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 reduction in losses due to fraud equal to at least $744 million.

JEL codes: D22, D73, G38, H25, H32, H81
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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). It is ultimately an empirical question whether the fraud deterrence effect of screening will dominate potential reductions in program take up among legitimate beneficiaries. 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); Gray (2019); Homonoff and Somerville (2021) find that application and reporting requirements inhibit poor and marginalized households from accessing social protection benefits to which they are legally entitled. Understanding and identifying when costs of the administrative requirements are low relative to their value as a tool for sanctioning fraud is crucial for the design of public benefit schemes.

There are good reasons to expect that the trade-off between administrative burden and fraud reduction will vary according to program context. The potential costs of exclusion due to screening requirements are likely highest in settings where the pool of potential beneficiaries is poor; the value from fraud reduction is meanwhile low when the benefits allocated are small in monetary terms. In contrast, the relative benefits of fraud reduction may loom larger in settings where potential beneficiaries enjoy relatively high levels of human capital and solicit large sums of money. The nature of the trade-off is also a

[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), and 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.

function of the timeline over which government support is needed. Emergency relief programs, for instance, may feature more limited versions of screening than programs enacted in normal circumstances due to the significant welfare costs to delay. Whereas existing scholarship has focused on the burden-fraud reduction trade-off in normal times with a poor recipient pool, we focus here on the trade-off in the context of emergency relief with a relatively affluent recipient pool.

We study screening in one of the largest economic relief programs in US history: the Paycheck Protection Program (PPP), an $814-billion stimulus package adopted as part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act enacted on March 27, 2020. The PPP permitted small businesses, nonprofits, and 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). We proceed in three steps. First, we present evidence on the magnitude of the effects of screening requirements on loan irregularities. Second, we investigate the mechanisms behind the effects, by developing a theoretical framework that offers precise empirical implications about the behavior of borrowers one would expect to observe when the fraud-deterring effect of screening is large relative to its administrative burden. Finally, we evaluate the implications of the framework by examining the borrowing behavior of fraudulent versus legitimate firms before and after screening, finding that the evidence is consistent with the hypothesis that the fraud-deterring effect of screening exceeded its administrative burden.

The PPP was popular and widely accessed. Loan approval tracked the demand for loans: 94% of firms that were formally eligible for the loan received it (Autor et al., 2022a). 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 (Section 1106, CARES Act, 2020). Moreover, there were weak incentives for due-diligence by lenders, since loans were 100% guaranteed by the SBA as long as the lenders acted in good faith (Section 1102, CARES Act, 2020). The primary oversight mechanism was ex-post auditing 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).

To study the impact of screening requirements on loan irregularities, we exploit a change in loan eligibility documentation in phase 2 of the PPP for those firms that wanted to borrow for a second time.[3] The new rule was imposed on all firms which had previously received a PPP loan and were requesting loans greater than $150K in phase 2 of the

[3]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).

program. Such firms 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.[4] Note that the change only affected screening, and *not* eligibility, requirements: firms requesting loans of ≤$150K were also only eligible if they experienced a 25% or more reduction in gross receipts, but they were not required to provide up front documentation. Finally, in phase 1 there were no differences in required documentation by loan size.[5]

Given variation in screening requirements by program phase and loan value, we employ a difference-in-differences estimation strategy to compute the impact of screening on 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 documentation requirement in their second loan applications. We use the administrative rule and define our exposed firms (i.e. exposed to the rule change) 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 non-exposed firms are those that received a loan ≤$150K *in phase 1* and reapplied in phase 2. We discuss below and in Appendix section C.3 screening-induced exit from the program; in brief, exposed firms were not more likely to exit. Moreover, we show that for legitimate firms, requested loan sizes across phases were basically identical, indicating that any dynamic factors driving loan needs remained consistent throughout both stages.

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.[6] For each measure, we find an economically meaningful and statistically significant reduction in irregular loans attributable to the screening requirement. Our results are robust to defining the firms exposed to screening

[4]According to the SBA these could include any *one* of the following: quarterly financial statements, quarterly or monthly bank statements showing deposits from the relevant quarters, or the annual IRS income tax filings (this was required if the firm used an annual reference period). Such documentation requirements are straightforward and presumably not burdensome for any legal businesses regularly paying taxes.

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

[6]Note that these measures are similar to, and highly correlated with, the larger array of outcomes indicating fraud used in Griffin et al. (2023). See subsection 3.2 for more details.

as those that received loans between \$151K-200K and firms not exposed to screening as those with loans between \$100K-\$150K in phase 1 respectively, to clustering standard errors at different levels, to variation in matching algorithms, and to different assumptions about differential pre-treatment trends (Rambachan and Roth, 2023).

To elucidate the mechanisms underlying these results, and to clarify 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 documentation requirements; the costs for fraudulent firms increase discretely with documentation requirements given the prospect of legal sanction upon discovery of their fraud. We show that a set of firms will respond to the documentation requirements by strategically requesting ≤\$150K in phase 2, leading to "bunching" of the frequency distribution at and under \$150K; however, if the administrative costs of the documentation requirements are low relative to their value as a tool for sanctioning fraud, the likelihood of bunching in phase 2 will be higher for those firms that had loan irregularities in phase 1. In other words, the *marginal response* to the rule change will be systematically higher for fraudulent than legitimate firms.

Consistent with our framework, we find that relative to legitimate firms, borrowers with loan irregularities in phase 1 who would have been subject to screening in phase 2 disproportionately 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 significant discontinuity at this threshold.[7] This suggests that many borrowers strategically set their loan requests just below the threshold to avoid submitting documentation. The firms exhibiting irregularities in phase 1 of the program were significantly more likely to bunch than others, indicating that the fraud deterrent created by the requirement was large relative to its administrative burden.

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 \$744 million, or 88% of the total

[7] An advantage of our setting is that the empirical distribution of loans in phase 1 offers a counterfactual distribution. An absence of such a distribution is a recognized challenge in identifying bunchers in the public economics and labor literature (Blomquist et al., 2021; Jakobsen et al., 2020; Londoño-Vélez and Avila-Mahecha, 2020).

reduction in overpayment between phases 1 and 2.[8] 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 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 from reduced fraud, ceteris paribus.

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. Yet satisfying such a documentation requirement generates information that could potentially be utilized to identify and sanction bad actors who illicitly exploit the program. Consistent with this fact, fraudulent firms were strongly deterred from procuring more funds by the documentation requirement.

Although our empirical strategy is tailored to the institutional organization of the PPP, the implications of our paper extend beyond the US federal government's Covid-19 relief efforts. Our findings on the returns to screening are applicable to a critical set of public programs and government functions for which the timeliness of relief is essential, the potential participants are large in number, and the capacity to detect fraudulent intent is limited. Emergency relief programs, for instance, have all three features. 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.[9] Tax collection, another indispensable task of government, involves the latter two features.[10]

Our paper contributes to the literature on screening requirements for public benefits. The theoretical literature (Nichols and Zeckhauser, 1982; Besley and Coate, 1992; Kleven

[8]The documentation requirements led to an average reduction in overpayment by $3,551 per firm for 209,482 firms in the group of firms exposed to upfront documentation requirements (Table A.7).

[9]The warming climate has led to more extreme natural disasters, with concomitantly greater economic costs (Coronese et al., 2019; Estrada et al., 2015). Given this environmental and economic reality, the organization of emergency relief is poised to become an increasingly salient responsibility of government.

[10]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) and Pomeranz (2015). These upfront documentation requirements introduce additional hurdles and risk for those who would seek to evade their taxes, and contribute to broad compliance in spite of a low incidence of formal auditing.

and Kopczuk, 2011) clearly lays out the trade-offs involved. The empirical literature finds mixed evidence (Alatas et al., 2016; Deshpande and Li, 2019; Gray, 2019; Homonoff and Somerville, 2021).[11] Our results suggest that in settings where program scope or timing are such that rigorous ex-post auditing systems are costly and/or infeasible, screening can be a 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.

The current study also relates to the broader literature on regulation in law and economics.[12] 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.[13] In contrast, the empirical literature is limited, with exceptions being 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), and Eliason et al. (2021), who show that requiring prior authorization for ambulance reimbursements reduced health care fraud. 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 scope.

[11] Finkelstein and Notowidigdo (2019) show that an intervention designed to reduce the cost of applying to the SNAP mostly benefited richer beneficiaries, although they suggest that this reflected the poor targeting of the intervention rather than the general properties of screening requirements.

[12] 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).

[13] 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).

Finally, our paper also contributes to a small but growing literature on COVID relief funds, in particular the PPP.[14] Our paper is most closely related to two studies that examine fraud in the PPP, with Griffin et al. (2023) suggesting that FinTech lenders were responsible for much of this fraud, while Beggs and Harvison (2022) find that 6% of PPP funds that went to investment management firms likely consisted of overallocations. However, our paper is distinct from other works in the literature in that it provides empirical evidence on the consequences of changes in institutional design for fraud in this program.[15]

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 second phase was established under the Economic Aid Act and lasted from January 2021 until May 2021. The second phase 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 (Section 1106, CARES Act 2020). 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).[16] 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.[17] In terms of economic criteria, firms applying in phase 1 of the program

[14]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., 2022b; Granja et al., 2022; Chetty et al., 2023); 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) and that Black-owned firms were less likely to do so than similar white-owned firms (Chernenko et al., 2023).

[15]For example, President Biden announced a number of steps to combat fraud in pandemic relief programs: see `https://www.whitehouse.gov/briefing-room/statements-releases/2022/03/01/fact-sheet-president-biden-to-announce-new-steps-to-combat-criminal-fraud-and-identity-theft-in-pandemic-relief-programs/`, accessed February 23, 2023

[16]There was a penalty on firms if they retained fewer workers or reduced their total wages by over 25%.

[17]The ceiling was set at three hundred employees in phase 1 for housing cooperatives, member-based

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 of this as part of the application.[18] These documents could include relevant tax forms, including annual tax forms, or quarterly financial statements or bank statements (SBA's Interim Final Rule 13 CFR Parts 120 and 121). Others had to retain these documents and might be asked for these later by the SBA.[19] 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 (SBA's Interim Final Rule 13 CFR Parts 120 and 121). This announcement came five months after the conclusion of the first phase of the PPP program (August 8, 2020).

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

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 (Section 1102, CARES Act 2020).

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

professional organizations, and tourist boards.

[18]In phase 1 of the program documentation requirements were not linked to loan amounts.

[19]Initially, borrowers applying for loan forgiveness for their second loan (of $150,000 or less) were mandated to provide documents indicating a decline in gross receipts exceeding 25% in 2020 compared to 2019 as part of their loan forgiveness application. However, starting in July 2021, the Small Business Administration (SBA) implemented the COVID Revenue Reduction Score to evaluate eligibility. This score was determined based on industry, geography, and the size of the business applying for loans. Each Second Draw PPP Loan of $150,000 or less was assigned a score. If the score exceeded the specified value required to validate the borrower's revenue reduction, it was deemed sufficient to meet the documentation requirement for proving revenue reduction. Only in cases where the score fell short of the required value were borrowers mandated to submit original documents demonstrating a reduction in gross revenue exceeding 25% in 2020 compared to 2019 (refer to SBA's Interim Final Rule 13 CFR Parts 120, docket number: SBA-2021-0015).

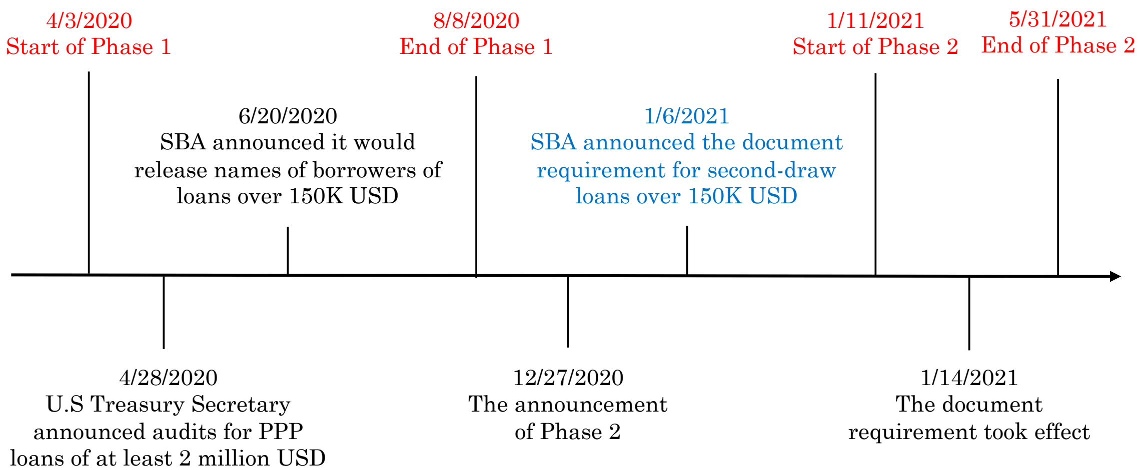

Figure I: Timeline of the Paycheck Protection Program.

adherence to 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.[20] 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.[21] 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 were

[20]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.

[21]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 (Cowley, 2020).

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 applications and all related documents for four to six years after the disbursement of the loans. Yet the frequency of auditing and the degree of scrutiny it would entail were not publicly disclosed. Only loans above $2 million were guaranteed to be audited.

Evidence of widespread fraud in the program is now abundant.[22] 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. Another notable case was that of former NFL wide receiver Joshua Bellamy, who was sentenced 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. FinTech lenders appear to have approved a disproportionate number of fraudulent loans identified by the Justice Department.[23]

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.

[22]A New York Times article 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.

[23]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. For details on the Sah case, see Indictment, *United States of America v. Dinesh Sah*, (N.D. Tex. 2020) (No. 3:20CR0484-S). On FinTechs and the incidence of fraud, see Griffin et al. (2023).

3 Data and variable coding

3.1 Data sources

Our primary data is the universe of PPP loans approved across the two phases of the program, made available by the Small Business Administration (SBA).[24] 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 A.1 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 or fewer employees.[25]

It is important to highlight that having unique firm-level identifiers is important for our analysis since the advanced documentation requirement was levied on second loans by firms that had already taken a PPP loan once. Moreover, such identifiers are also important to classify 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. Appendix C describes details of our string matching algorithm and also offers robustness checks on our findings.

3.2 Key variables

Below we describe the construction of our outcome measures: the extensive and intensive margins of overpayment in loans, and the receipt of multiple loans. We consider these as strong indicators of fraud, while accepting that they do not provide a comprehensive account of fraud in the PPP.[26] In addition to these measures, we considered numerous

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

[25] 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.

[26] For example, they would miss more sophisticated types of fraud like multiple loans for the same business under different names; or the misuse of funds for personal consumption rather than employee wages. For this reason, we do not attempt to estimate any aggregate measures of fraud.

other measures indicating fraud, including those used in the literature (Griffin et al., 2023) as well as others relying on matching external data sources. In Appendix D we show that many of these other measures are strongly correlated with our measures, and explain why the ones we use are most appropriate for the analysis in the paper.

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 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 two 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. Figure B.1 plots the distribution of overpaid amounts conditional on a firm reapplying in phase 2 and having

[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).

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 A.2. 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. 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.[28] Below we describe the process of identifying firms with multiple loans in the data.

As discussed, the SBA does not provide a unique 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 used 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 (see Appendix C for details on our string matching algorithm).[29] A firm could have received multiple loans in violation of the rules (Section 1102, CARES Act 2020) 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).

Although the number of such firms is relatively small, they were collectively granted a sum of $2.8 billion in PPP loans. The average number of loans issued to a firm with multiple

[28]Griffin et al. (2023) also use multiple loans as an indicator of fraud.

[29]Our definition of a firm is the same as the U.S. Bureau of Labor Statistics' definition of an establishment, i.e. a single physical location where one predominant activity occurs.

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 A.2).

4 Did screening affect fraud in PPP?

4.1 Identification strategy

The advance documentation requirement in phase 2 stipulated that all firms that had previously received a PPP loan and were requesting loans greater than \$150K in phase 2 must submit documentation showing a reduction in gross receipts of more than 25% in 2020 relative to 2019. Those with loan requests of ≤\$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 classify firms as *exposed* to screening if they had applied for a loan greater than \$150K in phase 1. Assuming that the underlying economic conditions that determined the loan requests by the firms remained constant, these are the firms that were affected by the changes in the documentation requirement. The panels in Figure 1 lend support to this assumption. These graphs plot the loan amounts in the two phases of the program for different types of firms: those with no loan irregularities, and those with overpaid and multiple loans in phase 1, respectively. We can see that for legitimate firms the loan amounts remained almost identical across the two phases, suggesting that the firm fundamentals determining loan need and eligibility remained constant across the two phases. On the other hand, firms with either type of loan irregularity reduced their loan amounts in phase 2 relative to phase 1. We classify firms as *non-exposed* if they had loan amounts ≤\$150K in phase 1 and were, therefore, not subjected to the screening requirement.

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 (i.e., exposed) group. We assess the evidence in support of this assumption using an event-study plot prior to conducting the main analysis.

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

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

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). $Exposedfirms_i$ is an indicator variable equal to 1 for those those firms with a loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2, 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 as that is the level at which the documentation requirement was levied (Abadie et al., 2023).[30] The coefficient ρ_g estimates the effect of belonging to the group of firms that were exposed to screening 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 2 and Figure 3 plot ρ and 95% confidence intervals for each month-year of the PPP program.[31] Figure 2 (top panel) shows the effect on the overpayment rate[32], and Figure 2 (bottom panel) presents the result for the overpayment dummy. Figure 3 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 exposed and non-exposed firms, 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:

$$Y_{it} = \alpha_i + \omega Phase2_t + \gamma Exposedfirms_i \times Phase2_t + e_{it} \quad (2)$$

[30] Appendix Table A.3 to Table A.5 presents results clustering at the lender location level. The results remain unchanged when we cluster at this level.

[31] Since phase 2 started four months after the first phase we have adjusted the x-axis to reflect this.

[32] In case a firm had more than one loans, overpayment rate is the maximum rate among all the loans it was approved for.

where Y_{it} stands for irregularities in PPP loans defined in the ways described earlier. In addition to our treatment indicator ($Exposedfirms_i$), 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. Firm fixed effects (α_i) are included to control for any firm-specific unobserved heterogeneity. Standard errors (e_{it}) are clustered at the firm level.

4.3 Results

Appendix Figure B.2 through Figure B.4 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 firms exposed to screening. This was true across both phases. Second, average outcomes for these measures declined across phases for all types of firms; however, the fall was much greater for the firms that were exposed to screening than those that were not. Finally, the incidence of multiple loans was slightly higher among the non-exposed firms than firms exposed to screening in phase 1. Yet the incidence of multiple loans declined sharply in phase 2 for the firms that were exposed to screening while it increased for the non-exposed firms.

Table 1 moves beyond 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 exposed 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 0.5 percentage point reduction in the probability of overpayment relative to firms that were not subjected to it. 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 percentage points (more than 5 times the control mean) for exposed firms relative to those that were not exposed to the screening requirement 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 percentage points (an effect equal to the value of the control mean). This effect is both statistically and economically significant. In sum, our findings consistently indicate that the introduction of the documentation requirement led to substantial reductions in loan irregularities indicative of fraud.

Robustness using the "more credible approach". Following recent developments in differences-in-differences estimation (see Roth et al., 2023 for a review), we use the "more credible approach" to parallel trends suggested by Rambachan and Roth (2023). We use this to investigate the robustness of our results to alternative assumptions about different outcome trends for treated (firms that were exposed to upfront documentation requirement and that had loan amounts greater than \$150K in phase 1 of the program) and control firms (firms that had loan amounts ≤\$150K in phase 1 of the program and were not exposed to the upfront documentation requirement).

Rambachan and Roth (2023) 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.[33] We, therefore, bound the change in slope of the differential trend between treated and control firms using the following formula suggested by Rambachan and Roth (2023):

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

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 B.8 and Figure B.9, respectively, while those for multiple loans are in Figure B.10. 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=0.005. In the case of multiple loans dummy this value is M=0.0025.

As discussed by Rambachan and Roth (2023), nothing in the data itself can place an

[33] Figure B.5 through Figure B.7 displayed a pre-trend in fraud in firms requesting more than \$150K and those requesting ≤\$150K in loans in phase 1 that appears to be roughly linear.

upper bound on the parameter M. We follow the methodology in Dustmann et al. (2022) to analyse and benchmark the value of M. For each of the outcomes, we use only phase 1 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.[34] 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 B.8, Figure B.9, and Figure B.10.

Other checks. In this section we present further checks that support a causal interpretation of our estimates. Appendix Table A.6 presents the results defining firms exposed to screening as those with a loan amount between \$151K - \$200K and the non-exposed firms as those with loan amounts between \$100K - \$150K *in phase 1*.[35] 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 behavior. The results are in the same direction as those in Table 1, albeit of smaller magnitude as the sample is restricted. This result is also reassuring as it helps to rule out the possibility that our overpayment measure, used to detect fraud, is solely based on the size of the firm.[36]

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 I). The announcement of the documentation requirement was on January 6, 2022, right before the start of phase 2 of the program.[37]

Another worry can be non-random attrition of the fraudulent firms subject to screening. For instance, if those fraudulent actors with loan amounts greater than \$150K in phase 1 were more likely to exit the program as a result of the upfront documentation requirement

[34]For a graphical representation see Figure B.5 to Figure B.7. 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.

[35]If a firm had been approved for multiple loans, the firm would be included in the sub-sample if the majority of the approved loans were between \$100-200K.

[36]Since the overpaid loan calculations uses a maximum annual salary threshold of \$100,000, the loan overpayment variables might classify larger business with higher annual salaries as fraudulent rather than smaller ones.

[37]This announcement was made via the release of a rule change. The documentation requirement was effective for second time PPP loans made after January 14, 2021. `https://www.sba.gov/sites/default/files/2021-01`

then that can result in overestimation of the treatment effect. Since we have the universe of firms in both phases we can directly check for treatment induced exit. We find that firms that were exposed to the upfront documentation requirement and those that had irregularities in phase 1 were *less* likely to exit the program than those firms not exposed to the documentation requirement, suggesting that our estimates are downward biased and conservative (details of the estimation and results can be seen in Section 6).

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

The findings presented thus far establish that the PPP's documentation requirement was successful 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 cost of fraud at $\widetilde{g}$ can be conceptualized as reflecting firms' beliefs about the likelihood that fraud will be discernible 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 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:

$$\eta_L^1 \equiv \eta_i | u_i(g_i^{1*}(\eta_i)) = u_i(\widetilde{g}) \qquad (10)$$
$$\eta_L^0 \equiv \eta_i | u_i(g_i^{0*}(\eta_i, \tau)) = u_i(\widetilde{g})$$

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:

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

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.[38]

Figure 4 depicts the theoretical impact of the documentation requirement on the density of all requests for the good from the program.[39] 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

[38]There is a large public economics (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013) and labor (Burtless and Hausman, 1978; Aaron and Pechman, 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.

[39]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].

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, when τ is high and ϕ is low, the proportion of bunchers that are comprised of fraudulent firms will be higher than the proportion of non-bunchers that are comprised of fraudulent firms.*

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 fraudulent firms are more likely to bunch than the legitimate firms. 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 for firms with a loan amount greater than \$150K in phase 1 that were exposed to the screening requirement. 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 and would have been subject to the documentation requirement 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 fraudulent firms to manipulate the details of their loan applications so as to avoid crossing the \$150K threshold in phase 2. 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:

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

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 amount of the loan; 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 ($Exposedfirms_i$) 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.

The main assumption is that in the absence of the screening requirement, the likelihood of exit or the loan amounts granted in phase 2 would not have exhibited systematic differences between fraudulent and legitimate firms with loan amounts above or below $150,000 in phase 1.[40]

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).[41]

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, had a 91% probability of continuing to ask for loans greater than $150K. The loan amounts they received were also larger in phase 2 than non-exposed firms (Columns (3)). In contrast, 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

[40] Since the outcomes are observed in phase 2 we cannot present a standard event study plot as before. However, Figure 1 provide some evidence in favor of our assumption.

[41] Since the share of firms with multiple loans is five times less than the share of overpaid loans (see Table A.2), we have less power to detect effects in Table 2b. Therefore, while the sign of the coefficients are in the same direction, firms which received multiple loans in phase 1 and were subject to the documentation requirement were no more or less likely in a statistical sense to exit than other firms.

loans were 11% and 13.7% less likely to receive a loan amount greater than $150K (in phase 2), respectively than other firms. 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 at or just below the $150K cut-off. As in our formal model, we refer to borrowers who pursue such a 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 fraudulent firms should have a higher likelihood of bunching than legitimate firms.

Due to the fact that we observe loan allocations across the two phases of the program, our dataset is 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.[42] If bunchers engaging in strategic evasion do exist, then we should observe specific patterns in the distributions of loan values in the

[42] 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).

two phases. First, in phase 2 one should observed a sharp upward spike in the density of loans immediately at and 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 5 presents the density of loan amounts for all phase 1 and phase 2 loans. Figure B.11 presents the same information, but breaks down the phase 2 loans into first and second time borrowers.[43] 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 at and just to the left of the \$150K cutoff and a missing mass of borrowers above the cutoff. The spike at and 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.[44]

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 B.12 presents the density as well as the p-value from a McCrary density test (McCrary, 2008).[45] 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.[46] There are two key takeaways. First, we fail to reject the null of a continuous loan distribution in phase 1 at 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 distribution in phase 2 at the \$150K cut-off (p-value is close to zero). This finding is consistent with firms changing their behavior following the introduction of the advanced documentation requirement and "bunching" at or below the cut-off of \$150K.

Identifying "bunchers". While a visual inspection of Figure 5 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

[43]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 at or just below the \$150K cutoff.

[44]By reference effects, we refer to the concentration of loans in amounts that are easy to remember, typically "round" numbers that are factors of five thousand.

[45]Figure B.13 presents a similar test but breaks down phase 2 loans into first time and second time loans.

[46]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).

loans. Figure 6 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.

Who bunches? Proposition 1 describes a way in which we can test whether the documentation requirement was an effective one i.e., whether fraudulent firms were significantly more likely to engage in bunching behavior than legitimate firms. In this subsection we investigate this further. We use our data at the firm (i) level and estimate the following:

$$Buncher\ in\ Phase2_i = \pi + \theta Loan\ Irregularities\ in\ Phase1_i + v_i \tag{13}$$

where $Buncher\ in\ Phase2_i$ is an indicator variable equal to 1 for those second time borrowing firms that had PPP loans of greater than \$150K in phase 1, but who 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. $Loan\ Irregularities\ in\ Phase1_i$ includes the following variables measured in phase 1: whether the firm was overpaid in phase 1; the rate of said overpayment; and whether a firm received multiple loans in phase 1. Standard errors (v_i) are clustered at the firm level.

The coefficient θ captures whether loan irregularities in the first phase predict the likelihood of bunching in phase 2. If bad actors were more likely to be bunching firms, we should expect $\theta > 0$ for the measures of fraud or loan irregularities in our study.

Table 3 presents the results. We find that loan irregularities in phase 1 increase the likelihood of bunching in phase 2. Table 3 Column (1) shows that firms that received an overpayment in phase 1 were roughly 2.5 times more likely to be a buncher in phase 2 than firms that did not receive an overpayment. The effect of overpayment–equal to an increase of 1 percentage point in the likelihood of bunching–is statistically significant at conventional standards. Column (2) shows that a 1 percentage point change in the overpayment rate is associated with a 0.2 percentage point increase in the likelihood of bunching. The effect of such a change–equal to one third of the control mean of the outcome–is statistically significant. Although the effect of receiving multiple loans in phase 1 is less precise and insignificant, it is substantively similar to the effects of overpayment rate in Column 2. A firm that received multiple loans in phase 1 is 0.2 percentage points (33% of the control mean of the outcome) more likely to bunch in phase 2, than firms that did not receive multiple loans. Taken together, the results suggest that bunchers were disproportionately comprised of firms with a checkered history in the program. These results indicate that the deterrent effect of the screening requirement against fraud outweighed its associated administrative burden.

Even though bunchers had taken pains to avoid screening by setting their loan requests to values at or below the $150K cutoff, it appears the existence of the documentation requirement seems to have instilled greater caution, thereby reducing the incidence of irregularities. Figure B.14 shows the average irregularities in the loans of bunchers and non-bunchers across the two phases. Bunching firms had much high rates of loan irregularities in phase 1 and experienced a larger fall in irregularities in phase 2 as compared to non-bunching firms.

7 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 at or below a threshold value of $150K. A non-negligible mass of borrowers did precisely that. Even so, the advent of screening 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)
Exposed firms	0.013***		0.019***		-0.000041	
	(0.00032)		(0.0011)		(0.000098)	
Phase 2	-0.0029***	-0.0029***	-0.0021***	-0.0021***	0.00060***	0.00060***
	(0.000086)	(0.00012)	(0.000059)	(0.000083)	(0.000052)	(0.000073)
Exposed firms × Phase 2	-0.0053***	-0.0053***	-0.017***	-0.017***	-0.0015***	-0.0015***
	(0.00036)	(0.00051)	(0.0011)	(0.0016)	(0.000100)	(0.00014)
Control mean of outcome	0.00775	0.00775	0.00323	0.00323	0.00178	0.00178
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. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2. Phase 2 is a dummy variable that takes a value of 1 for phase 2 of the PPP, remains zero otherwise. 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)
Exposed firms	-0.032***	0.91***	140850.2***	-4.39***	10.1
	(0.00062)	(0.00058)	(2390.8)	(0.082)	(11.5)
Overpaid in phase 1	0.014***	-0.0048***	-7398.3***	2.38***	-22372.9***
	(0.0023)	(0.0011)	(309.7)	(0.071)	(207.2)
Exposed firms × Overpaid in Phase 1	-0.037***	-0.11***	-59343.9***	20.6***	-18639.6***
	(0.0048)	(0.0060)	(6160.1)	(0.59)	(1579.5)
Control mean of outcome	0.713	0.0165	40273.7	-0.443	844.8
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)
Exposed firms	-0.033***	0.91***	140061.0***	-3.93***	-686.1***
	(0.00061)	(0.00058)	(2387.0)	(0.081)	(37.5)
Multiple loans in phase 1	-0.042***	-0.0051**	2493.6	0.44*	945.9***
	(0.0056)	(0.0022)	(3115.8)	(0.26)	(135.5)
Exposed firms × Multiple loans in Phase 1	-0.0092	-0.14***	-28115.8*	3.47*	-740.2
	(0.016)	(0.022)	(16678.6)	(1.87)	(493.8)
Control mean of outcome	0.713	0.0165	40326.6	-0.425	669.8
Observations	5128185	1494052	1494052	1494052	1494052
Fixed effects	No	No	No	No	No
Phase 1 control	No	No	Yes	No	No

Note: The unit of observation is the firm. For Columns (2)-(5) 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 1 values from phase 2 values to get an across-phase difference for each firm. Exposed firms consists of firms with a loan greater than $150,000 in phase 1 and were exposed to the upfront documentation requirement. Overpaid in phase 1 is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. 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 phase 1, and 0 otherwise. Standard errors are clustered at the firm level. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

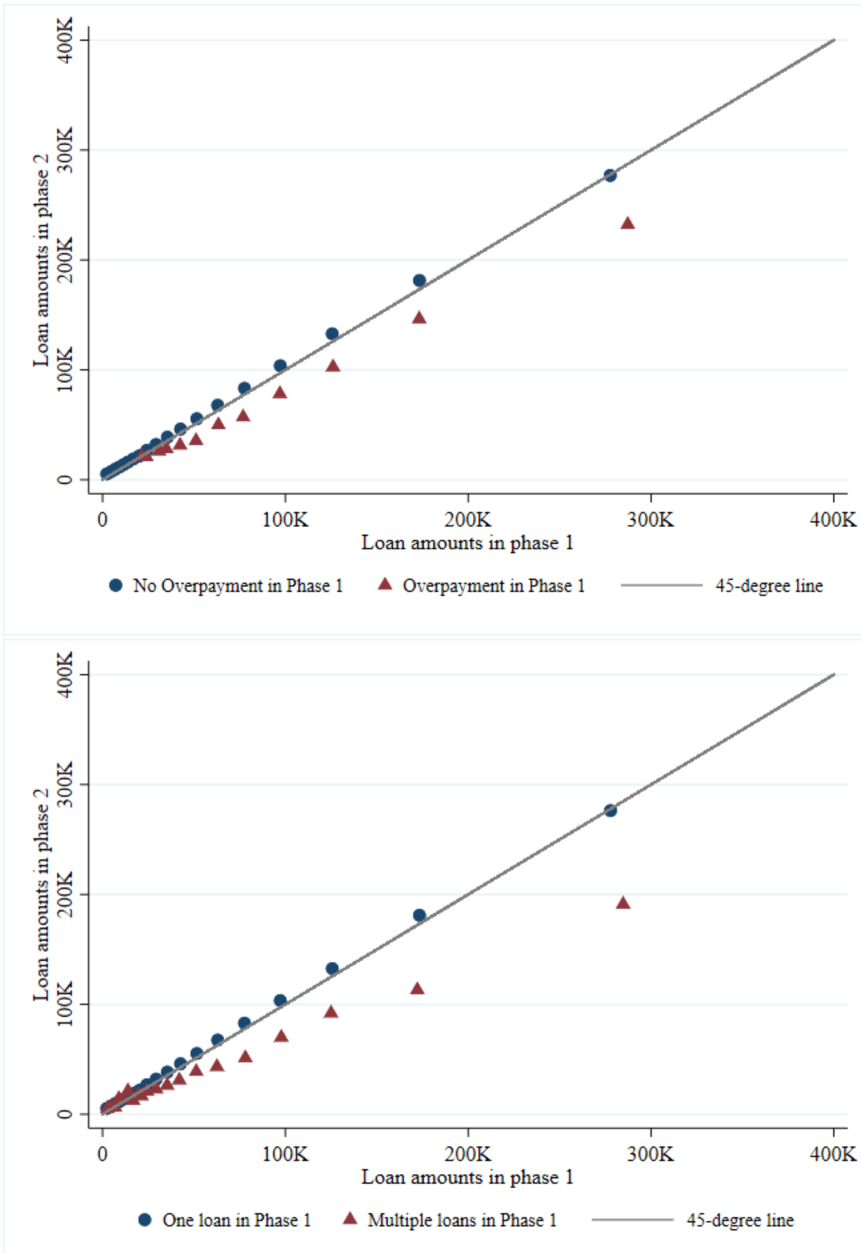
Table 3: Are suspect firms more likely to bunch when faced with upfront documentation requirements?

Dependent Variable:	**Likelihood of Bunching in Phase 2**		
	(1)	(2)	(3)
Whether Overpaid in Phase 1	0.0098***		
	(0.0010)		
Overpayment Rate in Phase 1		0.0021***	
		(0.00069)	
Whether received Multiple Loans in Phase 1			0.0016
			(0.0016)
Control mean of outcome	0.0055	0.0055	0.0055
Observations	1494052	1494052	1494052

Note: The unit of observation is at the firm level. The sample is restricted to firms that received loans in both phase 1 and phase 2. Bunching in phase 2 is an indicator variable equal to 1 for those second time borrowing firms that had PPP loans of greater than $150K in phase 1, but who 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. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. Overpayment rate is the overpaid amount divided by the maximum payment that a firm was eligible for, as measured in phase 1. 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 phase 1, and 0 otherwise. Standard errors are clustered at the firm-level. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

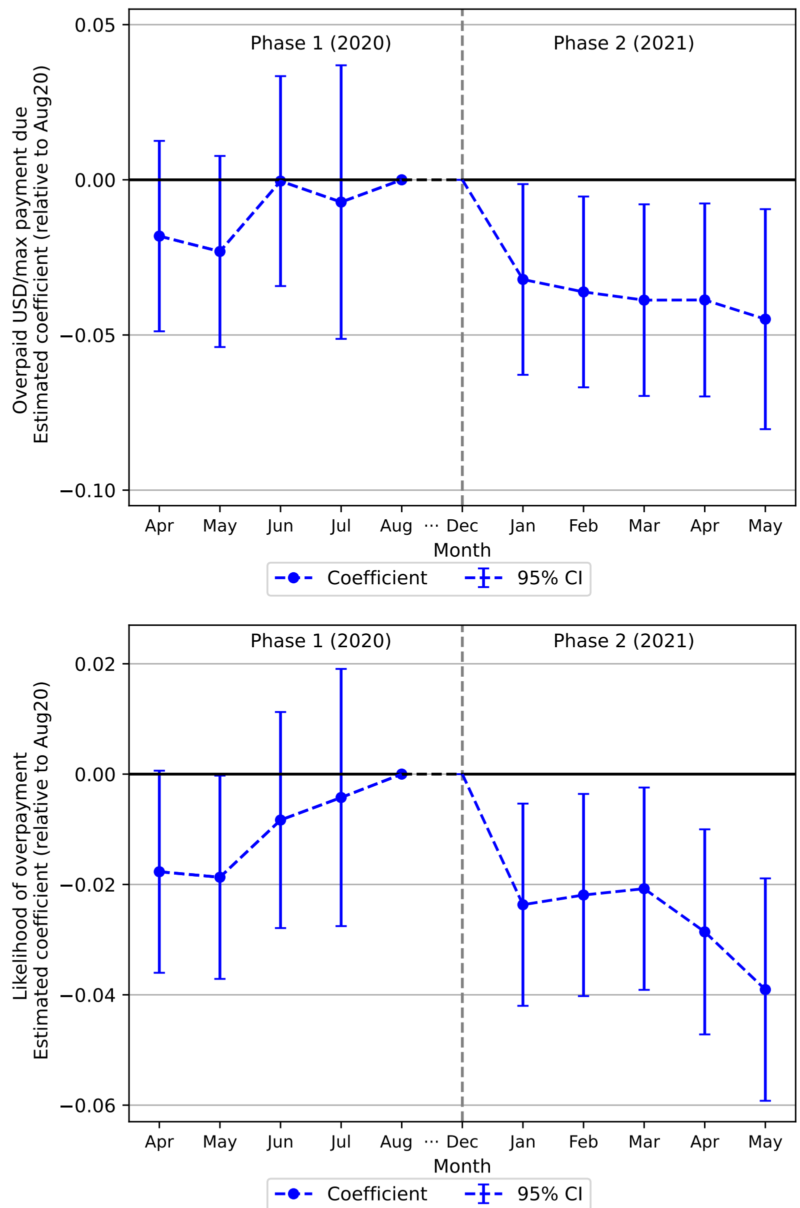
Figures

Figure 1: Loan amounts across phases of the program by firm type



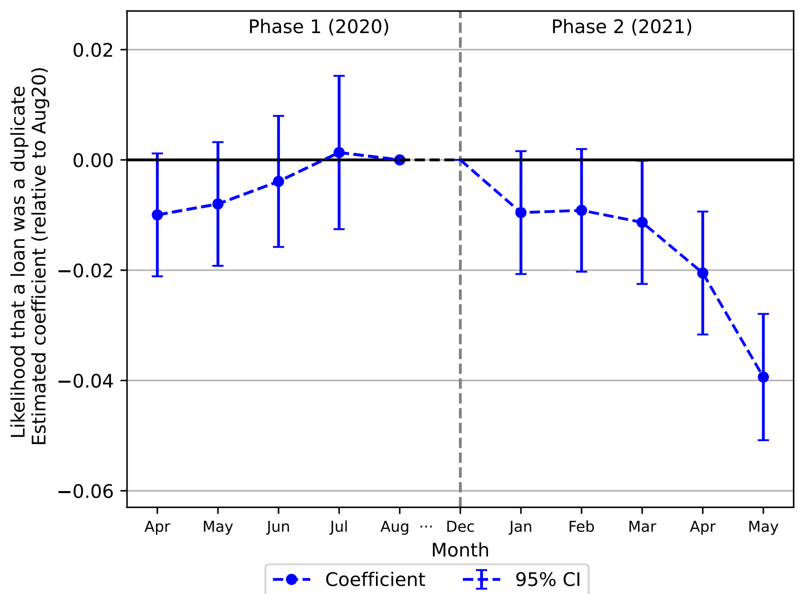
Notes. The top panel categorises firms with loan irregularities as those that had an overpayment in phase 1, while firms with no irregularities are those that had no overpayments. The bottom panel defines firms with loan irregularities as those that had multiple loans in phase 1, while firms with no irregularities are those with just one loan in phase 1.

Figure 2: Event study plot for the overpaid amount (in USD) as a fraction of maximum payment due per firm (top panel) and the likelihood of overpayment (bottom panel)



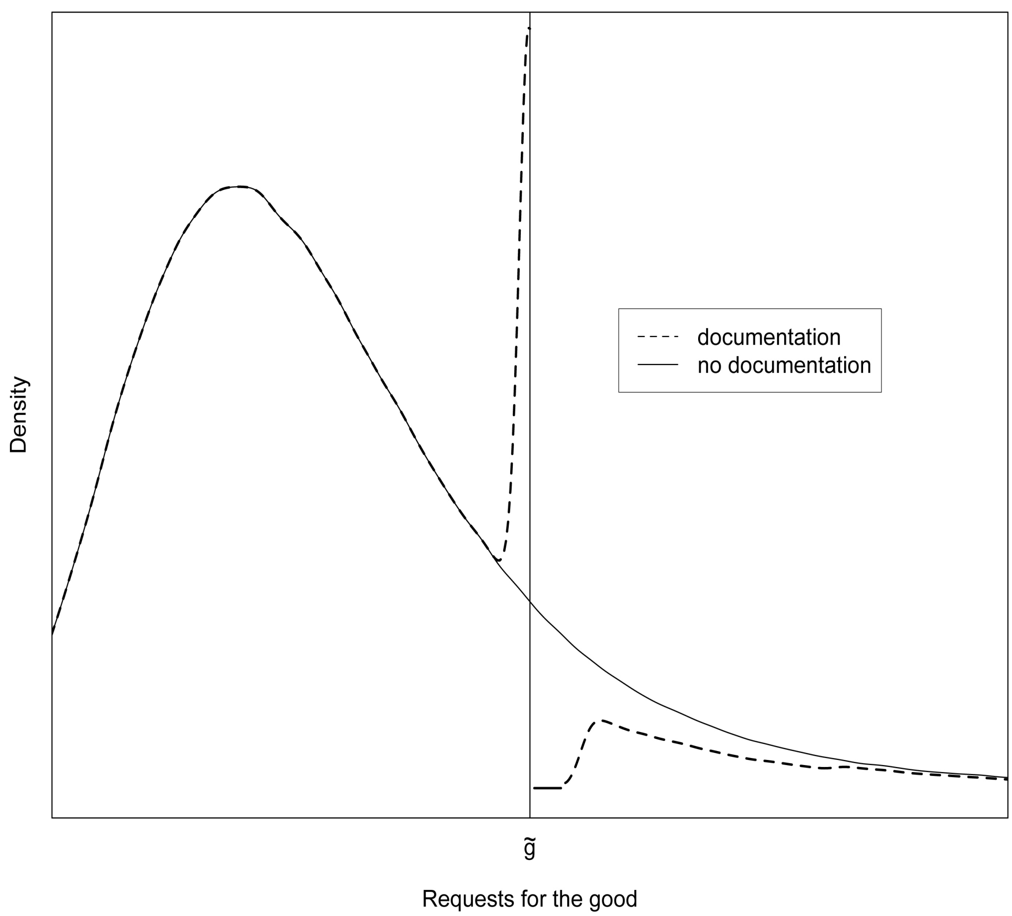
Notes. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1. The treatment group consists of those firms with at least one loan greater than $150,000 in phase 1 and that were exposed to the upfront documentation requirements.

Figure 3: Event study plot for the probability that a loan was a duplicate



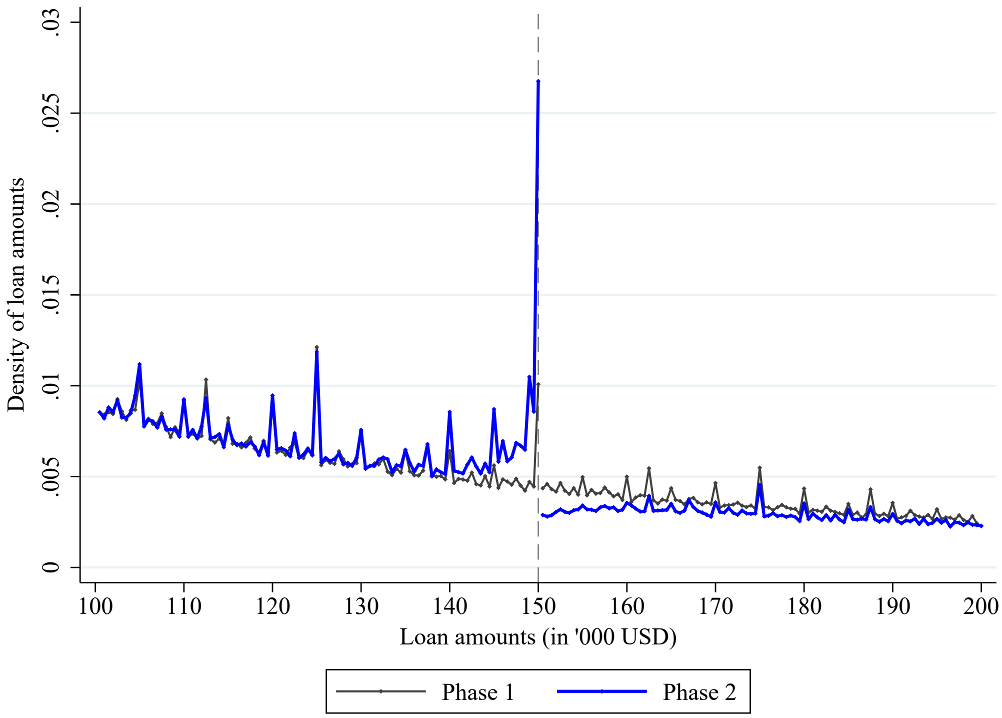
Notes. A duplicate loan is defined as any loan that is not the first loan issued to a firm. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1.The treatment group consists of those firms with at least one loan greater than $150,000 in phase 1 and that were exposed to the upfront documentation requirements.

Figure 4: The theoretical impact of the advanced documentation requirement on the density of all requests for the good from the program



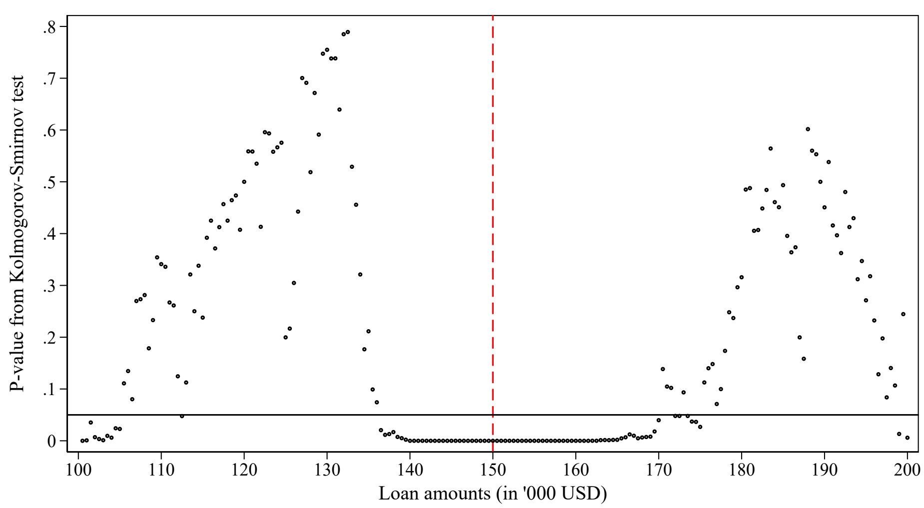
Notes. This graph was created by parameterizing the model as follows: $v(x) = \ln(x)$, $\pi(x) = x^2/2$, $\rho = 1/2$, $\tau = 1$, $\phi = 1/1000$, and F_1, F_0 are truncated normal densities with mean 5 and support [1,9].

Figure 5: Density of loan amounts across the two phases of PPP program



Notes. Phase 2 borrowers include both first and second time borrowers. The vertical dashed line represents the $150,000 threshold. Bin width is $500.

Figure 6: P-values from the Kolmogorov–Smirnov test to identify the bunching interval



Notes. The vertical red line denotes the $150,000 threshold. The horizontal black line marks the point where p-value equals 0.05.

Appendices

A Appendix tables

Table A.1: 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	5,128,185	1,494,052	8,256	1,485,796

Notes: The table summarizes the characteristics of all firms that took out loans. The unit of observation is the firm. "Bunchers" are firms that had PPP loans of greater than $150K in phase 1, but who chose to get PPP loans between $136-150k in phase 2. "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. Business size is the maximum number of employees a firm reported on its PPP applications. Information about registration dates is from the OC dataset.

Table A.2: Summary statistics of loan amounts and irregularities

	Phase 1	Phase 2
Approved amount per loan	101,590	42,748
	(348,642)	(141,715)
Share of overpaid loans	0.010	0.003
	(0.10)	(0.06)
Overpaid USD per loan	725.80	91.80
	(31,129)	(7,858)
Overpaid USD per overpaid loan	75,368	29,481
	(308,220)	(137,714)
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
	(28,260)	(4,789)
Share of firms with multiple loans	0.002	0.002
	(0.04)	(0.04)
Number of loans	5,136,454	6,338,537

Notes: The table shows the mean value. Standard deviation is in parentheses.

Table A.3: Did screening affect fraud in PPP loans? (standard errors clustered at the lender location level)

Dependent Variable:	**Overpayment dummy**		**Overpayment rate**		**Multiple loans dummy**	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed firms	0.013***		0.019***		-0.000041	
	(0.00088)		(0.0022)		(0.00028)	
Phase 2	-0.0029***	-0.0029***	-0.0021***	-0.0021***	0.00060*	0.00060
	(0.00030)	(0.00039)	(0.00017)	(0.00022)	(0.00035)	(0.00037)
Exposed firms × Phase 2	-0.0053***	-0.0053***	-0.017***	-0.017***	-0.0015***	-0.0015***
	(0.00070)	(0.00093)	(0.0021)	(0.0029)	(0.00032)	(0.00037)
Control mean of outcome	0.00775	0.00775	0.00323	0.00323	0.00178	0.00178
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. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2. Phase 2 is a dummy variable that takes a value of 1 for phase 2 of the PPP, remains zero otherwise. Standard errors are clustered at the lender location level. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.4: Did screening affect fraud through the extensive or intensive margin? (standard errors clustered at the lender location level)

(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)
Exposed firms	-0.032***	0.91***	140850.2***	-4.39***	10.1
	(0.0056)	(0.0029)	(3481.6)	(1.12)	(53.4)
Overpaid in phase 1	0.014**	-0.0048***	-7398.3***	2.38***	-22372.9***
	(0.0057)	(0.0016)	(767.3)	(0.35)	(519.9)
Exposed firms × Overpaid in Phase 1	-0.037***	-0.11***	-59343.9***	20.6***	-18639.6***
	(0.012)	(0.015)	(8762.5)	(1.29)	(2539.3)
Control mean of outcome	0.713	0.0165	40273.7	-0.443	844.8
Observations	5128176	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)
Exposed firms	-0.033***	0.91***	140061.0***	-3.93***	-686.1***
	(0.0056)	(0.0032)	(3480.8)	(1.11)	(92.4)
Multiple loans in phase 1	-0.042***	-0.0051**	2493.6	0.44	945.9***
	(0.011)	(0.0023)	(2556.0)	(0.37)	(110.8)
Exposed firms × Multiple loans in Phase 1	-0.0092	-0.14***	-28115.8**	3.47*	-740.2*
	(0.017)	(0.022)	(14203.1)	(1.91)	(418.5)
Control mean of outcome	0.713	0.0165	40326.6	-0.425	669.8
Observations	5128176	1494052	1494052	1494052	1494052
Fixed effects	No	No	No	No	No
Phase 1 control	No	No	Yes	No	No

Note: The unit of observation is the firm. For Columns (2)-(5) 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 1 values from phase 2 values to get an across-phase difference for each firm. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2. Overpaid in phase 1 is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. 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 phase 1, 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.

Table A.5: Are bunching firms disproportionately composed of bad actors? (standard errors clustered at the lender location level)

Dependent Variable:	**Likelihood of Bunching in Phase 2**		
	(1)	(2)	(3)
Whether Overpaid in Phase 1	0.0098***		
	(0.0012)		
Overpayment Rate in Phase 1		0.0021***	
		(0.00075)	
Whether received Multiple Loans in Phase 1			0.0016
			(0.0016)
Control mean of outcome	0.0055	0.0055	0.0055
Observations	1494052	1494052	1494052

Note: The unit of observation is at the firm level. The sample is restricted to firms that received loans in both phase 1 and phase 2. Bunching in phase 2 is an indicator variable equal to 1 for those second time borrowing firms that had PPP loans of greater than $150K in phase 1, but who 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. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. Overpayment rate is the overpaid amount divided by the maximum payment that a firm was eligible for. 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 phase 1, and 0 otherwise. Standard errors are clustered at the lender location level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.6: 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)
Exposed firms	0.0028***		0.0024***		-0.0028***	
	(0.00062)		(0.00093)		(0.00019)	
Phase 2	-0.0052***	-0.0052***	-0.0078***	-0.0078***	-0.0023***	-0.0023***
	(0.00038)	(0.00053)	(0.00046)	(0.00065)	(0.00016)	(0.00023)
Exposed firms × Phase 2	-0.0017**	-0.0017*	-0.0029***	-0.0029**	0.0022***	0.0022***
	(0.00069)	(0.00097)	(0.00095)	(0.0013)	(0.00019)	(0.00026)
Control mean of outcome	0.012	0.012	0.010	0.010	0.0032	0.0032
Observations	323316	323316	323316	323316	323316	323316
Firm FE	No	Yes	No	Yes	No	Yes

Note: 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 phase 1 loans between $100-200K bracket. 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. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2. Phase 2 is a dummy variable that takes a value of 1 for phase 2 of the PPP, remains zero otherwise. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

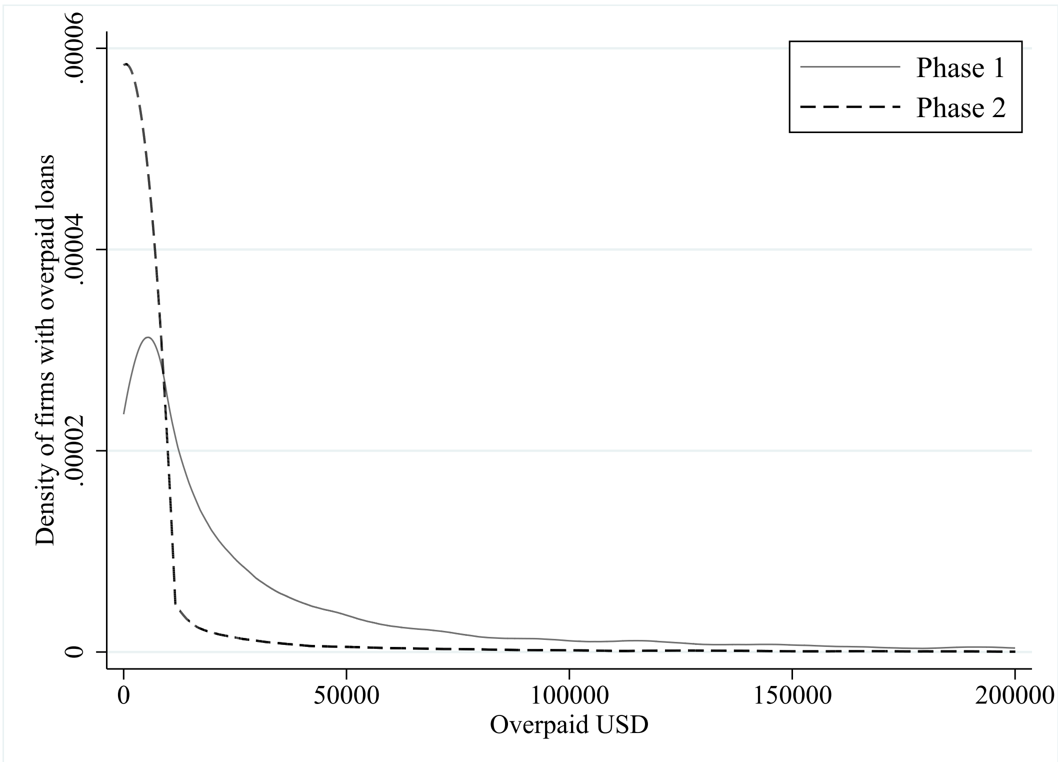
Table A.7: Did the screening requirement affect overpaid amounts?

Sample used:	All firms with loans in phase 1 and 2		Firms with loans between $100 - 200K$ in phase 1	
Dependent Variable:	**Overpayment amount**		**Overpayment amount**	
	(1)	(2)	(3)	(4)
Exposed firms	4549.5***		280.0***	
	(174.8)		(38.76)	
Phase 2	-69.59***	-69.59***	-318.7***	-318.7***
	(2.208)	(3.123)	(18.96)	(26.81)
Exposed firms × Phase 2	-3551.3***	-3551.3***	-224.9***	-224.9***
	(175.9)	(248.8)	(43.51)	(61.53)
Control mean of outcome	120.0	120.0	482.1	482.1
Observations	2988104	2988104	323316	323316
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 a firm-phase. Overpaid amount on a loan is the approved dollar value less the maximum payment due as per rules. 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. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2. Phase 2 is a dummy variable that takes a value of 1 for phase 2 of the PPP, remains zero otherwise. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

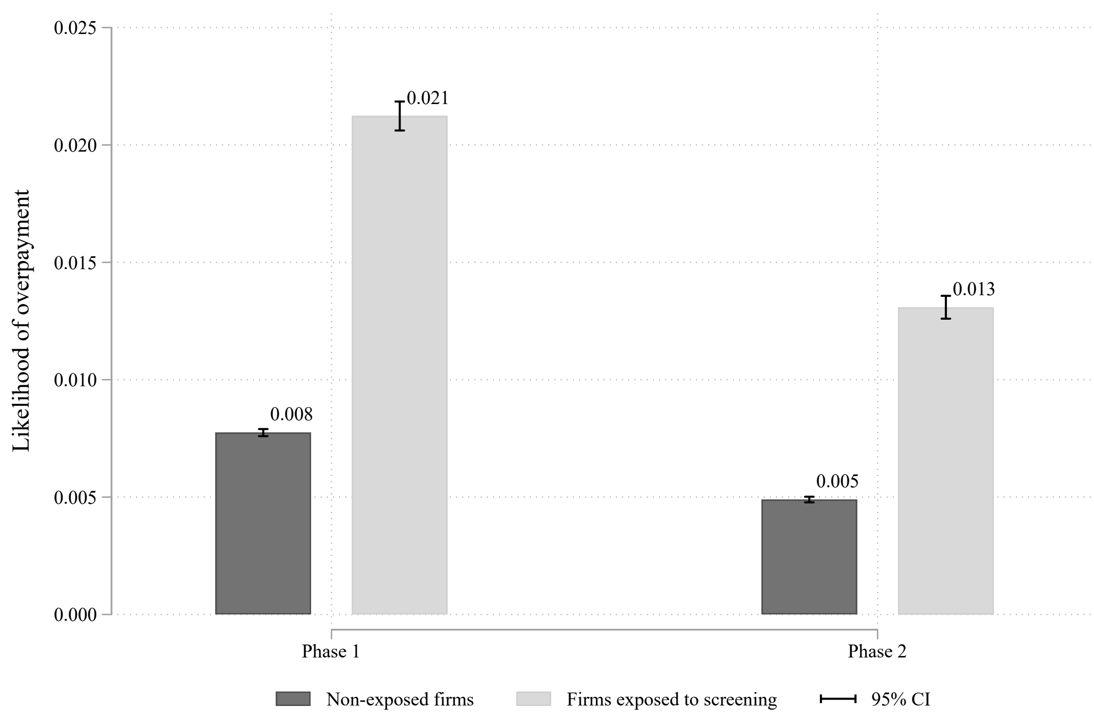
B Appendix figures

Figure B.1: The distribution of overpaid amounts issued to borrowers with overpayments in phase 1 and who reapplied in phase 2



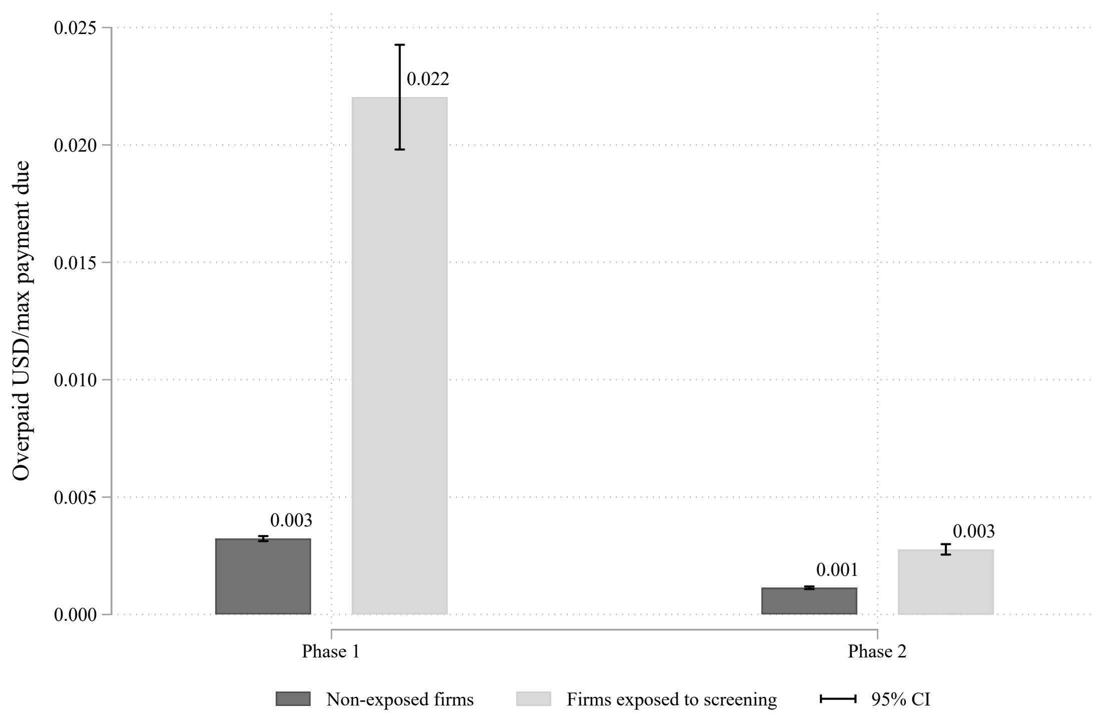
Notes. Data is at the firm-phase level, restricted to firms that applied in both phases and had at least one overpayment in phase 1. Overpaid amount on a loan is the approved dollar value less the maximum payment due. For each firm that took out more than one loan in a phase, the overpaid amount is the 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 B.2: Probability that a firm was overpaid by treatment status



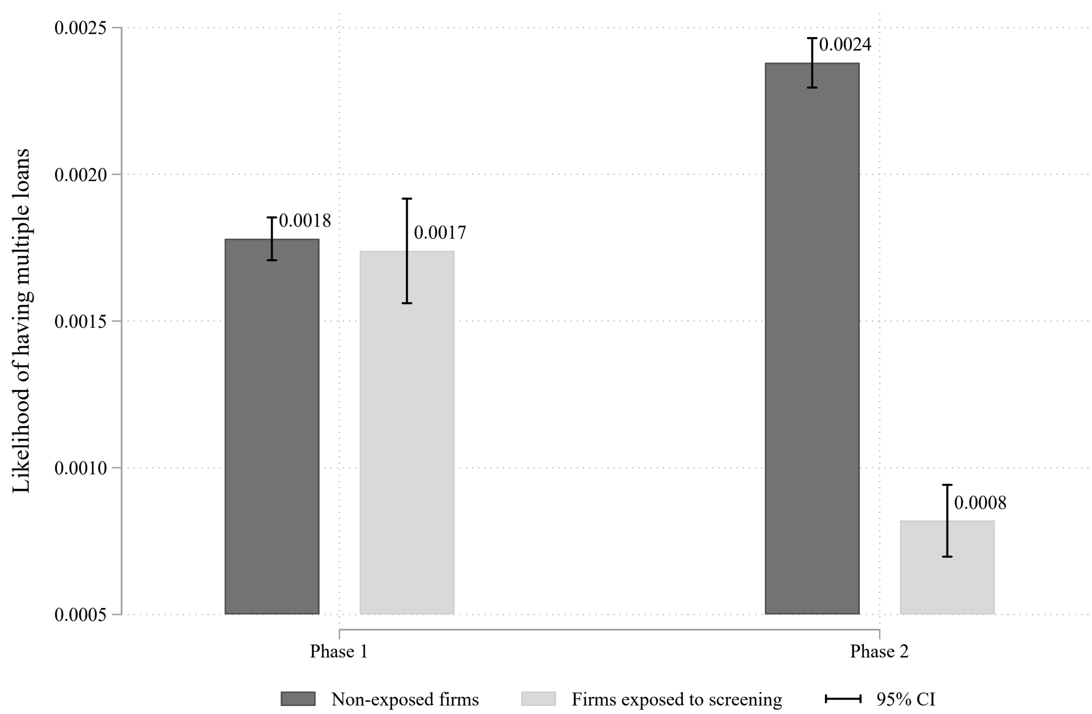
Notes. Data is at the firm-phase level. The sample is restricted to firms that took out loans in both phase 1 and phase 2.

Figure B.3: Overpaid amount (in USD) as a fraction of maximum payment by treatment status



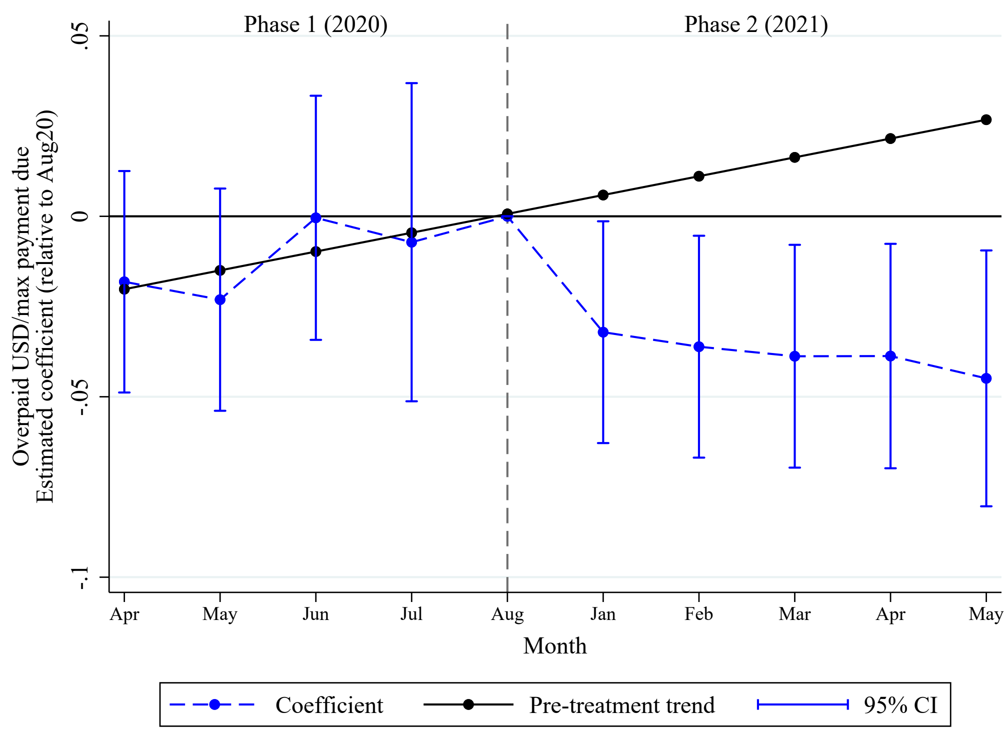
Notes. Data is at the firm-phase level. For firms with multiple overpaid loans in a phase, the overpayment rate plotted is the maximum rate among all those loans. The sample is restricted to firms that took out loans in both phase 1 and phase 2.

Figure B.4: Probability that a firm had multiple loans by treatment status



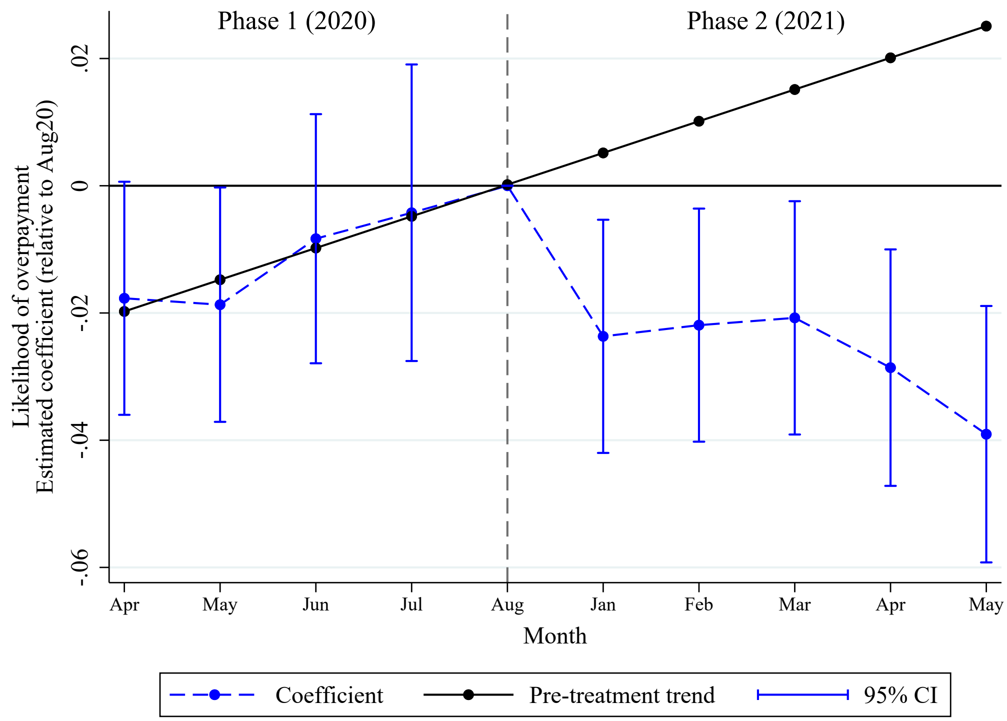
Notes. Data is at the firm-phase level. The sample is restricted to firms that took out loans in both phase 1 and phase 2.

Figure B.5: Event study plot for the overpaid amount (in USD) as a fraction of maximum payment due per firm



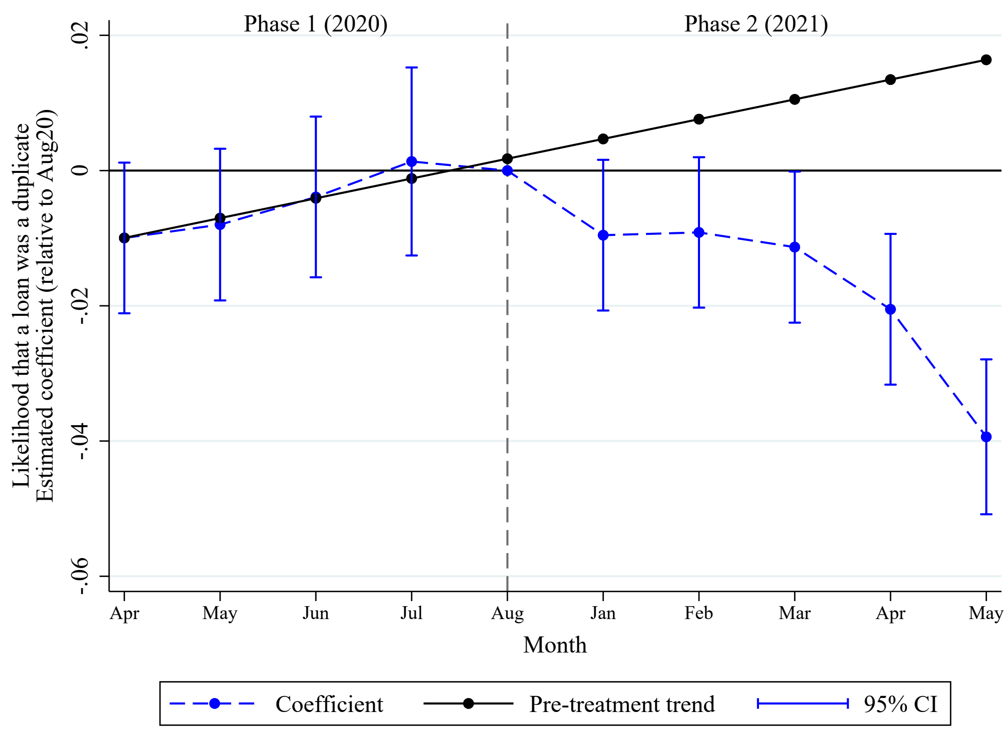
Notes. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1. The treatment group consists of those firms with at least one loan greater than $150,000 in phase 1 and that were exposed to the upfront documentation requirements. The trend line connects predicted monthly outcomes generated from regressing the pre-treatment coefficients on a linear term of months.

Figure B.6: Event study plot for the probability that a firm was overpaid



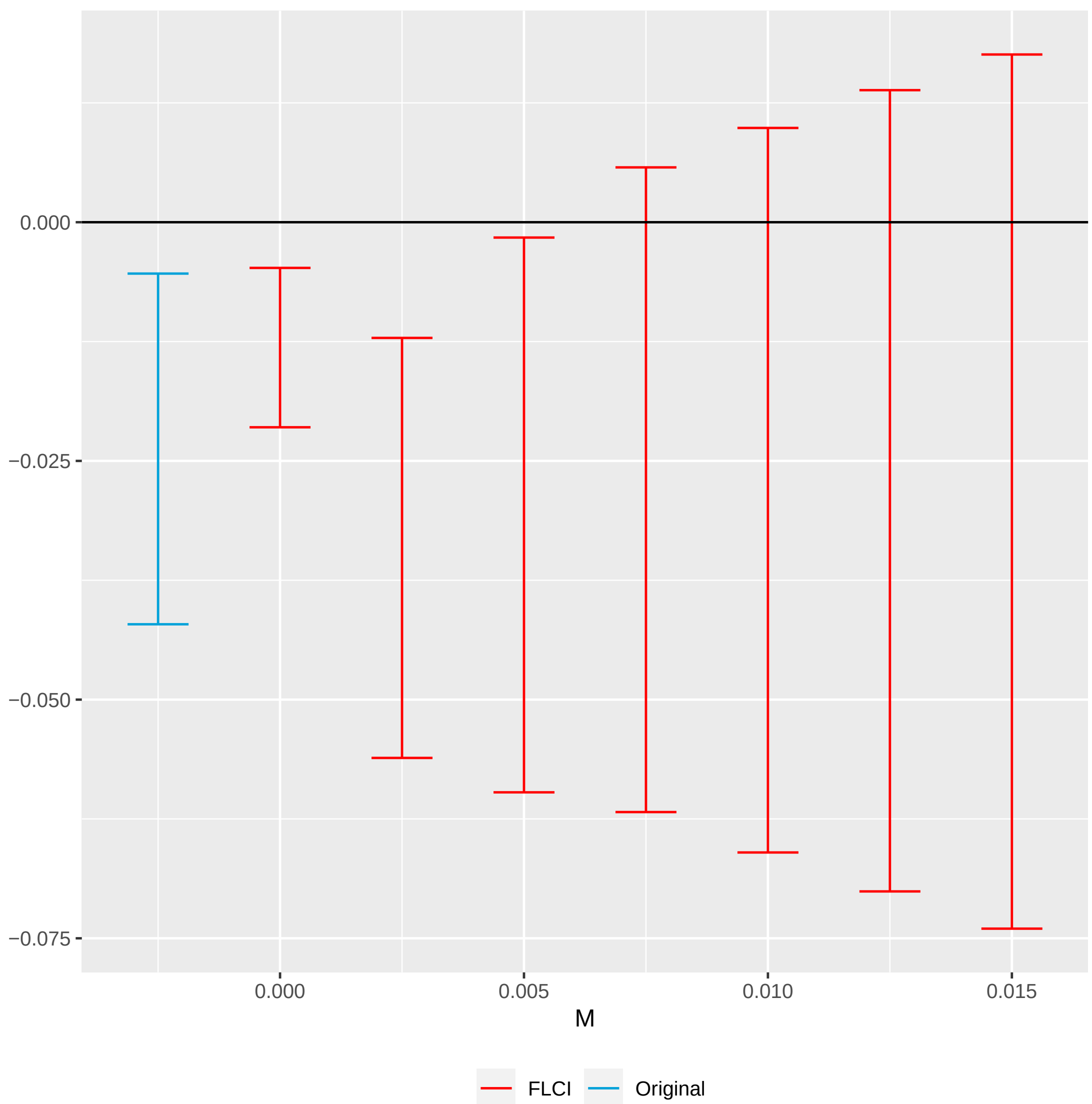
Notes. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1. The treatment group consists of those firms with at least one loan greater than $150,000 in phase 1 and that were exposed to the upfront documentation requirements. The trend line connects predicted monthly outcomes generated from regressing the pre-treatment coefficients on a linear term of months.

Figure B.7: Event study plot for the probability that a loan was a duplicate



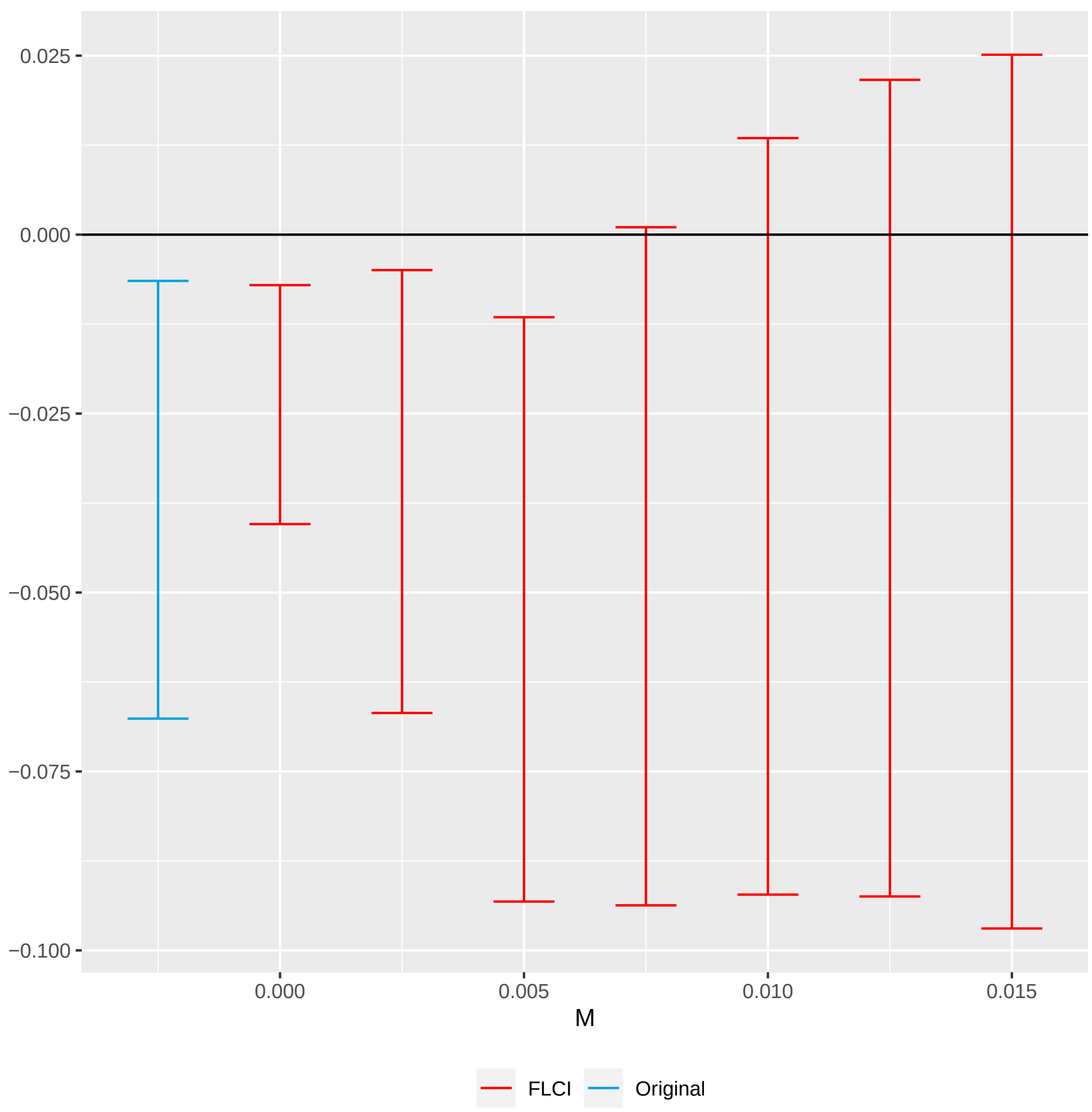
Notes. A duplicate loan is defined as any loan that is not the first loan issued to a firm. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1. The treatment group consists of those firms with at least one loan greater than $150,000 in phase 1 and that were exposed to the upfront documentation requirements. The trend line connects predicted monthly outcomes generated from regressing the pre-treatment coefficients on a linear term of months.

Figure B.8: Effects on the probability that a firm was overpaid - Robustness of the DID estimates using the "more credible approach" suggested by (Rambachan and Roth, 2023)



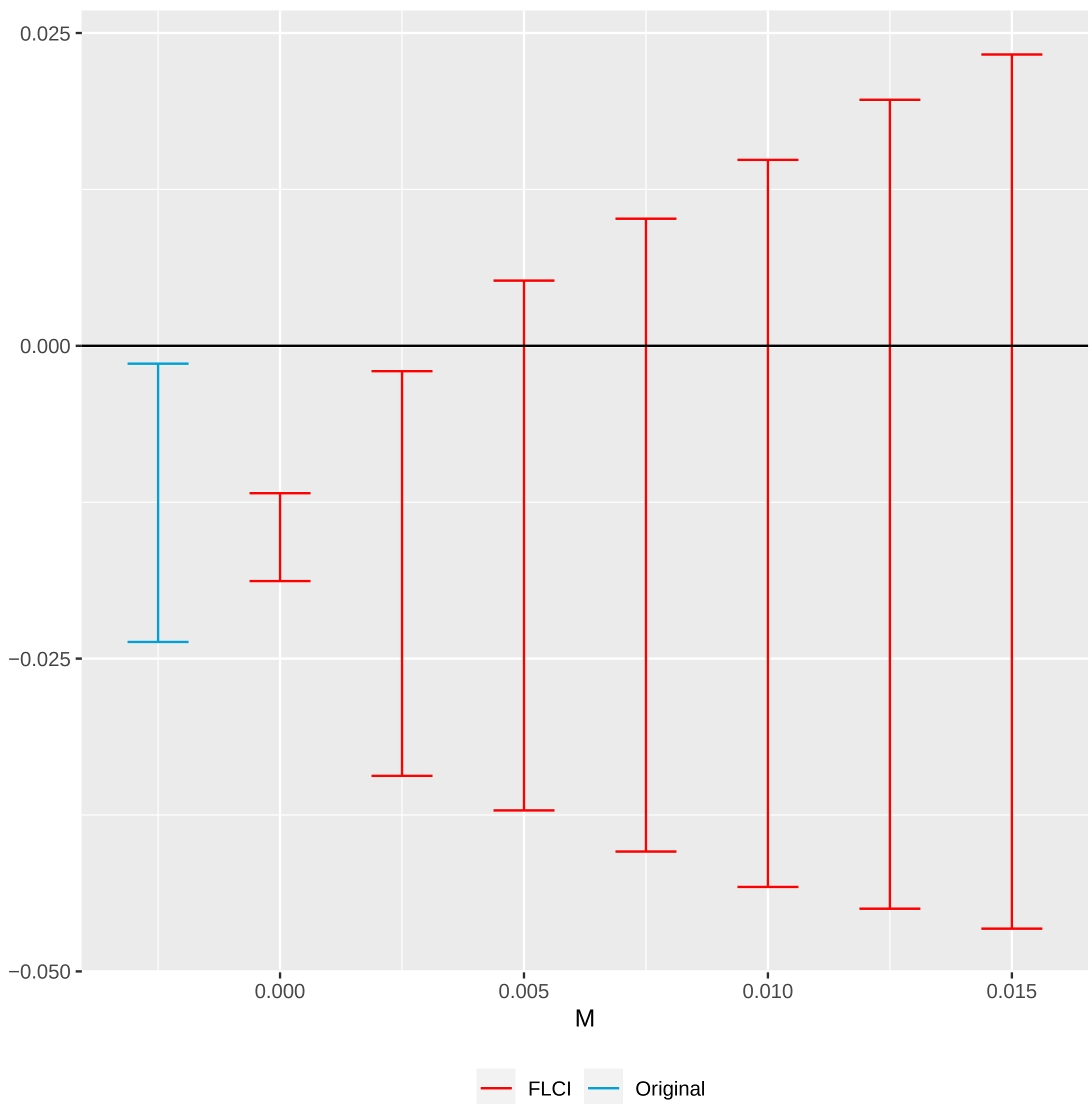
Notes. FLCI refers to fixed length confidence intervals. The blue line is the confidence intervals for the coefficient obtained from the interaction term between treatment and phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that in this version, only for phase 2 we let the treatment indicator interact with phase 2 instead with individual post-treatment months. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2.

Figure B.9: Effects on the overpaid amount (in USD) as a fraction of maximum payment due per firm - Robustness of the DID estimates using the "more credible approach" suggested by (Rambachan and Roth, 2023)



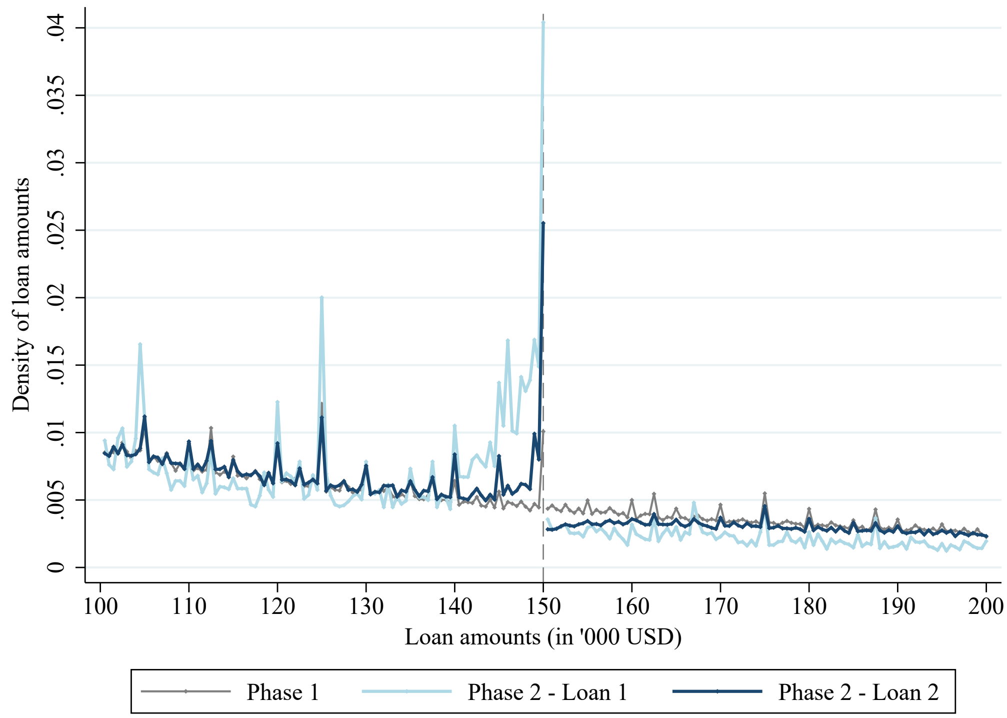
Notes. FLCI refers to fixed length confidence intervals. The blue line is the confidence intervals for the coefficient obtained from the interaction term between treatment and phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that in this version, only for phase 2 we let the treatment indicator interact with phase 2 instead with individual post-treatment months. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2.

Figure B.10: Effects on the probability that a loan was a duplicate - Robustness of the DID estimates using the "more credible approach" suggested by (Rambachan and Roth, 2023)



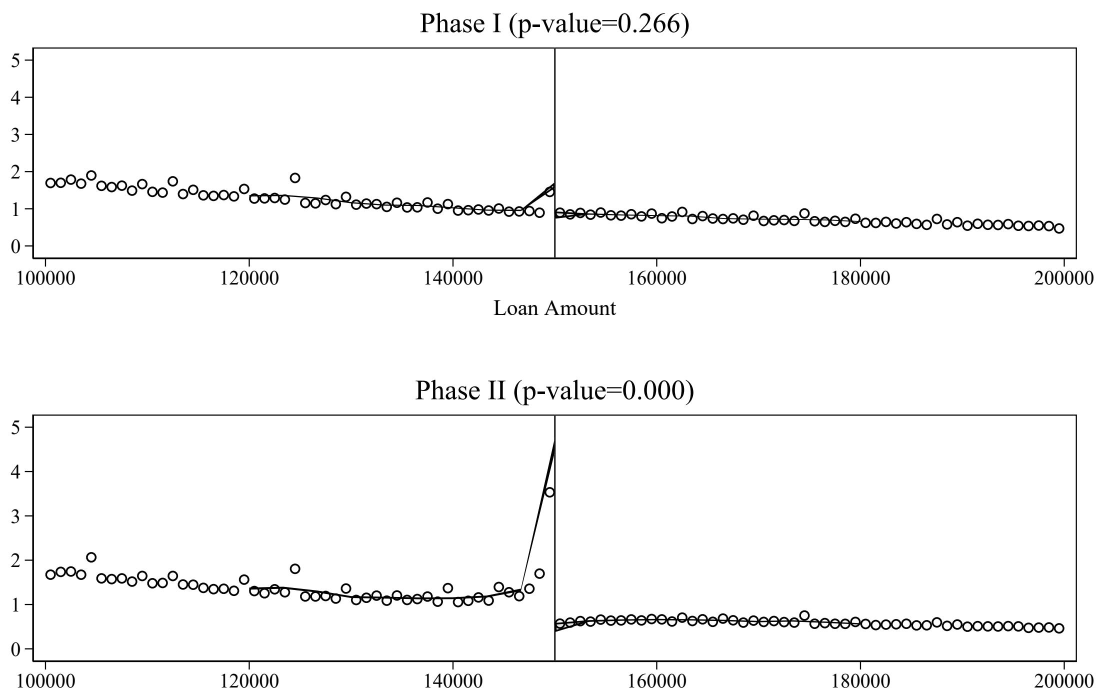
Notes. FLCI refers to fixed length confidence intervals. The blue line is the confidence intervals for the coefficient obtained from the interaction term between treatment and phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that in this version, only for phase 2 we let the treatment indicator interact with phase 2 instead with individual post-treatment months. Exposed firms are those with at least one loan greater than $150,000 in phase 1 and those that were exposed to the advance documentation requirement in phase 2.

Figure B.11: Density of loan amounts across the two phases of the PPP



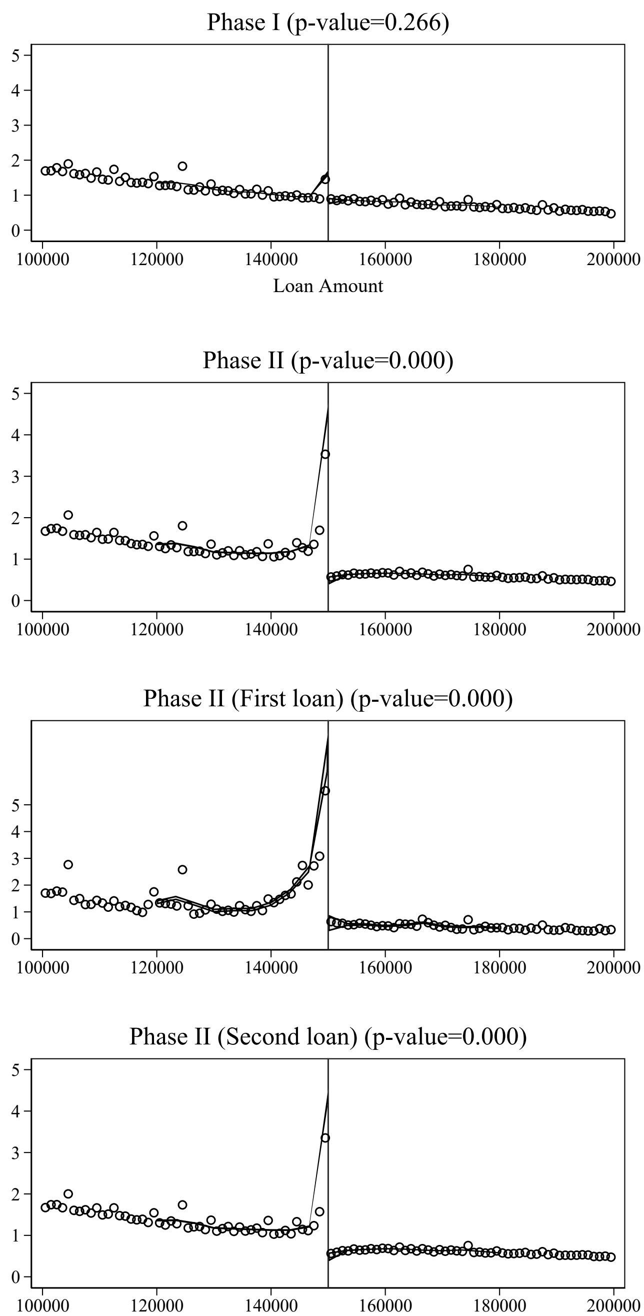
Notes. Loan 1 and loan 2 refers to whether a firm took a PPP loan for the first or second time. The vertical dashed line represents the $150,000 threshold. Bin width is $500.

Figure B.12: Distribution of approved loan amounts in phase I and phase II of PPP



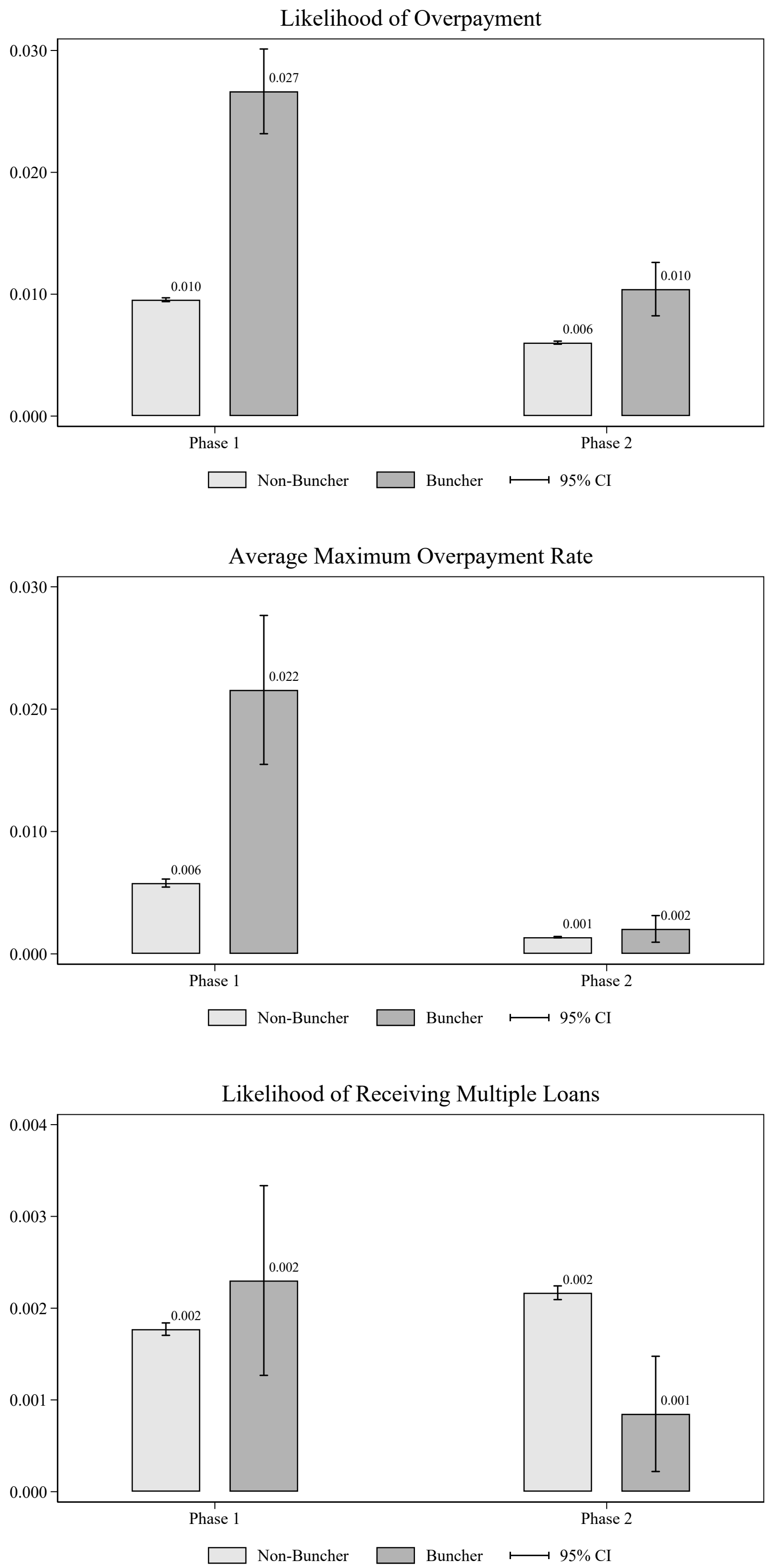
Notes. The *p*-value is from a McCrary Density Test (McCrary, 2008) of continuity of densities at the $150K cut-off

Figure B.13: Distribution of approved loan amounts in phase I and phase II of the PPP



Notes. The *p*-value is from a McCrary Density Test (McCrary, 2008) of continuity of densities at the $150K cut-off

Figure B.14: The behavior of bunching and non-bunching firms across phases



C Further Details on Firm Matching Across Phases

C.1 String matching to create a unique firm level identifier and identification of multiple loans to a firm in a given phase

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

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

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

3. Within each state and first or second time loans, we then string matched borrowers first on names using a similarity score cut-off of 0.9. For the subset of borrowers whose names were matched, we then string matched their addresses with other borrowers.[47] We used a similarity score cut-off of 0.7.[48] 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 the previous step. This helped us in creating a unique firm level identifier across both phases of the PPP.

Following this algorithm we were able to create a unique firm-level identifier for approximately 70% of all second time loans.[49] To verify the accuracy of these matches, we randomly sampled 1000 firms out of 2078901 total firms.[50] 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.

C.2 Geocode matching to create a unique identifier for unmatched firms from draw 2 of phase 2

The string matching exercise yielded unique firm-level identifiers for 72.21% of all second time loans. For the remaining 27.79% of second time loans which could not be matched to any first time loan, we adopted a new approach to find their first loan match. In this new

[47]Computationally the address matching was the more difficult part since the strings were long and borrowers were less consistent in their addresses than they were in their names.

[48]We used a lower cut-off for addresses as the data showed that borrowers were more prone to writing their addresses differently as opposed to their names. For instance, in one application they might write their address including their apartment number but not in the other.

[49]We were conservative in our matching algorithm. If a firm applied with either a different name in the two draws (for example personal name in the first draw and business name in the second draw) or if they applied with the same name but a different address in the two draws we dropped these firms from the analysis.

[50]Approximately 20 firms were chosen from each state.

algorithm, we replaced the string matching on firm addresses with geocode matching. Below we detail the steps undertaken.

1. For the 27.79% of unmatched second-time loans, we began with string matching on borrower's name with a similarity score cut-off of 0.9. For approximately 36% of this unmatched sample we could identify a first-time loan applicant with a similar name.

2. For this subsample of second-time applicants and their name-matches, we used the US Census Geocoder[51] to convert the loan applicant's address to geocoordinates, i.e., latitudes and longitudes. We obtained geocoordinates for approximately 55% of the subsample.

3. Using data on latitudes and longitudes, we computed the distance between the unmatched second loan applicant's address and all of its name matches obtained in (1), retaining the nearest match (the one with least distance).

4. We consider a pair of second-time unmatched loan applicant and its nearest location match from the set of first-time loan applicants as the same firm if the distance between their addresses is less than 1 kilometer.

The geocode algorithm helped us identify an additional sample of 39,301 firms which is roughly 5% of the unmatched second-time loans based on the 1 kilometer cut-off. We examined several cut-offs in the range of 50 metres (most conservative) to 5 kilometers (least conservative). The proportion of unmatched second-time loans we could identify by varying the distance threshold from most to least conservative ranged between 3.6% to 9%, however after manual checks, a threshold of 1 kilometer was ascertained best for this robustness analysis.

C.3 Implications of the string matching exercise for the classification of firm exit from PPP

A potential sample selection problem can arise from the string matching exercise we carried out to track firms' loan taking behavior across the two phases of the program. The algorithm allowed us to match 72.3% of the second time loans in phase 2 to their first time loans in phase 1. This means that for 27.7% of phase 1 firms in Table 2a and Table 2b "Exit from the program" is classified as one when in fact it is zero, implying that the probability of Type-I error (false positives) is 0.277.

We investigated this and addressed the issue in two ways. First, as described in subsection C.2, for the subset of firms that were matched on names across phase 1 and 2, instead of string matching their addresses, we used the US Census Geocoder to convert the addresses to geocoordinates and carry out a geocode matching. Tables C.1-C.3 present the results including this sample. The results are substantively identical to the main results presented in Tables 1-3.

[51]The US Census Geocoder is tool created by the United States Census Bureau that helps with converting physical addresses to geographic locations and vice-versa. It can be found here: https://geocoding.geo.census.gov/geocoder/

We next followed the methodology suggested by Hausman et al. (1998) for consistently estimating effects when the dependent variable has misclassification error.[52] Let Exit* be the true exit of firms from phase 2 of the program. We know from the matching exercise that Type-I error or the probability of false positives is $P(Exit = 1|Exit^* = 0) = 0.277$. Manual checks of the accuracy of the matches revealed that the probability of a Type-II error (false negative) is $P(Exit = 0|Exit^* = 1) = 0.002$. Let us define $\alpha_0 = P(Exit = 1|Exit^* = 0)$ and $\alpha_1 = P(Exit = 0|Exit^* = 1)$. For a cross-section of firms in phase 2 of the PPP program then:

$$\begin{aligned} E(Exit|X) &= P(Exit|X) \\ &= P(Exit^* = 1|X)P(Exit = 1|Exit^* = 1) + P(Exit^* = 0|X)P(Exit = 1|Exit^* = 0) \\ &= F(X'\beta)(1 - \alpha_1) + (1 - F(X'\beta))\alpha_0 = \alpha_0 + (1 - \alpha_0 - \alpha_1)F(X'\beta) \end{aligned} \tag{14}$$

where X includes a dummy for exposed firms, F^0 which is a dummy variable that 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, and an interaction of exposed firms and F^0 as in Equation 12. We can estimate Equation 14 by using maximum likelihood estimation. Following Hausman et al. (1998), we assume the errors are standard normally distributed. Results are presented in Tables C.4a and C.4b. Results produce coefficients of the same sign and significance as the main results in Table 2a and 2b.

[52]This method has been applied to various empirical issues, including patents (Palangkaraya et al., 2011), language indicators (Dustmann and Van Soest, 2002), education (Caudill and Mixon Jr, 2005) and smoking (Kenkel et al., 2004).

Table C.1: Robustness to Location Matches: Did screening affect fraud in PPP loans?

Dependent Variable:	**Overpayment dummy**		**Overpayment rate**		**Multiple loans dummy**	
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed firms	0.014***		0.019***		-0.000033	
	(0.00032)		(0.0012)		(0.000096)	
Phase 2	-0.0028***	-0.0028***	-0.0021***	-0.0021***	0.00059***	0.00059***
	(0.000085)	(0.000085)	(0.000058)	(0.000058)	(0.000051)	(0.000051)
Exposed firms × Phase 2	-0.0053***	-0.0053***	-0.017***	-0.017***	-0.0015***	-0.0015***
	(0.00036)	(0.00036)	(0.0012)	(0.0012)	(0.000097)	(0.000097)
Control mean of outcome	0.0077	0.0077	0.0032	0.0032	0.0017	0.0017
Observations	3064578	3064578	3064578	3064578	3064578	3064578
Firm FE	No	Yes	No	Yes	No	Yes

Note: The table shows how the advanced documentation requirement affects the main indicators of fraud after inclusion of an additional subsample of firms identified by the geocoding algorithm. 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. Exposed firms consists of firms with a loan greater than $150,000 in phase 1 and were exposed to the upfront documentation requirement. Phase 2 is a dummy variable that takes a value of 1 for phase 2 of the PPP, remains zero otherwise. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table C.2: Robustness to Location Matches: 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)
Exposed firms	-0.033***	0.91***	141308.5***	-4.39***	17.1
	(0.00062)	(0.00058)	(2350.5)	(0.081)	(11.4)
Overpaid in phase 1	0.015***	-0.0048***	-7241.1***	2.38***	-22387.4***
	(0.0024)	(0.0011)	(322.1)	(0.070)	(204.8)
Exposed firms × Overpaid in Phase 1	-0.038***	-0.11***	-60231.6***	20.7***	-18888.7***
	(0.0048)	(0.0059)	(6065.2)	(0.59)	(1595.5)
Control mean of outcome	0.71	0.017	40276.3	-0.44	844.6
Observations	5128218	1532289	1532289	1532289	1532289
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)
Exposed firms	-0.034***	0.91***	140501.4***	-3.92***	-685.2***
	(0.00062)	(0.00058)	(2346.9)	(0.080)	(37.8)
Multiple loans in phase 1	-0.035***	-0.0051**	2536.3	0.44*	950.3***
	(0.0056)	(0.0022)	(3124.0)	(0.26)	(135.7)
Exposed firms × Multiple loans in Phase 1	-0.0097	-0.14***	-28579.7*	3.46*	-744.4
	(0.016)	(0.022)	(16632.3)	(1.87)	(492.6)
Control mean of outcome	0.71	0.017	40330.5	-0.43	669.5
Observations	5128218	1532289	1532289	1532289	1532289
Fixed effects	No	No	No	No	No
Phase 1 control	No	No	Yes	No	No

Note: The table shows robustness of the main results after inclusion of an additional subsample of firms identified by the geocoding algorithm. Panel (a) shows the behavior of firms with overpayment in phase 1, and Panel (b) shows the behavior of firms with multiple loans in phase 1. The unit of observation is at the firm level. For Columns (2)-(5) 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 1 values from phase 2 values to get an across-phase difference for each firm. Exposed firms consists of firms with a loan greater than $150,000 in phase 1 and were exposed to the upfront documentation requirement. Phase 2 is a dummy variable that takes a value of 1 for phase 2 of the PPP, remains zero otherwise. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table C.3: Robustness to Location Matches: Are suspect firms more likely to bunch when faced with upfront documentation requirements?

Dependent Variable:	**Likelihood of Bunching in Phase 2**		
	(1)	(2)	(3)
Whether Overpaid in Phase 1	0.0098***		
	(0.0010)		
Overpayment Rate in Phase 1		0.0020***	
		(0.00064)	
Whether received Multiple Loans in Phase 1			0.0017
			(0.0016)
Control mean of outcome	0.0055	0.0055	0.0055
Observations	1532289	1532289	1532289

Note: The unit of observation is at the firm level. The sample is restricted to firms that received loans in both phase 1 and phase 2. The table shows robustness of the main results after inclusion of an additional subsample of firms identified by the geocoding algorithm. Bunching in phase 2 is an indicator variable equal to 1 for those second time borrowing firms that had PPP loans of greater than $150K in phase 1, but who 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. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. Overpayment rate is the overpaid amount divided by the maximum payment that a firm was eligible for, as measured in phase 1. 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 phase 1, 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.

Table C.4: Addressing and adjusting for misclassification of firm exit from PPP using the methodology suggested by Hausman et al. (1998)

(a) By overpayment status of firms

Dependent Variable:	**Exited from Phase 2**
	(1)
Exposed firms	-0.11***
	(0.0022)
Overpaid in phase 1	0.051***
	(0.0085)
Exposed firms × Overpaid in phase 1	-0.13***
	(0.017)
Control mean of outcome	0.709
Observations	5128185

(b) By multiple loan approval status

Dependent Variable:	**Exited from Phase 2**
	(1)
Exposed firms	-0.11***
	(0.0021)
Multiple loans in phase 1	-0.15***
	(0.020)
Exposed firms × Multiple loans in phase 1	-0.029
	(0.056)
Control mean of outcome	0.709
Observations	5128185

Note: The unit of observation is at the firm level. 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. Exposed firms consists of firms with a loan greater than $150,000 in phase 1 and were exposed to the upfront documentation requirement. Overpaid in phase 1 is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. Multiple loans in phase 1 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 phase 1, and 0 otherwise. The probability of a false positive is 0.27 and the probability of a false negative is 0. The analysis was carried out using Stata's mrprobit command. Standard errors are in parenthesis. Computational constraints restricted the use of clustered standard errors. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

D Other measures of fraud

We conducted a cross-validation exercise to investigate how well the measures of fraud used in this paper correlate with additional measures of fraud presented in Griffin et al. (2023). We compared our measures to two measures utilized therein: abnormally high implied compensation per employee relative to the industry benchmark[53] and large inconsistencies in jobs reported with another government program, i.e., the COVID-19 Economic Injury Disaster Loan (EIDL) Advance.[54]

Table D.1a and Table D.1b presents the results: Panel (a) reports correlations of our measures with the implied compensation of employees in PPP as a fraction of average industry compensation reported by the United States Census Bureau, and Panel (b) presents correlations of our measures with discrepancies in job numbers reported in EIDL Advance and PPP. Following Griffin et al. (2023), the measures examined in this panel are: a dummy equal to 1 when the number of jobs reported in EIDL is greater than that reported in PPP, and a dummy equal to 1 when the difference in jobs reported is greater than or equal to 3. The tables show that our measures are strongly positively correlated with other measures that flag irregularities in PPP. This gives us added confidence that our measures of loan irregularities reflect—in the aggregate—fraudulent intent on the part of borrowers.

Nonetheless, given that our treatment is defined as receiving a loan equal to or greater than $150K in the first phase of the PPP, the measures of fraud based on abnormally high implied compensation per employee and discrepancies between employees reported on the PPP and EIDL Advance were not suitable for our analysis. Due to the $100K ceiling on reporting employee compensation in the PPP, the abnormally high compensation measure requires limiting the analysis to firms in very low paying industries and regions; see Griffin et al. (2023), pp.1788–1789. As explained previously, the PPP-EIDL job report discrepancy measure is only meaningful for firms with ten or fewer employees. Since small and low paying firms rarely procured loans of $150K or greater[55], treatment effects estimated using the aforementioned measures would provide little insight into the aggregate affects of screening.

Despite this cross-validation, the measures used in this paper by no means represent a comprehensive account of fraud in the PPP. Our variables do not capture 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 - such as buying luxury consumption goods rather than spending it on employee wages. Given

[53]The average industry compensation data was accessed from `https://www2.census.gov/programs-surveys/susb/datasets/2020/us_state_6digitnaics_2020.txt`. The PPP and Census Bureau data were matched on 6-digit NAICS code, size of the enterprise, and the state in which the firm resides.

[54]The EIDL Advance program was run directly by the SBA. This program provided firms with the opportunity to receive a forgivable loan of up to $10,000. In 2020, this amount was calculated as $1,000 per employee (up to the $10,000 maximum). Therefore, for this analysis, the PPP sample was restricted to those firms that had reported no more than ten employees. There are no unique firm-level identifiers in either dataset. To merge the two, we employed a string matching algorithm that matched EIDL advance and PPP data on firm names and addresses. Data was accessed from `https://www.sba.gov/funding-programs/loans/covid-19-relief-options/eidl/eidl-data`.

[55]Across the two phases of the program, out of all the firms reporting ten employees or less only 0.6% get loans greater than a $150K.

these limitations, we will not attempt to utilize our measures to estimate an aggregate level of fraud in the PPP. Instead, we employ them in order to estimate the magnitude of the effects of screening on several serious irregularities indicative of (certain types of) fraud. They are well-suited for this purpose, since—unlike other potential measures—they do not require us to limit our analysis to subsamples of the data based on firm characteristics that are correlated with loan size.

Additional data attempts. We attempted to incorporate other data to measure additional types of fraud, but owing to the quality of the data were not successful in these attempts. For example, businesses were required to have been in existence prior to February 15, 2020, and we attempted to find firms registered after this date that received loans. However, the OpenCorporates business registry database we used did not contain data on firm addresses for a number of large states, invalidating this strategy. States without addresses include Alabama, Delaware, Illinois, Maine, Michigan, Mississippi, New Jersey, Nevada, Ohio, Oklahoma, South Carolina, and Wisconsin.

Since loan amounts were dependent on the number of employees, we worked with additional data on firm employees to quantify the gap in employees reported on the PPP application with the actual figures. For that purpose we combined PPP data with DNB proprietary data on firm employees. However, the quality of the DNB data precluded its use for the main analysis. DNB does not provide a date at which the employees of a given firm are observed.[56] Moreover, records on any particular firm are updated with variable frequency.

Finally, 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.

[56]While we could see that the record on a firm is updated at a particular date there is little clarity on whether any update refers to an update of the number of employees or some other characteristic of that firm. We therefore, do not base our analysis on measures from these additional data.

Table D.1: Correlation of our measures of loan irregularities in phase 1 with other measures of fraud used in Griffin et al. (2023)

(a) PPP compensation versus average industry compensation

Dependent Variables:	PPP compensation as a fraction of avg industry compensation			PPP compensation > 2 std dev avg industry compensation		
	(1)	(2)	(3)	(4)	(5)	(6)
Overpayment Dummy	0.298***			0.428***		
	(0.00573)			(0.00435)		
Overpayment Rate		0.289***			0.104***	
		(0.0147)			(0.0245)	
Multiple Loans Dummy			0.0175***			0.0305***
			(0.00116)			(0.00262)
Control mean of outcome	0.0991	0.0991	0.0991	0.0295	0.0295	0.0295
Observations	1340130	1340130	1340130	1340130	1340130	1340130

(b) Reported number of employees in EIDL Advance versus PPP

Dependent Variables:	Whether reported EIDL empl greater than PPP empl			Whether reported EIDL empl greater than PPP empl by at least 3		
	(1)	(2)	(3)	(4)	(5)	(6)
Overpayment Dummy	0.151***			0.145***		
	(0.00990)			(0.00840)		
Overpayment Rate		0.0921***			0.0989***	
		(0.0148)			(0.0151)	
Multiple Loans Dummy			0.241***			0.172***
			(0.0230)			(0.0195)
Control mean of outcome	0.185	0.185	0.185	0.0580	0.0580	0.0580
Observations	193842	193842	193113	193842	193842	193113

Notes: The unit of observation is at the firm level. Data is restricted to phase 1. In Panel (b) the sample is restricted to those firms that had reported no more than ten employees. This is because a comparison with EIDL advance only makes sense for this subset, since EIDL advance was capped at $10k, allowing for $1k per employee. *PPP compensationas a fraction of avg industry compensation* is dummy that takes the value of 1 if the implied compensation in the PPP loan is greater than the industry average, and 0 otherwise. *PPP compensation > 2 std dev avg industry compensation* is a dummy that takes the value of 1 if the implied compensation in PPP is at least 2 standard deviations hgiher than the industry average, and remains zero otherwise. *Whether reported EIDL empl is greater than PPP empl* is a dummy that takes the value of 1 if the jobs reported in the EIDL data for any particular firm is greater than those reported in PPP, and 0 otherwise. *Whether reported EIDL empl greater than PPP empl by at least* 3 is a dummy that turns on 1 if the jobs reported in the EIDL data for any particular firm is greater than those reported in PPP by at least 3 employees, and 0 otherwise. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, 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 in phase 1, 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.