Screen Now, Save Later? The Trade-Off between Administrative Ordeals and Fraud*

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Abstract

Screening requirements are common features of fraud and corruption mitigation efforts around the world. Yet imposing these requirements involves trade-offs between higher administrative costs, delayed benefits, and exclusion of genuine beneficiaries on one hand and lower fraud on the other. We examine these trade-offs in one of the largest economic relief programs in US history: The Paycheck Protection Program (PPP). Employing a database that includes nearly 11.5 million PPP loans, we assess the impact of screening by exploiting temporal variation in the documentation standards applied to loan applications for loans of different values. We find that screening significantly reduced the incidence and magnitude of various measures of loan irregularities that are indicative of fraud. Moreover, our analysis reveals that a subset of borrowers with a checkered history strategically reduced their loan application amounts in order to avoid being subjected to screening. Borrowers without a checkered history engaged in this behavior at a much lower rate, implying that the documentation requirement reduced fraud without imposing an undue administrative burden on legitimate firms. All told, our estimates imply that screening led to a $737 million reduction in losses due to fraud.

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1 Introduction

Corruption and fraud plague public programs around the world (Glaeser and Saks, 2006; Olken and Pande, 2012; Finan et al., 2017).[1] Public benefits may be captured by ineligible beneficiaries, or beneficiaries may obtain benefits greater than those to which they are entitled (Becker et al., 2005; Olken, 2006; Niehaus and Sukhtankar, 2013; Fang and Gong, 2017). To prevent this leakage, governments can impose screening requirements ex-ante, potentially including small "ordeals" in order to induce ineligible applicants to select out of the program before injury to the public fisc has taken place (Nichols and Zeckhauser, 1982; Besley and Coate, 1992).[2]

However, these requirements may reduce the timeliness of delivery, impose added costs for both the government and beneficiaries, and could lead to the exclusion of legitimate beneficiaries (Currie, 2006; Kleven and Kopczuk, 2011; Herd and Moynihan, 2018). Whether the costs of higher administrative expenses, delayed benefit transfers, and increased exclusion (Type I error) are higher than the savings from lower inclusion (Type II) errors is ultimately an empirical question. Prior evidence is mixed. Alatas et al. (2016) find that small ordeals improved the targeting of benefits by dissuading ineligible beneficiaries from applying for cash transfers in Indonesia. Yet in the US, Deshpande and Li (2019) find that application costs reduce targeting efficiency by dissuading poorer and more severely disabled beneficiaries from applying for disability benefits, and Gray (2019) and Homonoff and Somerville (2021) similarly show that reporting and interview requirements undermine retention among legally eligible households in the Supplemental Nutrition Assistance Program (SNAP). Understanding and identifying when costs of the administrative requirements are low relative to their value as a tool for sanctioning fraud is thus important in maintaining the integrity and viability of public programs.

We examine these issues in one of the largest economic relief programs in US history: The Paycheck Protection Program (PPP), a \$814-billion small business stimulus program adopted as part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act signed into law on March 27, 2020. The PPP permitted small businesses, nonprofits and certain other entities, typically with 500 or fewer employees, to apply for federally-backed loans administered by banks and other private lenders on behalf of the Small Business Administration (SBA). These loans in essence operated as grants, since program rules stipulated that repayment would not be required of borrowers as long as the funds were used for purposes prescribed by the federal government.[3] Moreover, there were weak incentives for due-diligence by lenders, since loans were 100% guaranteed by the SBA if the borrower defaulted for "legal purposes".[4] The primary oversight mechanism was ex-post auditing

[1]Starting with the seminal work by Becker and Stigler (1974), a number of scholars have put forth conceptual frameworks to assist our understanding of corruption, including but by no means limited to Shleifer and Vishny (1993); Banerjee (1997); Banerjee et al. (2012).

[2]An alternative to ex-ante screening is to deliver benefits first and audit ex-post. Auditing serves a punitive, and *ipso facto*, deterrent function. While a large literature demonstrates that rigorous ex-post auditing reduces corruption (Di Tella and Schargrodsky, 2003; Olken, 2007; Bobonis et al., 2016; Avis et al., 2018; Zamboni and Litschig, 2018), our focus in this paper is on the marginal value of screening in addition to performing audits.

[3]Loan forgiveness for eligible businesses was built into the program through Section 1106, CARES Act, 2020.

[4]Section 1102, CARES Act, 2020. Lenders were held harmless if they acted in good faith and all documents were

by the SBA. The PPP was divided into two distinct phases: phase 1 (April 2020 to August 2020) and phase 2 (January 2021 to May 2021).

In order to study the impact of screening requirements on loan irregularities, we exploit a sudden change in the rule regarding proof of loan eligibility announced in phase 2 of the PPP.[5] The new rule stipulated that all firms who had previously received a PPP loan and were requesting loans greater than $150K were required to submit with their PPP application documentation proving that they had experienced a reduction in gross receipts in excess of 25% in 2020 relative to 2019. Firms requesting loans of ≤$150K were also only legally eligible to receive funds from the program if they experienced a 25% or more reduction in gross receipts. Yet they were not required to provide up front to the lender any documentation proving this in order to have their applications processed. In phase 1, there were no differences in the documentation that firms were required to provide up front to lenders based upon the size of the loan for which they were applying.[6]

Given variation in screening by program phase and loan value, we employ a difference-in-differences estimation strategy to compute the impact of screening on serious irregularities in the receipt of loans from the PPP. Since our data consists of the full corpus of 11.5 million PPP loans allocated over the course of the two phases of the program, this allows us to use *the universe of borrowers in phase 1* to determine the set of firms that were subjected to the advance documentation requirement in their second loan applications. We define our treated firms as those that had received a loan greater than $150k in *phase 1*, i.e., before the announcement of the rule change, and reapplied in phase 2. The control firms are those firms that received a loan ≤$150k *in phase 1* and reapplied in phase 2.

We utilize as primary outcomes three measures of irregularities that are indicative of (though not dispositive of) fraudulent behavior on the part of the borrower: the receipt of a loan that exceeded the maximal permissible payment given firm characteristics presented in the loan application, the amount of overpayment expressed as a fraction of the maximal permissible payment (overpayment rate), and the receipt of multiple loans from the PPP during a single phase of the program. For each measure, we find an economically meaningful and statistically significant reduction in irregular loans attributable to the screening requirement.

To investigate the mechanisms underlying these results, and to better understand the trade-off between the administrative burden of complying with upfront documentation requirements and fraud prevention, we present a simple conceptual framework to demonstrate the expected impact of the rule change. In our model, legitimate and fraudulent firms both face some administrative costs of complying with either upfront or ex-post documentation requirements; the costs for fraudulent firms increase discretely with an upfront requirement given the prospect of legal sanction upon discovery of their fraud. We show that firms will respond to the documentation requirements

complete.

[5]The SBA made an announcement on January 6, 2021; the change was effective for loans made after January 14 2021 (13 CFR Parts 120 and 121).

[6]Other requirements also more or less remained the same across phases 1 and 2, as well as for loans above and below $150K.

by strategically requesting less than $150K in phase 2, leading to "bunching" of the frequency distribution under $150K; however, if the costs of the administrative requirements are low relative to their value as a tool for sanctioning fraud, the bunchers will be composed disproportionately of fraudulent firms.

Consistent with our framework, we find that borrowers with loan irregularities in phase 1 who would have been subject to screening in phase 2 responded to the new upfront documentation requirement by reducing their loan requests (as opposed to exiting the program). We explore this intensive margin response further and find strong evidence of the strategic evasion of the documentation requirements associated with screening. In phase 2 of the program, we observe bunching in the number of borrowers receiving loans right below and just at the $150K threshold that determined the use of screening. In phase 1, by contrast, there was no discontinuity at this threshold. This suggests that many borrowers strategically set their loan requests just below the threshold to avoid submitting documentation.

The shift in the locus of fraud to lower loan values resulted in considerable savings to the PPP: We estimate that it reduced overpayment by $737 million, or 88% of the total reduction in overpayment between phases 1 and 2.[7] Moreover, even firms that appear to have strategically evaded screening nonetheless engaged in less fraudulent activity once it had been introduced. The strong behavioral reaction to screening that we document, concentrated as it was among borrowers with previous loan irregularities, implies that screening was perceived of as a genuine risk for borrowers who may have been defrauding the program. Simply applying the same screening requirement in phase 1 would have resulted in overall savings of $1.5 billion, ceteris paribus. An across-the-board advance documentation requirement - for all loan values - would likely have had an even more significant impact in reducing losses due to fraud.

Our results indicate that substantial savings may be generated by forms of screening that impose minimal bureaucratic hurdles. The administrative burden of providing proof of eligibility in phase 2 of the PPP was quite low. Any legally functioning enterprise that pays taxes must of necessity have documentation that could be used to show recent changes in revenues. Thus, simply requiring program participants to present documents which they must have in order to satisfy their other legal obligations–a minimal ordeal–can be highly effective in reducing the participation of fraudulent actors without imposing an undue burden on legitimate participants.

These findings have implications that extend beyond the context of the US federal government's Covid-19 relief efforts. This includes programs for which the timeliness of relief is essential, the potential participants are large in number, and the capacity to detect fraudulent intent is limited. For instance, emergency relief programs have these features.[8] Another example of a critical

[7] Amounts calculated based upon the estimates presented in Table A4.

[8] The incidence and scope of such relief programs is only likely to grow in the future, not just due the prospect of new variants of Covid or future pandemics, but due to the realities of climate change. The warming climate has led to more extreme natural disasters, with concomitantly greater economic costs (Coronese et al., 2019; Estrada et al., 2015). Evidence suggests that it has also reduced economic growth (Dell et al., 2012; Kotz et al., 2021). Given this environmental and economic reality, the organization of emergency relief is poised to become an increasingly salient responsibility of government.

government task that shares these features is tax collection.[9]

Our paper contributes to the literature on screening requirements for public benefits. The theoretical literature (Nichols and Zeckhauser, 1982; Besley and Coate, 1992; Kleven and Kopczuk, 2011) clearly lays out the trade-offs involved. The empirical literature finds mixed evidence. In addition to the papers cited above, Finkelstein and Notowidigdo (2019) show that an intervention designed to reduce the cost of applying to the SNAP in the US mostly benefited richer beneficiaries, although they suggest that this reflected the poor targeting of the intervention rather than the general properties of screening requirements. Our results suggest that in general, in settings where program scope or timing are such that rigorous ex-post auditing systems are costly and/or infeasible, screening can be a particularly valuable tool in mitigating fraud. This is especially the case if the documentation requirements that characterize screening are easy for legitimate program participants to satisfy at a reasonable cost.

Our paper also relates to the broader literature on regulation in law and economics.[10] In particular, our work has a natural link to studies that compare ex-ante regulation with ex-post enforcement of harmful behavior, both in the private and public sectors. There is a long-standing and large theoretical literature that describes the conditions under which screening/regulation and auditing/enforcement act as complements or substitutes.[11] In contrast, the empirical literature is extremely limited. To the best of our knowledge the only study that empirically investigates this issue is Behrer et al., 2021, who show that water quality improved when ex-post oversight mechanisms were replaced by ex-ante regulation by the 1972 Clean Water Act (CWA). In a related vein, the distinction between ex-ante and ex-post monitoring features prominently in the political science literature on legislative oversight of the executive branch. The central concern of this literature is understanding how legislators can mitigate opportunism by bureaucrats in light of informational asymmetries and the prospect of ex-post monitoring by constituents and interest groups (McCubbins and Schwartz, 1984; Lupia and McCubbins, 1994). Our contribution to these related bodies of work lies in empirically establishing an approach for assessing the trade-offs entailed by greater ex-ante regulation, which we apply to a government program of unprecedented

[9] In the five year period from 2015-2019, the audit rate of individual tax returns was only 0.6% (TIGTA, 2021), suggesting that the risk of a formal audit may be unlikely in and of itself to dissuade many actors from engaging in tax evasion. Yet the payment of taxes requires ample ex-ante documentation (e.g., W2 forms for wage earners), facilitating the use of automated systems to detect discrepancies or irregularities. A large literature examines the role of this type of third-party document reporting in tax compliance; see for example Kleven et al. (2011); Pomeranz (2015). These upfront documentation requirements introduce additional hurdles and risk for those who would seek to evade their taxes, and surely contribute to broad compliance in spite of a low incidence of formal auditing.

[10] A large theoretical body of work investigates optimal regulation. Laffont, 1994 and Estache and Wren-Lewis, 2009 present an excellent review of the key ideas. Some studies have focused on the possibility of collusion between the regulator and the regulated (Stigler, 1971; Posner, 1974; Burgess et al., 2012; Jia and Nie, 2017). Along these lines, several papers study the problem of regulation from the lens of incentives of regulators (Glaeser et al., 2001; Duflo et al., 2013, 2018), while others investigate how the selection of the regulator affects social welfare (Besley and Coate, 2003).

[11] Theoretically, whether ex-ante regulation is better than ex-post control depends on, for example, the relative costs of enforcement (De Chiara and Livio, 2017; Strausz, 2005), in particular transaction costs (Coase, 1960) and whether there are fixed cost of lawsuits (Posner, 1998); heterogeneity in offense severity and limits on the violator's ability to pay (Shavell, 1984a,b); the degree of uncertainty in potential harm and uncertainty in whether and to what extent the legal system will penalize the violator (Kolstad et al., 1990; Mookherjee and Png, 1992); and the possibility of ex-post subversion of justice by the potential violator (Glaeser and Shleifer, 2003).

scope.

Finally, our paper also contributes to a small but growing literature on COVID relief funds, in particular the PPP. Much of this literature examines its impact on employment and business survival, with some evidence that it boosted both outcomes but debate over magnitudes (Hubbard and Strain, 2020; Autor et al., 2020; Granja et al., 2020; Chetty et al., 2020); as well as on appropriate targeting, with evidence that larger firms were better able to access the program (Bartik et al., 2020; Humphries et al., 2020; Balyuk et al., 2021). Our paper is most closely related to two studies that examine fraud in the PPP, with Griffin et al. (2021) suggesting that FinTech lenders were responsible for much of this fraud, while Beggs and Harvison (2021) find that 6% of PPP funds that went to investment management firms likely consisted of overallocations. Our paper corroborates much of these two papers' findings about systemic fraud, while highlighting the importance of institutional design.

2 Background

A reaction to the economic disruptions created by Covid-19, the PPP was designed to provide small businesses with large influxes of money in a very short period of time. The first phase of the program was established under the CARES Act and lasted from April 2020 until August 2020. The Economic Aid Act established the second phase of the PPP program that lasted from January 2021 until May 2021. The second phase of the program operated under the same terms and conditions as phase 1 with a few important exceptions that we outline below.

Although nominally structured as loans, loan forgiveness for eligible businesses was built into the program and widely advertised across both phases of the program.[12] The monies disbursed under the program did not need to be repaid if used for certain purposes (such as payroll costs, payments on covered mortgage obligations, payments on covered lease obligations, or covered utility payments).[13] The program was formally managed by the Small Business Administration (SBA), an independent federal agency. Private-sector financial institutions (henceforth lenders) played a central role as intermediaries in the program.

The program had several eligibility criteria for borrowers under the PPP. Since the program was oriented towards small businesses, there were ceilings on the size of firms: Eligible firms had to employ five hundred employees or less in phase 1 and three hundred or less in phase 2.[14] In terms of economic criteria, firms applying in phase 1 of the program had to certify that "current economic uncertainty makes this loan request necessary to support the ongoing operations of the Applicant." To receive a PPP loan for the second time, eligibility was limited to firms that had experienced a reduction in gross receipts in excess of 25% in 2020 relative to 2019, but, as explained above, only firms requesting loans above $150K in value were required to provide documentation

[12] Section 1106, CARES Act 2020.

[13] There was a penalty on firms if they retained fewer workers or reduced their total wages in excess of 25%.

[14] The ceiling was set at three hundred employees in phase 1 for housing cooperatives, member-based professional organizations, and tourist boards.

of this as part of the application.[15] These documents could include relevant tax forms, including annual tax forms, or, if relevant tax forms are not available, quarterly financial statements or bank statements could be used.[16] Others had to retain these documents and could be asked for these later by the SBA or if the borrower applied for loan forgiveness. The SBA announced the change in the documentation requirement for loans greater than 150K on January 6, 2021 via the release of a rule change; it became effective for second time loans made after January 14, 2021.[17] This announcement came five months after the conclusion of the first phase of the PPP program (August 8, 2020).

Figure 1 describes the timeline of the Paycheck Protection Program with key events and attempts to mitigate fraud.

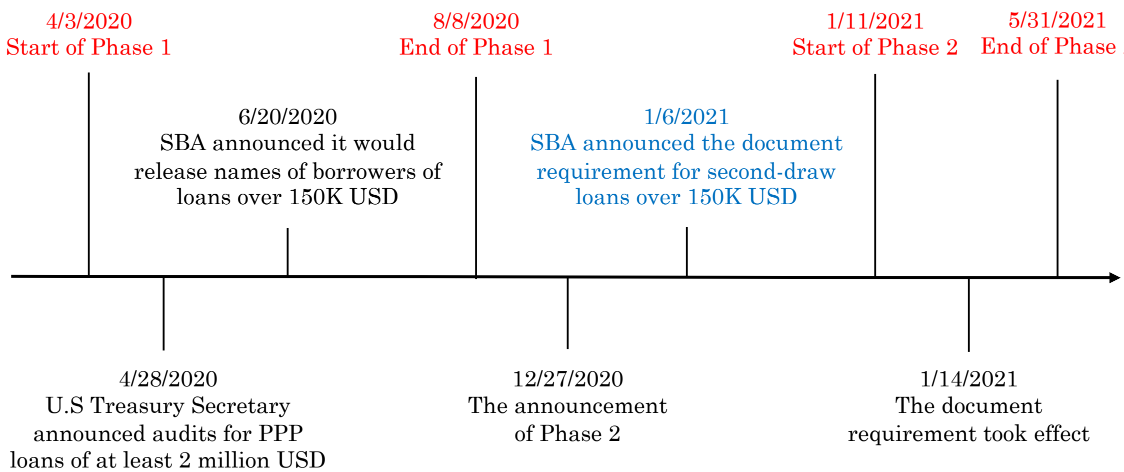

Figure 1: Timeline of the Paycheck Protection Program.

In addition to eligibility criteria, PPP rules specified the maximum loan amount that firms applying to the program could obtain. The maximum amount a firm could receive in phase 1 of the program was equal to the average employee compensation (salary and benefits) during the previous twelve months multiplied by 2.5. For the purposes of this calculation, employee salaries were capped at $100K. In this phase, no firm was permitted to receive more than $10 million from the program. The caps stayed much the same in phase 2, with the following exceptions: 1) restaurants and other firms within the accommodation and food services sectors could receive 3.5 times the average monthly compensation of their employees; 2) no firm could receive more than $2 million. Rules also stipulated that borrowers could not receive multiple loans for the same purpose.[18]

Given the emphasis on injecting capital in the private sector as quickly as possible, adherence to

[15] In phase 1 of the program documentation requirements were not linked to loan amounts.
[16] SBA's Interim Final Rule 13 CFR Parts 120 and 121.
[17] SBA's Interim Final Rule 13 CFR Parts 120 and 121.
[18] Section 1102, CARES Act 2020.

the eligibility criteria outlined above and even compliance with loan maximums appears to have followed an honor system. There were weak incentives for due diligence by lenders. Lenders provided the loans to recipients using money fully backed by the federal government. They were charged with processing the loan applications and verifying that the proper attestations were submitted by applicants. In exchange, the lenders received a fee from the SBA for each loan that they administered, with the size of the fee expressed as a percentage of the loan.[19] Crucially, the CARES Act contained a "hold harmless" clause stipulating that lenders that had received attestations from borrowers that the loans were used for authorized purposes could neither be subject to enforcement actions nor penalties related to said loans. Moreover, since the loans were backed by the federal government, lenders would not be on the hook in instances where borrowers needed to repay but failed to do so. In this way, the SBA allocated significant fees to lenders for managing loans for which they incurred zero risk.[20] In total, 5,460 lenders participated in the PPP.

The default oversight mechanism was ex-post auditing by the SBA. The SBA Administrator could review borrower eligibility for loans and loan forgiveness, loan amounts, and whether a loan was used for the permitted purposes at any time. PPP loans that are either taken for a first or a second time by firms could be subject to the review. To allow for ex-post auditing, borrowers were required to retain their application and all related documents for four to six years after the disbursement of the loan. Yet the frequency of auditing and the degree of scrutiny it entailed were never publicly disclosed. Only loans above $2 million were guaranteed to be audited.

Evidence of widespread fraud in the program is now abundant.[21] On March 26, 2021 the Justice Department announced that it had charged nearly five hundred defendants with engaging in fraud related to the PPP and other pandemic relief programs. Cases recently concluded with convictions illustrate the weak financial controls instituted in the PPP and give a sense of how the schemes operated. Dinesh Sah was sentenced in July 2021 to more than eleven years in prison for fraudulently obtaining over $17 million in funds from the PPP, using the money to purchase multiple homes and luxury automobiles, as well as to send millions overseas through wire transfers. By his own admission, he had filed fifteen fraudulent applications to eight different lenders, claiming employees and payroll expenses in his businesses that were vastly at odds with the true figures. To give a sense of the ease of fraud, Sah registered a fake company on May 18, 2020 (well after the stipulated February 15 cutoff date) then filed a PPP application for the company that same day. One day later, he received $2 million in his account for the non-existent firm. Another notable case was that of former NFL wide receiver Joshua Bellamy, who was sentenced

[19]Until February 2021, lenders received as a fee 5% of the loan amount for loans of $350K or less, 3% for loans between $350K and $2 million, and 1% for loans greater than $2 million. After February 2021, lenders received either a fee of 50% of the loan amount or a fixed payment of $2,500, whichever was smaller, for loans of $50K or less. For other loans, the fee schedule remained unchanged. For loans ineligible for forgiveness, lenders also receive a 1% interest rate.

[20]The fees do not represent pure profits, however, due to the administrative costs of managing the loan applications. Early reporting suggests there was significant variation across lenders in the profitability of PPP loan administration. See Cowley (2020).

[21]A recent article from the New York Times bemoans the fact that prosecutors are unable to process the "tidal wave of pandemic fraud." `https://www.nytimes.com/2022/08/16/business/economy/covid-pandemic-fraud.html`, accessed August 16, 2022.

in December 2021 to three years in prison for fraudulently obtaining a $1.2 million loan for his company, Drip entertainment, much of which was spent on jewelry, hotels, and other personal expenses. The federal complaint alleges that Bellamy was part of a group of conspirators who worked with an intermediary who prepared the loan applications and falsified paperwork in exchange for kickbacks on the PPP loan proceeds. FinTech lenders appear to have approved a disproportionate number of fraudulent loans identified by the Justice Department.[22]

In addition to fraud related to overstatements of job figures and employee compensation, there have been reports of companies receiving more than one PPP disbursement in the same round. On March 15, 2021, the Office of Inspector General (OIG) released a report in which it found 4,260 borrowers were approved for more than one loan as of August 31, 2020, which cost the program approximately 692 million U.S dollars.

3 Data and variable coding

3.1 Data sources

PPP loans. Our primary data is the universe of PPP loans approved across the two phases of the program. This data is made available by the Small Business Administration (SBA).[23] The total number of approved loans is 11,475,004 (5,136,454 in phase 1 and 6,338,537 in phase 2). The data includes details on the names and addresses of the borrowers, the loan approval date, whether the loan is a first or a second time loan, the borrower's industry (NAICS codes), the number of employees reported by the borrower, the loan amount, the status of the loan (whether paid in full or charged off), loan maturity, whether SBA guaranteed the loan, the purpose for which the loan is sought, the business type, the congressional district of the borrower and the names and address (only headquarter) of the lenders. It also includes other information on the borrowers such as race, ethnicity, gender, veteran status, whether the firm is located in a rural or urban area, and whether the firm is a non-profit. Table A1 shows descriptive statistics of firms that received PPP loans across the two phases. The majority of firms that received loans were urban and were either corporations or limited liability companies. Over seventy percent of firms had 10 employees or less.[24]

Dun & Bradstreet (DNB) data on number of employees. Since loan amounts were based on the number of employees there was an incentive for firms to misreport their employee numbers on the PPP application. To observe whether there are any such discrepancies, we use the firm employee figures compiled by DNB. DNB maintains two sets of data on firms and their number

[22]For information on cases brought by the Department of Justice for PPP fraud, see the website of the COVID-19 Fraud Enforcement Task Force, www.justice.gov/criminal-fraud/cares-act-fraud. On FinTechs and the incidence of fraud, see Griffin et al. (2021).

[23]We retrieved this data from the SBA's website on the 24th of November 2021.

[24]Demographic questions had a low response rate on PPP applications. However, those who did report their race were mainly white. Similarly, more men than women received PPP loans.

of employees: one is verified and based on actual figures from the firms and the other is imputed by DNB. We use the verified part of their data on 1,691,849 million PPP loan applicants.[25] This dataset was verified and updated in July 2021. For the firms in this dataset, we have a snapshot of each firm's number of employees both in phase 1 of the PPP program as well as in phase 2.[26]

3.2 Key variables

Overpayment on PPP Loans. We classify the approval of a loan that exceeded the maximum permissible payment as an overpayment on a PPP loan. To observe such overpayments, we combine PPP loan level data with SBA's rule on disbursement of funds. Using information presented in the loan applications about the number of employees and the industry in which the firm was operating, we first compute the maximum payment for which the firm was eligible as per SBA's rules.

In Phase 1, the following maximum payment method was applied to every firm with employees:

$$Maximum\ payment = No.\ of\ employees \times (2.5 \times \$100,000/12 + \$9,166),$$

where $100,000 is the maximum annual salary for each employee that firms can report on their PPP applications. For self-employed workers without employees, the maximum payment was calculated without the $9,166 that is the average benefit spending on each employee.[27]

In Phase 2, the maximum payment is calculated similarly for firms except those from the Accommodation and Food Services industry that took out loans for a second time. For these firms, SBA set a higher threshold. Therefore, we use the following method was instead:

$$Maximum\ payment = No.\ of\ employees \times (3.5 \times \$100,000/12 + \$9,166)$$

We then compare the maximum payment due with the actual approved amount and define overpayment on PPP loans in three different ways. The first is an indicator variable, *overpayment dummy*, equal to 1 if any of a firm's approved loans in a phase is above the maximum amount due, 0 otherwise. The second is the variable *overpayment rate*, equal to the amount of overpayment expressed as a fraction of the maximum payment that was due to borrowers. If a firm had multiple loans with overpayments in a phase, we used the maximum overpayment rate across the set of loans in any given phase. The third is the variable *overpayment amount*, equal to the total dollar amount by which all the loans in any phase exceeds the maximum. Figure 2 plots the distribution

[25]The DNB dataset was matched to the PPP dataset by DNB: 3.3 million observations were matched in total, out of which 1.7 million were verified and not imputed.

[26]While the data we use was updated overall in 2020 and 2021 by DNB, there is heterogeneity in the date that any particular firm's record was updated. Therefore, we place less weight on this data in our analysis and caution readers that some of the discrepancies between jobs reported in PPP applications and those in the DNB data may be due to other factors besides fraud.

[27]The $9,166 benefit spending amount was derived from the SBA's method of calculating maximum loan payment as presented in their January 2021 report (https://www.sba.gov/sites/default/files/2021-01/SBA%20OIG%20Report-21-07.pdf).

of overpaid amounts conditional on a firm reapplying in Phase 2 and having overpaid loans in phase 1. The figure shows that the distribution of overpaid amounts in Phase 2 shifted to the left of those in Phase 1 of the program. Further descriptive statistics for the overpayment indicators are shown in Table A2. The approved amount per loan was on average $101,589 USD in phase 1. The amount approved per loan fell in phase 2 to $42,748 USD. Both the number of overpaid loans as well as the amount overpaid fell in phase 2. The share of loans that had overpayments was 0.01 in phase 1, while it was 0.003 in phase 2. Since the total number of loans was 5,136,454 in phase 1, this suggests that almost 50,000 loans that were approved had payments above the maximum stipulated by law. This number fell to more than 19,000 overpaid loans in phase 2. Similarly, the overpaid amount per loan was $725 USD in phase 1, while it was much lower ($91 USD) in phase 2.

Multiple loans to the same borrower. The SBA does not provide a numerical firm identifier in the version of data for public access. Therefore, to determine whether two or more loans were disbursed to the same company, we use string matching on business name and address to assign a unique firm identification number to each group of loans associated with the same business name and address (the appendix for online publication has more details on our string matching algorithm). A firm could have received multiple loans in violation of the rules in any of the following ways:

- if a firm that participated solely in Phase 1 of the program received more than one loan in phase 1 (5,290 firms).
- if a firm that participated in both Phase 1 and Phase 2 of the program received more than one loan in any given phase (3,320 firms).
- if a firm borrowed for the first time in Phase 2 and received more than two loans (7,099 firms).

The average number of loans issued to a firm with multiple loans is 2 in Phase 1 and 3 in Phase 2. Nevertheless, the incidence of duplicate loans is low when compared to overpayment. Borrowers with duplicate loans make up only 0.15 percent and 0.18 percent of all participating firms in Phase 1 and Phase 2, respectively (Table A2). Since we were interested in looking at changes in firm behavior across phases in response to the screening requirement, we define multiple loans as a dummy variable equal to 1 when a firm that participated in both Phase 1 and Phase 2 of the program received more than one loan in any given phase.

Gap in jobs reported. Firms report their number of employees on the PPP applications and these figures are used to compute the loan amount to any firm. We combine this data with DNB data on employee figures and define the gap in jobs reported in two ways. First, we employ the simple difference in employee numbers across the two data sources (*job gap*). Second, we utilize the percentage by which PPP employee figures (positively) deviate from the DNB figures, and on this basis create a series of dummy variables that capture instances of extreme deviations of PPP figures from those contained in the DNB database (>300%, 400% or 500%). While evidence from both outcomes can be interpreted as suggestive of fraud, in our interpretation of the results we place more weight on the latter measure than the former one.[28]

Note that the above measures by no means represent a comprehensive account of fraud in the PPP. Our variables would miss any more sophisticated types of fraud - for example registering the same business with distinct names - and they do not account for what individuals actually did with the money - for example buying luxury consumption goods rather than spending it on employee wages. One should thus consider our measures as capturing some - relatively crude - aspects of fraud.[29]

4 Did screening affect fraud in PPP?

4.1 Identification strategy

The advance documentation requirement in phase 2 stipulated that firms with loan applications greater than $150k must submit documentation showing a reduction in gross receipts of more than 25% in 2020 relative to 2019. Those with loan requests of less than $150k were not required to submit such documentation with their loan applications, but they were required to retain said documents should the SBA later request it.

We use a differences-in-differences approach to estimate the effect of the screening requirement on fraud in the PPP. We define our treatment group as the set of firms that had applied for a loan greater than $150k in phase 1. Presuming that the firm fundamentals determining loan need and eligibility remained constant across the two phases, these are the firms that were affected by the changes in the documentation requirement. Our control group is the set of firms that had loan amounts less than $150k in phase 1.

The identification assumption motivating the differences-in-differences estimation strategy is *parallel trends*, i.e. firms whose loan amounts were greater than $150k in phase 1 would have experienced, on average, the same changes in fraudulent behavior across phases as those firms whose loan amounts were ≤$150k in phase 1, were it not for the fact that the documentation requirement was imposed on the former (treatment) group. As suggested in the literature, we assess the evidence in support of this assumption using an event-study plot prior to conducting the main analysis.

[28]This is the case since the DNB data on any particular firm is updated with variable frequency and there is little clarity on whether any update refers to an update of the number of employees that work in a given firm or some other characteristic of that firm.

[29]We attempted to incorporate other data to measure other types of fraud, but were not successful in these attempts. For example, businesses were meant to have been in existence prior to February 15, 2020, and we attempted to find firms who registered after this date yet received loans. However, the OpenCorporates business registry database we used does not contain data on a number of large states. In addition, we tried to match loan recipients to firms listed on the Federal "Do Not Pay" list, a list of firms and individuals previously found to be fraudulent by the federal government, but could only find about 100 matches.

We estimate the relationship between treatment group status and our outcomes in the months prior to and after the imposition of screening. Specifically, we estimate the following equation,

$$Y_{imt} = \tau_i + \gamma_{mt} + \sum_{g \neq Aug20} \rho_g T_i^0 \times [\mathbb{1}(g = mt)] + e_{imt} \tag{1}$$

where Y_{imt} is one of our three loan irregularity overpayment measures (*overpayment dummy*, *overpayment rate*, and *multiple loans dummy*), which corresponds to a particular firm (i) that receives a loan in a given month (m) during a given year (t). T_i^0 is an indicator variable equal to 1 for treatment firms, i.e. those that had loan amounts greater than \$150k in phase 1 of the program, 0 otherwise. τ_i is a firm fixed effect and γ_{mt} is a month-year of approval fixed effect. Standard errors (e_{imt}) are clustered at the firm level. The coefficient ρ_g estimates the effect of belonging to the treatment group for each month-year from April 2020 until May 2021. The reference category is the last month in phase 1 of the PPP program (August 2020). If ρ is statistically insignificant for all months of phase 1, then this lends support to the validity of the parallel trends assumption.

Figure 3 through Figure 5 plot ρ and 95% confidence intervals for each month-year of the PPP program. Figure 3 shows the effect on the overpayment rate, Figure 4 presents the result for the overpayment dummy, and Figure 5 plots the findings for multiple loans. In all the figures one can see that before the advanced documentation requirement was introduced in phase 2 (January 2021) for loans >150k, there are no statistically significant differences in outcomes across treatment (T_i^0) and control groups, lending support to the parallel trends assumption. At the end of this section we discuss other checks on the validity of our identifying assumptions.

4.2 Estimation

For firms (i) with a PPP loan approved at date (t) we estimate the following equation:

$$Y_{it} = \alpha_i + \omega Phase2_t + \gamma T_i^0 \times Phase2_t + e_{it} \tag{2}$$

where Y_{it} stands for irregularities in PPP loans defined in the ways described earlier. In addition to our treatment indicator (T_i^0), we include the indicator variable Phase2_t, equal to 1 for loans in phase 2 of the program, 0 otherwise. The parameter of interest is γ, which captures the impact of the screening requirement for firms whose previous loan amount indicates that they were subject to it. We consider specifications of our model with fixed effects (α_i), which we specify at the firm level. Standard errors (e_{it}) are clustered at the firm level.

4.3 Results

Figure 6 through Figure 8 graphically present the raw mean differences in differences in the data. There are several takeaways from these figures. First, for the overpayment measures one finds that both the volume and frequency of overpayment were concentrated among the treatment firms. This was true across both phases. Second, average outcomes for these measures declined across phases for both treatment and control firms; however, the fall was much greater for the former than the latter. Finally, the incidence of multiple loans was slightly higher among the control firms than treatment firms in phase 1. Yet the incidence of multiple loans declined sharply in phase 2 for the treatment firms while it increased for the control firms.

Table 1 moves beyond raw mean differences in the data and presents the results of estimating equation 2. Columns (1)-(2) present results for the overpayment dummy, Columns (3)-(4) for the overpayment rate, while Columns (5)-(6) present results for multiple loans as a dummy variable. Columns (1), (3) and (5) present results without firm fixed effects while Columns (2), (4) and (6) include them.

Results in Column (2) show that firms that were subject to the upfront documentation requirement, i.e. firms that had loans greater than a \$150k in phase 1 and reapplied in phase 2, had a 5% reduction in the probability of overpayment relative to firms that were not subjected to the screening requirement. The results are statistically and economically significant. Given the relative infrequency of overpayments, this is a large effect, equal to 63% of the value of the control group mean.

Findings for the rate of overpayment were even more pronounced. As shown in column (4), the upfront documentation requirement led to a statistically significant reduction in the rate of overpayment of 1.7% (more than 5 times the control mean) for treated firms in phase 2 of the program.

The results for multiple loans point in a similar direction. Column (6) shows that the upfront documentation requirement reduced the probability of receiving multiple loans by 0.2% (an effect equal to the value of the control mean). This effect is both statistically and economically significant.

Robustness using the "honest approach." Following recent developments in differences-in-differences estimation (see Roth et al., 2022 for a review), we use the "honest approach" to parallel trends suggested by Rambachan and Roth, 2019. We use this to investigate the robustness of our results to alternative assumptions about different outcome trends for treated (firms that had loan amounts greater than \$150k in phase 1 of the program) and control firms (firms that had loan amounts $\leq$\$150k in phase 1 of the program).

Rambachan and Roth, 2019 suggest that restrictions on the possible violations of parallel trends must be specified by the researcher, and the choice should depend on the economic context. In our case, one might be concerned about violations of parallel trends due to secular trends that evolve smoothly over time. We, therefore, bound the change in slope of the differential trend between treated and control firms using the following formula:

$$\Delta^{SD} := \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t\}$$

where δ_t refers to the difference in trends between the treated and control firms at time t. M is the maximum possible error of the linear extrapolation of the pre-trend. If M=0 the difference in trends between treated and control firms would be exactly linear, while $M > 0$ relaxes the assumption of exact linearity.

The estimates for probability of overpayment and overpayment rates are summarized in Figure 9 and Figure 10, respectively, while those for multiple loans are in Figure 11. The data used for these plots are at the loan - day level, while the treatment is defined at a firm level. These figures show that for the overpayment dummy and the overpayment rate the results remain robust for at least an M=0.0025 and M=0.005, respectively. In the case of multiple loans dummy this value is M=0.0025.

As discussed by Rambachan and Roth, 2019, nothing in the data itself can place an upper bound on the parameter M. For us to interpret these values of M we use data from phase 1 of the program. For each of the outcomes, we use only this data to create a linear trend that is extrapolated to phase 2 of the PPP program. We calculate the median of the absolute deviations of the coefficient ρ (see Equation 1) from this linear trend in phase 1 of the PPP program.[30] For the overpayment dummy this method leads to a value of M=0.0015, while for the overpayment rate and the multiple loans dummy the values are M=0.0026 and M=0.001, respectively. These are lower than the M at which our results are robust according to Figure 9 and Figure 10.

Other checks. In this section we present further checks that aim to support a causal interpretation of our estimates. Appendix Table A3 presents the results defining the treatment firms (T^0) as those with a loan amount between \$151K-\$200K and the control firms as those with loan amounts between \$100K - \$150K *in phase 1*.[31] Firms that are on either side of the \$150k threshold in a sample of those firms whose approved loan amounts were between \$100-200k in phase 1 are likely to be similar to each other in their fundamentals and the baseline probability of fraudulent behaviour. The results are in the same direction as those in Table 1, albeit of smaller magnitude as the sample is restricted to firms whose approved loan amounts were between \$100-200k in phase 1.

Next we check whether there is attrition of firms in Phase 2 of the program and investigate the direction of the bias. Since we can observe the universe of firms that received PPP loans in phase 1, i.e. before the announcement of the advanced documentation requirement in January 2021, if any firm does not reapply in phase 2 of the PPP program due to the advanced documentation requirement we can observe this in the data. Results show that those with loan amounts greater than \$150k and with multiple or overpaid loans in phase 1 were *more* likely to reapply in phase 2 of the program (see Table 2a and 2b Column (1) below). This suggests that we have a conservative estimate of the true effect of screening.

In this context one might also be worried about anticipation effects of an upfront documentation requirement based on loan size. However, the documentation requirement based on the \$150k threshold was not announced or even discussed by policy makers in phase 1 and phase 1 is when treatment firms are defined (see timeline in Figure 1). The announcement of the documentation requirement was on January 6, 2022, right before the start of phase 2 of the program.[32]

[30]For a graphical representation see Figure 3 to Figure 5. These figures plot the coefficient ρ from Equation 1 for each month in phase 1 and 2 of the PPP program. The black line is a linear trend using only phase 1 data.

[31]If a firm had been approved for multiple loans, T^0 would take on a value of one if the firm had at least one loan amount greater than \$150K in Phase 1.

[32]This announcement was via the release of a rule change and advanced documentation was effective for second time

5 A Model of Fraud with a Value-Based Documentation Requirement

The findings presented thus far establish that there were benefits from the PPP's documentation requirement in terms of fraud reduction. Yet such requirements typically impose administrative burdens on all program participants in addition to dissuading fraud. Thus, it is important to verify that the documentation requirement adopted in the PPP was effective in reducing losses due to the participation of fraudulent firms without imposing undue burdens on firms with a legitimate right to participate in the program. We develop here a theoretical framework that provides precise empirical implications about when this will be the case, which we subsequently evaluate using our loan data.

Consider a relief program that distributes a highly valued good (e.g., money) among firms in the economy. A given firm i participates in the program by submitting an application for the good in the amount $g_i \in [0, \overline{g}]$, where $\overline{g}$ is the maximum level of support for any firm permitted by the program. The economy contains two types of firms, legitimate firms and fraudulent firms, with the former equal to a proportion $\zeta \in (0, 1)$ of all firms. One can conceptualize fraudulent firms as shell corporations that engage in no legitimate economic activity, or, alternatively, as firms that engage in economic activity but are ineligible to participate in the program based on the criteria stipulated by the program. Crucially, due to the need to rapidly provide relief in order to mitigate the emergency which gave rise to the program, firm type cannot be discerned by program administrators prior to allocating the good. Yet the status of firms may become apparent during an ex-post review of the program after the emergency has abated, with penalties potentially assigned to fraudulent firms that received the good. At the time of application, each firm is privy to its eligibility status $s_i \in \{0, 1\}$, where $s_i = 1$ indicates that firm i is legally eligible to participate (legitimate firm) and $s_i = 0$ indicates that it is not eligible (fraudulent firm).

Utility from participation in the program varies by firm type. For legitimate firms, participation in the program entails no risk of punishment, so demand for the good is mediated only by idiosyncratic tastes for asking the government for support and the fixed cost of submitting an application. For fraudulent firms, who are officially barred from the program, the prospect of punishment at some point after the program has concluded is a distinct possibility, so this fact will shape demand for the good.

In line with the institutional structure of the PPP, we consider a documentation requirement that is based on the amount of the good being solicited from the program. Specifically, we consider a documentation requirement imposed on all loan applications greater than the amount $\widetilde{g}$. This requirement has two consequences. First, all firms seeking values of the good above this amount will incur a cost $\phi > 0$, which represents the administrative burden associated with satisfying the documentation requirement. Second, the likelihood that a fraudulent firm will be identified as such upon post-program review increases discretely with the documentation requirement, implying that for fraudulent firms the cost of fraud shifts upward at this point. This discrete increase in the

PPP loans made after January 14, 2021. `https://www.sba.gov/sites/default/files/2021-01` cost of fraud at $\widetilde{g}$ can be conceptualized as reflecting firms' beliefs about the likelihood that fraud will be discernable based on the information contained in the document itself, or, alternatively, firms' beliefs about the resolve of program administrators to more stringently audit firms that receive amounts of the good in excess of $\widetilde{g}$.

These considerations lend themselves to the following characterization of firm utility:

$$u_i = \begin{cases} v(g_i) - \eta_i g_i - \phi \mathbb{I}(g_i > \widetilde{g}) & \text{if } s_i = 1 \\ v(g_i) - c(g_i) - \eta_i g_i - \phi \mathbb{I}(g_i > \widetilde{g}) & \text{if } s_i = 0 \end{cases} \tag{3}$$

where $v(0) = 0, v' > 0, v'' < 0, v'(0) = +\infty$, and η_i captures idiosyncratic tastes for procuring government support. We assume η_i is distributed according to a continuous density F_1 with support $[\underline{\eta}, \overline{\eta}]$ for legitimate firms, and according to a continuous density F_0 with identical support for fraudulent firms. $\mathbb{I}(x)$ is an indicator function equal to 1 if the expression x is true (0 otherwise).

For a fraudulent firm, $c(g_i)$ represents the cost of soliciting the good in amount g_i given that the firm is not entitled to participate in the program. We characterize the cost function as follows:

$$c(g_i) = \pi(g_i)[1 + \tau \mathbb{I}(g_i > \widetilde{g})], \tag{4}$$

where π satisfies $\pi(0) = 0$, $\pi' > 0$, $\pi'' > 0$. The parameter $\tau > 0$ captures the discrete jump in the cost of fraud at $\widetilde{g}$. The cost function reflects a setting in which in which the punishment for fraud is a smooth and convex increasing function of the level fraud but the likelihood of detection jumps discretely upwards for all levels of fraud greater than the amount $\widetilde{g}$.

To fix ideas, we start with the scenario in which there is no discrete change in documentation requirements at $\widetilde{g}$, i.e. $\tau = \phi = 0$. The optimally selected value of the good for a legitimate firm is equal to:

$$g_i^{1*}(\eta_i) = \begin{cases} \widehat{g}_i \equiv g_i | v'(g_i) = \eta_i & \text{if } \widehat{g}_i \leq \overline{g} \\ \overline{g} & \text{if } \widehat{g}_i > \overline{g} \end{cases}, \tag{5}$$

which implies that g_i^{1*} is (weakly) decreasing in η_i.

In contrast, the optimal request of a fraudulent firm is equal to:

$$g_i^{0*}(\eta_i, \tau = 0) = \begin{cases} g_i^{\dagger} \equiv g_i | v'(g_i) - \pi'(g_i) = \eta_i & \text{if } g_i^{\dagger} \leq \overline{g} \\ \overline{g} & \text{if } g_i^{\dagger} > \overline{g} \end{cases}, \tag{6}$$

where again $g_i^{0*}$is decreasing in η_i.

Now define the threshold points η_U^1, η_U^0 as follows:

$$\begin{aligned} \eta_U^1 &\equiv \eta_i | g_i^{1*} = \widetilde{g} \\ \eta_U^0 &\equiv \eta_i | g_i^{0*} = \widetilde{g}. \end{aligned} \tag{7}$$

All legitimate firms with a taste parameter above η_U^1 optimally request amounts of the good below

$\widetilde{g}$, so they are not affected by the documentation requirement. Similarly, all fraudulent firms with a taste parameter above η_U^0 optimally request amounts of the good below $\widetilde{g}$.

For all firms for which the taste parameter lies below the relevant threshold, on the other hand, the documentation requirement may affect their requests. Any such firm can set $g_i = \widetilde{g}$, thereby ensuring a utility disbursement equal to:

$$u_i(\widetilde{g}) = \begin{cases} v(\widetilde{g}) - \eta_i \widetilde{g} & \text{if } s_i = 1 \\ v(\widetilde{g}) - \pi(\widetilde{g}) - \eta_i \widetilde{g} & \text{if } s_i = 0 \end{cases} \tag{8}$$

For a legitimate firm with $\eta_i \leq \eta_U^1$ that sets $g_i \neq \widetilde{g}$, the best such request will be g_i^{1*}, defined by equation (5). Similarly, for a fraudulent firm with $\eta_i \leq \eta_U^0$ that sets $g_i \neq \widetilde{g}$, the best such request will be:

$$g_i^{0*}(\eta_i, \tau) = \begin{cases} g_i^{\ddagger} \equiv g_i | v'(g_i) - (1+\tau)\pi'(g_i) = \eta_i & \text{if } g_i^{\ddagger} \leq \overline{g} \\ \overline{g} & \text{if } g_i^{\ddagger} > \overline{g} \end{cases} . \tag{9}$$

Naturally, the question arises as to whether it would be optimal for firms to avoid documentation by requesting $\widetilde{g}$ or to solicit more from the program in spite of the added administrative burden and/or greater risk of sanction. Define the threshold points η_L^1, η_L^0 as follows:

$$\begin{aligned} \eta_L^1 &\equiv \eta_i | u_i(g_i^{1*}(\eta_i)) = u_i(\widetilde{g}) \\ \eta_L^0 &\equiv \eta_i | u_i(g_i^{0*}(\eta_i, \tau)) = u_i(\widetilde{g}) \end{aligned} \tag{10}$$

where the utilities on the LHS of the equalities above incorporate the fixed cost of the documentation requirement (ϕ) and, for fraudulent firms only, also incorporate the parameterized jump in the cost function (τ). Firms with a taste parameter equal to the relevant threshold above will be indifferent between setting $g_i = \widetilde{g}$ and optimally choosing a higher value of g_i that is subject to the documentation requirement. Firms with a taste parameter below the relevant threshold will choose a value of g_i above $\widetilde{g}$. Specifically, all legitimate firms with a taste parameter equal to or above η_L^1 but equal to or below η_U^1, i.e. all i such that $s_i = 1$ and $\eta_i \in [\eta_L^1, \eta_U^1]$, will set $g_i = \widetilde{g}$. Moreover, all fraudulent firms with a taste parameter equal to or above η_L^0 but equal to or below η_U^0, i.e. all i such that $s_i = 0$ and $\eta_i \in [\eta_L^0, \eta_U^0]$, will also set $g_i = \widetilde{g}$. Thus, the existence of the documentation requirement creates a spike in the mass of requests at $g_i = \widetilde{g}$ equal to:

$$\Delta = \zeta[F_1(\eta_U^1) - F_1(\eta_L^1)] + (1-\zeta)[F_0(\eta_U^0) - F_0(\eta_L^0)], \tag{11}$$

resulting in a concomitant reduction in the mass of requests above $\widetilde{g}$. Following the nomenclature employed by the public finance and labor literatures, we refer to firms that contribute to this spike as 'bunchers' and Δ as the bunching mass.[33]

[33] There is a large public economics (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013) and labor (Burtless and Hausman, 1978; Aaron et al., 1981; Chetty et al., 2011) literature that uses discrete changes in the level and slope of choice sets as a way to estimate elasticity of behavioral responses like income, wealth and labor supply. See Kleven, 2016 for an excellent survey of this work.

Figure 12 depicts the impact of the documentation requirement on the density of all requests for the good from the program.[34] The initial density of requests–without the documentation requirement–is shown by the solid line. Note that the density is smooth throughout its range. The density of requests with documentation required for all requests above $\widetilde{g}$ is shown by the dashed line. This density is characterized by a large upward spike at $\widetilde{g}$, reflecting the choice of a subset of legitimate firms to avoid the administrative costs of the documentation requirement and the choice of a subset of fraudulent firms to simultaneously evade the risk of detection created by documentation requirement and to avoid its administrative costs. Relative to the no documentation density, the density of requests with the documentation requirement has a large excess mass at $\widetilde{g}$ and too little mass for an interval to the right of this point. Otherwise, at points sufficiently far to the left and right of $\widetilde{g}$, the two densities overlap perfectly.

It is instructive to consider how the parameters representing the administrative burden of the documentation requirement (ϕ) and its value as a tool for sanctioning fraud (τ) affect the size and composition of the bunching mass. The proposition below summarizes these relationships.

Proposition 1. *a) An increase in the administrative burden of the documentation requirement (ϕ) results in a larger bunching mass due to increased strategic avoidance of the requirement by both legitimate and fraudulent firms; b) An increase in the fraud sanctioning value of the documentation requirement (τ) results in a larger bunching mass due strictly to an increase in strategic avoidance of the documentation requirement by fraudulent firms seeking to reduce their risk of sanction. Thus, the bunching mass will be disproportionately populated by illegitimate firms when τ is high and ϕ is low.*

Proof. Note that Δ is decreasing in η_L^1 and η_L^0, each of which may depend on τ and ϕ. Thus, changes in parameters that lower both of these points or that lower one but leave the other unaffected will unambiguously increase Δ. Using the equalities in (10), implicit differentiation and application of the envelope theorem reveals that an increase in ϕ lowers both η_L^1 and η_L^0. This implies that greater ϕ leads to a larger bunching mass, and that it does so because greater numbers of both legitimate and illegitimate firms engage in strategic avoidance. Repeating the same procedure for τ reveals that η_L^0 is decreasing in τ but η_L^1 is unaffected by changes in this parameter. This implies that greater τ leads to a larger bunching mass, but that it does so solely due to the fact that greater numbers of illegitimate firms engage in strategic avoidance. □

The proposition has immediate implications for program design. An effective documentation requirement is one for which compliance costs are low, but which significantly raises the real or perceived risk of engaging in fraud. If a documentation requirement is effective in this sense, then the composition of the bunchers will be quite different from that of the overall population of firms: bunchers will consist of a disproportionate number of fraudulent firms. If a documentation requirement is ineffective, on the other hand, then the composition of the bunchers will mirror that

[34]This graph was created by parameterizing the model as follows: $v(x) = \ln(x)$, $\pi(x) = x^2/2$, $\rho = 1/2$, $\tau = 1$, $\phi = 1/1000$, and F_1, F_0 are truncated normal densities with mean 5 and support [1,9].

of the overall population. For this reason, empirical analyses which can detect bunching and elucidate the composition of bunchers may be highly revealing about the efficacy of a documentation requirement. We investigate these in more detail in the next section.

6 Mechanisms: Extensive or intensive margin effects?

In this section, we examine mechanisms that might explain why the advanced documentation requirement led to a systematic fall in overpayment and multiple loans, particularly on loans greater than \$150k in phase 2. The introduction of screening may have affected firm behavior along the extensive margin, by which we refer to the decision of firms to exit the program. Specifically, those firms whose phase 1 loan indicated that they may have participated in fraud could have reacted to the prospect of screening in phase 2 by abandoning the program entirely. Alternatively, as elucidated by our formal model, the introduction of screening may have affected firm behavior along the intensive margin, by which we refer to the decision of firms to manipulate the details of their loan applications so as to avoid crossing the \$150k threshold. Firms with past indicators of fraud may have continued using the PPP but simply asked for less out of the program in order to avoid the documentation requirement. To the extent that fraudulent firms reduced their requests in this manner at a significantly greater rate than legitimate firms, this constitutes evidence that the fraud reducing effect of the documentation requirement dominated the administrative burden it may have generated. Below we provide evidence on both types of mechanisms. We show that the behavioral reaction of firms to the documentation requirement was overwhelmingly located on the intensive margin, and that the nature of the behavioral response indicates that the fraud reduction properties of the requirement dominated its compliance costs.

6.1 Estimation

For this section we use the cross-section of firms (i) in phase 2 of the PPP program and estimate the following equation:

$$Y_i = \kappa + \theta F_i^0 + \mu T_i^0 + \pi T_i^0 \times F_i^0 + \varepsilon_i \tag{12}$$

where Y_i includes the following outcomes: an indicator variable equal to 1 if firm i exited in phase 2 of the PPP program (i.e., did not reapply for PPP loans); an indicator variable equal to 1 if the firm applied to a loan in phase 2 that is greater than \$150k; the dollar loan amount itself; the difference in the number of jobs reported from phase 1; and the difference in the loan amount per job. While the first outcome describes changes in firm behavior along the extensive margin, the last four outcomes present evidence on the intensive margin. To assess whether screening resulted in behavioral changes for firms that may have participated in fraud, we include the indicator variable F^0. The variable F^0 is equal to 1 for firms that were paid above the maximum permissible amount under the PPP rules or firms that received multiple loans in phase 1. We interact F^0 with our treatment (T_i^0) in order to specifically assess how potentially fraudulent firms that were most likely to be affected by the documentation requirement responded to it. Standard errors (ε_i) are clustered at the firm level.

6.2 Results

Table 2a and Table 2b present the findings. Table 2a shows the results classifying suspect firms as those that were paid a PPP loan amount greater than the permissible amount in phase 1 of the program. Table 2b presents the results classifying such firms as those that received multiple PPP loans in phase 1 of the program.

Screening does not appear to have prompted bad actors to leave the program (Column (1)). To the contrary, firms that were subjected to upfront documentation and that had previously obtained loans in excess of the maximum or received multiple loans were more likely than other firms to obtain loans in phase 2 of the PPP program (9.6 and 5.9 percentage points of the control mean, respectively).

However, screening did appear to have important implications for behavior along the intensive margin, i.e. for the content of the loans firms received in phase 2. There are two key takeaways. First, these results show that, unlike for bad actors, there was continuity in loan requests for those firms that did not have any irregularities in phase 1. Firms with loan amounts greater than $150k in phase 1 but no overpayments or multiple loans, continued to ask for loans greater than $150k (approximately 90%). The loan amounts they received in phase 2 were also larger in phase 2 than control firms (Columns (2) - (3)). Second, Column (2) in both Panels (a) and (b), show that firms that were subjected to screening and that had previously obtained loans in excess of the maximum or received multiple loans were 11% and 13.7% less likely to receive a loan amount greater than $150k, respectively. The effects are similar for loan amounts as an outcome (Column (3)).

Table 2a Columns (4) and (5) show that the suspect firms did not request smaller loan amounts because they reported that they had fewer employees to support. In fact, they reported significantly greater growth in the number of employees relative to phase 1 than did other firms (Column (4). What distinguished these firms is that they reduced the loan amounts they requested per employee much more than did other firms. Table 2a Column (5) shows that the differences in this respect are substantial. Relative to firms that received a loan amount in excess of $150K in phase 1 but were not overpaid, overpaid firms in the same category received approximately $18k less per employee in phase 2 than they did in phase 1. Table 2b Columns (4) and (5) present a similar trend. Thus, given the specter of screening for high loan requests, firms with past loan irregularities were inclined to cut their requests down to levels carrying less risk.

6.3 Strategic Evasion of Screening

The fact that the upfront documentation requirement in phase 2 was made conditional on the value of the loan request introduces the possibility of strategic evasion of screening. Borrowers who sought to maximally exploit the program but wanted to avoid providing evidence of a fall in their gross receipts could do so by following a simple strategy: Set phase 2 loan requests just below the \$150k cut-off. As in our formal model, we refer to borrowers who pursue such as strategy as 'bunchers'. If strategic evasion was truly systemic, then the data should reveal the existence of a large mass of such bunchers. Moreover, if the fraud reduction properties of screening dominated its administrative burden, then the firms belonging to any such bunching mass should be significantly more likely to have indicators of past wrongdoing than non-bunching firms.

Due to the fact the we observe loan allocations across the two phases of the program, our data are exceptionally well suited for detecting the existence of bunchers. Since in phase 1 there was no documentation requirement activated for loans greater than \$150K, the distribution of loan values from that phase serves as a credible counterfactual distribution, i.e. a distribution capturing what the loan values in phase 2 would have been had screening not been implemented.[35] If bunchers engaging in strategic evasion do exist, then we should observe specific patterns in the distributions of loan values in the two phases. First, in phase 2 one should observed a sharp upward spike in the density of loans immediately below the \$150K cutoff, and concomitant reduction in the density above the cutoff. Second, in phase 1 we should not observe any large spikes in the density around the \$150K cutoff, as the screening requirement was not operative at this time.

Figure 13 presents the density of loan amounts for phase 1 and phase 2 loans. Figure 14 presents the same information, but breaks down the phase 2 loans into first and second time borrowers.[36] The figures provide evidence of systemic strategic evasion. In both figures, one observes that relative to the counterfactual (phase 1) distribution, there is a marked excess mass just to the left of the \$150K cutoff and a missing mass of borrowers above the cutoff. The spike immediately to the left of the \$150K cutoff is exceptionally stark and dwarfs the magnitude of other bumps in the distribution attributable to reference effects.[37]

We complement our visual inspection of loan amounts with a formal test of the continuity of the distribution of loan amounts around the \$150k cut-off in the two phases of the PPP program. Figure 15 presents the density as well as the p-value from a McCrary density test (McCrary, 2008). The McCrary density test tests the null hypothesis of the continuity of the density of loans against the alternative of a jump in the density function at the \$150k cut-off.[38] There are two key takeaways. First, we fail to reject the null of a continuous loan distribution in phase 1 around the \$150K cut-off (p-value=0.266). This lends support to our use of the phase 1 loan distribution as a counterfactual for the behavior of the firms in phase 2. Second, we can reject the null of a continuous loan

[35]A common challenge in identifying bunchers in the public economics and labor literature is the absence of a counterfactual distribution reflecting aggregate behavior in the absence of the reform (Blomquist et al., 2021; Jakobsen et al., 2020; Londoño-Vélez and Avila-Mahecha, 2020)

[36]Naturally, incentives for firms that were applying for the first time in phase 2 were similar to those for repeat borrowers. Any new program participant who wished to garner maximal benefit from the PPP but avoid upfront documentation of a fall in gross receipts could do so by locating just below the \$150K cutoff.

[37]By reference effects, we refer to the concentration of loans in amounts that are easy to remember, typically numbers that are factors of ten or five.

[38]The McCrary density test (McCrary, 2008) is the standard test of discontinuities in the conditional density of the forcing variable in regression discontinuity designs (Imbens and Lemieux, 2008).

distribution in phase 2 around the \$150K cut-off (p-value=0). This finding is consistent with firms changing their behavior following the introduction of the advanced documentation requirement and 'bunching' below the cut-off of \$150K.

Identifying "bunchers". While a visual inspection of Figure 13 and Figure 14 can be utilized to identify the interval of loan values whose frequency was inflated by bunching due to strategic evasion, we formally test and locate the bunching interval using a Kolmogorov–Smirnov (KS) test. The KS-test tests the equality of the loan distributions in phase 1 and phase 2 loans. Figure 16 plots p-values from this test on the y-axis with loan amounts on the x-axis. The figure suggests that the two distributions across phase 1 and phase 2 for second time borrowers are statistically significantly different in the \$136-150K range. We therefore, define 'bunchers' as those firms that had PPP loans of greater than \$150K in phase 1, but then chose to get PPP loans between \$136-150K in phase 2.

The behavior of bunchers before-and-after screening. Having established the existence of systemic strategic evasion and having identified the set of firms that appear to have practiced it, two questions arise. First, do the bunching firms differ systematically from non-bunchers in terms of their behavior in the first phase of the program? Specifically, are they more likely to have received irregular loans or to have reported employment figures that appear to be inflated (in order to qualify for greater loans) in Phase 1 of the program? If so, then this would be consistent with the notion that bunching firms were disproportionately composed of bad actors, some of whom may have engaged in strategic evasion to avoid detection for both their past (phase 1) and present (phase 2) misdeeds. A finding of a disproportionate level of such fraudulent firms among the bunchers would indicate that the documentation requirement worked well, in the sense that it dissuaded participation in the PPP by bad actors to a greater degree than it dissuaded participation by legitimate firms. A second question that arises is to what extent, if any, did screening lead to a fall in irregularities among bunching firms? Findings linking screening to a reduction in fraud among bunchers might indicate that the introduction of screening may have induced extra caution on the part of bad actors, even though they had set their loan requests at lower level to avoid the documentation requirement.

To address these questions, we use our data at the firm (i) and phase (t) level and estimate the following:

$$Y_{it} = \pi + \theta Bunchers_i + \tau Phase2_t + \beta Bunchers_i \times Phase2_t + v_{it} \tag{13}$$

where Y_{it} includes the following outcomes: *overpayment dummy*; *overpayment rate*; the difference in the number of jobs reported on the PPP loan application as compared to the employees data compiled by DNB (*job gap*); a set of indicator variables denoting whether the percent gap in jobs reported in the PPP loan application as compared to the DNB employees data was greater than 300, 400 or 500%, respectively; and *multiple loans*. $Bunchers_i$ is an indicator variable equal to 1 for firms that had PPP loans of greater than \$150K in phase 1, but who then chose to get PPP loans between \$136-150k in phase 2, and equal to 0 for non-bunchers. Non-bunchers are any firms that fell outside the bunching interval of \$136-150K in phase 2 of the program, irrespective of their loan amounts in phase 1. Standard errors (v_{it}) are clustered at the firm level.

The coefficient θ captures the outcomes for bunching firms in phase 1. If bunching firms were disproportionately comprised of bad actors, we should expect $\theta > 0$ for the measures of fraud in our study. The coefficient β allows us to quantify the extent to which the new documentation requirement was correlated with a reduction in such outcomes among bunching firms in phase 2.

Table 3 presents the results. For nearly all outcomes, we find that bunchers were more likely than non-bunchers to have been involved with loan irregularities in phase 1. Table 3 Column (1) and (2) show that relative to non-bunchers, they were significantly more likely to have received an overpayment on their loans (1.7% or nearly twice the average for the control group) and the rate of overpayment on their loans was higher (1.6% or 2.7 times the control group's average). Columns (3) and (4)-(6) show that the gap between the number of jobs bunchers reported in their PPP applications and that recorded by DNB was higher (column (3)), and the likelihood that said gap was extreme–exceeding a percentile difference of 300%, 400%, or 500%–was also higher (columns (4)-(6)). In all these cases, the differences between bunchers and non-bunchers were statistically significant by any conventional standard. The one outcome for which there was no statistically significant difference between bunchers and non-bunchers was the incidence of multiple loans (although the estimated impact is non-trivial in substantive terms). This one partial exception notwithstanding, it appears clear that bunchers were disproportionately comprised of firms with a highly checkered history in the program.

The table also shows that the screening requirement was associated with a significant fall in irregular activities by bunchers in phase 2. With the introduction of screening in phase 2, relative to phase 1, bunchers became significantly less likely to receive loans with an overpayment (1.3% or 1.3 times the control group mean), the rate of overpayment on their loans declined (1.5% or 2.5 times the control group mean), the gap between the number of jobs they reported in their applications and the DNB figures declined (1.9 jobs or half the control group mean), and the likelihood of extreme gaps between these sources declined as well (approximately 1% fall or a fall of 10-23% of the control group mean in the category of percent job gap of >300%, >400% and >500%, respectively). The probability of multiple loans by bunchers also fell by 0.2% (100 pp of the control group mean) in phase 2 relative to phase 1.

These results suggest that the advance documentation requirement was able to screen out the bad actors from loan brackets greater than >\$150k and in the process significantly reduced the irregularities committed by suspect firms. The effects are economically large and statistically significant. In fact, the declines of bunchers were significantly larger in magnitude than the declines experienced by non-bunchers. Thus, even though bunchers had taken pains to avoid screening by setting their loan requests to values below the \$150K cutoff, it appears the existence of the documentation requirement seems to have instilled greater caution, thereby reducing the incidence of fraud.

Conclusion

The findings of our paper establish the utility of screening as a means of reducing fraud in large-scale public programs. Examining changes over time in the behavior of borrowers in the Paycheck Protection Program (PPP), we find that screening was effective in reducing irregularities in loan disbursements that are indicative of fraud. However, due to the peculiarities of the implementation of screening in the PPP, it was possible for borrowers to strategically evade the documentation requirements it imposed by setting their loan requests below a threshold value of $150K. A non-negligible mass of borrowers did precisely that. Even so, the advent of screening nonetheless marked a reduction in the aggregate level of fraud, including among those borrowers who had engaged in strategic evasion. In our judgement, this speaks to the merit of screening in programs like the PPP that have extensive scope and tight timelines.

The PPP is far from unique in this regard. There are varied circumstances when it is simply impossible to have a robust oversight apparatus up-and-running prior to engaging with the potential beneficiaries from a public program. Emergency relief programs by their very nature face this challenge, since the welfare improvements they convey are highly contingent upon the timeliness with which funds are allocated to recipients. And yet such programs cannot possibly be effective unless they are able to channel resources to legitimate program participants and prevent debilitating levels of fraud. In precisely these types of contexts, this paper shows that a little bit of screening can go a long way.

A key trade-off for policymakers is the timeliness of support versus the extent to which said support is used for its intended purpose. Screening requirements that can be satisfied by program participants promptly and at low cost, but which are also effective in discriminating between actors who should or should not have access to the program hit the veritable sweet spot. Actors who are legitimate program participants should be able to put together the relevant documentation quickly—meaning that funds can go out the door quickly to those who need them. Conversely, fake documentation should be difficult to create and/or easy to detect, thereby deterring criminally-minded actors from attempting to defraud the program.

Of course, it is important to recognize that there is no free lunch in implementing screening. The introduction of documentation requirements and other verification procedures can easily dissuade legitimate beneficiaries of a public program from taking the steps necessary to receive the benefits intended for them by policymakers. The returns to fraud reduction offered by screening should always be contemplated with this point in mind. Yet our paper demonstrates that there are contexts in which the trade-off between fraud reduction and uptake by legitimate beneficiaries can be empirically assessed, thereby informing future changes in program design. In particular, the approach developed in this paper should be applicable to any program in which screening requirements are a function of the value of the good solicited by program participants. In these settings, the identification of a bunching mass and an analysis of the characteristics of bunchers can provide useful information about the fraud-inhibiting effect of screening relative to its compliance costs.

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Tables

Table 1: Did screening affect fraud in PPP loans?

Dependent Variable:	**Overpayment dummy**		**Overpayment rate**		**Multiple loans dummy**	
	(1)	(2)	(3)	(4)	(5)	(6)
T^0	0.013***		0.019***		-0.000	
	(0.000)		(0.001)		(0.000)	
Phase 2	-0.003***	-0.003***	-0.002***	-0.002***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$T^0 \times$ Phase 2	-0.005***	-0.005***	-0.017***	-0.017***	-0.002***	-0.002***
	(0.000)	(0.001)	(0.001)	(0.002)	(0.000)	(0.000)
Control mean of outcome	0.008	0.008	0.003	0.003	0.002	0.002
Observations	2988104	2988104	2988104	2988104	2988104	2988104
Firm FE	No	Yes	No	Yes	No	Yes

Note: The table shows how the advanced documentation requirement affects the main indicators of fraud. The unit of observation is at the firm-phase level. The sample is restricted to firms that received loans in both Phase 1 and Phase 2. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in a phase, and 0 otherwise. Overpayment rate on each loan is the overpaid amount divided by the maximum payment that a firm was eligible for. For firms with multiple overpaid loans, the regression was run with the maximum rate among those loans. Multiple loans dummy is a dummy variable that takes a value of 1 when a firm is identified to have been approved for more than one loan in any given phase, and 0 otherwise. T^0, the treatment group, consists of firms with at least one loan greater than $150,000. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table 2: Did screening affect fraud through the extensive or intensive margin?

(a) Suspect firms defined based on overpayment in phase 1

Dependent Variable:	**Exited from Phase 2**	**Whether loans >150k in Phase2**	**Average loan amount in Phase 2**	**Δ average no. jobs across phases**	**Δ average loan amount/job across phases**
	(1)	(2)	(3)	(4)	(5)
T^0	-0.032***	0.911***	140850.21***	-4.392***	10.06
	(0.001)	(0.001)	(2390.79)	(0.082)	(11.50)
Overpaid in phase 1	0.014***	-0.005***	-7398.27***	2.384***	-22372.91***
	(0.002)	(0.001)	(309.69)	(0.071)	(207.21)
T^0 × Overpaid in Phase 1	-0.037***	-0.111***	-59343.86***	20.618***	-18639.64***
	(0.005)	(0.006)	(6160.15)	(0.592)	(1579.51)
Control mean of outcome	0.713	0.017	40273.74	-0.443	844.78
Observations	5128185	1494052	1494052	1494052	1494052
Fixed effects	No	No	No	No	No
Phase 1 control	No	No	Yes	No	No

(b) Suspect firms defined based on multiple loans in phase 1

Dependent Variable:	**Exited from Phase 2**	**Whether loans >150k in Phase2**	**Average loan amount in Phase 2**	**Δ average no. jobs across phases**	**Δ average loan amount/job across phases**
	(1)	(2)	(3)	(4)	(5)
T^0	-0.033***	0.909***	140061.00***	-3.928***	-686.10***
	(0.001)	(0.001)	(2387.03)	(0.081)	(37.53)
Multiple loans in phase 1	-0.042***	-0.005**	2493.60	0.440*	945.95***
	(0.006)	(0.002)	(3115.85)	(0.258)	(135.49)
T^0 × Multiple loans in Phase 1	-0.009	-0.137***	-28115.77*	3.466*	-740.19
	(0.016)	(0.022)	(16678.58)	(1.871)	(493.77)
Control mean of outcome	0.713	0.016	40326.64	-0.425	669.78
Observations	5128185	1494052	1494052	1494052	1494052
Fixed effects	No	No	No	No	No
Phase 1 control	No	No	Yes	No	No

Note: The table shows how the advanced documentation requirement affected the behavior of firms with overpayment (Panel (a)) and with multiple loans (Panel (b)) in Phase 1. The unit of observation is at the firm-phase level. The sample is restricted to firms that received loans in both Phase 1 and Phase 2. Exited from Phase 2 is a dummy variable that takes a value of 1 when a firm showed up only in Phase 1, and 0 otherwise. Whether loans greater than $150K in Phase 2 is a dummy variable that turns on when at least one of a firm's loans in Phase 2 exceeds $150,000. The average loan amount is calculated from all the loans issued to a firm in Phase 2. The average number of jobs is computed using the numbers of employees reported by a firm on its applications. The average loan amount per job is calculated by dividing the total dollar amount of loan by the total number of employees reported in the same phase. For the last two outcomes, we further subtract Phase 2 values from Phase 1 values to get an across-phase difference for each firm. T^0, the treatment group, consists of firms with at least one loan greater than $150,000. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Were outcomes of bunching firms different from others?

Dependent Variable:	**Overpayment dummy**	**Overpayment rate**	**Job gap between PPP and DNB**	**Percent job gap PPP - DNB**			**Multiple loans dummy**
				>300%	>400%	>500%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bunchers	0.017***	0.016***	1.713***	0.066***	0.057***	0.055***	0.001
	(0.002)	(0.003)	(0.354)	(0.006)	(0.005)	(0.005)	(0.001)
Phase 2	-0.004***	-0.004***	-1.083***	-0.005***	-0.005***	-0.005***	0.000***
	(0.000)	(0.000)	(0.024)	(0.000)	(0.000)	(0.000)	(0.000)
Bunchers × Phase 2	-0.013***	-0.015***	-1.903***	-0.010***	-0.009***	-0.014***	-0.002***
	(0.002)	(0.003)	(0.310)	(0.003)	(0.003)	(0.003)	(0.001)
Control mean of outcome	0.010	0.006	3.674	0.096	0.073	0.059	0.002
Observations	2988104	2988104	970440	970440	970440	970440	2988104
Fixed effects	No	No	No	No	No	No	No

The table shows how the outcomes of bunching firms differ from non-bunching firms. The unit of observation is at the firm-phase level. The sample is restricted to firms that received loans in both Phase 1 and Phase 2. Bunchers are firms with at least one loan greater than $150K in Phase 1, all loans at $150K and below in Phase 2, and at least one loan in Phase 2 falling between $136K and $150K. Overpayment dummy is a dummy variable that takes a value of 1 when a loan is identified as overpayment, and 0 otherwise. Overpayment rate is the overpaid amount divided by the maximum payment that a firm was eligible for. Job gap between PPP and DNB is calculated by subtracting the number of employees shown on a firm's DNB record (as of July 2021) from the maximum number of employees that firm reported on its PPP applications in a phase. Note that DNB data shows the sum of all employee figures across all locations of a firm, while the number reported on PPP applications can be from one of the offices. Therefore, to increase the comparability of that the two job numbers, we exclude from the regression observations with large, negative (< -50) gaps between PPP and DNB figures and observations with DNB job numbers exceeding 500. For each firm, we further estimate the size of this job gap as a fraction of its DNB employee number, and report dummy outcomes indicating very large, positive differences. Multiple loans dummy is a dummy variable that takes a value of 1 when a firm is identified to have been approved for more than one loan in any given phase, and 0 otherwise. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Figures

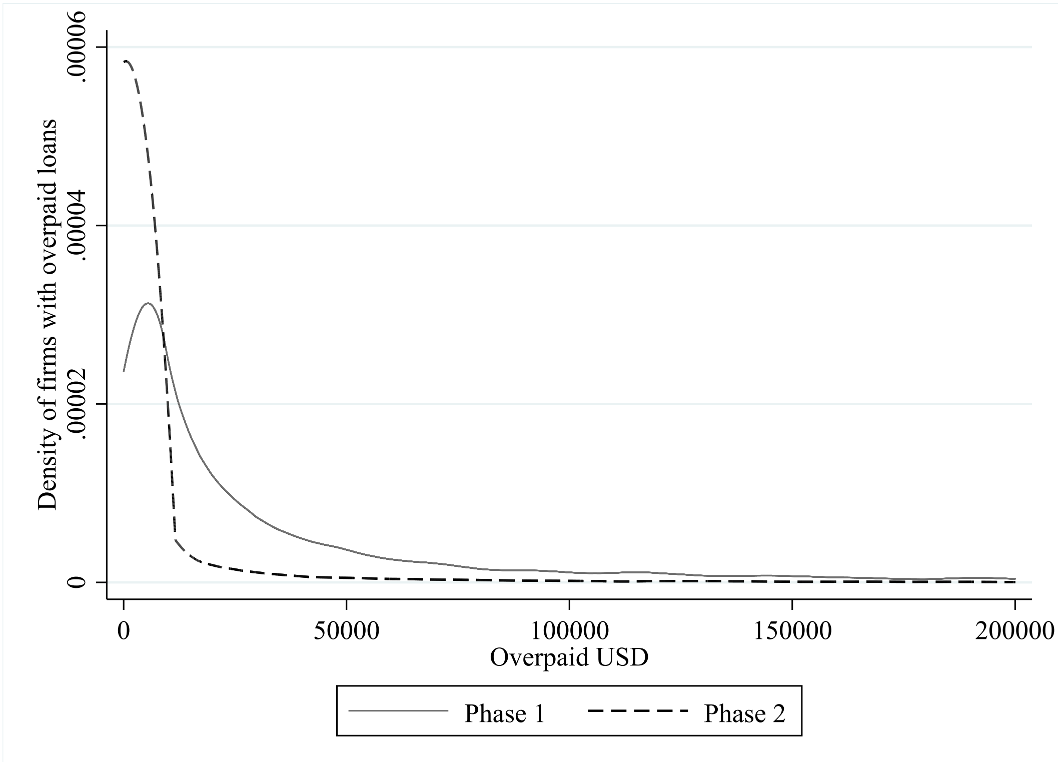


Figure 2: The distribution of overpaid amounts issued to borrowers with overpayments in Phase 1 and who reapplied in Phase 2. Data is at the firm-phase level, restricted to firms that applied in both phases and had at least one overpayment in Phase 1. Overpaid amount on a loan is the approved dollar value less the maximum payment due. For each firm that took out more than one loan in a phase, the overpaid amount is the total overpaid value across all the loans the firm was approved for in that phase. While the plot only shows up to $200,000 of overpayment, there is a small number of firms with overpaid amounts exceeding this level.

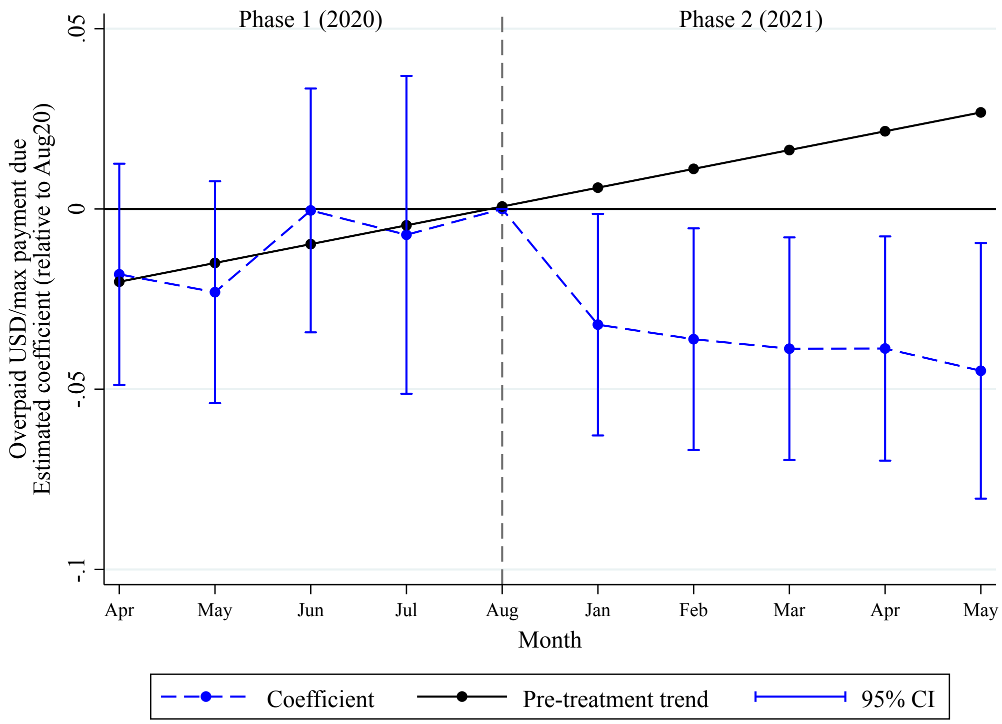


Figure 3: Event study plot for the overpaid amount (in USD) as a fraction of maximum payment due per firm. Data is at the loan-date level, restricted to firms that took out loans in both Phase 1 and Phase 2. For each firm with multiple loans, overpayment rate is the maximum rate among all the loans it was approved for. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in equation Equation 1. The treatment group consists of firms with at least one loan greater than $150,000 in Phase 1. The trend line connects predicted monthly outcomes generated from regressing the pre-treatment coefficients on a linear term of months.

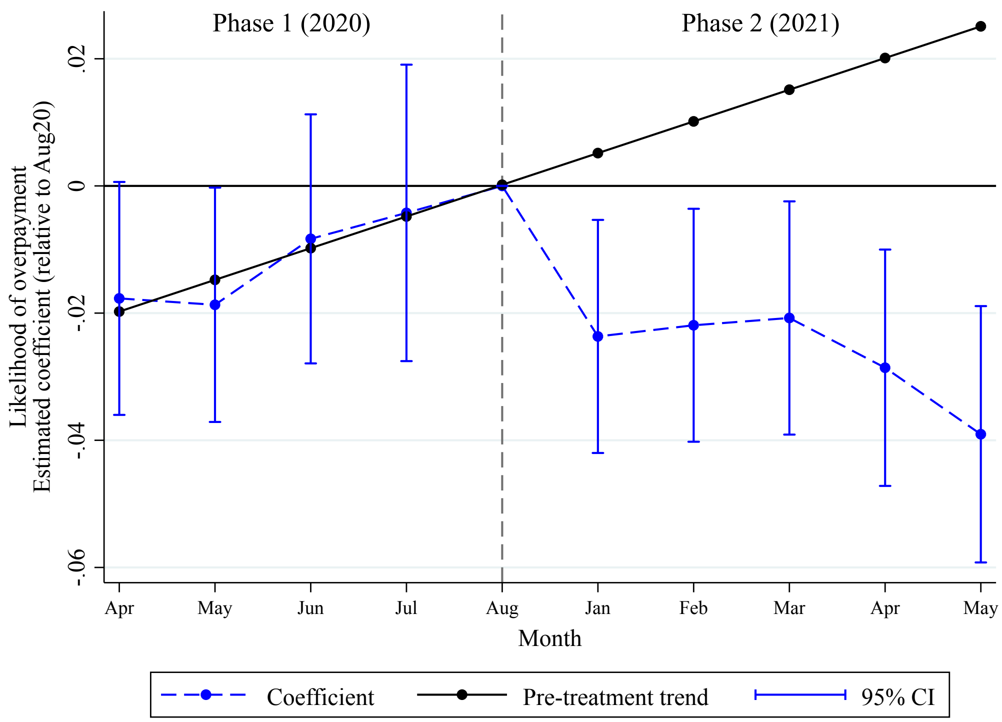


Figure 4: Event study plot for the probability that a firm was overpaid. Data is at the loan-date level, restricted to firms that took out loans in both Phase 1 and Phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in equation Equation 1. The treatment group consists of firms with at least one loan greater than $150,000 in Phase 1. The trend line connects predicted monthly outcomes generated from regressing the pre-treatment coefficients on a linear term of months.

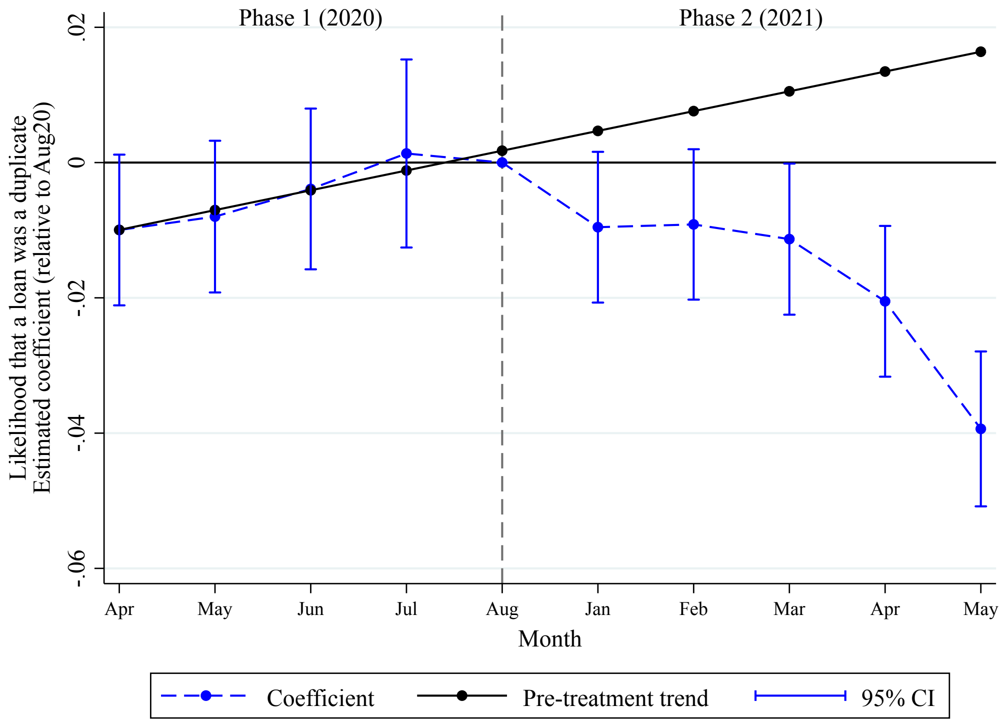


Figure 5: Event study plot for the probability that a loan was a duplicate. A duplicate loan is defined as any loan that is not the first loan issued to a firm. Data is at the loan-date level, restricted to firms that took out loans in both Phase 1 and Phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in equation Equation 1. The treatment group consists of firms with at least one loan greater than $150,000 in Phase 1. The trend line connects predicted monthly outcomes generated from regressing the pre-treatment coefficients on a linear term of months.

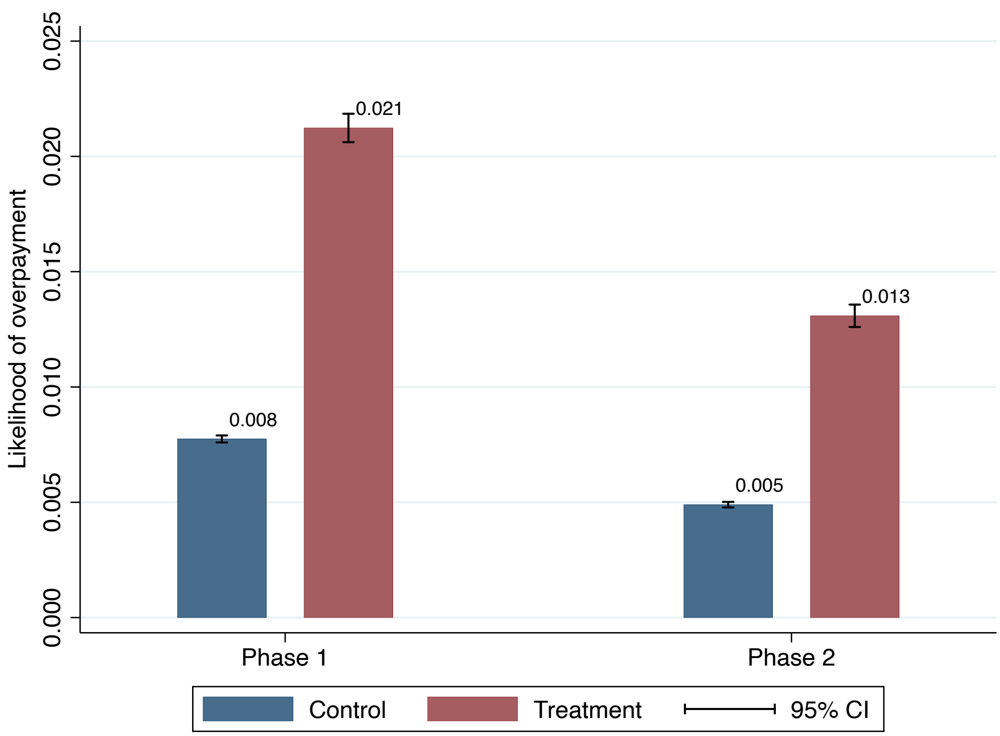


Figure 6: Probability that a firm was overpaid. Data is at the firm-phase level. The sample is restricted to firms that took out loans in both Phase 1 and Phase 2.

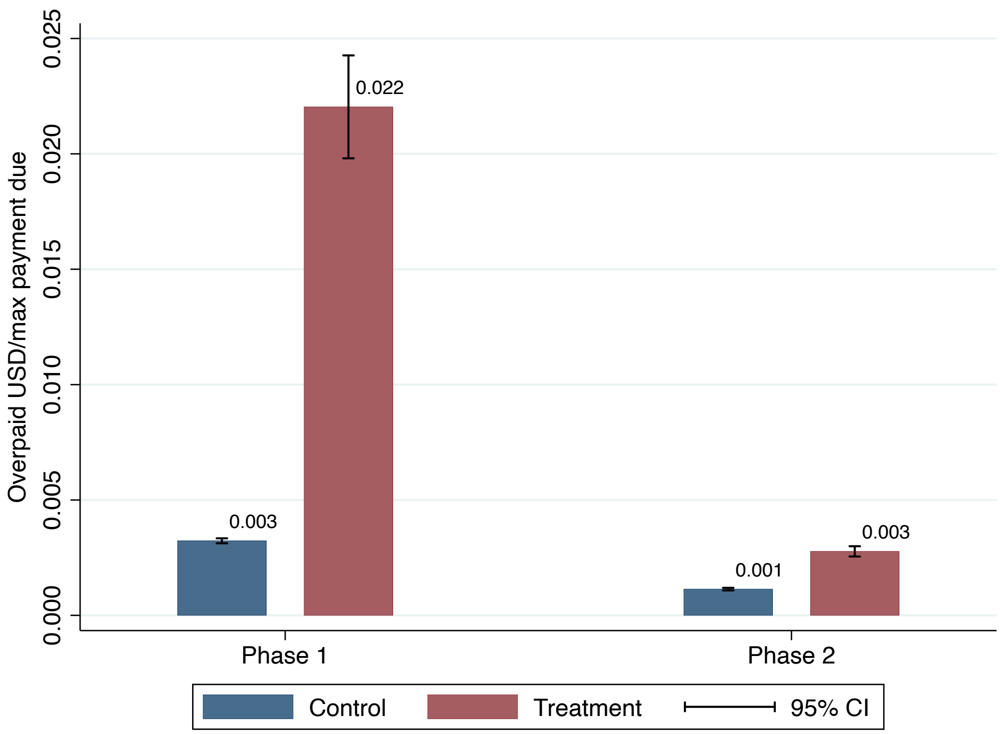


Figure 7: Overpaid amount (in USD) as a fraction of maximum payment due as per rules. Data is at the firm-phase level. For firms with multiple overpaid loans in a phase, the overpayment rate plotted is the maximum rate among all those loans. The sample is restricted to firms that took out loans in both Phase 1 and Phase 2.

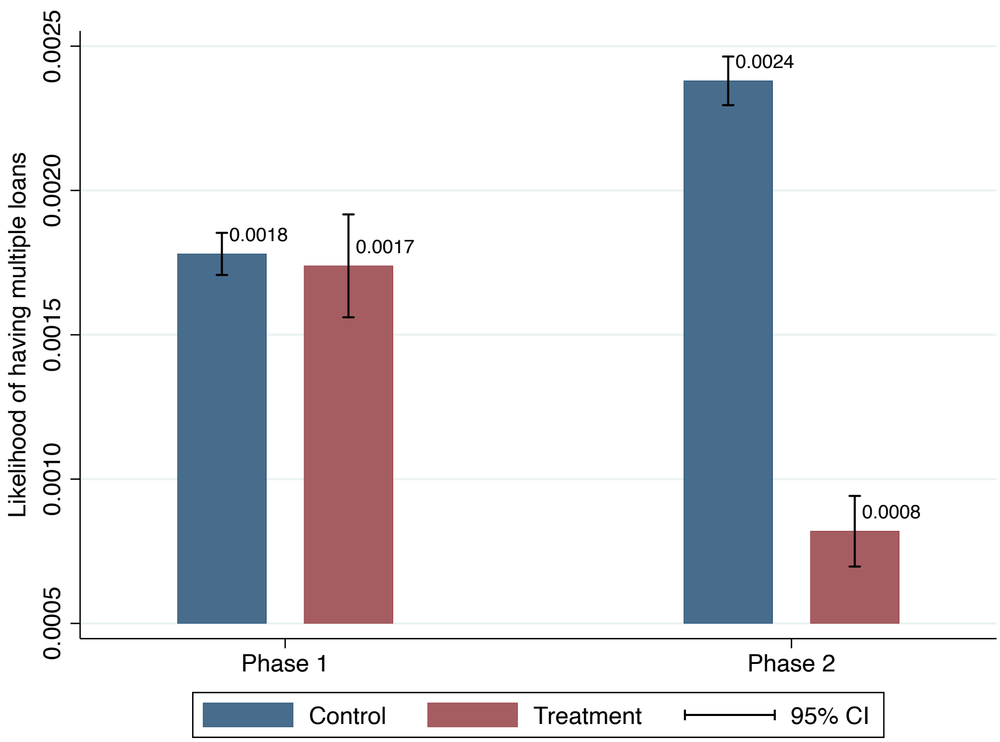


Figure 8: Probability that a firm had multiple loans. Data is at the firm-phase level. The sample is restricted to firms that took out loans in both Phase 1 and Phase 2.

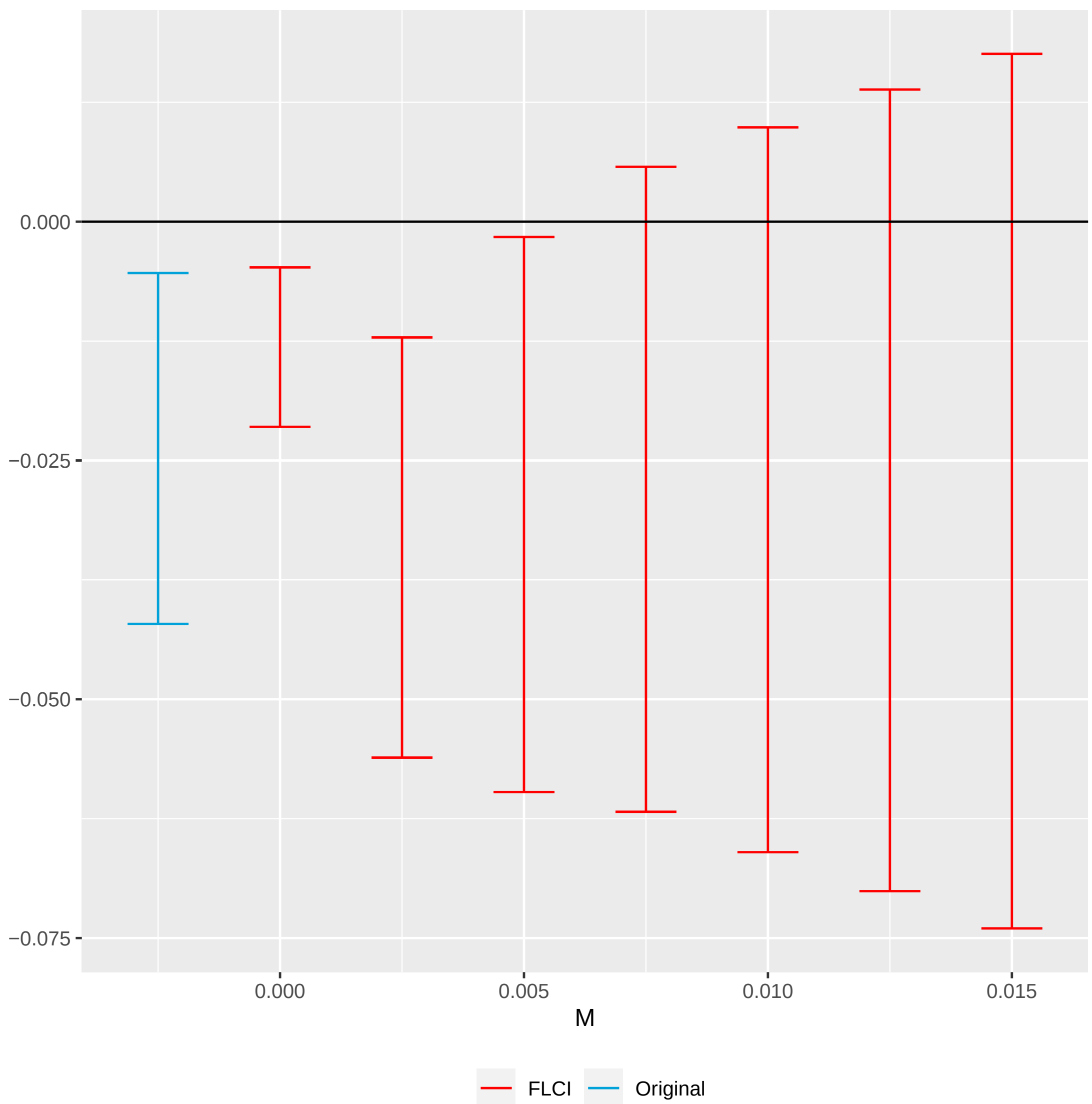


Figure 9: Honest DID plot for the probability that a firm was overpaid.The blue line denotes the coefficient obtained from the interaction term between treatment and Phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that in this version, we let the treatment indicator interact with Phase 2 instead with individual post-treatment months. The treatment group consists of firms with at least one loan greater than $150,000 in Phase 1.

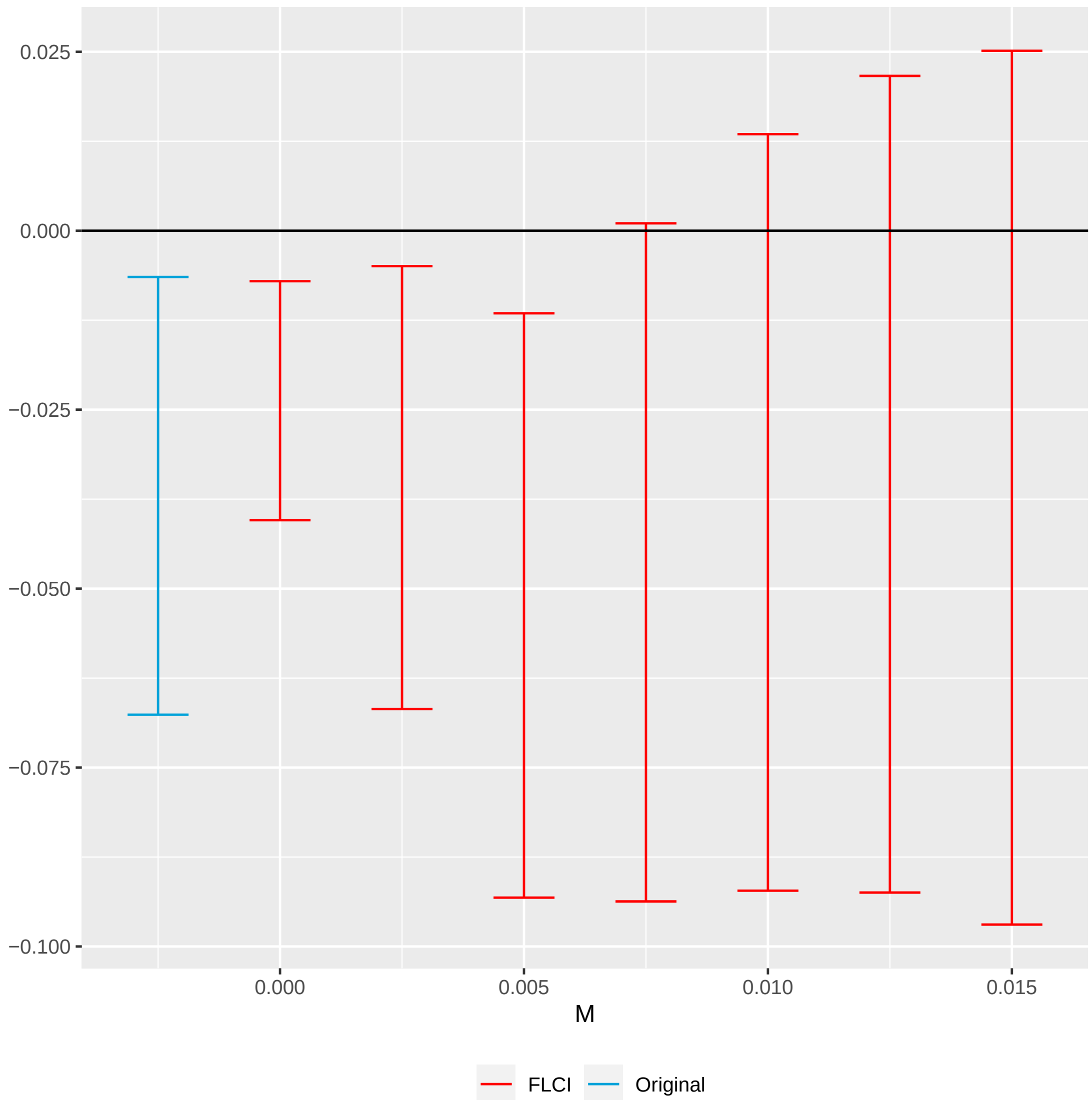


Figure 10: Honest DID plot for the overpaid amount (in USD) as a fraction of maximum payment due per firm. The blue line denotes the coefficient obtained from the interaction term between treatment and Phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that in this version, we let the treatment indicator interact with Phase 2 instead with individual post-treatment months. The treatment group consists of firms with at least one loan greater than $150,000 in Phase 1.

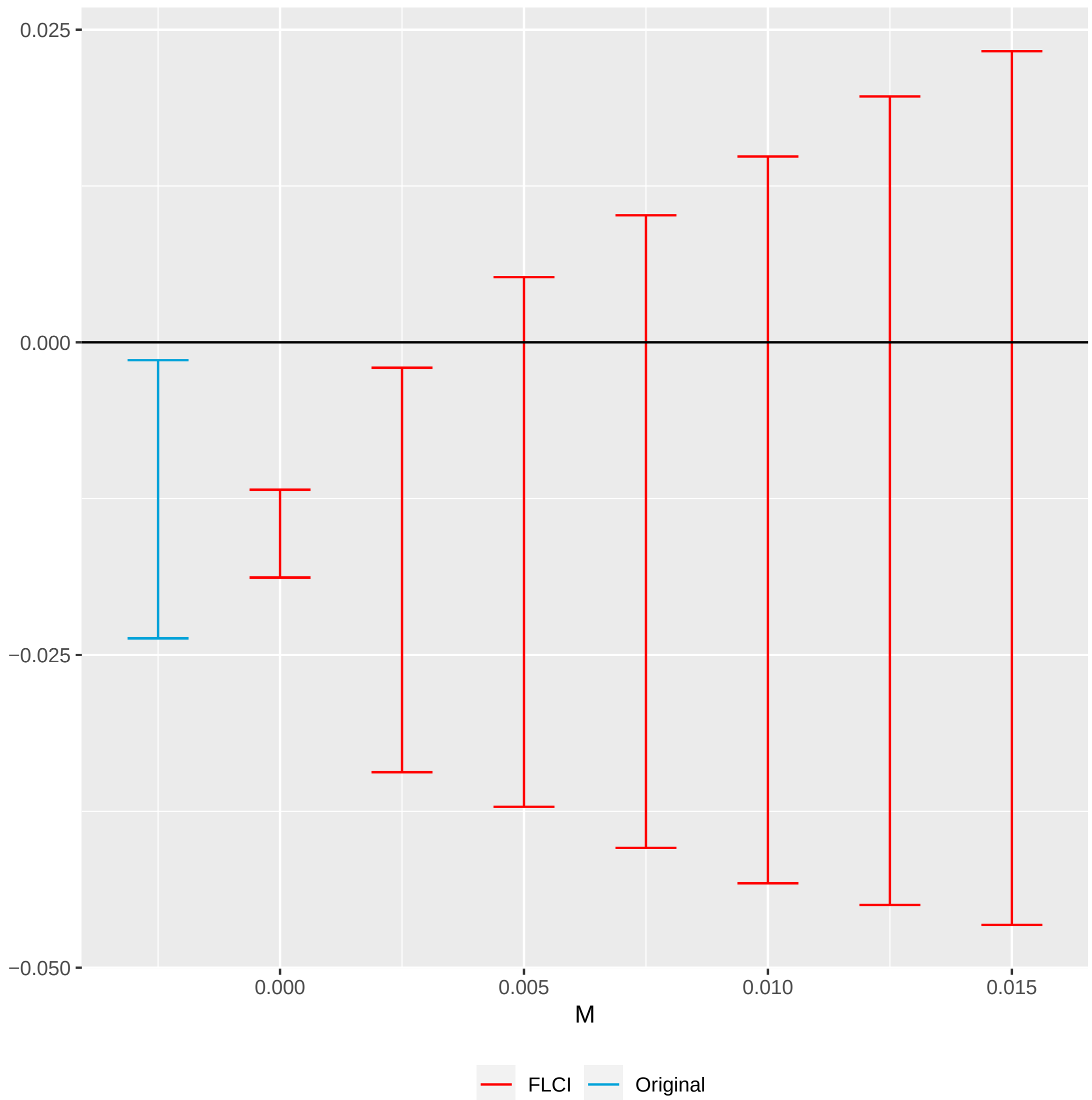


Figure 11: Honest DID plot for the probability that a loan was a duplicate. A duplicate loan is defined as any loan that is not the first loan issued to a firm in any given phase. Data is at the loan-date level, restricted to firms that took out loans in both Phase 1 and Phase 2. The blue line denotes the coefficient obtained from the interaction term between treatment and Phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that in this version, we let the treatment indicator interact with Phase 2 instead with individual post-treatment months. The treatment group consists of firms with at least one loan greater than $150,000 in Phase 1.

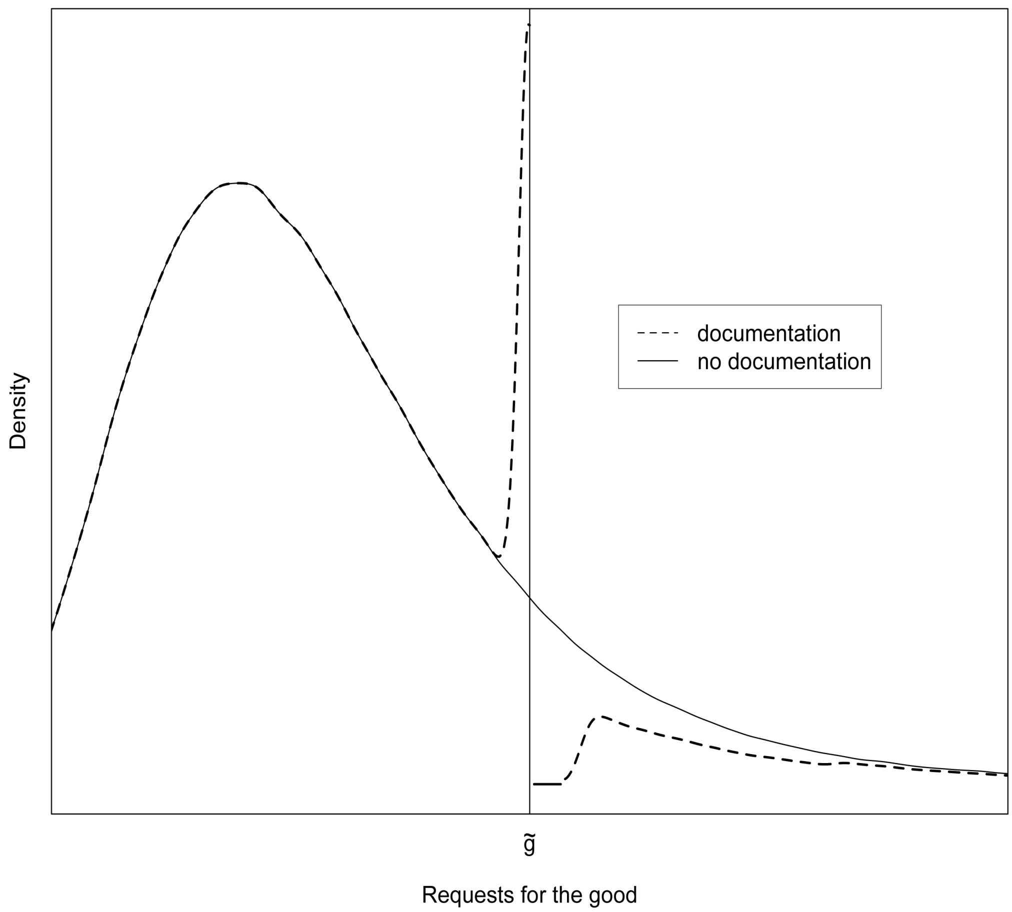


Figure 12: The theoretical impact of the advanced documentation requirement on the density of all requests for the good from the program. This graph was created by parameterizing the model as follows: $v(x) = \ln(x)$, $\pi(x) = x^2/2$, $\rho = 1/2$, $\tau = 1$, $\phi = 1/1000$, and F_1, F_0 are truncated normal densities with mean 5 and support [1,9].

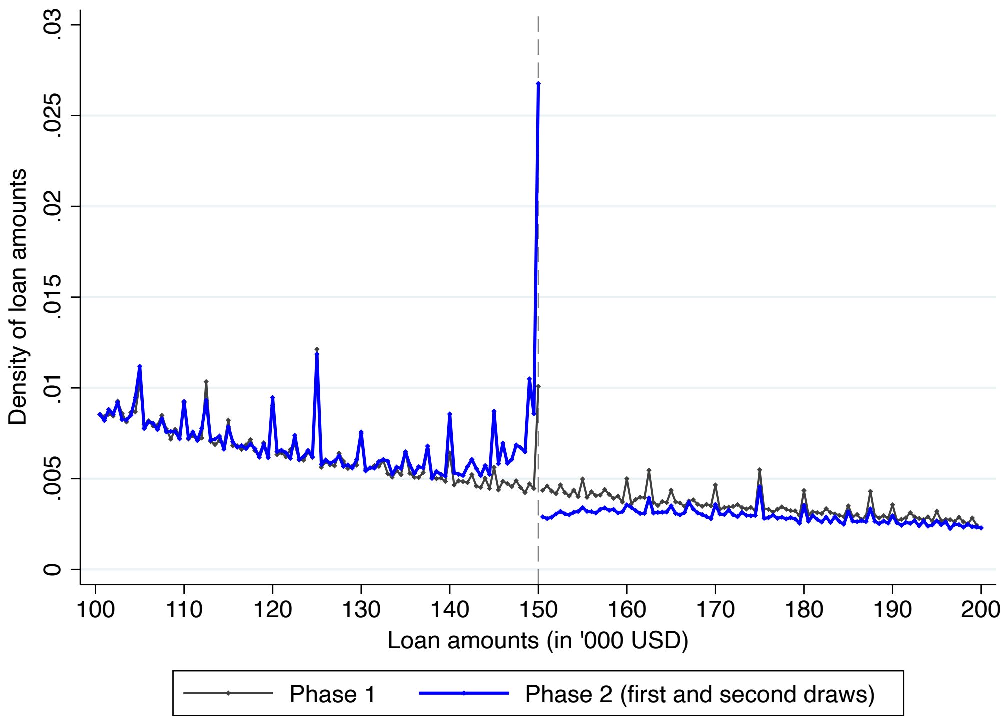


Figure 13: Density of loan amounts across the two phases of PPP program. Phase 2 borrowers include both first and second time borrowers. The vertical red line represents the $150,000 threshold. Bin width is $500.

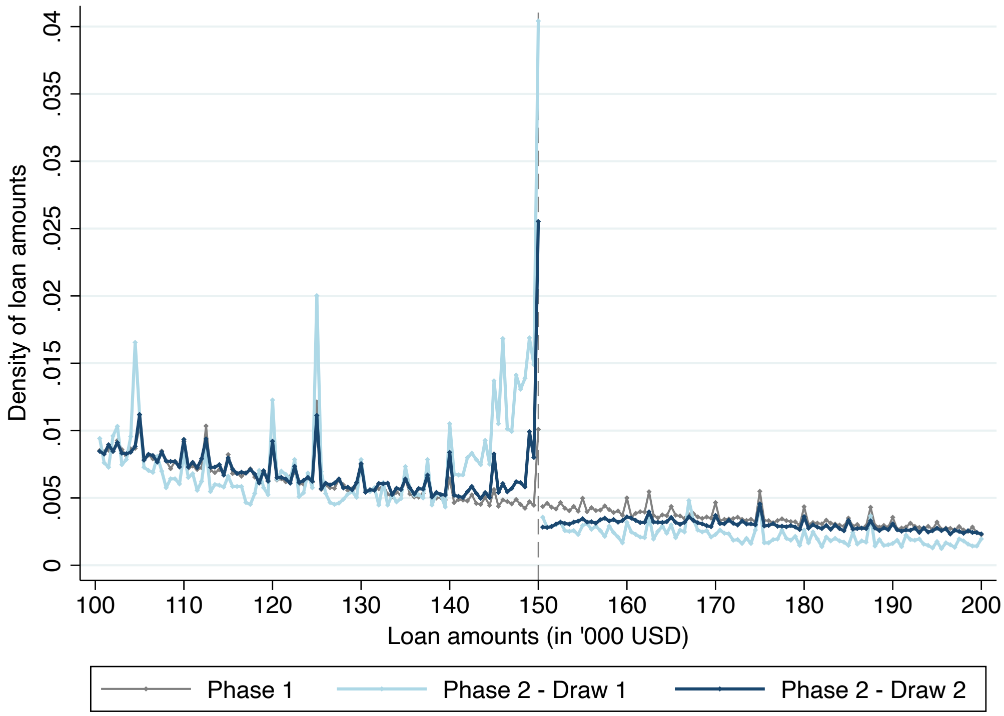


Figure 14: Density of loan amounts across the two phases of PPP program. First and second draw refers to whether a firm took a PPP loan for the first or second time. The vertical red line represents the $150,000 threshold. Bin width is $500.

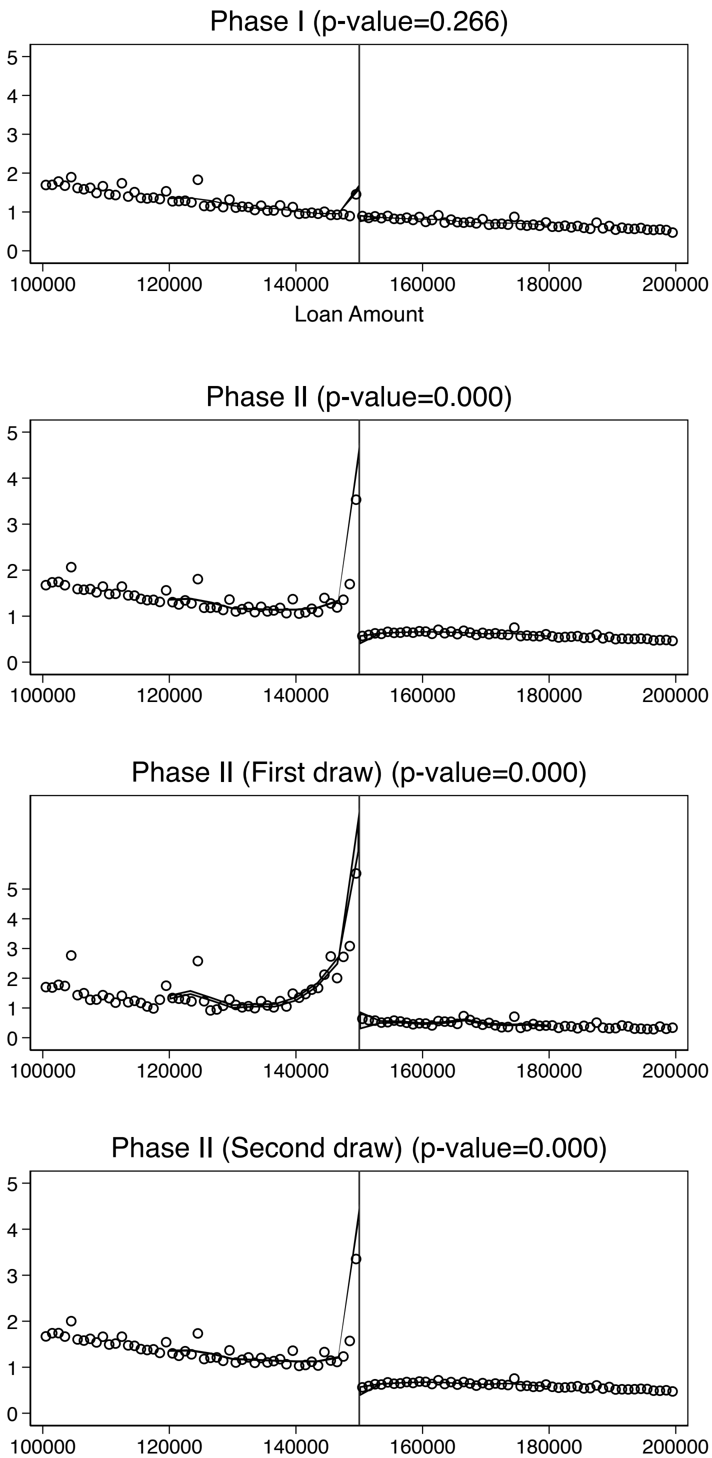
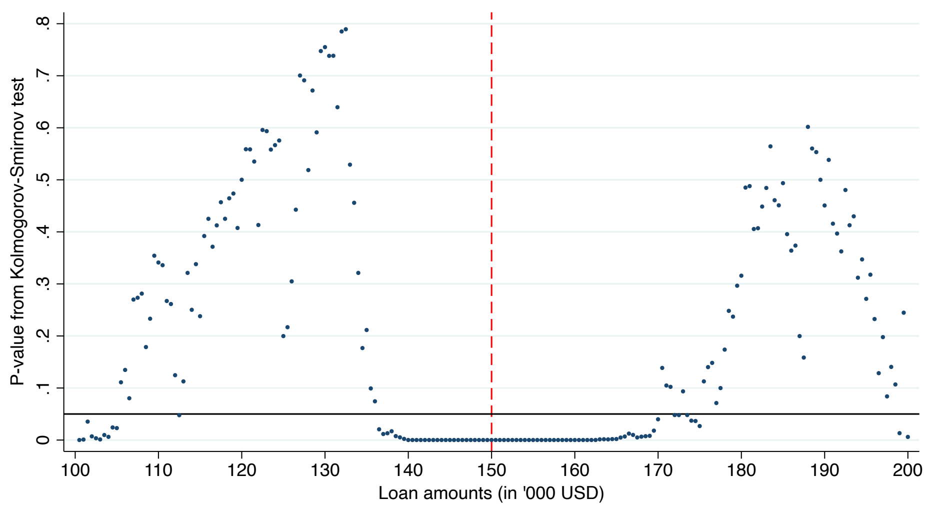


Figure 15: Distribution of approved loan amounts in phase I and phase II with p-value from a McCrary Density Test (McCrary, 2008) of continuity of densities around the \$150k cut-off



Notes: The vertical red line denotes the $150,000 threshold; the horizontal black line marks the point where p-value equals 0.05. As shown in the plot, the bunching window for second drawers in Phase 2 is between $136,000 and $175,000.

Figure 16: P-values from the Kolmogorov–Smirnov test.

Appendix tables

Table A1: Characteristics of borrowers

	Firms in Phase 1	Firms reapplied in Phase 2	Bunchers	Non-bunchers
	Business profile in Phase 1			
Business type				
Corporation	0.295	0.349	0.483	0.348
Limited Liability Company	0.283	0.299	0.250	0.300
Subchapter S Corporation	0.134	0.137	0.169	0.137
Sole Proprietor/Self-employed	0.198	0.131	0.014	0.132
Others	0.090	0.083	0.084	0.083
Business size				
At most 10 employees	0.775	0.740	0.210	0.743
11-20 employees	0.107	0.128	0.491	0.126
21-50 employees	0.076	0.092	0.250	0.091
More than 50 employees	0.042	0.040	0.048	0.040
Industry				
Construction	0.096	0.096	0.129	0.096
Professional, Scientific, and Tech.	0.131	0.126	0.136	0.126
Healthcare and Social Assistance	0.103	0.100	0.154	0.099
Accommodation and Food Services	0.074	0.106	0.039	0.107
Retail trade	0.091	0.079	0.059	0.079
Others	0.480	0.475	0.473	0.475
Unanswered	0.026	0.019	0.011	0.019
Location				
Urban	0.802	0.829	0.897	0.829
Rural	0.198	0.171	0.103	0.171
Registration date				
Before February 2020	0.997	0.998	0.999	0.998
After February 2020	0.003	0.002	0.001	0.002
	Share of loan proceeds in Phase 1			
Payroll	0.958	0.956	0.962	0.956
Utilities	0.013	0.014	0.011	0.014
Rent	0.015	0.016	0.016	0.016
Debt interest	0.001	0.001	0.001	0.001
Observations	5,128,815	1,494,052	8,256	1,485,796

The table summarizes the characteristics of all firms that took out loans in Phase 1. The unit of obseration is at the firm level. Bunchers are firms with at least one loan greater than $150K in Phase 1, all loans at $150K and below in Phase 2, and at least one loan in Phase 2 falling between $136K and $150K. Business size is the maximum number of employees a firm reported on its Phase 1 applications. Information about registration dates is from the OC dataset.

Table A2: Summary statistics of loan amounts and overpayment indicators

	Phase 1	Phase 2
Approved amount per loan	101,589.58	42,748.19
	(348,642.24)	(141,714.77)
Share of overpaid loans	0.010	0.003
Overpaid USD per loan	725.80	91.80
	(31,129.07)	(7,858.05)
Overpaid USD per overpaid loan	75,367.62	29,480.93
	(308,219.80)	(137,714.31)
Overpaid USD per $10k of max payment	66.04	8.57
	(2,852.92)	(308.07)
Overpaid USD per $10k of max payment for overpaid loans	6,857.39	2,751.02
	(28,259.83)	(4,789.10)
Share of firms with multiple loans	0.002	0.002
Number of loans	5,136,454	6,338,537
Number of firms	5,128,185	5,745,589

Note: The table shows the mean value and standard deviation (values in parentheses).

Table A3: Did the screening requirement affect fraud in PPP loans? (Loans between $100-200K in Phase 1)

Dependent Variable:	**Overpayment dummy**		**Overpayment rate**		**Multiple loans dummy**	
	(1)	(2)	(3)	(4)	(5)	(6)
T^0	0.003***		0.003***		0.000**	
	(0.001)		(0.001)		(0.000)	
Phase 2	-0.005***	-0.005***	-0.008***	-0.008***	-0.000	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)
$T^0 \times$ Phase 2	-0.002***	-0.002*	-0.003***	-0.003**	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Control mean of outcome	0.012	0.012	0.010	0.010	0.000	0.000
Observations	322752	322752	322752	322752	322752	322752
Firm FE	No	Yes	No	Yes	No	Yes

Note: The table shows how the screening requirement affects the main indicators of fraud. The unit of observation is at the firm-phase level. The sample is restricted to firms that received loans in both Phase 1 and Phase 2 with more than half of the loans in Phase 1 falling between $100K and $200K. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in a phase, and 0 otherwise. Overpayment rate on each loan is the overpaid amount divided by the maximum payment that a firm was eligible for. For firms with multiple overpaid loans, the regression was run with the maximum overpayment rate. Multiple loans dummy is a dummy variable that takes a value of 1 when a firm is identified to have been approved for more than one loan, and 0 otherwise. T^0, the treatment group, consists of firms with at least one loan greater than $150,000. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A4: Did the screening requirement affect overpaid amounts?

Dependent Variable:	**Overpayment amount**		**Overpayment amount (robustness check)**	
	(1)	(2)	(3)	(4)
T^0	4549.48***		282.75***	
	(174.80)		(38.74)	
Phase 2	-69.59***	-69.59***	-316.55***	-316.55***
	(2.21)	(3.12)	(18.94)	(26.79)
$T^0 \times$ Phase 2	-3551.33***	-3551.33***	-226.96***	-226.96***
	(175.93)	(248.81)	(43.49)	(61.51)
Control mean of outcome	119.97	119.97	479.07	479.07
Observations	2988104	2988104	322752	322752
Firm FE	No	Yes	No	Yes

Note: The table shows how the screening requirement affects the overpaid amounts issued to firms. The unit of observation is at the firm-phase level. The sample is restricted to firms that received loans in both phases of the program (columns 1-2), in addition to having more than half of the loans in Phase 1 falling between $100K and $200K (columns 3-4). Overpaid amount on a loan is the approved dollar value less the maximum payment due. For each firm that took out more than one loan in a phase, the overpaid amount is the total overpaid value across all the loans the firm was approved for in that phase. T^0, the treatment group, consists of firms with at least one loan greater than $150,000. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Appendix: For Online Publication

String matching to create a unique firm level identifier and identification of multiple loans to a firm in a given phase

The advanced documentation requirement was levied on second loans by firms that had already taken a PPP loan once. Our analysis also required that we identify firms that had taken multiple loans in a given phase. Since there were no unique firm level identifiers in the PPP data, we carried out a string matching exercise. This was both a time and computation heavy activity.

Using names and addresses that were submitted by borrowers with their PPP application, the following steps were used.

1. PPP loan data had an identifier for first or second time loans. We created two separate datasets using this identifier.

2. To reduce computation burden, we further split the data by states.

3. Within each state and first or second time loans, we then string matched borrowers on names and then addresses with other borrowers. This helped us identify borrowers with multiple loans to the same firm.

4. For each state, borrowers were then matched across first and second time loans using the same matching algorithm as in step 1. This helped us in creating a unique firm level identifier.

Following this algorithm we were able to create a unique firm-level identifier for approximately 70% of all second time loans. To verify the accuracy of these matches, we randomly sampled 1000 firms out of 2078901 total firms.Approximately 20 firms were chosen from each state. To check for false positives, we manually checked whether the names and addresses of borrowers that were identified as belonging to the same firm were in fact correct. If the addresses were different we used Google Maps to check whether the address were very different in terms of distance. Of the 1000 firms, we could only find 2 firms for which there were mismatches in addresses.