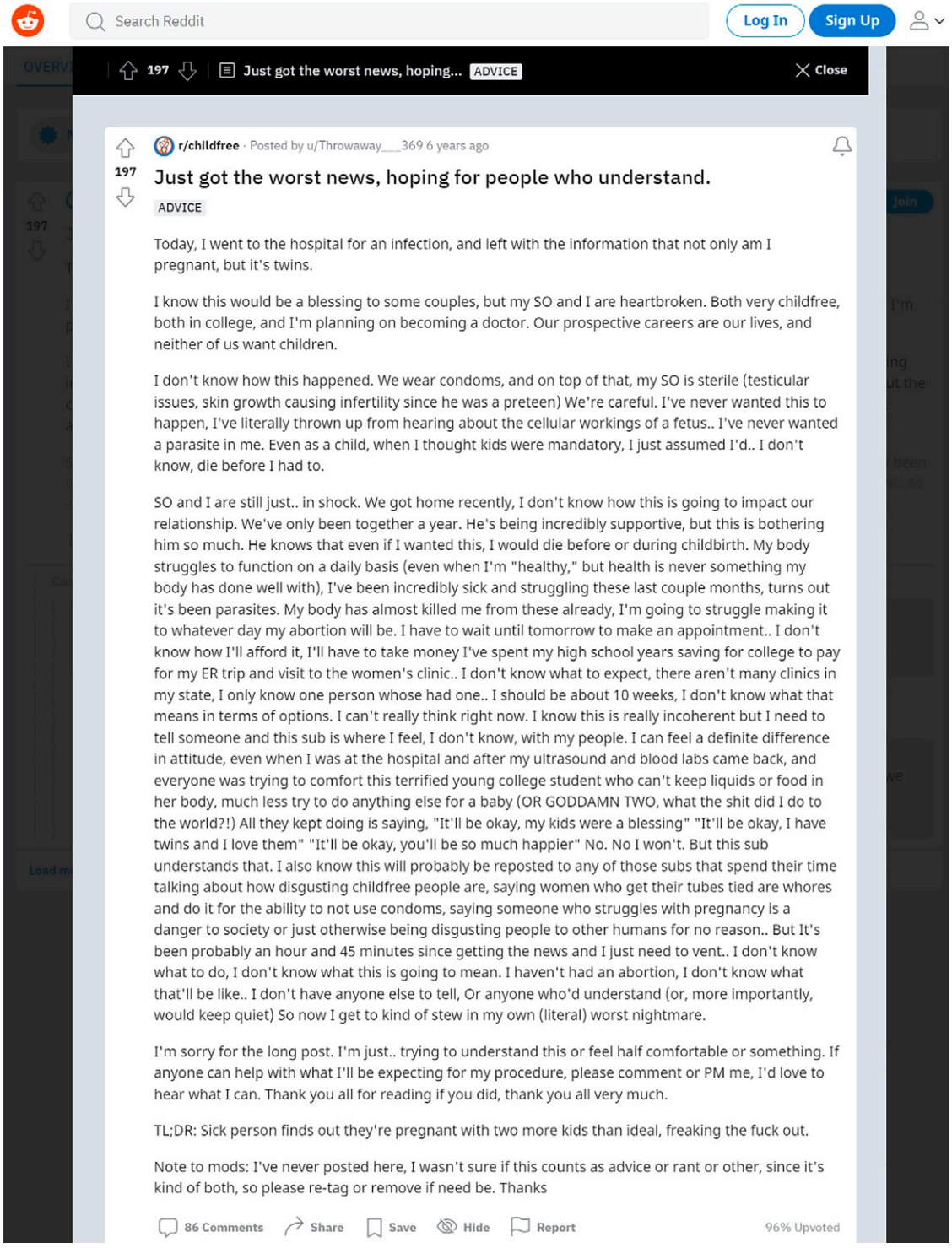
Figure 1. (Color online) Anecdotal Supporting Evidence for Financial and Social Barriers to Abortion and Attempts to Overcome Them Through Digital Means



mandated, the effects of platform entry are also lower. This may suggest diminished need for abortion services when education, particularly sex education, is higher. Further, in counties with higher levels of religious affiliation, the effect of entry is larger, suggesting that localized abortion-related stigma in these areas may reduce the availability or use of other sources of funding. These results add nuance to our central finding that P2P lending appears to facilitate access to abortion services by highlighting the effect of local social barriers.

Our study contributes to two streams of literature. First, we contribute to the literature on digital platforms, particularly in the healthcare context. As platforms attract more attention, academic work has expanded to examine their broader economic and social implications (Greenwood and Agarwal 2016, Greenwood and Wattal 2017, Wang and Overby 2021). Yet, much of this work has focused on either the economic implications of platform participation (Burtch et al. 2018, Wang and Overby 2021) or health outcomes resulting from platform participation (Chan and Ghose 2014, Greenwood and Wattal 2017), rather than how the democratization of capital might affect healthcare decision making more broadly. We extend the literature on P2P lending to illustrate how persons facing decisions involving financial and social hardship may circumnavigate liquidity barriers by leveraging such capital. In doing so, we speak directly to the impact that the availability of capital has in obviating healthcare-related financial and social frictions. Our findings further contribute to the literature in

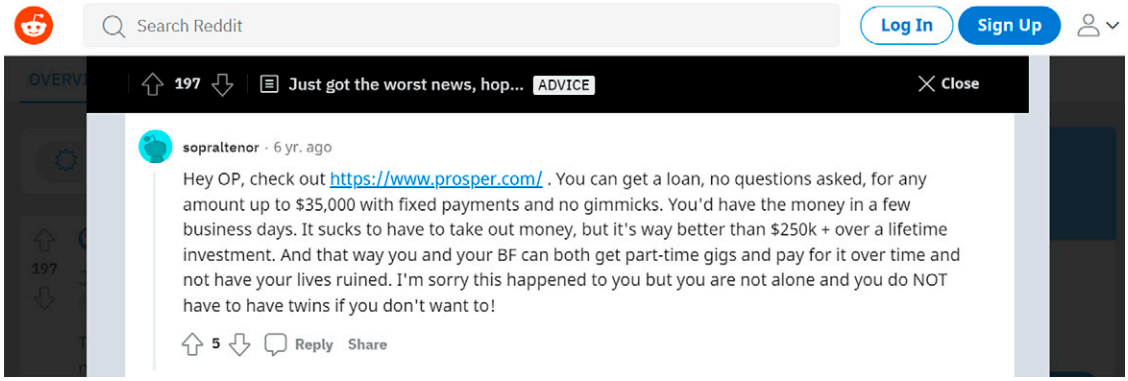
Figure 1. (Continued)



public health by implicating P2P lending as an alternative and viable source of capital, especially where insurance markets are thin. Inasmuch as cost continues to be a significant barrier to access a wide range of critical health services (Gohmann and Ohsfeldt 1993, Medoff 2008, Wilson 2009), our work provides evidence for the role that technology platforms may play.

Finally, our work further contributes to the ongoing legal conversation in the United States related to abortion regarding what constitutes an "undue burden."

Figure 1. (Continued)



Since *Roe v. Wade* (1973), the decision establishing a woman's right to choose, the legal test for when the State can advocate on behalf of the unborn has changed dramatically. Chiefly, in *Planned Parenthood v. Casey* (1992), the Court held that the State could regulate the procedure prior to viability as long as it did not place an "undue burden" on the mother (see Schneider 1993 and Borgmann 2009 for discussions).[10] Yet, most empirical work on the economic effects of abortion has focused on downstream implications stemming from legality (Donohue and Levitt 2001, Bitler and Zavodny 2002), rather than microeconomic factors that might prevent women from accessing care at the point where this care is delivered. Although our work cannot speak to the legal definition of an undue burden, and does not seek to, we believe it is important to provide rigorous empirical evidence on how restrictions affect women's access to care, and to provide information to the legal community to better inform decisions. We hope this serves as a call for work examining factors that materially impact women's ability to access health services.

Related Literature

> [D]isparity in funding by the State clearly operates to coerce indigent pregnant women to bear children they would not otherwise choose to have, and just as clearly, this coercion can only operate upon the poor, who are uniquely the victims of this form of financial pressure.
>
> —Thurgood Marshall (*Maher v. Roe*, 1977)

Digital multisided platforms create markets by facilitating transactions and efficiently charging the participants who elect to use the platform as an economic space without taking ownership of the transacting entities (Rochet and Tirole 2003, 2004; Rysman 2009; Hagiu and Wright 2015). In doing so, these platforms reduce market inefficiencies by decreasing information asymmetries and transaction costs (Bakos 1997, Brynjolfsson and Smith 2000, Rysman 2009). In addition to increasing market efficiency, successful multisided platforms create and rely on network externalities that benefit both buyers, through lower fees and increased match quality, and sellers, through increased market size (Rochet and Tirole 2004, Armstrong 2006). As a result, in markets where platforms emerge, incumbents often face increased competition, or even displacement, while platform participants benefit from increases in market efficiency (Parker and Van Alstyne 2005, Chakravorti and Roson 2006, Tucker and Zhang 2010, Seamans and Zhu 2014, Zervas et al. 2017).

In practice, these effects are easy to observe, and have been widely noted by the academy and the popular press. Examples of successful digital platforms span both traditional online retailers like eBay and Amazon (Brynjolfsson et al. 2003, Dellarocas and Wood 2008), which allow economic agents to costlessly operate and browse digital storefronts, as well as de novo gig-economy platforms like Uber and Airbnb (Horton 2015, Greenwood et al. 2017, Zervas et al. 2017), which allow individuals to sell their services as they spread the cost of their capital investments across the market (Gong et al. 2017).

Whereas much of the early research on two-sided platforms related to their economic benefits (Brynjolfsson and Smith 2000, Brynjolfsson et al. 2003), recent work has begun to focus on the societal implications of platforms. Studies have shown their impact on a variety of issues, including: charitable giving, dating, drunk driving, sexually transmitted infections, and bankruptcy (Bapna et al. 2016, Greenwood and Agarwal 2016, Greenwood and Wattal 2017, Burtch et al. 2018, Wang and Overby 2021). Yet, despite this growing literature, two notable gaps persist in the context of financial platforms. First, whereas research has shown the economic implications of financial platforms, their noneconomic societal impact has received

comparatively less attention. Put another way, even though crowdfunding and microloan platforms like Kickstarter and Kiva have received attention, the focus has been on either matching outcomes or economic impacts (Stephen and Galak 2012, Burtch et al. 2013, Mollick 2014, Burtch et al. 2018, Younkin and Kuppuswamy 2018); the notable exception being Burtch and Chan (2019), who study charitable giving. This is notable because these platforms may be able to help circumvent the liquidity barriers faced by disenfranchised groups. Thus, it is not surprising that they may generate significant social externalities.

Second, much of the existing research has focused on the outcomes associated with participation on the platforms at the individual level, rather than on broader changes in the social environment. The process by which individuals use the platform to reallocate or redistribute resources can, in aggregate, lead to material changes in social outcomes. Once again, there are some notable exceptions. Burtch et al. (2018) show how the entry of the Uber ride-sharing platform reduces rates of necessity-based entrepreneurship. In perhaps the closest analogue to our work, Wang and Overby (2021) show that the entry of P2P lending markets affects bankruptcy filings by allowing individuals to raise short-term capital. Strikingly, they find that P2P platforms increase bankruptcy filings, as people financially overextend when capital is more easily available. We address these gaps by focusing on the social implications of P2P lending platforms. These lending platforms increase liquidity by creating a viable secondary debt-based financial market that can be accessed by individuals with short-term need. P2P platforms do so by removing layers of financial intermediation, decreasing social costs, and offering increased availability of capital (Warschauer 2004, Barry 2013, Morse 2015, Iyer et al. 2016). We next describe these platforms and their existing connection to reproductive care.

P2P Lending and Accessing Reproductive Care

Before discussing the direct effect of P2P lending on abortion services, it is worth discussing evidence of the effect that financial barriers can have on reproductive care more broadly. Perhaps unsurprisingly, financial barriers are consistently an issue in the United States when accessing clinical care of any kind (Osborn et al. 2017) but can specifically be an issue when it comes to reproductive health (Hock-Long et al. 2003) across a variety reproductive treatments—ranging from in vitro fertilization (IVF) (Connolly et al. 2008), to embryo transfer (Henne and Bundorf 2008) and even adoption (Hollingsworth 2000)—often due to heterogeneity in insurance requirements that exist across states (Flores et al. 2021). Whereas empirical work showing the clear effect of P2P lending markets in such contexts has lagged, significant anecdotal evidence exists to suggest that patients and providers are embracing these options for reproductive services. Indeed, P2P lenders such as Lending Club[11] and Prosper[12] directly discuss how their platforms may be utilized for accessing reproductive financial support, and this language is directly aimed at would-be parents and interested couples. These discussion points are also repeated in online support groups aimed at individuals or couples looking for financial options. Lastly, such discussions also occur on traditional care forums[13] as well as in general discussion boards like Reddit.[14] Interestingly, the options provided by P2P platforms have become commonplace enough that individual providers of fertility and family services (reproductive endocrinology,[15] IVF,[16] and adoption[17]) highlight them to would-be patients and parents when discussing financing options. Quoting one fertility provider in the American Northwest: "Lending Club Patient Solutions makes it easy to say YES to treatment with low monthly payments, no money down, and no payment for 3–7 weeks."[18]

It is also worth noting that whereas these procedures are highlighted by individual healthcare providers and platforms, these procedures do not face the same form of social or political debate that is associated with abortion. Given the stigma around abortion, medical providers and platforms are understandably less willing to provide open endorsements of specific financial options, or even highlight successful instances of how women may have benefited from their services. Thus, clear anecdotal evidence of how P2P funding, as a new way to access short-term capital, may assist those looking for financial support is not striking in its absence. However, the body of anecdotal evidence showing how P2P platforms are used in other forms of reproductive care raises the possibility that it is likely to be used for the more stigmatized forms of reproductive care, such as abortion. We next discuss why this may be true in detail.

P2P Lending and Abortion

Why might the availability of P2P lending influence the ability of women to secure access to an abortion? As a start, we note that, unlike traditional debt-based lenders (e.g., banks), P2P platforms do not require borrowers to disclose information about the reason for the loan, beyond indicating a simple category under which the loan is solicited. Instead, the platform assesses the risk of the loan and assigns a grade to the borrower's request using codifiable personal financial data.[19] This stands in contrast to the interview process at other financial institutions, where a loan officer's biases may influence willingness to extend the loan, notably if they do not approve of how the individual will use the funds. These particular aspects of P2P

lending make them appealing for debt consolidation and medical loans in general. Indeed, a popular category of loans on these platforms is for medical procedures; and anecdotal evidence corroborates unforeseen medical expenses as a reason why people borrow on the platform.[20]

With respect to the specific issue of abortion, there are two distinct, yet related, factors associated with abortion that are relevant to why P2P platforms may be attractive: (i) securing access to capital, as the out-of-pocket costs of terminating a pregnancy are often nontrivial, and (ii) the social stigma that accompanies abortion in the United States shapes decisions regarding how funding may be accessed. With respect to the first factor, there is copious evidence that indicates systemic unmet financial need when women try to access abortion services (Gohmann and Ohsfeldt 1993, Medoff 2008). This is unsurprising, as abortion services are often not covered by private insurance. In some states (e.g., Utah and Michigan), private employer-based health insurance is prohibited from covering the procedure outright.[21] Further, the Hyde Amendment (18 U.S.C. §3006A) restricts the use of federal dollars from covering the procedure, largely limiting federal workers and Medicaid recipients in all 50 states from coverage. As a result, 69% of women pay out of pocket when opting to terminate a pregnancy, even if only 36% lack insurance during the sample (Jones et al. 2013).

Beyond difficulties with insurance, the financial costs are nontrivial—the median (mean) cost varies between $490 ($506) and $1,750 ($1,874) in the first trimester (Jones and Kooistra 2011, Jones et al. 2013, Roberts et al. 2014), without considering costs of travel, lost wages, and any postprocedure complications. Further, in some states, women are compelled to take time from work, travel sometimes hundreds of miles to a clinic, engage in a counseling session, wait a mandatory 24 to 72 hours, and then repeat the trip to the clinic before the procedure can take place. After the first trimester, costs increase significantly due to the extra time, skill, and resources required. The associated costs for a procedure between 9 and 20 weeks of pregnancy increase significantly to between $500 and $3,000, and 16 weeks or later can be $15,000 or more.[22] This is considerable when compared with the fact that 63% of Americans lack the savings to cover even a $500 emergency.[23] Needless to say, these financial barriers are even more pressing for lower-income women, who comprise 75% of the patient pool (Jerman et al. 2016).

With respect to the second factor, stigma and concerns around privacy can influence how women may choose to acquire short-term capital. Requesting funding from family and friends can be difficult if the procedure itself is delegitimized or considered immoral. Short-term loans from financial institutions like banks can expose the individual to the possibility of bias from bank officials. In contrast, P2P platforms do not require borrowers to provide detailed information about the medical issue being addressed (or even that a medical issue is being addressed). As discussed above, this is unlike other financial institutions where detailed interviews can be, and often are, conducted as part of routine due diligence. In contrast, loans can be applied for through the platform with repayment schedules provided algorithmically.

When considering these two factors, P2P lending platforms appear to be well positioned to address both financial needs as well as the need for privacy that women face. As it relates to available capital, loans originating on P2P lending platforms are typically greater than $1,000, and can be much larger,[24] thereby making them structurally adequate to cover the cost of the procedure and incidentals. They also protect the borrower from having to provide needless details about the specific medical procedures, especially for women seeking capital for abortions. These ideas are clearly laid out in Figure 1, which describes how such barriers are perceived. In the first example, a woman posts on the popular online forum Reddit about how to access capital, alongside an identified sum of money needed ($2,500). Whereas costs may vary across individuals, the amount requested is typically in the range offered by lenders on P2P platforms. In the second, a woman tells her story of visiting the hospital for an unrelated infection and leaving with the information that she is pregnant with twins, seeking advice. Another Reddit user offers peer-to-peer lending as a solution with the following line "You can get a loan, no questions asked, for any amount up to $35,000 with fixed payments and no gimmicks. You'd have the money in a few business days." Based on these narratives, we argue that P2P platforms may be good candidates for accessing short-term capital when a woman is faced with the need to terminate pregnancy.[25]

Alternate Forms of Capital

It is worth considering if other sources of short-term liquidity might yield similar effects. Here, structural differences between P2P lending and other sources of capital make it unlikely that they offer comparable options for persons considering an abortion. Consider, for instance, the possibility of crowdfunding. The difference between crowdfunding and P2P lending is that crowdfunding requires the investee (borrower) to give up equity, whereas P2P lending requires the payment of interest on a principal. In addition, unlike a double-blind P2P lending process, crowdfunding breaches a borrower's privacy by design when requiring the revelation of the borrower's motives for

funding through various narratives. These differences make P2P lending a better fit for the funding of medical expenses, because there is no equity to be shared and the social stigma connected to the procedure would disincentivize disclosure. Whereas some P2P lending platforms (e.g. Prosper) operated like crowdfunding platforms in their early days (Lin et al. 2013, Lin and Viswanathan 2016), they have since evolved to operate as financial marketplaces (Wei and Lin 2017). These differences have led to an explosion in the size of P2P markets. In 2017, the entire crowdfunding market totaled less than $900 million in the United States, while a single P2P lending platform, LendingClub, intermediated $11.2 billion in capital.[26]

Yet another potential source of short-term capital is payday loans, which are often cited as an easy and convenient source of short-term capital. However, the median payday loan amount in the United States is only $350 and is capped at $500 in most states (Consumer Financial Protection Bureau 2016). These limitations, coupled with their predatory nature (i.e., annualized interest rates between 400% and 1,000%; Stegman 2007), make them both unattractive and insufficient. Credit cards are another possibility. However, the average U.S. household already carries almost $7,000 in debt,[27] which is more than the average credit limit with a Subprime credit score, and only marginally less that the average limit for someone with a Prime score. This means that the available credit would likely not cover the cost of the procedure, let alone indirect costs like loss of pay, travel costs, and the possible need for postprocedure care.[28] In contrast, interest rates on LendingClub do not exceed 36%, meaning that whereas some women might be excluded from the market entirely, the loans are not predatory in nature, once granted. Finally, as discussed above, one might consider a short-term medical loan from a bank. Terminating a pregnancy is not a category of medical loan applications at most banks, even though adoption, infertility treatments, cosmetic surgeries often are.[29] Moreover, banks often require detailed information, which may create privacy concerns (Jones and Kooistra 2011). For similar reasons, personal loans from family and friends for the procedure may be impractical and effectively infeasible.

Qualitative Evidence

The above paints a compelling picture for why P2P lenders might be able to obviate the capital constraints that women might face when seeking care. However, in the absence of clear narratives from the academic literature about the role that digital platforms may play in women's health, more direct qualitative evidence is needed to bolster the prima facie relationship. Therefore, we conducted a series of semistructured interviews with representatives of abortion providers, fund managers, and support groups to obtain firsthand accounts of the decision making involved when women were faced with financial constraints in accessing abortion services. Note that, for privacy reasons, we were not able to interview any women who considered and/or had an abortion with those providers. With IRB approval, we contacted nine organizations, of which six responded. Details regarding the interview protocol and responses are in Online Appendix B. Interviews were conducted on the phone and were based on a semistructured interview protocol. The interviewees were primarily counselors or administrators at providers, or policy experts at organizations that worked on issues relating to women's health. In total, six interviews were conducted. Our objective was not to reach theoretical saturation, but to gain a deeper understanding of the contextual factors that influence women facing the decision to terminate a pregnancy. These interviews reiterated the influence of financial barriers that women when securing abortion services as well as the significant influence of social stigma attached to the procedure.

All interviewees stated that financial constraints could be severe for women facing the decision to terminate a pregnancy, especially on short notice,[30] and that the limited availability of the funds often represented a significant barrier to securing care before it was prohibited by statute. These situations often required counselors to help coordinate multiple sources to bridge financial gaps for their patients. The approach described amounted to cobbling together the required funds from a variety of sources (e.g., charity networks, women's health organizations, abortion funds, family, and friends). Our interviewees also indicated that women were often apprehensive about approaching people in their social network for fear of stigma or disapproval. These agencies worked with women to help them acquire funds from nonprofit organizations, while also protecting them from predatory lending practices of payday loans.[31] One interviewee indicated the importance of coaching women to create plausible reasons (e.g., fixing a car or replacing a major appliance) if they did apply for a loan, as to avoid informing the lending agency of the loan's true purpose. Although credit cards were also mentioned as potential options, the interviewees discussed how many women did not have the required credit limits, or the means to pay them off in reasonable time. When asked directly about the issue of P2P funding, many counselors were not aware of these options. However, these counselors did direct patients to support groups that explicitly and openly discuss such options for procuring funding online. In general, the central role of financial barriers was repeatedly highlighted across our interviews.

Additionally, much of the reason for seeking capital from charitable sources, rather than banks or family,

was the inability to openly seek capital due to the stigma associated with terminating a pregnancy. As a result, women were compelled to access the funds without leaving a trail, with no notes of the interaction kept. For emphasis on stigma, we once again note that a quarter of women will use such services before the age of 45 and that 18% of pregnancies in the United States end via termination.[32] This suggests that the need for such services is pervasive in the United States but almost never discussed openly. Finally, the stigma surrounding providers (including counselors and staff members at abortion clinics) was also strong. Many funds and providers do not list contact information on their websites, let alone offer public guidance to overcome financial barriers. Phone numbers to their offices often go directly to voicemail, where volunteers screen callers before engaging with them. As one provider noted, "We receive a many of suspicious voicemails, often from individuals posing as researchers and medical professionals." Given the social stigma, and even physical violence that providers are subject to (Potts 2010), such caution is warranted. As a result, it is also not surprising that limited anecdotal evidence exists in the public forum about alternate sources of financial and social support, outside of an actual care provider. Empirically, this suggests that uncovering any relationship between the entry of P2P funding, as a short-term solution to financial barriers, and abortion rates will need to emerge from an analysis of existing data, rather than based on the presence of large amounts of anecdotal narratives.

Taken in sum, interviews with care providers and policy experts, as well as extant research, suggest that the entry of P2P lending platforms may increase the rate at which women elect not to carry to term by obviating financial barriers. The structure of these platforms offers benefits that ameliorate the concerns discussed above, while also making it easier to access capital. First, these platforms reduce economic frictions to quickly accessing capital. Second, they substitute for largely nonexistent insurance coverage. Third, they do not require in-person interviews or multiple phone calls with charitable organizations, thereby reducing transaction and search costs. Finally, they lessen the privacy concerns by allowing women to keep information private and avoid stigma. As a result, we expect the entry of LendingClub to be associated with an increase in the rate at abortions. In lieu of formal hypotheses, we allow the empirical analysis to guide us.

Data and Methodology

Funding Options for Abortions

Before proceeding into formal empirical analysis, a broader discussion of abortion-related trends and financing is warranted, notably given that the overall rate of abortions in the United States has been declining since the 1970s, to roughly 17 per 1,000 women aged 15–44 in 2011 (Boonstra 2016), around the time of our analysis. The steady decline has primarily been attributed to a reduction in unintended pregnancies (Dreweke 2016). Although the number of abortions has been reduced, a clear disparity exists in terms of the populations that have experienced this reduction. Among women with incomes below the federal poverty level, the unintended pregnancy rate in 2011 was 112 per 1,000 women, as compared with 20 per 1,000 women in the bracket with income 200% of the poverty level or higher (Boonstra 2016), illustrating the increasingly concentrated impact of the need for abortion services among the socioeconomic lower class. Further, in 2014, 49% of abortion recipients were *below* the poverty level, as compared with 27% in 2000 (Boonstra 2016).

These circumstances bring the ability to pay for abortion services to the fore, given federal- and state-level restrictions on private and public insurance (Jerman et al. 2016). Statutory restrictions on the use of Medicaid payments, for example, limit payments to cases of incest, rape, and threat to the life of the mother (Roberts et al. 2014). In 2014, the period of our study, 33 states followed the federal standard, prohibiting the state from paying for abortions in almost all circumstances, and a further eight states had prohibitions on private coverage (Roberts et al. 2014). The Guttmacher Institute goes on to report that whereas most women have health insurance, most women also pay out of pocket for the procedure (57%), either because the procedure would not be covered by insurance or there is a perceived need to keep knowledge of the procedure secret (Jerman et al. 2016). Thus, even in the presence of private insurance coverage, most women were compelled to pay out of pocket, and those without coverage are either compelled to carry to term or turn to charity networks like the National Network of Abortion Funds (NNAF).

Given our focus on funding for abortion, some discussion of organizations like the NNAF is warranted. The NNAF, and similar organizations, operate a network of independent groups across the country that provide capital and resources to women in need of financial support to pay for abortions. In 2019, NNAF reports that it received 215,573 calls from women looking for support, of which 26% (56,155) were provided some support. In most cases, this financial support was not complete but represented small amounts to help defray overall costs (e.g., transportation, childcare, medication) or, in some cases, help with payment for medical costs directly to the abortion provider. Most fundraising by these organizations is done by local philanthropy, as well as fundraising events that are attended by pro-choice activists and

supporters. Our own qualitative interviews with such organizations attested to the fact that patients often cobble together small amounts from diverse sources before they can pay for the abortion.

An alternative, and compelling, source of information on the costs borne by women is the Turnaway Project (Dobkin et al. 2014, Miller et al. 2020), a longitudinal academic study that recruited women seeking abortions at 30 different clinics in 21 states between 2008 and 2010. The goal was to study the effects of receiving an abortion versus being denied an abortion on women's physical, economic, and mental health. Using these data, Roberts et al. (2014) analyzed the effects on 725 women who secured an abortion, yielding insights into the financial restrictions borne by a diverse sample. Fifty-seven percent of participants were treated in a state where Medicaid covered the procedure, while 54% resided in such a state. The median cost during the first trimester was $490, while the median price between 14 and 20 weeks was $750. At 20 weeks, the median price was $1,750. Roberts et al. (2014) further report that 71% of women received some financial assistance. Private insurance paid for 7% of the procedures, state Medicaid dollars covered 34% of participants, and nonprofit groups assisted 29%. The remainder received no financial assistance. Later-term procedures were more likely to receive financial support. However, even with assistance, most patients had out-of-pocket expenses, the median being $340 in the first trimester and $650 between 14 and 20 weeks. In addition to the procedure costs, women reported paying a mean of $54 for out-of-pocket travel costs. Interestingly, the total out-of-pocket costs (including medical and travel costs) were equivalent to more than one third of monthly personal income for 56% of the patients. The median personal income reported was $800 (mean= $1,039). Finally, we note that 54% of the surveyed women reported that raising money for the procedure caused delays in accessing care, with non-White women being particularly prone to delays due to financial barriers.

Taken in sum, while it is challenging to provide a clear description of where the totality of abortion funding comes in the United States, there are clearly financial barriers that are experienced by women seeking these services (Boonstra 2016). The presence of taxpayer-funded services is extremely small (15.6% of procedures; Conover 2015) because of statutory restriction. Whereas some argue that support may be higher in practice, this reasoning is largely based on the tax exemptions of medical plans that cover the procedure (Conover 2015). Furthermore, to the extent that there is an obvious skew in need toward women of lower socioeconomic status, the public benefits are de minimis and, in practice, negligible, compelling women to find sources of capital like their social networks or the NNAF. Taken in sum, we note that whereas the reasons why women seek abortions remains a complex question, those who seek the procedure are constrained by financial resources, timing, and the presence of stigma (Roberts et al. 2014, Miller et al. 2020, Addante et al. 2021).

Empirical Setting

To identify the effect of P2P platform entry on the localized abortion rate, we leverage the registration of LendingClub (LC) in different locations at different times. As of January 2020, LC was the largest online peer-to-peer lending platform in the United States, ensuring that the platform is viewed as a legitimate source of capital rather than a fringe player. It is worth giving a brief overview on how LC works. First, a borrower prepares a loan request on the platform. Personal data are used for identification, but the data are not shared with lenders. Financial information is then made available to the LC algorithm. From this, the algorithm determines the loan terms available to the borrower. The loan request is then listed on the site for two weeks, visible to all users, alongside a credit grade (A, B, C, D, E, F, G). Once again, the request does not contain any personally identifying information; only credit data are shown to investors for their use in evaluating loan requests. Investors then have the option to subsidize or, as is more common, spread capital over several notes payable. Typical annual percentage rates range from 5.99% to 35.89% (well below credit cards and payday lenders). LC finances itself through an origination fee of 1%–6%. Loans are paid back over three- to five-year windows.

To identify the effect of P2P platform entry on the localized abortion rate, we leverage the state-level registration of LendingClub (LC). In doing so, we treat the rollout of the platform as quasiexperimental, allowing us to execute a difference-in-differences estimation. This identification strategy requires clarification on two related questions before we can proceed to the actual analysis. First, we need to understand the process by which the platform is licensed to operate in local jurisdictions, so potential issues of reverse causality are addressed. Second, we need a clearer examination of changes in the regulatory environment concerning abortion in states, so as to clarify the presence of any bias. We discuss the former issue below, while the regulatory environment is discussed in Online Appendix A.

Regarding the entry of LC in specific states, the platform attempted to enter every jurisdiction in the United States in 2008 (simultaneously). However, guidance from the Securities and Exchange Commission requires that lenders be authorized on a state-by-state basis, the reason being that borrowers are executing promissory notes. As a result, LC—or, more specifically, WebBank (the Utah state-chartered bank

that backs LC)—is subject to chartering statutes in each state in which they operate. Recall that although banks are subject to federal oversight, their legal requirements are largely defined at the state level. Therefore, LC was prohibited from operating in several states because the near-uniform way it operates across markets was incompatible with state statute. Reasons for denial included state-level recordkeeping requirements, restrictions on loan origination, disclosure requirements, and other ministerial issues. A detailed discussion of these denials, and their reasoning, is available in Sinha and Snow (2017). More importantly, there are no data to suggest that the requirements for operating in each state were related to medical reasons, medical costs, or medical procedural requirements in that state. Therefore, it is unlikely that such factors would bias our estimations. Further, it is worth noting that the regulatory schemes within counties are consistent throughout the panel and will be absorbed by the county fixed effects. Finally, we observe no relationship between abortion prevalence and entry (as shown in Table 15), suggesting that state-level entry by LC is primarily related to unchanging state statute. In 2008, the platform was registered in 40 states, as well as the District of Columbia (Online Appendix C). Nine additional states later registered the platform (Kansas and North Carolina in 2010; Indiana in 2012; Tennessee in 2013; Mississippi in 2014; Nebraska, North Dakota, and Maine in 2015; and Idaho in 2016).[33]

To execute the difference-in-differences analysis, we construct a novel data set, drawing on multiple sources of data. Consistent with Wang and Overby (2021), information on the time and location of LC entries is drawn from LC's blog and firm documents. Microloan data are captured through LC's API. As LC entered many states in 2008, we center our analysis on this event. This allows us to circumvent the identification issues that characterize multisite entries (Goodman-Bacon 2021). County-level data on the number of abortions per year are drawn from Johnston's Archive.[34] State-level data are drawn from the Centers for Disease Control and Prevention (CDC), and the Alan Guttmacher Institute (AGI). Both data sources report only yearly figures. Although there are some missing data at the county level, we conduct our analysis at this level because it allows us to examine heterogeneous effects attributable to county-specific differences that are beyond coarse state-level variations. Results are consistent at the state level. Further data on county-level factors (e.g., average income, number of banks) are drawn from the Census Bureau's American Community Survey as well as the Area Health Resource File provided by the Department of Health and Human Services. The unit of analysis is the *county-year*, with 2,367 treated counties and 779 untreated counties acting as control. The range of our analysis is from 2005 to 2010, with LC's entry into treated counties occurring in 2008. We initiate the sample in 2005 to provide at least three years of pretreatment data for examining pretreatment trends. We close the sample when additional entries are observed (2010). In robustness tests, we extend our analysis to 2000, which allows for a longer pretreatment period. Results are consistent and are in Table D1 in Online Appendix D.

Variable Definitions

Dependent Variables. The primary dependent variable (DV) for the analysis is the number of abortions performed at licensed clinics in the county-year. This is the most granular unit of time available, as abortion data are only reported by year and county. Primary analyses leverage data from Johnston's Archive, which compiles data from state health departments and related agencies by county. Replication using the AGI and CDC at the state-year level yields consistent results. County population is set as exposure, thereby transforming the dependent variable into a rate.

Independent Variables. The key independent variable of interest is a 0/1 indicator that LC has been authorized to operate in county i at time t. We focus on LC, as the largest operating lender. Still, it is possible that other P2P platforms may have a confounding effect. Specifically, we consider the case of Prosper, an alternative P2P platform that started operating in 2005. Due to a series of legal actions, Prosper was forced to cease operations in 2008, but it restarted in 2009 under a different business model. We address the possible impact of Prosper in robustness tests described below (Table 11). Other crowdsourcing platforms like Kickstarter are present but represent a significantly different funding model, largely based on personal narratives from borrowers where identities are provided. These are unlikely to be useful in the context of abortions due to the lack of anonymity for the borrower or available equity. Furthermore, the availability of these platforms is also constant across the sample and should be subsumed by the fixed effects.

Control Variables. In addition to the county and time fixed effects that are used to complete the difference-in-differences estimation, we use several sets of controls to absorb any endogenous process that might be correlated with both LC entry and within county abortion rates. First, we include county-level population, number of live births, married women, and women aged 15–44 to proxy fertility (intended or unintended). We then include financial factors such as

the number of banks and financial institutions, the amount of bank deposits, and unemployment, all of which might be correlated with financial need and the decision to terminate a pregnancy (Medoff 2008, Jones and Jerman 2014). We also include demographic factors, including personal income per capita, poverty rates, the proportion of high school graduates (education), and county racial makeup. Descriptive statistics are in Table 1. Correlations are in Table 2.

Estimation Procedure

We use a difference-in-differences (DD) approach to identify the effect of LC's entry on county-level abortion rates. The DD approach has been widely used in prior research on studying the effects of platform implementation (Greenwood and Wattal 2017, Burtch et al. 2018, Wang and Overby 2021). The idea behind a DD is to mimic an experimental design using secondary data. By leveraging location fixed effects to control for time-invariant heterogeneity across counties, and time fixed effects to account for trends in the DV over time, we identify the effect of platform entry by comparing the change in the dependent variable for untreated locations with the change in treated locations. Formally,

$$Y_{it} = \beta_1 LC_{it} + T_t + S_i + \gamma' X_{it} + \varepsilon_{it},$$

where Y_{it} is the number of abortions in county i in year t; LC_{it} is a dummy equal to 1 if LC is available to borrowers in county i during year t, and 0 otherwise; T_t are year fixed effects; S_i are county fixed effects; X_{it} is a vector of controls; γ is the vector of coefficients; and ε_{it} is the error term. Errors are robust and clustered at the county level. Year fixed effects account for systematic changes in the number of procedures over time within the United States, which are generally trending down. County fixed effects account for time-invariant factors. The parameter of interest is β_1, which captures the DD. The estimator is the Poisson quasi-maximum likelihood estimator (QMLE), which resolves the well-known issues with logged counts DVs (Arvis and Shepherd 2013). County population is set as exposure, which permits us to interpret the change as a rate. Results are in Table 3.

Before discussing the results, it is important to note that the DD is subject to several assumptions, the most important of which is that trends in the dependent variable are parallel for the treated and untreated locations prior to treatment (Bertrand et al. 2004). Nonparallel pretreatment trends would undermine the argument that changes in the dependent variable are a result of platform entry. To ensure that this assumption is met, we first employ matching to increase the similarity between treatment and control. To do so, we use a coarsened exact match (CEM) (Blackwell et al. 2009). In robustness tests, we also use a propensity score matching (PSM) approach, with fully consistent results.

Matching Procedure

To execute the matching process, we pair treated and untreated counties using 2007 data, the year prior to treatment (entry of LC). Specifically, we match on the per-capita number of procedures, the number of live births (to capture fertility rates), the number of banks and financial institutions, the amount of deposits held (to capture the level of savings), the unemployment rate, personal income, and education level. Research indicates that these factors are related to the regional

Table 1. Descriptive Statistics

Variable	Definition	*N*	Mean	Standard deviation	Median	Min	Max
Number of procedures	Number of procedures	14,167	287.002	1,383.74	28	0	29,077
LC Entry	Entry of LendingClub (state level)	18,876	0.376	0.484	0	0	1
Prosper Entry	Entry of Prosper (state level)	18,876	0.316	0.465	0	0	1
Live births per capita	Number of live births per capita	15,956	0.013	0.004	0.012	0	0.189
Unemployment %	Rate of unemployment	18,870	6.52	3.019	0.012	0	29.7
Personal income per capita	Personal income per capita	18,504	31,757.19	8,594.811	30,223	11,749	156,195
Education %	Proportion of high school or higher	11,483	0.832	0.072	0.847	0.461	0.993
Banks per capita	Number of banks per capita	18,857	0	0	0	0	0.004
Deposits per capita	Amount of deposits per capita (million $)	18,857	0.016	0.014	0.014	0	0.548
Employment in Finance	Percentage of people employed in Finance	18,870	0.945	0.161	0.505	0.031	1.78
Mortgage delinquency	Mortgage delinquency rates	18,732	3.332	0.839	3.356	1.342	5.04
Population #	Number of people	18,857	96,338.48	310,000	25,457	40	9,848,011
Planned P. clinics	Split variable for Planned Parenthood clinics—2010	18,858	0.26	0.881	0	0	19
Title X clinics	Split variable for Title X Health clinics—2010	18,858	0.694	1.092	1	0	24
Education split	Split variable for education (high school)	10,932	0.833	0.070	0.846	0.492	0.975
Sex education split	Split variable for sex education mandates	18,876	0.351	0.477	0	0	1
Religious affl. split	Split variable for religious affiliations	18,564	0.519	0.176	0.505	0.031	1

Table 2. Pairwise Correlations

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1) *Number of procedures*	1.000																
(2) *LC Entry*	0.044	1.000															
(3) *Prosper Entry*	0.008	0.587	1.000														
(4) *Live births per capita*	0.089	−0.063	−0.086	1.000													
(5) *Unemployment %*	0.009	0.421	0.619	−0.059	1.000												
(6) *Personal income per capita*	0.228	0.145	0.099	−0.058	−0.205	1.000											
(7) *Education %*	0.027	−0.075	−0.098	−0.197	−0.332	0.511	1.000										
(8) *Banks per capita*	−0.128	−0.119	−0.039	−0.188	−0.253	0.133	0.159	1.000									
(9) *Deposits per capita*	0.157	0.027	0.041	−0.022	−0.138	0.383	0.145	0.316	1.000								
(10) *Employment in Finance*	0.160	0.054	−0.027	−0.015	−0.192	0.226	0.223	0.178	0.205	1.000							
(11) *Mortgage delinquency*	0.009	0.013	0.062	0.113	0.251	−0.260	−0.422	−0.315	−0.152	−0.309	1.000						
(12) *Population #*	0.795	0.045	0.012	0.104	0.009	0.265	0.074	−0.169	0.133	0.108	0.022	1.000					
(13) *Planned P. clinics*	0.544	0.045	0.004	0.089	−0.008	0.267	0.129	−0.153	0.126	0.152	−0.059	0.772	1.000				
(14) *Title X clinics*	0.340	0.022	0.019	0.064	0.096	0.008	−0.091	−0.181	0.026	−0.155	0.196	0.373	0.184	1.000			
(15) *Education split*	0.025	0.020	−0.019	−0.196	−0.271	0.506	0.986	0.136	0.128	0.225	−0.418	0.076	0.140	−0.095	1.000		
(16) *Sex education split*	−0.030	−0.082	−0.046	0.040	0.166	−0.073	−0.095	−0.123	−0.037	−0.212	−0.029	0.004	−0.013	0.127	−0.098	1.000	
(17) *Religious affl. split*	−0.034	−0.045	−0.023	0.125	−0.185	0.067	−0.099	0.380	0.194	0.134	−0.086	−0.047	−0.069	−0.091	−0.160	−0.051	1.000

Table 3. Poisson Estimation of LendingClub Entry on County-Level Procedure Rate

Variable	(1) Base Poisson: Number of procedures	(2) Matched Poisson: Number of procedures	(3) Matched Poisson: Number of procedures	(4) Matched Poisson: Number of procedures
LC Entry	0.0420***	0.0183***	0.0365***	0.0401***
	(0.00333)	(0.00421)	(0.00389)	(0.00416)
Live births per capita			0.639***	0.632***
			(0.0947)	(0.0961)
Unemployment %			−0.00818***	−0.0101***
			(0.00124)	(0.00136)
Personal income per capita			2.01e-06**	2.40e-06**
			(9.35e-07)	(1.01e-06)
Education %			−0.900***	−1.239***
			(0.136)	(0.157)
Banks per capita			154.9**	97.53
			(74.49)	(83.72)
Deposits per capita			−1.986***	−2.075***
			(0.209)	(0.212)
Median age (women)				−0.00301
				(0.00262)
Women proportion %				0.516
				(0.328)
Women btw 15&44%				0.112
				(0.180)
Women married %				1.642***
				(0.235)
Women divorced %				1.309***
				(0.411)
Poverty (household) %				1.348***
				(0.271)
White proportion %				−0.262***
				(0.0921)
Number of observations	13,382	9,656	5,827	2,287
Number of counties	2,330	1,680	1,541	388
Matching weights	N/A	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

abortion rates (Jones et al. 2010, Jerman et al. 2016). The default binning algorithm, Sturge's rule, is used. After matching, the number of treated counties reduced from 2,367 to 1,318, and the number of untreated counties reduced from 779 to 600. A characteristic of CEM and other matching procedures is that the algorithm can create strata with unequal numbers of treated and control units (a single treated county is sometimes matched to more than one control county and vice versa). To accommodate this, CEM generates weights for each county, which are included in the estimation. The matching results in superior balance between treatment and control (Online Appendix Table C2). The univariate distances are nearly even across the match and control samples after the matching process. However, to the extent that some imbalance may remain between treatment and control groups, we include the matching variables in the estimation as controls. Results from the baseline matched samples, without the additional matching covariates, are in column 2 of Table 3. In column 3, we add the matching covariates.[35]

Results

As mentioned, we use a QMLE as our baseline estimator. This model offers notable benefits over the logged ordinary least squares or a fixed-effect negative binomial estimation (see Allison and Christakis 2006 and Silva and Tenreyro 2006 for a complete discussion). Results in column 1 of Table 3 show a strong and significant increase in the abortion rate following the entry of LC. Recall that population is used as exposure, constraining the LC coefficient to 1. Setting population as exposure also means that the coefficient should be interpreted as a rate. Column 2 contains the matched sample, again indicating a significant and positive relationship. Economically, results suggest an increase of approximately 0.2851 procedures per county-year in the base estimations and 0.1196 procedures per county-year in the matched estimations (675 or 283 procedures annually across the 2,367 treated counties) after the entry of LC.[36] Finally, column 3 shows the results of the matched sample with additional controls. Once again, we observe a significant relationship. These results lend support to the hypothesis that P2P lending, as an alternative form of short-term capital supply, may alleviate the financial barriers faced by women choosing to terminate their pregnancies.

Before proceeding with further estimations, we first examine whether our estimated treatment effects are reasonable, given the policy implications of a significant effect and the divisiveness of the topic in U.S. politics (Matthews et al. 1997). As we contextualize these results, we acknowledge the constraints of the data that we employ and underscore the need for further study. Further, it is important to underscore that our estimates may be conservative, notably if LC entry is facilitating contraception and pregnancy avoidance in the short term (or women's access to education and entrepreneurship in the long term).[37] First, we consider whether the effect is consistent with the size of the lending market on LC. That is, are the number of loans issued on LC in line with the observed effect? The number of loans issued at the close of our sample is roughly 12,500, well above the number of incremental abortions that we estimate. Furthermore, in 2010 alone, there were 765,000 abortions reported to the CDC. The estimated effect is well within these numbers (a fraction of 1%), suggesting that it may be realistically attributed to the presence of LC. Though the effect size that we uncover may be small relative to the total number of abortions reported in the United States per year, each of these procedures represents an important and consequential decision. Therefore, it is worth acknowledging the impact of this incremental effect, on individuals and families collectively.

Next, it is worth considering whether the loans studied on LC are issued under the category of "Medical Loans," as relatively few loans are issued within this category relative to more ambiguous categories like "Other." Given the absence of any mechanism to confirm the purpose of the loan on the platform, it is possible that women looking for short-term capital may apply under categories like "Other" so as to avoid specifying the details of their requirements. Indeed, from a privacy perspective, women may be more likely to choose the "Other" category, especially since loan categories have no bearing on the interest rates or the probability of receiving a loan. More than 50% of the loans on LC in our sample are classified as "Other."

One final concern that might emerge with these initial estimates is that, despite the fixed-effect structure and the matching, ongoing unobserved time-variant factors might be correlated with LC entry. These correlations, if present, are likely to bias the estimated treatment effects. To mitigate these concerns, we include a slew of additional controls in the model. These include the median age of local women and the number of women between ages 15 and 44, the number of married and divorced women (to account for the possible rate of pregnancy in a more proximal manner), the proportion of households living in poverty (to proxy economic need), and controls for the racial mixture of the county. Due to missing data, the addition of these controls leads to considerable reductions in sample size. This reduction notwithstanding, results in column 4 of Table 3 show that LC entry

continues to be strongly associated with an increase in terminations.

Robustness Tests

Whereas matching, combined with a two-way fixed-effect strategy, provides nontrivial initial evidence of the effect that P2P lending has on a woman's decision to access care, it is important to ensure that our results stand up to empirical scrutiny. In what follows, we detail a series of falsification tests that provide corroborative evidence of the observed effect, while also providing some evidence of the mechanisms at play.

Pretreatment Trends

The key assumption of the DD estimation is that the trends in the dependent variable are parallel prior to treatment (Bertrand et al. 2004). To assess the validity of this assumption, we take two approaches. First, we replicate the Danaher and Smith (2014) placebo test in which we shift the panel and treatment forward in time (i.e., move it earlier in time) and drop all post-treatment observations. Intuitively, this test allows us to examine if the treatment and control groups are significantly different prior to treatment (Hydari et al. 2019). We expect this pseudotreatment's effects to be insignificant. Second, we replicate a common variant of the Autor (2003) leads and lags model, which allows us to show that, until the treatment occurs, both control and treatment groups are trending similarly. By taking such an approach we model the effect semiparametrically, capturing the trend in the effect pre- and posttreatment. Again, our expectation is to obtain no significant difference prior to treatment, with a significant effect being observed posttreatment. Before discussing the result, two clarifications are warranted. First, whereas our baseline data set starts in 2005, the results from the leads and lags model provides consistent results, even when extended prior to 2005, as shown in Online Appendix D. Second, as the treatment occurs contemporaneously for *all* treated observations, we avoid the potential inverse weighting problems discussed by Callaway and Sant'Anna (2021) and Goodman-Bacon (2021).

In executing the Danaher and Smith test, we move the treatment up by both one year and two years before actual treatment (2007 and 2006, respectively), dropping all observations following the original treatment year (including the treatment year itself). Results in Table 4 show no significant effect in either sample, regardless of the inclusion of controls, bolstering the notion that counties receiving the LC treatment were not demonstrably different than those that did, once conditioned upon controls. Turning to the results of the Autor model (Table 5), we observe little in the way of pretreatment trends. In the base model, we observe some differences three years before treatment,

Table 4. Poisson Estimation of LendingClub Entry on County-Level Procedure Rate for Pretreatment Trends (2006 and 2007)

	Treatment in 2006		Treatment in 2007	
Variable	(1) Matched Poisson: Number of procedures	(2) Matched Poisson: Number of procedures	(3) Matched Poisson: Number of procedures	(4) Matched Poisson: Number of procedures
LC Entry	−0.00844	−0.00708	−0.0075	−0.00690
	(0.00618)	(0.00612)	(0.00621)	(0.00604)
Live births per capita		−0.226**		−0.174
		(0.109)		(0.106)
Unemployment %		0.0147***		0.0161***
		(0.00470)		(0.00477)
Personal income per capita		−5.45e-06**		−5.36e-06**
		(2.21e-06)		(2.21e-06)
Education %		−0.223		−0.233
		(0.185)		(0.185)
Banks per capita		1,201***		1,191***
		(168.4)		(168.1)
Deposits per capita		−1.533***		−1.533***
		(0.457)		(0.457)
Number of observations	4,954	1,316	4,954	1,316
Number of counties	1,655	440	1,655	440
Matching weights	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 5. Relative Time Model of County-Level Procedure Rate on LendingClub Entry

Variable	(1) Base Poisson: Number of procedures	(2) Matched Poisson: Number of procedures	(3) Matched with controls: Number of procedures
$Treated_{t-3}$	0.0292***	0.0161*	0.00329
	(0.00573)	(0.00723)	(0.00668)
$Treated_{t-2}$	−0.000428	−0.000345	0.00788
	(0.00564)	(0.00712)	(0.00674)
$Treated_{t-1}$	Omitted base case		
$Treated_{t0}$	0.0220***	−0.00172	0.00793
	(0.00567)	(0.00716)	(0.00630)
$Treated_{t+1}$	0.0485***	0.0227**	0.0349***
	(0.00575)	(0.00728)	(0.00643)
$Treated_{t+2}$	0.0856***	0.0521***	0.0814***
	(0.00581)	(0.00739)	(0.00650)
Live births per capita			0.662***
			(0.0979)
Unemployment %			−0.00850***
			(0.00125)
Personal income per capita			1.42e-06
			(9.38e-07)
Education %			−0.898***
			(0.136)
Banks per capita			165.7*
			(74.56)
Deposits per capita			−1.921***
			(0.209)
Number of observations	13,382	9,656	5,827
Number of counties	2,330	1,680	1,541
Year fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

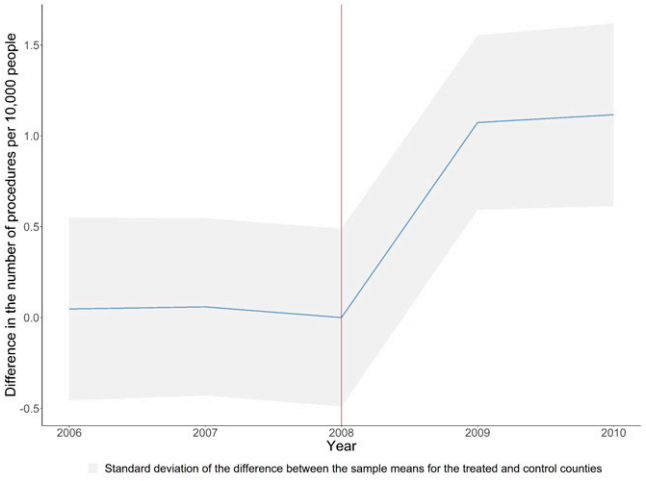
although the coefficients are positive. As we shift to the matched and fully controlled samples, any pretreatment differences fall away. Finally, we note that the main effect of LC's entry is significant and grows over time. This is consistent with a growing network effect that such platforms rely on. Figure 2 shows the difference between the number of procedures in the treated and control counties, during both pre- and posttreatment trends graphically in the matched sample. As is evident, the effects of LC's entry on abortion rates in the posttreatment period are easily discernible.

Number of Clinics

We have thus far emphasized financial costs as critical to the observed change in behavior. One simple way to corroborate this mechanism is to artificially perturb the financial burden that women face. Intuitively, the effect of LC's entry in a region should be smaller if the financial burden is higher, because the funding provided by the platform would be less likely to bridge the financial gap. The location of clinics, and the distance women must travel to them, is one such factor that can lead to variations in financial burdens faced by women. To test such an effect, we split our sample into two groups, counties where there are no clinics that offer abortion services, and counties in which there is at least one clinic. In counties without clinics, the relative effect of LC's entry is likely to be smaller, since the costs of travel and the procedure are relatively higher.

To determine the number of clinics in a county, we use two different definitions of a "clinic." In the first, we use the presence of a Planned Parenthood (PP). PP is currently the largest provider of reproductive care in the United States.[38] As PP is only a single provider, we also replicate our estimations using the number of Title X–funded health (TiX) clinics, splitting the data again based on the presence or absence of a clinic. Title X clinics are federally funded clinics with the target of providing affordable birth control and reproductive healthcare for women, notably those who cannot afford it on their own. Data on both PP and Title X–funded health clinics is drawn from the AGI. Results of the split-sample analyses are in Table 6. As can be seen, the effect is notably smaller when there are no clinics in the focal county (columns 1 and 3), emphasizing the role of LC on reducing financial barriers for women.

Figure 2. (Color online) Pre- and Posttreatment Trends of the Differences in the Number of Procedures per 10,000 People Between the Treated and Control Counties in the Matched Sample



Household Income

Another way to corroborate the cost mechanism is by proxying financial need by the relative wealth of the treated area.[39] The intuition is simple—in areas of lower relative wealth, the effect of LC's entry is likely to be higher because there will be a greater unmet financial need among women in the county. In contrast, relatively wealthier counties may not experience the same extent of financial barriers. Whereas there is within-county heterogeneity in terms of wealth distribution, we would still expect some broad effects to manifest in the aggregate when comparing affluent counties to those that are less affluent.

To execute this test, we replicate our estimations while splitting the sample based on the median county household income of the sample. Data on household income is drawn from the U.S. Census. Results are in Table 7. As can be seen, there is an effect across both relatively wealthier and relatively poorer counties. However, the effect of LC's entry is noticeably larger in less affluent counties (column 1) as compared with more affluent counties (column 2). An interaction of the treatment with county wealth (column 3) confirms that the effect is stronger in counties with a lower-than-median county household income. Taken in sum, these estimations further corroborate the cost mechanism and underscore the pervasiveness of the effect.

Issues of Serial Correlation

As noted by Bertrand et al. (2004), one critical problem with panel data sets and DD estimations is the potential for serial correlation in the standard errors. This is concerning because it artificially increases the probability of a significant effect manifesting purely by chance. One recommended approach to determine if serial correlation is a problem is a random implementation test, also referred to as a *placebo test*. To implement the test, we replicate our estimations using

Table 6. Falsification Test Using Clinic Availability (Poisson Estimation)

Variable	(1) Planned Parenthood clinics not present: Number of procedures	(2) Planned Parenthood clinics present: Number of procedures	(3) Title X health clinics not present: Number of procedures	(4) Title X health clinics present: Number of procedures
LC Entry	0.0288***	0.0415***	0.0173**	0.0407***
	(0.00617)	(0.00503)	(0.00701)	(0.00488)
Live births per capita	−9.139***	0.645***	−10.23***	0.611***
	(2.662)	(0.0959)	(3.282)	(0.0959)
Unemployment %	−0.0207***	0.00386**	0.0113***	−0.0182***
	(0.00190)	(0.00169)	(0.00215)	(0.00156)
Personal income per capita	−3.92e-07	3.65e-06***	1.26e-05***	−5.19e-06***
	(1.62e-06)	(1.18e-06)	(1.67e-06)	(1.21e-06)
Education %	−0.677***	−1.160***	−0.754***	−0.674***
	(0.184)	(0.202)	(0.220)	(0.174)
Banks per capita	444.9***	−219.9**	−212.9*	412.6***
	(105.1)	(108.6)	(117.9)	(97.71)
Deposits per capita	6.124***	−2.040***	−1.600***	−4.042***
	(1.217)	(0.214)	(0.223)	(0.662)
Number of observations	4,466	1,361	2,863	2,964
Number of counties	1,286	255	731	810
Matching weights	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 7. Falsification Test Using Median Income (Poisson Estimation)

Variable	(1) Income below median: Number of procedures	(2) Income above median: Number of procedures	(3) Income median interaction: Number of procedures
LC Entry	0.0550***	0.0422***	0.0532***
	(0.00871)	(0.00391)	(0.00454)
LC Entry & Median Split Interaction			−0.0127***
			(0.00327)
Live births per capita	0.168	0.797***	0.822***
	(0.918)	(0.210)	(0.204)
Unemployment %	0.0335***	0.000800	0.00654***
	(0.00163)	(0.000711)	(0.000645)
Education %	−0.855***	0.391***	−0.0651
	(0.192)	(0.123)	(0.103)
Banks per capita	1,445***	170.6***	421.3***
	(119.7)	(55.70)	(50.18)
Deposits per capita	−3.139**	−0.507***	−0.532***
	(1.383)	(0.0598)	(0.0597)
Number of observations	2,906	2,921	5,827
Number of counties	865	676	1,541
Matching weights	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

randomly assigned treatments. The intuition behind this test is that if a significant effect exists even in the placebo, then there is a strong likelihood that within-county serial correlation exists and that the estimation likely suffers from weak identification (Bertrand et al. 2004, Donald and Lang 2007). Thus, the expectation is to observe no significant effect.

To execute these tests, we randomly assign treatment to counties using the same proportion of treated to control counties as the original sample. We apply this procedure in two ways. In the first, we randomly and proportionately assign treatments to any county in the data set, effectively randomizing counties into treatment and control. In the second, we randomly and proportionately assign treatments only to counties that are eventually treated, but we randomly perturb the point in time when the county was treated. We then estimate our baseline model. This process is replicated 10,000 times, with the coefficient of interest being stored each time. Results are in Table 8. Columns 2 and 3 show that

Table 8. Falsification Test Using Randomly Assigned Treatment Groups (Poisson Estimation)

Variable	(1) Original model: Number of procedures	(2) Random in all: Number of procedures	(3) Random in treated: Number of procedures	(4) Difference between (1) and (2)	(5) Difference between (1) and (4)
LC Entry	0.0420***	0.0008	1.5e-05	N/A	N/A
	(0.00333)	(0.0008)	(0.0009)	N/A	N/A
z-score	12.64	0.03	<0.01	12.03	12.17
p-value	<0.001	0.512	0.500	<0.001	<0.001
Number of replications	N/A	10,000	10,000	N/A	N/A
Number of observations	13,382	13,382	13,382		
Number of counties	2,330	2,330	2,330		
Matching weights	N/A	N/A	N/A		
Year fixed effects	Yes	Yes	Yes		
County fixed effects	Yes	Yes	Yes		
Population as exposure	Yes	Yes	Yes		

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

the placebo coefficients are not statistically different from zero. Columns 4 and 5 compare the coefficients from the placebo tests to the observed effect from Table 3, indicating that the estimated effect is significantly larger. These results give us confidence that it is the entry of LC, and not simply the error structure, driving the observed treatment effect.

In addition to placebo tests, Bertrand et al. (2004) provide several additional tests to ensure that DD estimations do not suffer from serial correlation in the standard errors. One standard approach is to include lagged values of the outcome variable in the estimation, the intuition being to explicitly account for serial correlation that may exist across one or two periods of the panel. Results are in Table 9. In executing these estimations, we first include a single lag and then a second lag. As would be expected, there is significant within-county consistency in the number of procedures that occur each year. However, and importantly, the effect of LC entry remains consistent (i.e., positive and significant) in the presence of these lags. In panel data models with fixed effects such as the one in Table 9, the fixed-effects estimator may suffer from a bias due to limited time periods in the panel and autoregressive terms (Nickell 1981).[40] To check the robustness of our results against such a bias, we further developed dynamic panel data models in which we estimate the lagged number of procedures using the twice-lagged outcome as an instrument (Anderson and Hsiao 1981, Arellano and Bond 1991). The results support our findings and are available upon request from the authors.

Finally, as including the lagged dependent variable may not completely alleviate the extent to which the standard errors are underestimated (Bertrand et al. 2004), we run a third test in which we collapse the panel into two periods, pre- and posttreatment. Simulations suggest that such an implementation eliminates bias due to serial correlation if and when all entities are treated at the same time in a panel (Bertrand et al. 2004). The DV therefore becomes the average number of procedures (and all other variables) for each county, before and after entry. We then replicate our estimations using the two-period panel. Results are in Table 10 and indicate a positive, statistically significant, effect of platform entry on the procedure rate.

Prosper

Although LC is the dominant service, at least in terms of dollars lent, it is worth considering the effect of other similar platforms. It is plausible that Prosper, a similar platform, has a similar effect on abortion rates, since lenders and borrowers are subject to the same

Table 9. First- and Second-Order Lags in the Poisson Estimations of LendingClub Entry

Variable	(1) Original Model: Number of procedures	(2) With the first lag: Number of procedures	(3) With the second lag: Number of procedures	(4) With the first and second lags: Number of procedures
LC Entry	0.0365***	0.0400***	0.0424***	0.0426***
	(0.00389)	(0.00430)	(0.00521)	(0.00521)
Abortion First Lag		0.000202***		0.000222***
		(7.78e-06)		(1.04e-05)
Abortion Second Lag			4.68e-05***	2.07e-05**
			(9.26e-06)	(9.32e-06)
Live births per capita	0.639***	0.821***	−4.780*	0.828
	(0.0947)	(0.0989)	(2.609)	(2.623)
Unemployment %	−0.00818***	−0.00639***	−0.00219	−0.000138
	(0.00124)	(0.00134)	(0.00158)	(0.00158)
Personal income per capita	2.01e-06**	1.12e-05***	1.42e-05***	1.19e-05***
	(9.35e-07)	(1.06e-06)	(1.22e-06)	(1.23e-06)
Education %	−0.900***	−0.0993	−0.116	−0.182
	(0.136)	(0.185)	(0.244)	(0.244)
Banks per capita	154.9**	−233.6***	281.8***	435.8***
	(74.49)	(87.69)	(107.0)	(107.3)
Deposits per capita	−1.986***	−0.982***	−1.248***	−1.116***
	(0.209)	(0.251)	(0.268)	(0.267)
Number of observations	5,827	5,366	4,926	4,920
Number of counties	1,541	1,531	1,531	1,530
Matching weights	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 10. Before/After Treatment Panel for the Poisson Estimation of LendingClub Entry

Variable	(1) Original panel: Number of procedures	(2) Before/after treatment panel[a]: Number of procedures
LC Entry	0.0365***	0.0183***
	(0.00389)	(0.00672)
Live births per capita	0.639***	4.951***
	(0.0947)	(0.404)
Unemployment %	−0.00818***	−0.0215***
	(0.00124)	(0.00272)
Personal income per capita	2.01e-06**	−5.69e-06**
	(9.35e-07)	(2.27e-06)
Education %	−0.900***	−2.045***
	(0.136)	(0.392)
Banks per capita	154.9**	−518.5***
	(74.49)	(159.3)
Deposits per capita	−1.986***	−3.345***
	(0.209)	(0.619)
Number of observations	5,827	2,038
Number of counties	1,541	1,019
Matching weights	Yes	Yes
Year fixed effects	Yes	Yes
County fixed effects	Yes	Yes
Population as exposure	Yes	Yes

Note. Standard errors in parentheses.
[a]In the before/after treatment panel, the variables are averaged per county (see the text for details).
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

theoretical mechanisms. Therefore, we could conduct a similar analysis with Prosper's entry. However, in the case of Prosper, several caveats are required. First, whereas Prosper was universally available in 2005 (the starting point of our analysis), the platform was associated with several legal challenges to its business model between 2006 and 2010. These challenges led to volatility in its operations, which makes it challenging to cleanly estimate treatment effects. Second, in 2008, the Securities and Exchange Commission (SEC) found Prosper in violation of the Securities Act and required the platform to cease operations, shutting the site down across the board. All financial transactions on the platform were suspended for this time period, again reducing the extent to which a clean econometric model could be estimated. The platform reopened in 2009 but with restrictions on who could lend money on the platform. Finally, when the platform opened in 2009, the process by which loans and interest rates were issued also changed. Prior to its temporary closure, Prosper operated on a variable rate model where lenders and borrowers determined interest rates based on a Dutch auction system. In 2009, it changed its business model to one where loan rates were set by the firm based on credit ratings and an internal algorithm. Given the significant changes in the way that the platform operated over time, this creates challenges in replicating the analyses for Prosper using the same sample as Lending Club.

These issues notwithstanding, it is possible to ensure that the premise of our analysis holds even with Prosper. We thus limit our analysis of Prosper's effect on abortion rates to the years 2008 onward, when a stable business model existed for the platform. Although this limits the observable pretreatment window, it does allow us to exploit the resolution of legal challenges in 2009, at which point there was a reintroduction of Prosper across the United States, except for the three states (Iowa, Maine, and North Dakota). This partial treatment provides us with a different DD. Thus, we use the relaunch of Prosper in 2009 after reregistration by the SEC.[41] Counties in Iowa, Maine, and North Dakota serve as our controls. Results are in Table 11 and show a positive effect of Prosper's reentry on the number of procedures, consistent with LC. However, given the obvious differences in launch, we caution against direct comparisons of these results with those from LC.

Additional Robustness Tests

In addition to these tests, we run a series of additional falsification tests to establish the robustness of our results. To conserve space, we briefly describe these tests here. Additional information is available upon request. First, while initially constructing our sample using a CEM, other matching models exist, such as propensity score matching (PSM). To ensure robustness, we replicate our analysis using Stata's default PSM weighting options, comparing counties based on the propensity to be treated. Results remain consistent (Table 12). Second, in an extension of the CEM matching procedure, we match on trends in the dependent

Table 11. Effect of Prosper Entry on the Number of Procedures

Variable	(1) Poisson model: Number of procedures
Prosper	0.087***
	(0.022)
Unemployment %	0.004***
	(0.001)
Income per capita	0.000***
	(0.000)
Education %	1.410***
	(0.231)
Deposits per capita	0.069***
	(0.089)
Number of observations	14747
Year fixed effects	Yes
County Fixed Effects	Yes
Population as Exposure	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

variable prior to treatment and replicate our estimations. This analysis captures the notion of trajectory-based matching used in prior research (Haviland and Nagin 2005), so that counties on similar trajectories are matched. Results remain consistent in both significance and effect size (Table 13). Third, we test the effects of LC's entry at the *state* level, using data provided by both the AGI and CDC. Results are in Table 14 and are consistent with those at the county level. Fourth, we use a logit model to examine if LC entry is related to changes in the number of procedures. In doing so, we regress our independent variable if interest (entry) on the number of annual county-level procedures using a logit estimator (conditioned upon controls). Results indicate no significant relationship, as shown in Table 15. Finally, to account for general within-county trends, we create time splines for each county and include them in the estimation. The intuition is to partial out any trend in the dependent

Table 12. Poisson Estimation of LendingClub Entry on County-Level Procedure Rate Using Propensity Score Matching

Variable	(1) Base Poisson: Number of procedures	(2) Matched Poisson: Number of procedures	(3) Matched Poisson: Number of procedures	(4) Matched Poisson: Number of procedures
LC Entry	0.0420***	0.0538***	0.0458***	0.0395***
	(0.00333)	(0.00148)	(0.00157)	(0.00171)
Live births per capita			0.728***	0.462***
			(0.0533)	(0.0542)
Unemployment %			−0.00162***	−0.00285***
			(0.000569)	(0.000608)
Personal income per capita			−2.43e-07	−2.32e-07
			(1.64e-07)	(1.68e-07)
Education %			−1.042***	−1.025***
			(0.0787)	(0.0870)
Banks per capita			436.3***	162.4***
			(36.76)	(40.18)
Deposits per capita			−0.696***	−0.704***
			(0.0307)	(0.0319)
Median age (women)				0.0171***
				(0.00148)
Women proportion %				0.916***
				(0.136)
Women btw 15&44%				0.959***
				(0.0905)
Women married %				0.130
				(0.132)
Women divorced %				−2.118***
				(0.245)
Poverty (household) %				2.786***
				(0.160)
White proportion %				1.013***
				(0.0433)
Number of observations	13,382	7,917	6,396	3,170
Number of counties	2,330	1,365	1,361	538
Matching weights	N/A	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 13. Poisson Estimations of LendingClub Entry on County-Level Procedure Rate Matched on Procedure Trends

Variable	(1) Base Poisson: Number of procedures	(2) Matched Poisson: Number of procedures	(3) Matched Poisson: Number of procedures
LC Entry	0.0420***	0.0182***	0.0365***
	(0.00333)	(0.00421)	(0.00397)
Live births per capita			0.638***
			(0.0954)
Unemployment %			−0.00816***
			(0.00125)
Personal income per capita			2.07e-06**
			(9.41e-07)
Education %			−0.904***
			(0.137)
Banks per capita			148.06**
			(75.04)
Deposits per capita			−1.997***
			(0.213)
Number of observations	13,382	9,460	5,774
Number of counties	2,330	1,636	1,519
Matching weights	N/A	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

variable that is idiosyncratic to the location, but unrelated to the treatment, in addition to the average-time fixed effects. Results are in Table 16 and show that the coefficient is statistically significant and consistent in magnitude in the presence of these county-level trends.

Regional Differences and Privacy Concerns—Tests of Moderation

Thus far, significant evidence suggests that the entry of P2P lending closes financial gaps associated with accessing reproductive care. However, it is worth considering heterogeneity in the effect across different social environments. The fact that social barriers can unduly influence women when making their decision is critical, because many women seeking an abortion cannot do so in a vacuum and must rely on other parties. These range from the woman's partner, to family members, to physicians and medical staff, to even members of the local community (e.g., church leaders and personal mentors; Norris et al. 2011). The cost of privacy being breached during the decision-making process, for both the woman and the extended set of stakeholders, is not uniform for all women. Literature in public health refers to the costs of this social judgement regarding abortion as *abortion stigma*, defined as an "attribute of an individual that is deeply discrediting;" making someone "tainted" or "discounted" (Kumar et al. 2009, p. 626). Largely considered a "concealable" stigma, since it is unknown until disclosed (Quinn and Chaudoir 2009), the effect is to create social rules of secrecy (Ellison 2003). However, when women make the decision to undergo the procedure, the social costs of disclosure are starkly heterogeneous. Part of this is caused by psychological and social factors; for example, there are often higher

Table 14. Replication of the Base Model at the State Level Using Two Data Sources (Poisson Estimation)

Variable	Data source: AGI Base Poisson: Number of procedures	Data source: CDC Base Poisson: Number of procedures
LC Entry	0.0207***	0.0360***
	(0.00346)	(0.00291)
Number of observations	200	292
Number of states	50	49
Year fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Population as exposure	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 15. Logit Model of County-Level Procedure Rate on LendingClub Entry (Entry Model)

Variable	(1) Logit entry model: *LC Entry*
# Procedures	−0.0001
	(0.0001)
Live births per capita	−107.555***
	(25.452)
Unemployment %	−0.082**
	(0.037)
Personal income per capita	−0.00000
	(0.00001)
Education %	1.497
	(1.153)
Banks per capita	−1,959.248***
	(529.665)
Deposits per capita	3.112
	(5.753)
Population	0.00000***
	(0.00000)
Number of observations	5,499
Year fixed effects	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

social costs for women who live in rural or conservative communities (Doran and Nancarrow 2015). Similarly, nontrivial disinformation exists about the physiological and psychological after-effects of the procedure (such as unfounded claims of depression, impaired fertility, and breast cancer), with women who consume such disinformation being more likely to internalize stigma (Littman et al. 2009).[42] Finally, women who live in areas where sex education is underemphasized or abstinence is taught exclusively may be additionally stigmatized (Kumar et al. 2009).

Empirically, we explore two factors that may contribute to heterogeneous levels of stigma associated with the procedure and that are also likely to vary across counties in our data set: education (including sex education) and local religious affiliations. Education has been associated with increasingly pro-choice views, with more educated regions being more open to the procedure, thereby minimizing such stigma (Kumar et al. 2009). Thus, the extent to which LC's entry may be associated with a change in the procedure rate is likely to vary across counties with heterogeneous educations levels (measured in this case by the proportion of high-school graduates in the county). Within the context of education, the presence of mandatory sex education within schools can lead to broader acceptance of the procedure in general, and less social sanction associated with it (Kumar et al. 2009). That said, many U.S. states have opted for abstinence-only education within the ambit of sex education, which has enhanced the degree of stigmatization, while showing minimal impact on the rate of teenage pregnancies (Collins et al. 2002, Norris et al. 2011). Therefore, within the broader context of education, we also consider how mandatory sex education may affect the relationship.

Table 16. Replication of the Base Model with the County Time Splines (Poisson Estimation)

Variable	(1) Base Poisson: Number of procedures
LC Entry	0.0181***
	(0.00692)
Number of observations	13,382
Number of counties	2,330
County x time splines	Yes
Year fixed effects	Yes
County fixed effects	Yes
Population as exposure	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

In addition to education, prior research shows that religious affiliation is indicative of social barriers for women; evangelical doctrine holding that abortion is immoral, making it particularly risky for women to contact their community for support (Borgmann 2009, Sitzmann and Campbell 2021). Other orthodox groups have similar views. As a result, we would expect stronger social barriers to exist in areas with higher religious affiliations, and thus a higher reliance on outside funding. To capture this potential effect, we proxy religious affiliation using data from the Association of Religious Data Archives (thearda.com) (Bacon et al. 2018). These data capture the number of adherents to roughly 236 individual religious groups at the county level. The variable we use is the ratio between the total adherents to all religious groups and the population of the county as measured in 2010, thereby capturing differences in religiosity across counties.

Results are in Tables 17–19, where we conduct a split-sample analysis based on median values of the moderating variable. We see in Table 17 that counties with lower levels of education (defined as high school graduates) experience a stronger treatment effect, relative to those counties with higher education rates. Intuitively, this suggests that the social and financial barriers faced in areas with lesser education are larger. Thus, the introduction of a feasible source of capital with heightened privacy leads to a larger increase in abortion rates. The interaction term in column 3 of Table 17 provides corroboratory evidence in a single interaction model. Table 18 captures split-sample analysis based on mandatory sex education, a more specific test of the effect of sex education. The coefficient of interest is lower in the counties with mandatory sex education, where the social barriers are likely to be lower, and the difference in coefficients across the two subsamples is significant. Table 19 again shows the effects of social stigma—the effect being

Table 17. Effect Sizes of LendingClub Entry for Below and Above Median Education

Variable	(1) Education[a] below median: Number of procedures	(2) Education[a] above median: Number of procedures	(3) Education[a] median interaction: Number of procedures
LC Entry	0.0518***	0.0389***	0.0665***
	(0.00781)	(0.00475)	(0.00528)
LC Entry & Median Split Interaction			−0.0385***
			(0.00460)
Live births per capita	4.721	0.657***	0.643***
	(3.305)	(0.0957)	(0.0947)
Unemployment %	−0.00648***	−0.0154***	−0.00885***
	(0.00215)	(0.00164)	(0.00125)
Personal income per capita	−8.16e-06***	3.16e-06***	1.79e-06*
	(2.44e-06)	(1.06e-06)	(9.36e-07)
Education %	−0.983***	−1.054***	−0.929***
	(0.225)	(0.203)	(0.136)
Banks per capita	56.33	285.5***	148.0**
	(137.4)	(92.67)	(74.50)
Deposits per capita	−2.525***	−1.481***	−1.918***
	(0.400)	(0.248)	(0.209)
Number of observations	3,030	2,655	5,827
Number of counties	913	653	1,541
Matching weights	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes

Note. Standard errors in parentheses.
[a]Education variable is split at the median for the number of high school (or higher) graduates in a given county.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

significantly larger in more religious areas, as shown in column 3. This suggests that the social barriers associated with securing funding are stronger in highly religious areas. Of course, we cannot determine if the woman securing the procedure is religious or not, only that she lives in an area that has a high number of religious adherents. In summary, these results provide evidence for how social barriers can act,

Table 18. Effect Sizes of LendingClub Entry Depending on Sex Education

Variable	(1) Sex education is not mandated in state: Number of procedures	(1) Sex education is mandated in state: Number of procedures	(3) Sex education interaction: Number of procedures
LC Entry	0.0481***	0.0358***	0.0316***
	(0.00644)	(0.00519)	(0.00421)
LC Entry & Sex Education Median Split Interaction			−0.0126***
			(0.00414)
Live births per capita	0.547***	9.118***	0.639***
	(0.0976)	(2.933)	(0.0947)
Unemployment %	−0.0136***	−0.000439	−0.00817***
	(0.00164)	(0.00199)	(0.00124)
Personal income per capita	4.07e-06***	−1.29e-06	2.62e-06***
	(1.17e-06)	(1.77e-06)	(9.57e-07)
Education %	−1.046***	−0.845***	−0.910***
	(0.198)	(0.188)	(0.136)
Banks per capita	428.3***	−61.56	179.0**
	(100.2)	(117.4)	(74.92)
Deposits per capita	−9.138***	−1.563***	−1.984***
	(1.156)	(0.214)	(0.209)
Observations	3,304	2,523	5,827
Number of counties	884	657	1,541
Matching weights	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes

Note. Standard errors in parentheses.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 19. Effect Sizes of LendingClub Entry for Below and Above Median Religious Affiliations

	(1) Religion[a] below median: Number of procedures	(2) Religion[a] above median: Number of procedures	(3) Religion[a] median interaction: Number of procedures
LC Entry	0.0109**	0.0642***	0.0274***
	(0.00538)	(0.00577)	(0.00430)
LC Entry & Median Split Interaction			0.0199***
			(0.00399)
Live births per capita	−15.02***	0.569***	0.634***
	(2.706)	(0.0969)	(0.0947)
Unemployment %	−0.0107***	−0.00173	−0.00714***
	(0.00174)	(0.00196)	(0.00126)
Personal income per capita	1.07e-05***	−6.13e-06***	1.98e-06**
	(1.44e-06)	(1.29e-06)	(9.35e-07)
Education %	−0.990***	−0.723***	−0.919***
	(0.183)	(0.204)	(0.136)
Banks per capita	620.7***	−217.8**	160.0**
	(102.7)	(110.5)	(74.50)
Deposits per capita	−2.252***	−2.338***	−2.046***
	(0.229)	(0.551)	(0.209)
Number of observations	3,117	2,710	5,827
Number of counties	786	755	1,541
Matching weights	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Population as exposure	Yes	Yes	Yes

Note. Standard errors in parentheses.
[a]Religion variable is split at the median for the number of adherents (see the text for a definition) in a given county.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

through the pathway of financial barriers, to reduce access to medical care in situations where there is a potential for significant stigma.

Discussion and Conclusion

In this work, we examine how the entry of P2P lending platforms influences the rate at which women elect not to carry to term. The implications of access to capital on the ability to secure abortion services are considerable. Whereas there has been much discussion of the benefits and shortcomings of P2P lending, and detailed examinations of how such platforms perform and are governed, their societal implications are only now being examined. In this study, we extend this stream of research on the democratization of capital in the context of women's health. Our findings provide insights into a phenomenon hidden behind the data. The use of P2P lending for medical procedures that have financial barriers is intuitive. When a medical procedure has social barriers with a strong stigma attached, however, establishing such a relationship requires a detailed examination of data, combined with the evidence from the literature, and a qualitative investigation of the phenomenon by interviewing stakeholders. Future research, based on either primary data or detailed medical records, which are unavailable for privacy reasons, is needed to further corroborate and explore the relationship that we uncover here. To the extent that an open discussion of the effects of lending platforms on abortion rates in the public domain is difficult to observe, given the social conditions, our results represent a first attempt to uncover changes that manifest from the advent of platforms in sensitive social settings.

Notable social and theoretical implications stem from this work. From a social perspective, it is important to consider how these results may inform the abortion debate in a way that enhances welfare. Whereas prolife advocates raise ethical concerns with the procedure (Alcorn 2000), there is evidence that maintaining the legality of abortion has a meaningful impact on crime (Donohue and Levitt 2001), reduces unanticipated births or births in unstable environments (Akerlof et al. 1996), and decreases child maltreatment (Bitler and Zavodny 2002). Given the intensely divisive nature of the procedure (Matthews et al. 1997), we posit that any external factor that can significantly change abortion rates is important to study and document, for two reasons. First, any regulatory or market influence on these factors (such as the entry of peer-to-peer lending) should be conducted in the presence of full information. Legal battles pertaining to abortion in the United States have been argued in the U.S. Supreme Court multiple times in the last five years alone (see *Whole Woman's Health*

v. Hellerstedt, *June Medical Services LLC v. Russo*, and *United States v. Texas*); these debates must be based on empirical data and rigorous analysis.

Second, given the paucity of adequate data on this topic, and the significant implications involved, we believe new streams of research are warranted that provide empirical evidence, rather than rhetorical claims, on the subject. The estimates that we obtain here should be viewed as a starting point for more work on this subject rather than the definitive answer. In effect, for socially responsible policymaking, we posit that empirical work on the causal impact of platforms and markets on abortion rates is necessary, and that this work should build up to a coherent and influential program of research that can inform policymakers. Speaking directly to policymaking that affects the availability of abortion is outside the domain of this work. However, in terms of informing said policymakers, we show that some women are unable to access abortion services due to financial constraints, and those constraint appear to be lifted when P2P lending platforms enter the market. These platforms create a new channel by which short-term capital can be accessed in a way that protects privacy and shields individuals from stigma.

From a legal perspective, the implications of this paper depend on the perspective of the reader. Since *Planned Parenthood v. Casey*, the Supreme Court has held that the State, prior to viability, cannot place an undue burden on the mother when making her decision. And while the legal definition of "undue burden" has evolved significantly since *Casey* (Ziegler 2017), it is evident that an absence of freely available capital does place the procedure out of reach for some women. Hence, for prolife readers, our work provides corroborative evidence of the effectiveness of their approach in limiting the availability of the procedure. For advocates of reproductive rights, this work emphasizes the need for policies that decrease out-of-pocket costs for women, as well as the need to subsidize indigent women who lack the financial means to access abortions (e.g., the National Network of Abortion Funds), if they wish for policies to be effective in advancing their goals. As mentioned above, we hope that our work serves as a call for greater attention to the barriers that women may face when attempting to access reproductive care.

There are also implications for financial institutions and lenders. To the extent that the entry of P2P lending platforms is associated with changing procedure rates, it is evident that there is unmet demand for financing from individuals who may not feel comfortable accessing traditional sources of capital. If, as we conjecture, traditional lenders are not approached by women considering the termination of their pregnancies for fear of ill treatment, whether intentional or not, it follows that such lenders are failing to fully service the market. On the one hand, this may be viewed as a source of competitive disadvantage for these lenders, as they are losing customers to P2P platforms. Revising their procedures for lending, keeping this potential market in mind, may be necessary. On the other hand, if lenders are happy to cede this market to entities like P2P platforms based on taste, then there may be reactions on the part of shareholders, either positive or negative, especially in the sensitive context of abortion-based lending. Either way, our work provides implications for traditional lenders, where platforms serve as an alternative source of capital.

Theoretically, we contribute to the literature in two ways. First, whereas research on the social implications of platforms has expanded in recent years (e.g., Greenwood and Wattal 2017, Wang and Overby 2021), from both an economic and healthcare perspective, most work focuses on the direct implications of platform participation (e.g., Chan and Ghose 2014, Burtch et al. 2018, Wang and Overby 2021). We augment this work by showing how the democratization of capital can resolve social frictions that are indirectly associated with participation. We thus extend the literature on P2P lending to illustrate how traditionally marginalized financial groups, namely, women and persons of lower socioeconomic status, can use such platforms to circumnavigate liquidity barriers. We are also able to push work on the social implications of platforms. To date, most work has studied individual-level effects of other financial services platforms (e.g., Stephen and Galak 2012, Burtch et al. 2013, Mollick 2014, Younkin and Kuppuswamy 2018), with some notable exceptions. Whereas this is valuable, it is important to continue the abstraction to a more macro perspective. Finally, we show that the increasing availability of capital through secondary lending can obviate nontrivial market frictions by granting individuals facing financial and social barriers access to care.

There are limitations to this work that offer opportunities for future research. First, we cannot observe the individual woman who chooses to terminate a pregnancy. Although detailed data on individuals are available for specific states, granular data of this type are limited and tightly controlled for privacy reasons. In this same way, we cannot observe the impacts on other aspects of women's lives, such as increased access to education or startup capital, which accompany the democratization of financial markets. It should be noted that, during the execution of our semistructured interviews, we attempted to interview women who had secured an abortion. These requests were rebuffed for privacy reasons. Further attempts to contact women via support forums (e.g., r/abortion,

afterabortion.com, healthboards.com, and femaleforum.com) were universally taken down by site administrators within minutes of posting. Future work that can interface directly with women who have terminated a pregnancy will add credibility to any analysis. Similarly, data from each of our sources are only available at the year level. And while more granular data are preferable, investigations at the year level remain popular (Chan and Ghose 2014; Chan et al. 2016, 2019).

Second, we argue that financial barriers are a significant mechanism in this decision, in that platforms help reduce such barriers. However, to the extent that we do not directly observe the price paid by individuals for the procedure, this relationship remains inferred rather than observed. Empirical checks, such as a manipulation of the cost paid through the number of clinics in the county or household income, alleviate this to some extent. In this same vein, it is possible that some women leverage these platforms to help finance the delivery of a child. Although such an approach is not viable for raising a child, as the notes payable accrue interest and must be repaid, our analysis cannot rule this possibility out. Similarly, as discussed, the platform's emergence happens at a time of significant financial strife (i.e., the 2008 financial crisis). Although robustness checks and replication with another platform alleviate this somewhat, these remain limitations, nevertheless.

Third, we study steady-state results but cannot identify if individual behavior specifically changes upon entry of the platform. Specifically, does the easier access to capital provided by the availability of P2P lending induce riskier behavior on the part of individuals, thereby potentially necessitating more procedures, or increase women's access to education, which might undermine the need for the procedure? We do not have enough visibility to posit individual behavior, although this specific concern seems unlikely (i.e., P2P lending stimulating additional unprotected sexual encounters). Fourth, although we see an effect on the number of procedures, we cannot make assertions about the overall welfare effect of P2P lending. It is beyond the scope of this work to comment on the political and moral issues associated with a woman's right to choose. To the extent that peer-to-peer markets have political or ethical undertones, studying these requires a different analytical approach. Finally, we exploit a single platform, LC, during our investigation, although replications with Prosper provide consistent results. Inasmuch as other platforms also exist (e.g., Prosper and LendingTree), it is important to make two points. First, we focus on LC because it is the largest P2P lender by a considerable margin and should account for the lion's share of any effect. Second, other P2P lending platforms should affect decision making via the same mechanisms (i.e., increased liquidity coupled with greater privacy), meaning that any additional platforms should only further the observed effect.

In summary, we show how the entry of digital multisided platforms can have significant societal implications. We further provide evidence for how the entry of LendingClub affects women's reproductive decisions. To the extent that accessing medical services is often viewed as beneficial, our work shows how platforms can reduce the financial barriers in this marketplace. In future work, we hope that scholars are able to extend deeper into the legal, ethical, and political implications of digital platforms. We believe that extending the study of digital multisided platforms to noneconomic societal outcomes is much needed and represents a desirable trend in extant IS literature; our work contributes to and builds on this trend.

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Endnotes

[1] Quoted in Carmon and Knizhnik (2015, pp. 175–176).

[2] See https://trackbill.com/bill/us-congress-house-bill-7792-abortion-providers-loan-elimination-act/1941116/.

[3] See https://www.usnews.com/news/politics/articles/2021-10-13/we-have-to-be-heard-texas-women-travel-to-seek-abortions.

[4] See https://www.newyorker.com/news/news-desk/how-abortion-law-in-new-york-will-change-and-how-it-wont.

[5] See https://shoutyourabortion.com/

[6] See https://www.npr.org/2019/05/23/726294656/a-look-at-the-economics-of-ending-a-pregnancy

[7] See https://www.glamour.com/story/how-much-does-an-abortion-cost.

[8] See https://www.teenvogue.com/story/abortion-stories.

[9] See https://www.lifewomenshealthcenter.org/ and https://rgiohio.com/patient-resources/insurance-costs/.

[10] It is worth noting that the Court also changed the reviewing requirements to rational basis, rather than strict scrutiny, during the *Casey* decision. As a result, any restriction of the procedure need only be plausibly related to a legitimate government interest, rather than be narrowly tailored, and rigorously shown, to further a compelling governmental interest.

[11] See https://www.lendingclub.com/trypatientsolutions/fertility.

[12] See https://www.prosper.com/blog/fertility-treatments-in-the-united-states-sentiment-costs-and-financial-impact/.

[13] See https://www.embryo.net/financial-support and https://www.verywellfamily.com/best-ivf-loans-5092336.

[14] See https://www.reddit.com/r/infertility/comments/2ldbsm/ivf_loans/.

[15] See https://www.dreamababy.com/fertility_financing.php.

[16] See https://www.cnyfertility.com/ivf-financing-and-loans/.

[17] See https://adoptionsbygladney.com/resources/adoption-finances.

[18] See https://fertilitynw.com/financial-insurance-information-2/.

[19] See https://www.lendingclub.com/public/how-peer-lending-works.

[20] See https://www.lendingclub.com/loans/resource-center/top-reasons-to-get-a-personal-loan.

[21] See https://www.aclu.org/issues/reproductive-freedom/abortion/bans-insurance-coverage-abortion.

[22] See https://www.thecut.com/2018/11/how-much-does-an-abortion-cost.html.

[23] See https://www.forbes.com/sites/maggiemcgrath/2016/01/06/63-of-americans-dont-have-enough-savings-to-cover-a-500-emergency/.

[24] For more details, see https://www.lendingclub.com/loans/personal-loans or https://www.prosper.com/personal-loans.

[25] It is worth considering if P2P platforms might be used to assist with the out-of-pocket expenses associated with giving birth. Although plausible as a discrete financial outlay, it seems unlikely that platforms could subsidize the raising of a child given the disparities in overall cost. The USDA, for example, estimates that the cost of raising the child at approximately $233,000 (approximately $284,000 when adjusted for inflation) or approximately $13,000 per year. Given that it would be impossible to continually subsidize the child on an ever-increasing set of P2P loans, because these notes must be repaid with interest, it is easy to see how the long-term financial calculus is intractable. We thank an anonymous reviewer for raising this issue.

[26] LendingClub has served over 2 million borrowers nationwide, originating loans totaling over $33 billion so far.

[27] See https://www.nerdwallet.com/blog/average-credit-card-debt-household/.

[28] See https://www.smartasset.com/credit-cards/the-average-credit-card-limit.

[29] See https://www.bankrate.com/loans/personal-loans/top-reasons-to-apply-for-personal-loan/.

[30] Some interviewees indicated that financial shortfalls could be as severe as $20. While women seeking this level of assistance are unlikely to be funded by the platform (participants needing a minimum credit score of 600), participants with a "Fair" credit score (between 580 and 669) are plausible candidates for short-term financing because they are viable lendees and often have a significant credit utilization rate (meaning their liquid cash reserves are minimal, but they are capable of developing a financial plan).

[31] Avoidance of payday loans was reiterated constantly in interviews.

[32] See https://www.guttmacher.org/fact-sheet/induced-abortion-united-states.

[33] See https://help.lendingclub.com/hc/en-us/articles/213706208-Qualifying-for-a-personal-loan.

[34] See http://www.johnstonsarchive.net/policy/abortion/index.html.

[35] The financial crisis in 2008 could affect the matching process by creating additional heterogeneity between the control and treatment groups that the matching variables do not capture. We examine this issue in detail in Online Appendix E and find consistent results even after accounting for county-level financial factors in matching. We thank an anonymous reviewer for raising this point.

[36] To recover an estimate of the effect size, we condition on population, instead of setting it as exposure, and estimate time and location fixed effects explicitly using a standard Poisson. The effect is calculated with margins and other variables held at mean.

[37] We thank the anonymous reviewer for raising the possible countervailing effects of LC entry.

[38] See https://www.plannedparenthood.org/uploads/filer_public/71/53/7153464c-8f5d-4a26-bead-2a0dfe2b32ec/20171229_ar16-17_p01_lowres.pdf for details.

[39] We thank the anonymous panel for the suggestion of this robustness check.

[40] We thank our associate editor for guiding us to the resources regarding this potential bias.

[41] From 2005 to 2009, Prosper was available nationwide. Although its operations may have been disproportionally disrupted during the legal challenges that were resolved in 2009, these potential disruptions are not documented, hence preventing us from coding treatment and control groups before 2009. From 2009 to 2015, the residents of all states except the three states (Iowa, Maine, and North Dakota) have access to Prosper loans, hence providing us with our DD design.

[42] It is worth noting that no peer-reviewed research has established a credible causal link between legal abortion services, psychiatric morbidity, or other adverse health outcomes. Work in this space has been widely criticized for failing to adhere to proper guidelines for research (Abel et al. 2012). As discussed by Major et al. (2009, p. 863), "Major methodological problems pervaded most of the research reviewed. The most rigorous studies indicated that within the United States, the relative risk of mental health problems among adult women who have a single, legal, first-trimester abortion of an unwanted pregnancy is no greater than the risk among women who deliver an unwanted pregnancy." Further, to quote from the *AMA Journal of Ethics*: "17 states now mandate that clinicians provide women seeking abortions with scripted counseling that includes false information on at least one of the following topics: a link between abortion and breast cancer, the ability of a fetus to feel pain, and long-term mental health consequences for women who have abortions. These statements are not evidence-based and have been countered in the literature. To require that clinicians give inaccurate information to patients is, to say the least, unethical" (Steinauer and Sufrin 2014, p. 266).

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