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

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Abstract

Screening requirements are widely used to deter fraud in public programs but may impose administrative burdens and deter legitimate participation. We study this trade-off in the Paycheck Protection Program (PPP), an $814 billion COVID-19 relief program. Using administrative data on 11.5 million loans, we exploit a policy change that required upfront documentation for second-draw loans above $150,000. Difference-in-differences estimates show that screening reduced loan irregularities indicative of fraud by up to 68 percent. We develop a model that characterizes when the fraud-deterrence value of screening exceeds its compliance costs and show that effective screening generates bunching below documentation thresholds, disproportionately among fraudulent firms. Consistent with these predictions, borrowers strategically reduced loan requests to avoid screening, and firms with prior irregularities were significantly more likely to bunch. Calibration and revealed-preference estimates indicate that fraud-deterrence effects were large relative to administrative burdens. We estimate that screening reduced fraudulent disbursements by at least $832 million. These results demonstrate how targeted documentation requirements can improve targeting efficiency in large-scale emergency programs.

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1 Introduction

Corruption and fraud plague public programs around the world (Becker and Stigler, 1974; Glaeser and Saks, 2006; Olken and Pande, 2012). 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, 2013b; Fang and Gong, 2017). These issues are often exacerbated in emergency relief programs, where timeliness is of the essence and administrative systems must deliver benefits at scale under severe time constraints. 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).

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, 2011a; Herd and Moynihan, 2018). In emergency relief settings, these costs take on special weight: policymakers may rationally tolerate more imperfect targeting or weaker verification in order to disburse aid quickly, yet the scale and urgency of crisis programs can also magnify the fiscal and political consequences of fraud. 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. Similar findings are reported by Dupas et al. (2016) in Kenya and Ashraf et al. (2010) in Zambia. Yet evidence from the United States (Deshpande and Li, 2019; Homonoff and Somerville, 2021) and India (Muralidharan et al., 2025) shows that application and reporting requirements can prevent poor and marginalized households from accessing benefits to which they are legally entitled. The design of public benefit schemes thus stands to benefit from approaches that identify the value of screening in reducing fraud relative to the administrative costs it imposes on beneficiaries.

We study screening based on simple financial documentation requirements in one of the largest economic relief programs in US history: the Paycheck Protection Program (PPP), an \$814-billion stimulus package adopted in response to the COVID-19 pandemic through 2020-21. The PPP is a useful laboratory for emergency relief design because it combined extreme urgency, high transfer values, and a massive applicant pool, leaving limited scope for intensive ex-ante verification. The PPP permitted small businesses, nonprofits, and other entities to apply for federally-backed loans administered by private lenders on behalf of the Small Business Administration (SBA). The program was popular and widely accessed, with 11.5 million loans (~\$68,000 each) going to 94% of firms that were formally eligible (Autor et al., 2022a). For borrowers, loans in essence operated as grants, since repayment was not required if funds were used for approved purposes. Meanwhile, because loans were fully guaranteed by the SBA and relied heavily on borrower attestations, lenders bore no credit risk and faced weak incentives for intensive due diligence. Thus, the outcomes we examine are ultimately an equilibrium determined by interactions between borrowers and lenders. In combination, these design features facilitated rapid disbursement but also increased opportunities for misreporting and overpayment, thereby sharpening the trade-off between speed and program integrity.

Our analysis proceeds in four steps. First, we present evidence on the magnitude of the effects of screening requirements on loan irregularities. While theoretically, additional screening that increases probability of fraud detection would reduce fraud (Becker and Stigler, 1974), to what extent it matters empirically is unclear. Moreover, even if fraud is reduced, whether and to what extent screening affects non-fraudulent firms is important to understand. Consequently, in step two we develop 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. Next, we test the implications of our model by examining the borrowing behavior of fraudulent versus legitimate firms after screening. Finally, we attempt to quantify these trade-offs through different methods, including a model calibration exercise, a revealed preference approach, and estimates of time and wage costs of administrative compliance.

We begin by exploiting a change in loan eligibility documentation to study the impact of screening requirements on loan irregularities. The PPP was divided into two distinct phases: phase 1 (April 2020 to August 2020) and phase 2 (January 2021 to May 2021).[1] In phase 2, firms that wanted to borrow for a second time and were requesting loans greater than \$150K were required to submit with their PPP application documentation proving that they had experienced a reduction in gross receipts in excess of 25% in 2020 relative to 2019.[2] Note that while phase 2 introduced additional eligibility requirements for second-draw loans, the specific policy variation we exploit affected screening conditional on eligibility: firms requesting loans of ≤\$150K faced the same 25% gross-receipts-decline criterion, but were not required to provide documentation up front. Finally, in phase 1 there were no differences in required documentation by loan size.[3]

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

[1] Phase 1 comprised of the first two rounds or tranches, while phase 2 comprised of the third round.

[2] 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).

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

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 show that for legitimate firms, requested loan sizes across phases were basically identical, indicating that any secular 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 allowed by program rules, 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.[4] For each measure, we find an economically meaningful and statistically significant reduction in irregular loans attributable to the screening requirement. These results are robust to alternative definitions of treatment (firms receiving $151K–200K versus $100K–150K loans in Phase 1), to clustering standard errors at different levels, and to different assumptions about differential pre-treatment trends (Rambachan and Roth, 2023). We also find that, among firms subject to the documentation requirement, those with irregularities in phase 1 were more likely than legitimate firms to respond by reducing their loan requests rather than exiting the program. The shift in the locus of fraud to lower loan values resulted in considerable savings to the PPP: we estimate that it reduced loan irregularities by $832 million, or 76% of the irregularities in phase 1, or 3% of the total fraud in the PPP as estimated by the SBA (Small Business Administration, 2023).

We next present a simple conceptual framework to examine the trade-off between the administrative burden of complying with upfront documentation requirements and fraud prevention. In our model, there are two types of firms: legitimate and fraudulent. Each type has private information about their eligibility status. Both types 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. Our model shows that both types of firms will respond to the documentation requirements by strategically requesting amounts immediately below the threshold triggering screening, leading to "bunching" of the frequency distribution at and under that point. However, if the administrative costs of the documentation requirements are low relative to their value as a tool for sanctioning fraud, the proportion of bunchers that are comprised of fraudulent firms will be significantly higher than the proportion

[4] 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 and Appendix 4 for more details.

of non-bunchers that are comprised of fraudulent firms. On the other hand, if the administrative costs are high, the proportions of bunchers and non-bunchers comprised of fraudulent firms will be similar to each other.

We examine the data to test the implications of the model. 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.[5] This suggests that many borrowers strategically set their loan requests just below the threshold to avoid submitting documentation. Importantly, the proportion of bunchers comprised of firms exhibiting irregularities in phase 1 of the program was significantly greater than the proportion of non-bunchers exhibiting said irregularities, indicating that the fraud deterrent created by the requirement was large relative to its administrative burden.

Finally, we turn to provide quantitative evidence on the trade-off, beginning with a model calibration exercise. We search for the ratio of parameters representing fraud deterrence and administrative burden which is most consistent with the overall proportion of firms exhibiting irregularities and the proportion of bunching firms exhibiting said irregularities. We find that the fraud deterrence parameter is 434 times greater than the administrative burden parameter. In addition, we use a revealed-preference approach, focusing on firms exhibiting bunching, and compare changes in loan amounts between firms with and without loan irregularities across the two phases of the program. We find that bunching firms with phase 1 irregularities curtailed their average loan requests roughly three times more than firms without said irregularities. This lopsided behavioral reaction is again consistent with the notion that the risk of fraud detection significantly outweighed compliance costs for firms. Alternative estimates also point in the same direction: government paperwork reduction act estimates suggest only 8 minutes to comply with the documentation requirements,[6] amounting to a *total* compliance cost of $1.5 million that pales in comparison to the $832 million in fraud reduction. Overall, compliance costs would have to exceed $3,955 per firm in order to negate gains from fraud reduction.

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 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 to the prospect of new variants of COVID or future pandemics, but due to the realities of climate

[5] 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).

[6] `https://omb.report/icr/202101-3245-004/doc/107676901`, accessed September 2022

change.[7] Tax collection, another indispensable task of government, involves the latter two features.

Our paper contributes to the literature on screening requirements for public benefits.[8] As discussed earlier, the targeting effects of screening, or "ordeal," mechanisms vary widely across programs. Finkelstein and Notowidigdo (2019) suggest that this variation reflects the type of frictions ordeals impose. In neoclassical models (Nichols and Zeckhauser, 1982; Besley and Coate, 1992), low-cost ordeals can improve targeting by deterring low-benefit applicants with high opportunity costs, whereas behavioral models (Bertrand et al., 2004; Mani et al., 2013; Mullainathan and Shafir, 2013) emphasize that cognitive burdens and poverty-induced "bandwidth taxes" may disproportionately exclude high-need individuals. In our case, the screening requirement—uploading an existing tax document—imposed minimal time and cognitive costs on a relatively advantaged applicant pool of registered business owners, aligning more with neoclassical predictions of improved targeting. Our setting also illustrates the broader trade-off between inclusion and exclusion errors: while emergency programs often minimize screening to speed disbursement, high transfer values and sophisticated applicants can justify more stringent checks. The PPP's phase 2 screening requirement balanced urgency with fraud prevention, yielding targeting gains without imposing substantial participation costs.

The current study also relates to the broader literature on regulation in law and economics.[9] 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.[10] 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

[7]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.

[8]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; Cuneo et al., 2023), our focus in this paper is on the marginal value of screening in addition to performing audits.

[9]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).

[10]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).

(CWA), and Eliason et al. (2025), 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, applied to a government emergency relief program of unprecedented scale.

Finally, our paper also contributes to a small but growing literature on COVID relief funds, in particular the PPP.[11] 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. 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 and examines the trade-off between administrative burden and fraud prevention based on such a change.

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. PPP Loans were intended primarily to fund payroll costs, including benefits. Formally managed by the Small Business Administration (SBA), the program operated through private-sector financial institutions (lenders) acting as intermediaries. In total, 5,460 lenders participated in the PPP.

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). Figure I describes the timeline of the Paycheck Protection Program with key events.

[11] 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).

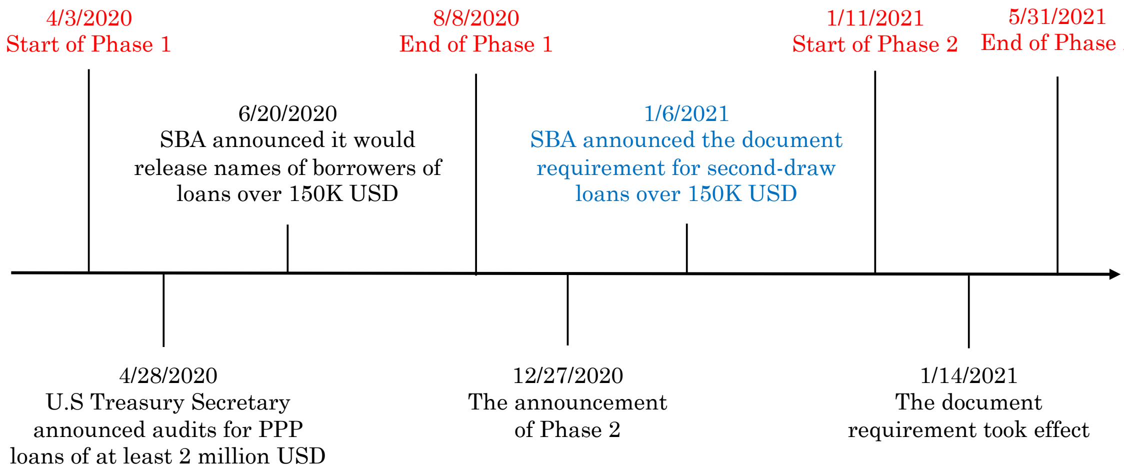

Figure I: Timeline of the Paycheck Protection Program.

We describe the two phases of the PPP below. We then discuss the program's oversight framework, including reported cases of fraud.

2.1 Phase 1

The first phase was established under the CARES Act (2020) and lasted from April 2020 until August 2020.

Eligibility and borrowing limits. To be eligible for phase 1 loans, firms had to employ five hundred or fewer employees (with minor exceptions). Firms had to certify that "current economic uncertainty makes this loan request necessary to support the ongoing operations." 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. No firm was permitted to receive more than $10 million from the program. Rules also stipulated that borrowers could not receive multiple loans (Section 1102, CARES Act 2020).

Documentation requirements. Applicants were required to certify eligibility and substantiate loan requests using historical payroll records. This process entailed submitting federal and state tax filings to verify wages and employer-funded benefits. Furthermore, borrowers had to validate the identity of significant owners and provide bank statements, book of record or invoices to prove the business was active as of February 15, 2020. In phase 1, there were no conditions related to a business having suffered a fall in gross receipts owing to the pandemic.

Audits. Audit authority rested with the SBA, which could review borrower eligibility and use of proceeds at any time. Loans above $2 million were automatically subject to SBA review, while smaller loans could be audited at the agency's discretion. Borrowers were required to retain supporting documentation for potential ex post review.

2.2 Phase 2

The second phase was established under the Economic Aid Act and lasted from January 2021 until May 2021. It operated under similar terms to phase 1, with important exceptions regarding eligibility and documentation.

Eligibility and borrowing limits. To be eligible for loans in phase 2, firms had to employ 300 employees or fewer. Firms were permitted to apply for a first-draw (new) loan in phase 2 regardless of whether they had applied in phase 1. However, receipt of a second-draw loan was restricted to firms that had experienced a decline in gross receipts of at least 25% in 2020 relative to 2019. Borrowing caps were largely unchanged from phase 1, with two notable exceptions: (i) restaurants and other firms in the accommodation and food services sectors could borrow up to 3.5 times average monthly payroll, and (ii) the maximum loan amount was capped at $2 million. Program rules continued to prohibit firms from receiving more than one loan per draw (Section 1102, CARES Act 2020).

Documentation Requirements. 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 firms requesting loans above $150K in value were required to provide documentation of this as part of the application.[12] 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. 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

[12] Documentation requirements were also reduced for loan forgiveness based on the same threshold, as noted by Griffin et al. (2023), who also examine the discontinuity across this threshold. However, their focus is on Fintech firms, and they do not examine changes over time. Analytically, ex-ante and ex-post documentation requirements around the same threshold would have the same incentive effects on firms.

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.

Audits. Audit rules in phase 2 were unchanged from phase 1 and applied to both First Draw and Second Draw loans. The SBA retained authority to review eligibility and use of proceeds, and loans above $2 million were automatically subject to audit.

2.3 Program Oversight

Given the program's emphasis on injecting capital into the private sector as quickly as possible, adherence to eligibility criteria and loan caps largely operated on an honor system. Compliance relied heavily on borrower attestations rather than ex ante verification. Lenders processed applications and confirmed that required certifications were submitted, but they faced weak incentives for intensive due diligence. The loans were fully backed by the federal government, so lenders did not bear credit risk. The CARES Act further limited lender liability through a "hold harmless" clause, which protected lenders from enforcement actions. 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. In exchange for administering loans, lenders received SBA fees proportional to loan size.[13] The SBA, therefore, allocated significant fees to lenders for managing loans for which they incurred zero risk. Neither fees nor penalties for loan administration changed after the screening requirement was introduced in phase 2, nor did they differ across the $150,000 threshold.

The default oversight mechanism was ex-post auditing by the SBA. However, the frequency and intensity of audits were not publicly specified. The SBA Administrator had the authority to review borrower eligibility, loan amounts, loan forgiveness claims, and whether funds were used for permitted purposes at any time. Both first-draw and second-draw loans were subject to review. To facilitate ex-post oversight, borrowers were required to retain their applications and supporting documentation for four to six years after disbursement. Loans with an original principal amount above $2 million were automatically reviewed. If the SBA determined that a borrower was ineligible or had misused loan proceeds, it could require repayment and refer the case for civil or criminal enforcement.

Reports from administrative agencies and criminal prosecutions document instances of fraud and misuse under the program. An SBA report notes that 744,000 cases amounting to $36 billion across

[13] 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.

$1.2 trillion in pandemic relief (PPP plus other programs) are likely fraudulent (Small Business Administration, 2023). Cases that have resulted in convictions illustrate the weak financial controls in the PPP and provide insight into how fraudulent schemes operated. In one case, a borrower (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; 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 case involved a professional athlete (Joshua Bellamy) who obtained a $1.2 million loan for a shell company (Drip Entertainment) and diverted much of the funds to personal consumption. He was convicted and sentenced in December 2021 to three years in prison. FinTech lenders appear to have approved a disproportionate number of fraudulent loans identified by the Justice Department.[14] Nonetheless, the PPP looks similar to other government programs when comparing fraud rates (Government Accountability Office, 2024).

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.

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 on the website of the Small Business Administration (SBA) [retrieved on November 24, 2021]. 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

[14] 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).

whether the firm is a non-profit.

Table 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.[15]

Unique firm-level identifiers are essential to our analysis, as the advanced documentation requirement applied to second draw loans taken by firms with a prior PPP loan. They also allow us to identify firms that received multiple loans within the same phase. Because the PPP data lack unique firm identifiers, we implemented a string-matching algorithm to link firm records. Appendix 3 details the algorithm and provides robustness checks for 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.[16] In addition to these measures, we considered numerous 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 4 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 under program rules 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.[17] 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 permitted under program rules.

[15] 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.

[16] For example, they would miss other types of fraud like the misuse of funds for personal consumption rather than employee wages; or inflating reported payrolls as documented in Griffin et al. (2023). For this reason, we do not attempt to estimate any aggregate measures of fraud or place a lot of weight on the estimates of *levels* of compliance costs of screening.

[17] Overclaiming benefits as fraud is common in government programs across the world, be it unemployment insurance in the US (Khetan et al., 2024) or workfare programs in India (Niehaus and Sukhtankar, 2013a).

For self-employed workers without employees, the maximum payment was calculated without the \$9,166 that is the average benefit spending on each employee.[18]

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. 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 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.1. 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. Yet the persistence of overpayments is striking: the maximum permissible amount under program rules could have been calculated directly from information in the loan application, so lenders could in principle identify when requests exceeded the limit. Nevertheless, tens of thousands of loans still slipped through, perhaps owing to weak incentives among lenders or other institutional frictions. 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. We define multiple loans as a dummy variable equal to 1 when a firm received more than one loan in any given phase, 0 otherwise. Below we describe the process of identifying firms with multiple loans in the data.

As discussed, the publicly available SBA data lack a unique firm identifier. Therefore, to

[18] 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).

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 3 for details on our string matching algorithm). We define a firm following the U.S. Bureau of Labor Statistics' definition of an establishment: a single physical location where one predominant activity occurs. 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).

While the number of such firms is relatively small, they were collectively granted a sum of $2.8 billion in PPP loans. 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.1). The average number of loans issued to a firm with multiple loans is 2 in phase 1 and 3 in phase 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 second draw loans greater than $150,000 must submit documentation showing a reduction in gross receipts of more than 25% in 2020 relative to 2019. Those with loan requests of ≤$150,000 were not required to submit such documentation with their applications, but were required to retain said documents should the SBA later request them for review.

We use a difference-in-differences approach to estimate the effect of the screening requirement. We define treatment exposure based on historical behavior, classifying firms as Exposed if they received a phase 1 loan greater than $150,000. While firms could enter the program for the first time in phase 2—receiving a first draw (exempt from documentation) and potentially a subsequent Second Draw (subject to documentation)—we exclude these firms from our main analysis. Focusing solely on firms that applied in both phases allows us to categorize treated or exposed firms based on behavior established before the screening mandate was announced. This ensures that assignment to the treatment or control group is determined by credit needs established prior to the announcement of the phase 2 rules, thereby mitigating concerns that treatment status is a self-selected response to the policy change itself.

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.[19] 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 upfront.[20]

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,

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

where Y_{imt} is one of our three loan irregularity 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 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

[19]Two main factors can explain why the loan amounts remained nearly identical. First, macroeconomic conditions remained roughly similar between the two phases of the PPP, with many industries, especially those dependent on in-person services, still operating below pre-pandemic levels due to social distancing and reduced demand. This meant that underlying loan needs remained the same. Second, PPP loan amounts were calculated using a formula, meant primarily to cover payroll and operational costs. Assuming firms accurately reported, calculated amounts, and requested loans in phase 1, and the costs for keeping workers on payroll and sustaining ongoing operations remained similar, one would expect firms - especially legitimate ones - to request similar amounts in phase 2.

[20]To the extent that the fraudulent non-exposed firms reduced their loan asks - or exited - because they feared an increased environment of accountability, our estimates would understate the effect of the screening requirement.

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). We also consider clustering at the lender location level (Appendix Table A.2), which does not change our results. 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 2a, Figure 2b and Figure 2c plot ρ and 95% confidence intervals for each month-year of the PPP program. Figure 2a (left top panel) shows the effect on the overpayment rate, Figure 2b (right top panel) shows the effect on overpayment dummy and Figure 2c plots the findings for multiple loans. Across all outcomes, the event-study estimates suggest a modest negative pre-trend in April–May, although the confidence intervals are wide. Importantly, these differences attenuate and are economically close to zero in the two months immediately preceding the policy change (June–July of phase 1). Relative to the post-treatment effects, the estimates nearest to treatment are small and display a similar pattern across the three outcomes we study. Nonetheless, to address the relevance of differential pre-trends, we complement the event-study evidence with additional robustness checks, including the approach of Rambachan and Roth (2023), which assesses sensitivity to alternative assumptions about violations of parallel trends. All additional robustness checks—including alternative treatment definitions that compare firms with phase 1 loan amounts of \$151K–\$200K to those with \$100K–\$150K, as well as analyses of potential anticipation effects and non-random attrition—are presented in the Appendix 5.

4.2 Estimation

The event study specification above examines how loan irregularities evolved around the introduction of the documentation requirement. We next estimate a difference-in-differences specification comparing exposed and non-exposed firms across the two phases of the program. For firms i with a PPP loan approved at date t, we estimate the following equation:

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

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 issued in phase 2 of the program and 0 otherwise. This captures any time trends across the two program phases. 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. 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.

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 2 moves beyond mean differences in the data and presents the results of estimating equation 2. 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 in phase 2 firms that were exposed to the upfront documentation requirement 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 68% of the value of the control group mean.

Findings for the rate of overpayment were even more pronounced. While average overpayment rates fell in phase 2 —in line with a drop in the average loan ask — 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 (column (4)).

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 documentation requirement was effective and that its introduction led to substantial reductions in loan irregularities indicative of fraud.

4.4 How did firms change behavior in response to screening?

Firms that would have been subject to the documentation requirement may have responded to the prospect of screening in phase 2 by exiting the program altogether (extensive margin). Alternatively, they may have continued participating in the PPP but requested amounts below $150,000 to avoid the upfront documentation requirement (intensive margin). These responses could vary depending on the firm's behavior in phase 1, specifically whether they engaged in any loan irregularities during that phase. To examine which reaction dominated, we estimate the following equation, using the cross-section of firms (i) in phase 2 of the program:

$$Y_i = \kappa + \theta F_i^0 + \mu Exposedfirms_i + \pi Exposedfirms_i \times F_i^0 + \varepsilon_i \tag{3}$$

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.

The key identifying assumption is that in the absence of the documentation requirement, the difference in outcomes between fraudulent and legitimate firms would have been the same for firms with loan amounts above or below $150,000 in phase 1.[21]

Figure 3 and Figure 4 show the effects on the intensive margin. Figure 3 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. Figure 4 presents the results classifying such firms as those that received multiple PPP loans in phase 1 of the program.

Results show that screening had 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, unlike for bad actors, there was continuity in loan requests for those firms that did not have any irregularities in phase 1. Exposed firms with loan amounts greater than $150K in phase 1 but no overpayments or multiple loans, had a 93% total probability of continuing to ask for loans greater than $150K. In contrast, firms that were subjected to screening and that had previously obtained loans in excess of the maximum or received multiple loans were 11% and 14% less likely to receive a loan amount greater than $150K (in phase 2), respectively than other firms. We find similar effects when using loan amounts as the outcome, although statistical power is lower for exposed firms with multiple loans in Phase 1. Firms reduced their loan ask by reducing the amount asked

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

per employee, rather than reducing the number of employees (see lower panels of Figure 3 and Figure 4).

Appendix Figure B.5 presents the results on the extensive margin effects of screening. It shows that screening does not appear to have prompted bad actors to leave the program. 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).[22] More details of the results can be seen in Appendix Table A.3a and Table A.3b. We consider clustering at the lender location level (Appendix Table A.4a and Table A.4b), which does not change our results. While this analysis of exit relies on our matching of firms across phases, in subsection 6.1 we examine an alternative test of whether the screening requirement resulted in firm exit; this test is based on changes in the distribution of loan sizes around the $150,000 threshold, inspired by the distributional accounting framework in Cengiz et al. (2019).

5 The targeting efficiency of a value-based documentation requirement

The findings presented thus far establish that the PPP's documentation requirement was successful in terms of fraud reduction, and fraudulent firms reduced their loan asks significantly in Phase 2. Yet such requirements typically impose administrative burdens on all program participants in addition to dissuading fraud. Thus, it is important to investigate the trade-offs induced by screening and assess whether the PPP's documentation requirement effectively reduced fraud without placing undue burdens on legitimately eligible firms. We develop here a theoretical model that provides precise empirical implications about when this will be the case.

The model serves three purposes in our paper. Firstly, it provides a framework for understanding how a value-based documentation requirement (i.e., screening) induces bunching behavior among recipients of government-provided benefits. Secondly, it clarifies how differential propensities to engage in bunching by fraudulent and legitimate firms relate to parameters that represent the added risk of engaging in fraud due to screening and the compliance costs due to screening. Finally, by using parameter values based on our data, the model provides a basis for quantifying the trade-off between fraud deterrence and administrative burden.

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

[22] Since the share of firms with multiple loans is five times smaller than the share of overpaid loans (see Table A.1), we have less statistical power to detect effects where fraudulent firms are defined as those that obtained multiple loans in Phase 1.

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:

$$u_i = \begin{cases} v(g_i) - \eta_i g_i - \phi \mathbb{I}(g_i > \widetilde{g}) & \text{if } s_i = 1 \\ v(g_i) - c(g_i) - \eta_i g_i - \phi \mathbb{I}(g_i > \widetilde{g}) & \text{if } s_i = 0 \end{cases} \tag{4}$$

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:

$$c(g_i) = \pi(g_i)[1 + \tau \mathbb{I}(g_i > \widetilde{g})], \tag{5}$$

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 of the government support, i.e. the level of fraud, but the likelihood of detection jumps discretely upwards for all amounts greater than $\widetilde{g}$.

The optimally chosen loan requests for legitimate and fraudulent firms with and without the documentation requirement are formally characterized in Appendix 6 of the Appendix. The key intuitions can be gleaned from Figure 5, which depicts the theoretical impact of the documentation requirement on the density of all requests for the good from the program.[23] The initial density of requests–without the documentation requirement–is shown by the solid line. Note that the density is smooth throughout its range. The density of requests with documentation required for all requests above $\widetilde{g}$ is shown by the dashed line. This density is characterized by a large upward spike at $\widetilde{g}$, reflecting the choice of a subset of legitimate firms to avoid the administrative costs of the documentation requirement and the choice of a subset of fraudulent firms to simultaneously evade the risk of detection created by documentation requirement and to avoid its administrative costs. Following the nomenclature employed by the public finance and labor literatures, we refer to firms that contribute to this spike as 'bunchers' and the spike itself as the bunching mass.[24] 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 latter of which reflects the targeting efficiency of the screening mechanism. The proposition below summarizes these insights.

Proposition 1. *a) An increase in the administrative burden of the documentation requirement (ϕ)*

[23] This graph was created by parameterizing the model as follows: $v(x) = \ln(x)$, $\pi(x) = x^2/2$, $\zeta = 0.99$, $\tau = 1$, $\phi = 1/500$, $\tilde{g} = 0.15$, $\bar{g} = 2.0$, and F_1, F_0 are uniform densities with support [1,70].

[24] 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.

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 significantly higher than the proportion of non-bunchers that are comprised of fraudulent firms. Contrariwise, when τ is low and ϕ is high, the proportions of bunchers and non-bunchers comprised of fraudulent firms will be similar to each other.

Proof. See subsection 6.1 in the Appendix. □

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 Model test and quantification

6.1 Empirical evidence on 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 were perceived of by firms as dominant relative to its administrative burden, then fraudulent firms should be disproportionately represented among bunchers compared to non-bunchers.

Due to the fact that we observe loan allocations across the two phases of the program, our dataset is well suited for detecting the existence of bunchers. This circumvents the common challenge in identifying bunchers in the public economics and labor literature — 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). 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 reasonable counterfactual distribution, i.e. a distribution capturing what the loan values in phase 2 would have been had screening not been implemented. If bunchers engaging in strategic evasion do exist, then we should observe specific patterns in the distributions of loan values in the two phases. First, in phase 2 one should observed a sharp upward spike in the density of loans immediately 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 that time.

Figure 7 presents the density of loan amounts for all phase 1 and phase 2 loans. The figures provide evidence of systemic strategic evasion. 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 rounding.

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 program. Figure B.7 presents the density as well as the *p*-value from a McCrary density test, which is the standard test of discontinuities in the conditional density of the forcing variable in regression discontinuity designs (McCrary, 2008; Imbens and Lemieux, 2008).[25] In this context, the McCrary density test evaluates 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.

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 documentation requirement and "bunching" at or below the cut-off of \$150K.

Finally, we also test whether the cut-off led to exit, using a distribution based test inspired by Cengiz et al. (2019). We estimate the extensive margin effect (Δe) as the sum of the "excess mass" (Δa) appearing at the threshold and the "missing mass" (Δb) disappearing from above the threshold. To implement this test while conducting inference, we adapted the binwise mass-based estimator of Cengiz et al. (2019) to a loan-level probability model. Similar to the previous extensive margin test

[25] Figure B.6 presents the loan density as in Figure 7 but breaks down the phase 2 loans into first and second time borrowers. 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. Figure B.8 presents a similar test as Figure B.7 but breaks down phase 2 loans into first time and second time loans.

in subsection 4.4, we find no evidence of screening-induced exit (full description of the methods and results are in Appendix 7).

Identifying "bunchers". While a visual inspection of Figure 7 can be utilized to identify the interval of loan values within which we observe bunching, we formally test and locate the bunching interval using a Kolmogorov–Smirnov (KS) test. The KS-test tests the equality of the loan distributions in phase 1 and phase 2 loans. Figure B.9 in Appendix 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 loans of greater than \$150K in phase 1, but then chose to get loans between \$136-150K in phase 2.

6.2 Are fraudulent firms more prevalent among bunchers than non-bunchers?

Proposition 1 provides a testable implication of the model: if the documentation requirement had a high fraud-deterrence value relative to its administrative burden, fraudulent firms should be disproportionately represented among bunchers compared to non-bunchers. If, instead, the administrative burden dominated, the prevalence of fraudulent firms should be similar across the two groups.

In this subsection we investigate this further. We use our data at the firm (i) level and estimate the following:

$$Irregular\ firms_i = \pi + \theta Buncher\ in\ Phase2_i + v_i \tag{6}$$

where $Irregular\ firms_i$ is defined based on phase 1 behavior of firms and includes the following variables: whether the firm was overpaid in phase 1; the rate of said overpayment; and whether a firm received multiple loans in phase 1. $Buncher\ in\ Phase2_i$ is an indicator variable equal to 1 for those second time borrowing firms that had loans of greater than \$150K in phase 1, but who chose to get loans between \$136-150k in phase 2, and equal to 0 for non-bunchers. Non-bunchers are any firms that fell outside the bunching interval of \$136-150K in phase 2 of the program, irrespective of their loan amounts in phase 1. Standard errors (v_i) are clustered at the firm level.[26]

The coefficient θ measures the additional probability that firms with loan irregularities in phase 1 are among the bunchers compared to non-bunchers i.e., $E(Irregular\ firms|Bunchers == 1)$ - $E(Irregular\ firms|Bunchers = 0) = \theta$. In line with Proposition 1, if the fraud sanctioning value of the documentation requirement (τ) is high relative to its administrative burden (ϕ) then we

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

should observe $\theta > 0$. Conversely, if the administrative burden dominates then we should observe a $\theta = 0$. This provides a test of the targeting effectiveness of the screening mechanism, specifically, whether it had a greater deterrent effect on fraudulent firms than on non-fraudulent ones.

Table 3 presents the results and Appendix Figure B.10 shows the corresponding graphical representation. We find that irregular firms are disproportionately represented among bunchers relative to non-bunchers. In the first column we see that the probability of an irregular firm among bunchers is 0.0266, compared to 0.0095 for non-bunchers. The difference of 0.017 is both economically meaningful and statistically significant.[27] The second column shows a similar pattern when irregularity is defined using the overpayment rate. When using multiple loan receipt to define irregularity (Column (3)), the results point in the same direction but are weaker. The probability of an irregular firm among bunchers is 0.0023, compared to 0.0018 among non-bunchers. This difference of 0.0005 is not statistically significant, though it represents 27.8% of the non-buncher mean.[28]

Taken together, the results suggest that bunchers were disproportionately comprised of firms with a checkered history in the program. In light of the model, these results indicate that the deterrent effect of the screening requirement against fraud was weighed more heavily by firms than its associated administrative burden.

6.3 Quantification of the trade-off

6.3.1 A model calibration exercise

Our evidence on the trade-off between fraud reduction and administrative burden has demonstrated that screening resulted in a disproportionate amount of bunching behavior by irregular firms. This is consistent with the expectations of our model for settings in which fraud deterrence looms large relative to compliance costs. Yet we have not to this point attempted to establish the relative magnitude of these two quantities. To do so, we conduct a calibration exercise that utilizes our theoretical model in order to assess the relative values of the parameters τ and ϕ that are most consistent with the behavioral patterns encountered in the data.

[27] We follow the methods suggested in St. Clair (2016) and Marx (2018) and investigate attrition around the bunching window. Results are presented in Appendix Table A.6 and corroborate the results discussed in Table A.3a and Table A.3b concerning the attrition of fraudulent actors subject to screening. Firms with loan amounts ranging from \$150-170K in phase 1, and those exhibiting irregularities, were 6% less likely to exit the program compared to those without irregularities in phase 1 but with similar loan amounts (\$150-170K). This implies that our results likely underestimate the true impact of screening.

[28] 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 instilled greater caution, thereby reducing the incidence of irregularities. Figure B.11 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.

We proceed as follows. Using the functional form and distributional specifications employed to construct Figure 5, we set legitimate firms as a proportion of all firms that participate in program equal to 0.99, as this reflects the proportion of non-overpaid firms in phase one. Consistent with our data, we set the number of firms to 11.5 million, $\widetilde{g} = 0.15$, and $\bar{g} = 2.0$ (the latter quantities reflecting the threshold for screening and the maximum loan request in millions, respectively). We then proceed to calculate the ratio τ/ϕ that is most consistent with the observed proportion of overpaid firms among the bunchers (0.027).[29] To accomplish this, we run a series of simulations where we set τ to values between 0.1 and 1, and then for each of these values, we calculate the ratio $\tau/\hat{\phi}$ that minimizes the squared difference between the model-based prediction of the proportion of fraudulent firms among the bunchers and 0.027. Our results are presented in Figure 6. Across our simulations, the value of τ/ϕ was calculated as being between 400 and 500, with a mean of 434. In other words, our model calibration exercise indicates that the increase in the perceived cost (to fraudulent firms) of engaging in fraud due to screening outweighed the individual firm compliance costs by more than 400 to 1. Such a large ratio of these parameters indicates that, for the typical firm, the screening-induced jump in perceived costliness of fraud was significantly greater than the administrative burden of compliance.

To assess the robustness of our finding of a large ratio of fraud deterrence to compliance costs, we evaluated a number of extensions of the model calibration exercise where the true proportion of illegitimate firms deviated from the 0.01 figure based on overpaid firms in phase 1. Specifically, we introduced a parameter γ into the simulation and set the true proportion of illegitimate firms to $\gamma \times 0.01$. We considered values of γ in the set $\Gamma \in \{0.5, 1, 2, 5, 10\}$, since we believe that the proportion of overpaid firms is more likely to underestimate the proportion of illegitimate firms than to overestimate it. In all of our simulations, we assumed that the proportion of illegitimate firms among the bunchers was $\gamma \times 0.027$, i.e., that the multiplicative increase in the proportion of illegitimate firms among bunchers (vis-à-vis the entire population) was the same as that observed with the overpaid firms. Figure B.12 in the Appendix presents our extensions. The ratio of parameters remains more or less the same for the scenario where the proportion of illegitimate firms is overestimated by overpaid firms ($\gamma = 0.5$), but it increases substantially in the scenarios where the proportion of illegitimate firms is underestimated ($\gamma > 1$). In the most extreme case, where the proportion of illegitimate firms is ten times the proportion of overpaid firms, our mean estimate of the ratio of the perceived costliness of fraud to compliance costs was 721.[30] Thus, these extensions reinforce the notion that for individual firms the fraud deterrence effects of screening were large relative to administrative burden.

[29] All parameterizations used in the calibration are presented in Table A.7 in the Appendix.

[30] The mean ratios varied from a minimum of 434 ($\gamma = 1$) to a maximum of 721 ($\gamma = 10$).

6.3.2 Alternative estimates

We complement the findings of our model calibration above with other sources of information that also shed light on the trade-off between fraud reduction and administrative burden.

First, using a revealed preference approach, we focus on bunching firms and compare changes in loan amounts between those with and without loan irregularities across the two phases of the program. This can serve as an additional test of the *relative* intensity of the perceived risk of fraud detection versus the cost of compliance. Such relative comparisons are central to our understanding of the targeting efficiency of the documentation-based screening requirement.

We calculate the average changes in loan amounts across the two phases of the PPP by bunchers that exhibited loan irregularities in phase 1 and bunchers that did not exhibit such irregularities (Panel A of Table A.8 in the Appendix). The difference in these two amounts is potentially indicative of the relative intensity of firms' concerns about sanction risk for fraud versus administrative burden, since firms with phase 1 loan irregularities should have held both concerns whereas the regular firms should have been concerned primarily with administrative burden of the documentation requirement. Results show that bunching firms with phase 1 irregularities curtailed their average loan requests roughly three times more than firms without said irregularities – $128,018 versus $43,093. This lopsided behavioral reaction is again consistent with the notion that fraud detection risk significantly outweighed compliance costs. We do note that a potentially nontrivial share of firms classified as "regular" may have engaged in other forms of fraud beyond overpayments or multiple loans[31] and may also have been responsive to the sanctioning risk associated with documentation requirements. This difference, therefore, likely understates the true effect, which is all the more notable given the conservative nature of our irregularity measure.

Our next attempt to quantify the trade-off uses more direct estimates of the time costs of compliance, for example those provided by the federal government (Panel B in Table A.8 in the Appendix). In keeping with the Paperwork Reduction Act, the SBA produced estimates of the total time it should take to complete the paperwork for PPP applications in the two phases as well as the dollar cost of doing so, using the salary and fringe benefits for a GS-11, Step 1 Federal employee as the benchmark ($45.5 per hour). According to the SBA's review, the "estimated time for each applicant to review the form, gather the necessary information and complete the application is 8

[31] For instance, inflating reported payrolls on the PPP application as documented by Griffin et al. (2023). This is why we place limited weight on estimates of absolute levels of compliance costs based on the reduction in loan ask by those we classify as regular firms ($43,093). Interpreting such estimates as meaningful measures of compliance costs relies on a key assumption: the firms categorized as "regular" are in fact legitimate and the intended beneficiaries of the program. This assumption is strong in our context. Accordingly, any resulting estimate should be interpreted as an upper bound on compliance costs. This interpretation is reinforced when we compare our estimates ($43,093) to those in the literature. For example, Benzarti (2020) estimate the compliance costs of itemizing income tax deductions to range between $175 and $591, while estimates for compliance costs associated with screening mechanisms reach a maximum of $5,833 (see Appendix Table A.10).

minutes." Based on that 8 minute figure, the compliance cost (due to screening) per application could not exceed \$6.05, for an aggregate total of no more than \$1.3 million for the 210,296 firms that needed to provide additional documentation.

With the \$6.05 per application compliance cost estimate in hand, we returned to our model calibration in order to more precisely pin down the perceived costliness of fraud. Specifically, we set $\phi = 6.05 \times 10^{-}6$ (since parameters that are monetary equivalents are expressed in millions) then searched for the value of τ that that minimized the squared difference between the model-based prediction of the proportion of fraudulent firms among the bunchers and the observed proportion based on overpayment, otherwise maintaining the parameterizations in the benchmark calibration outlined above. This resulted in an estimate $\hat{\tau} = 1.996 \times 10^{-}3$, indicating a fraud deterrence effect of \$1,996. The implied ratio of fraud deterrence to compliance costs of 330 to 1 is roughly consistent with the ratios calculated using calibrations without prior knowledge of ϕ, again indicating that fraud deterrence overwhelmed administrative burden at the firm-level.

We recognize that the 8 minute figure might underestimate the time needed to provide relevant documentation. Thus, our third step was to estimate how much money, on average, applicants would have needed to have spent on complying with the screening requirement in order to balance out the aggregate reduction in fraud due to screening (Panel C in Table A.8 in the Appendix). To calculate the aggregate fraud reduction statistic, we sum across the screening induced reductions due to overpayment and multiple loans. The documentation requirements led to an average reduction in overpayment by \$3,551 per firm for 209,911 firms in the group of firms exposed to upfront documentation requirements. It led to an average reduction in amounts from multiple loans by \$411 per firm for 210,296 firms in the the group of firms exposed to upfront documentation requirements (see Appendix Table A.9 for details). This gives us a total of \$832 million in fraud reduction.[32] The 210,296 firms would have to spend \$3,955 each on complying with the documentation requirement to fully negate the benefits from fraud reduction.

7 Discussion and conclusion

The findings of our paper establish the utility of screening as a means of reducing fraud in large-scale public programs. Contextualizing these results and our setting within the broader literature on screening might help elucidate our contribution; to this end, Appendix Table A.10 synthesizes evidence from a range of studies across diverse programmatic and institutional settings.

[32]This fraud reduction total figure of \$832 million in the PPP compares reasonably to similar figures for other programs of similar size. For example, the Food and Nutrition Service calculates fraud recoveries from the SNAP (Supplemental Nutrition Assistance Program) to be around \$390 million in Fiscal Year 2021 (SNAP Program Administration and Nutrition Division, 2021). Meanwhile, Khetan et al. (2024) estimate that using identity verification technology for Unemployment Insurance benefits resulted in \$1.8 billion lower disbursements to suspicious recipients.

Table A.10 shows no consistent pattern: ordeals sometimes improve targeting and sometimes worsen it. This aligns with Finkelstein and Notowidigdo (2019), who argue that the effects of screening depend on the type of friction imposed. In neoclassical models (e.g., Nichols and Zeckhauser (1982)), time-based hassles can improve welfare by screening out low-benefit applicants with high opportunity costs. In contrast, behavioral models highlight that cognitive frictions can disproportionately deter high-need individuals, especially those facing poverty-induced "bandwidth taxes" (Bertrand et al., 2004; Mani et al., 2013; Mullainathan and Shafir, 2013). Because public programs are often complex and burdensome (Kleven and Kopczuk, 2011b; Currie, 2006), these costs may fall most heavily on the very people policymakers aim to reach.

Two contextual features likely explain why screening improved targeting in our setting. First, the costs were minimal: applicants uploaded a standard tax form via a simple and clearly communicated process. Second, the eligible population—formal, tax-compliant business owners—was relatively advantaged and less susceptible to cognitive barriers. This context more closely aligns with neoclassical assumptions, where well-designed ordeals can improve targeting.

More broadly, our setting highlights the central policy trade-off between exclusion and inclusion errors. When transfers are small and recipients disadvantaged, minimizing exclusion typically takes priority. But for larger transfers or more advantaged populations, fraud prevention becomes more salient. In emergencies, urgency introduces a further constraint: delays in disbursement can generate significant welfare losses. The PPP faced all three pressures—high urgency, large transfers, and relatively sophisticated applicants—making the design challenge more complex.

Our findings thus speak to an overlooked scenario: emergency relief for middle- or high-income populations. In such cases, it is unclear whether speed or fraud prevention should dominate. Our results examining the PPP show that simple, low-cost screening can strike a balance—limiting fraud without significantly deterring legitimate applicants.

Of course, screening could have myriad costs. Even when take-up is unaffected, some participants may alter their behavior. In the PPP, some legitimate firms may have reduced loan requests to avoid documentation burdens. Policymakers must weigh such behavioral responses against the fraud reduction benefits.

Our approach offers a way to quantify these trade-offs. By leveraging threshold-based screening and applicant bunching, researchers can infer the relative deterrent effects on fraudulent versus compliant firms. Such evidence, combined with calibrated models as in this paper, can guide screening design in future programs—especially where full oversight is not feasible but timeliness remains critical. It is not obvious *a priori* whether governments should prioritize fraud prevention or disbursement speed, or what the optimal threshold for screening should be. We leave such question and their implications for program design in similar hybrid contexts for future research.

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Tables

Table 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
Observations	5,128,185	1,494,052	8,256	1,485,796
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
	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

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.

Table 2: 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.

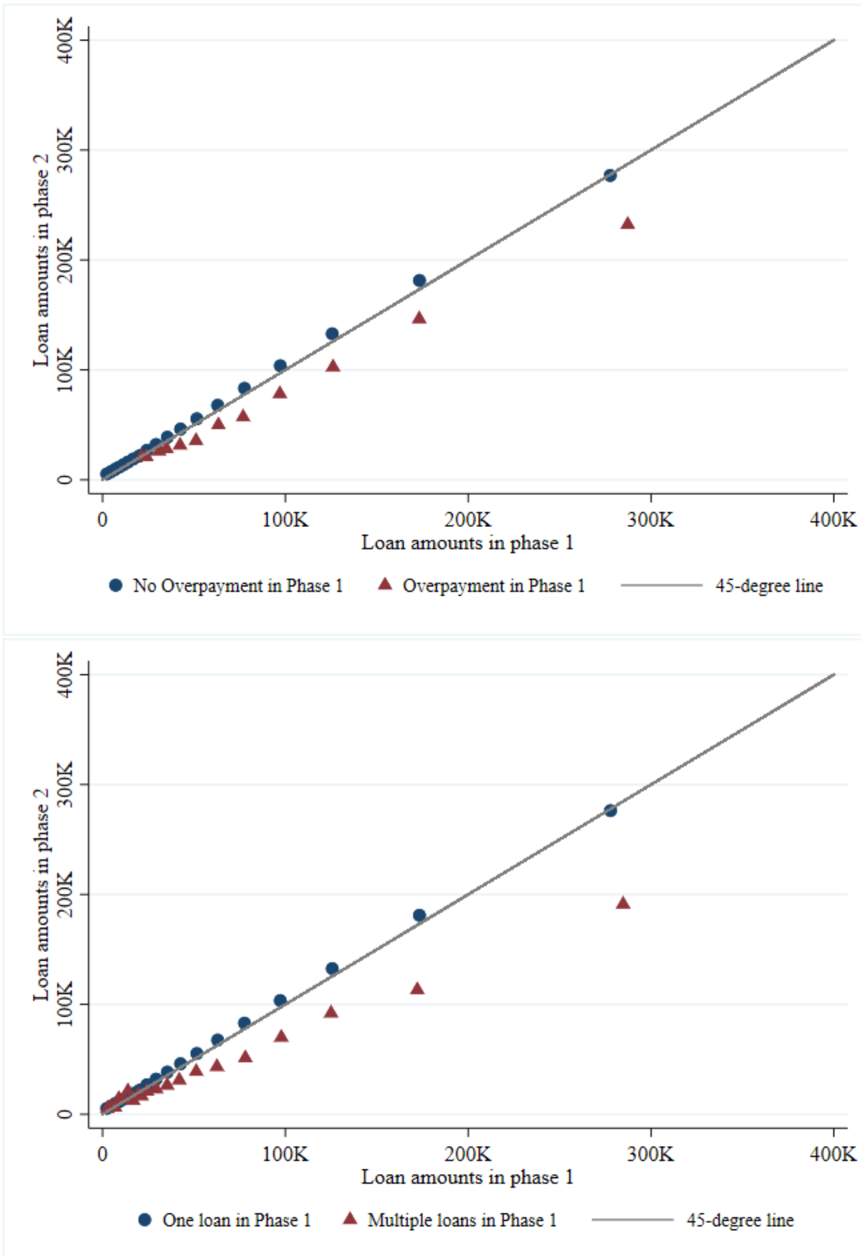
Table 3: Are fraudulent firms more prevalent among bunchers than non-bunchers?

Dependent Variable:	**Firms with irregular loans**		
	Whether Overpaid in Phase 1	Overpayment Rate in Phase 1	Whether received Multiple Loans in Phase 1
	(1)	(2)	(3)
Bunching firms in Phase 2	0.017***	0.016***	0.00053
	(0.0018)	(0.0031)	(0.00053)
Constant	0.0095***	0.0058***	0.0018***
	(0.000080)	(0.00017)	(0.000034)
Observations	1494052	1494052	1494052

Note: Note: The unit of observation is the firm. The sample is restricted to firms that received loans in both phase 1 and phase 2. Bunching firms 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. "Whether 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. "Overpayment rate in Phase 1" classifies firms as irregular based on their overpayment rate, which is defined as the overpaid amount divided by the maximum payment that a firm was eligible for (as measured in phase 1). "Whether received 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. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

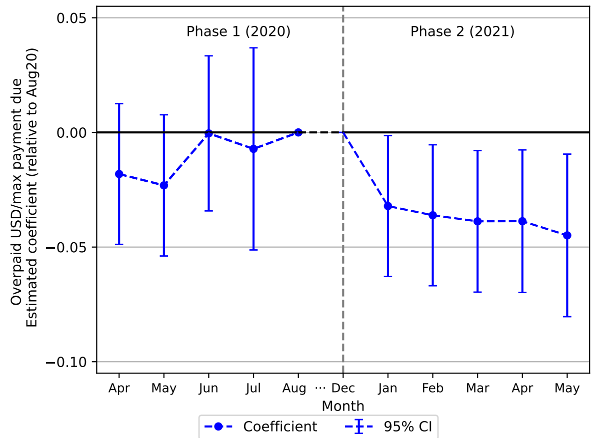
Figures

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

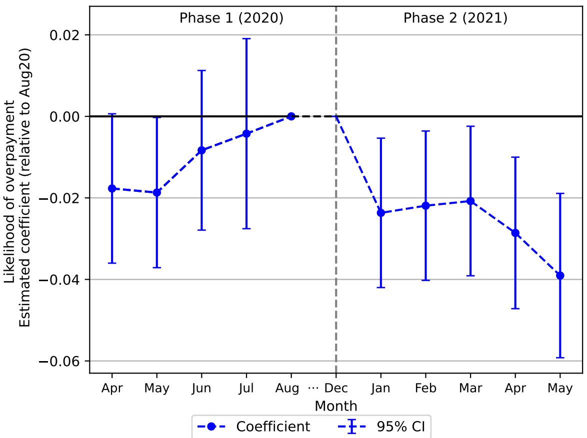


Notes. The top panel categorizes firms with loan irregularities as those that had an overpayment in phase 1, while firms with no irregularities are those that had no overpayments. The bottom panel defines firms with loan irregularities as those that had multiple loans in phase 1, while firms with no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

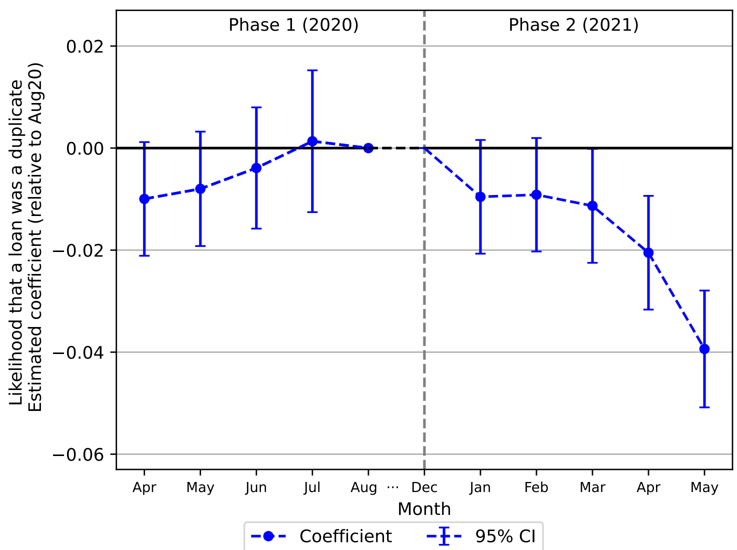
Figure 2: Event study plots for loan irregularities



(a) Overpayment rate



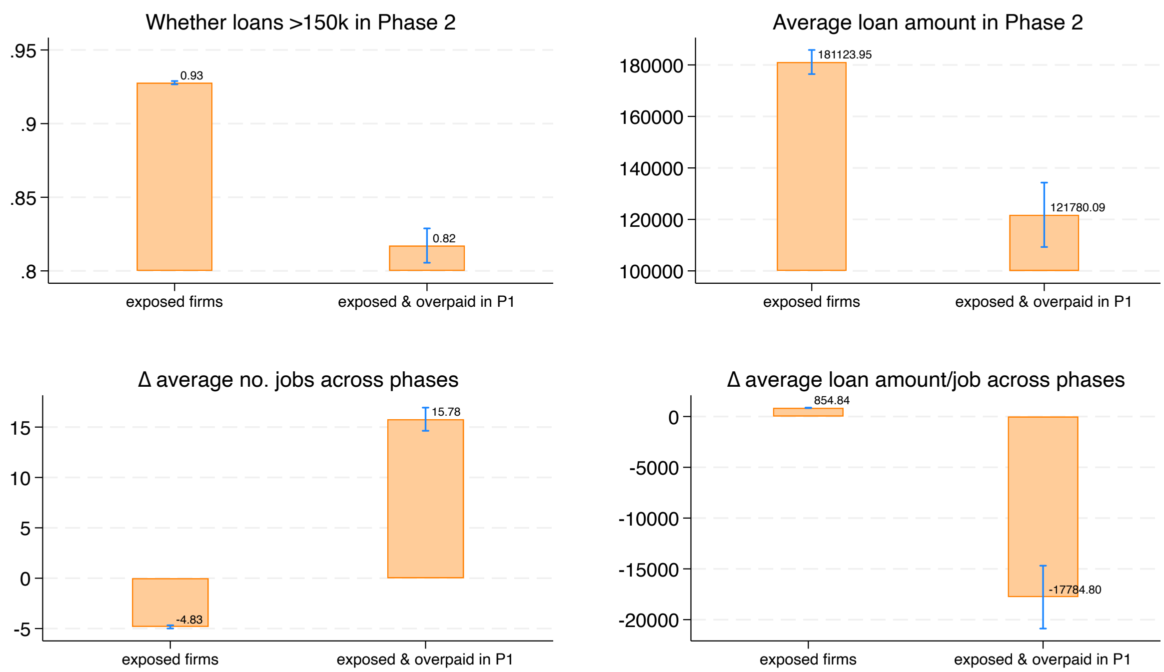
(b) Probability of overpayment



(c) Probability of multiple loans

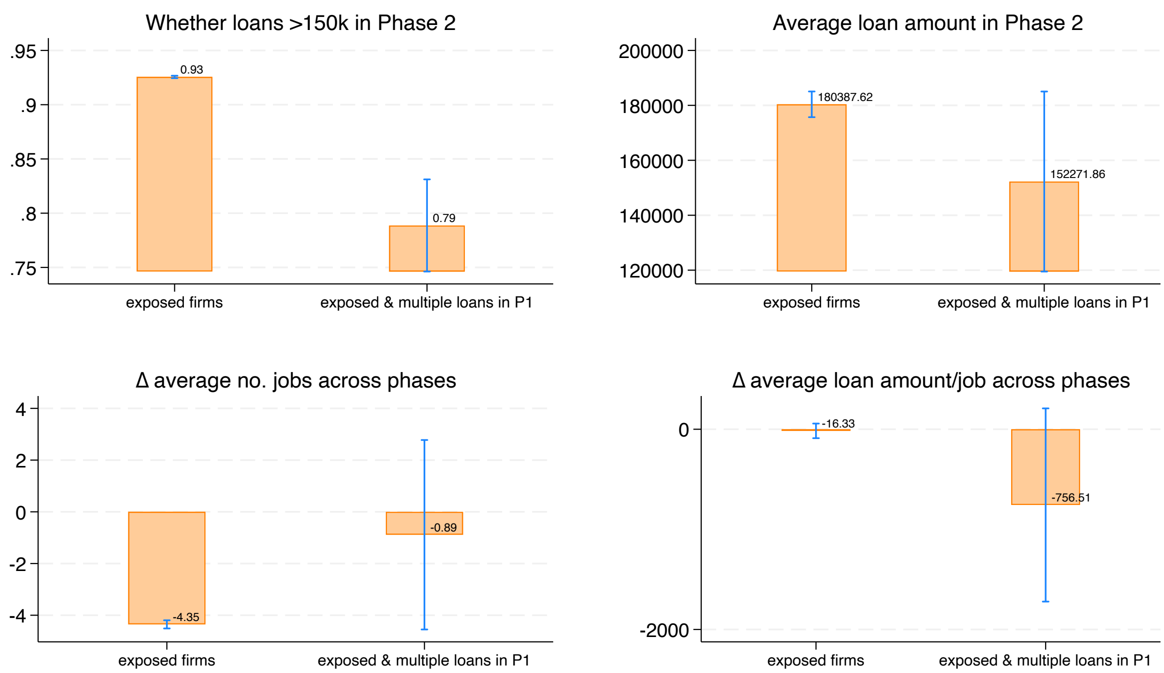
Notes. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1. The treatment group consists of firms with at least one loan greater than $150,000 in phase 1 and that were exposed to the upfront documentation requirements. We have adjusted the x-axis to reflect the fact that phase 2 started four months after the end of the first phase.

Figure 3: Did screening affect fraud through the intensive margin? Suspect firms defined based on overpayment in phase 1



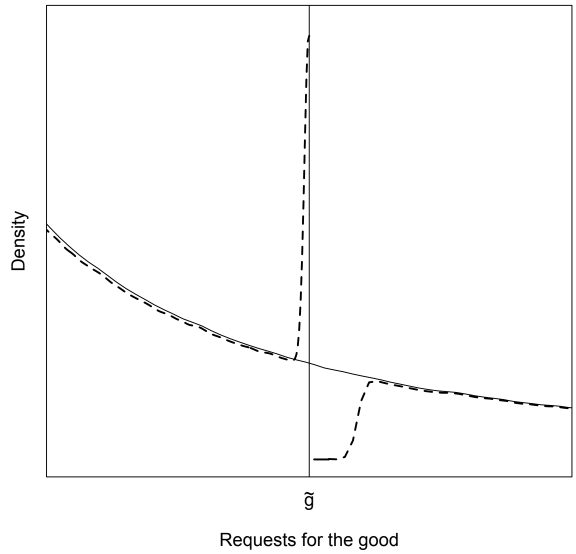
Notes. This graph visualizes columns (2) - (5) from Table A.3a. The unit of observation is the firm. The sample is restricted to those that received loans in both phase 1 and phase 2. Exposed firms are those with loans greater than \$150,000 in phase 1 that were subject to the upfront documentation requirement. "Overpaid in P1" is a dummy variable that takes a value of 1 when a firm received any overpayment in phase 1, and 0 otherwise. Whether loans greater than \$150K in phase 2 is a dummy variable that takes the value one 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 bottom two panel, we further subtract phase 1 values from phase 2 values to get an across-phase difference for each firm. 95% confidence intervals using standard errors clustered at the firm level are shown in blue.

Figure 4: Did screening affect fraud through the intensive margin? Suspect firms defined based on multiple loans in phase 1



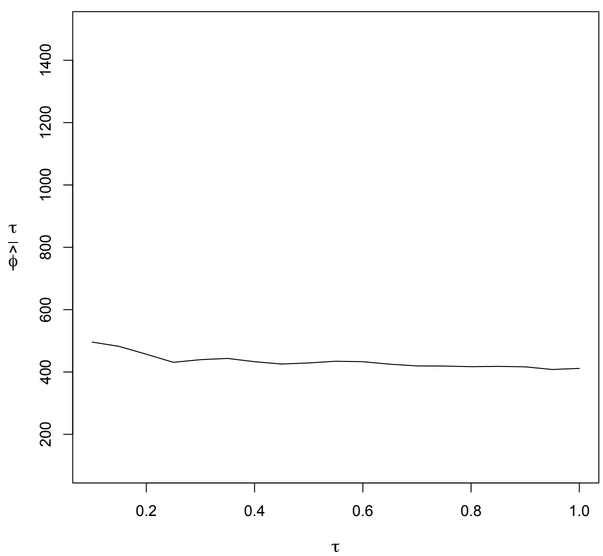
Notes. This graph visualizes columns (2) - (5) from Table A.3b. The unit of observation is the firm. The sample is restricted to those that received loans in both phase 1 and phase 2. Exposed firms are those with loans greater than $150,000 in phase 1 that were subject to the upfront documentation requirement. "Multiple loans in P1" 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. Whether loans greater than $150K in phase 2 is a dummy variable that takes the value one 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 bottom two panel, we further subtract phase 1 values from phase 2 values to get an across-phase difference for each firm. 95% confidence intervals using standard errors clustered at the firm level are shown in blue.

Figure 5: The theoretical impact of the advanced documentation requirement on the density of all requests for the good from the program



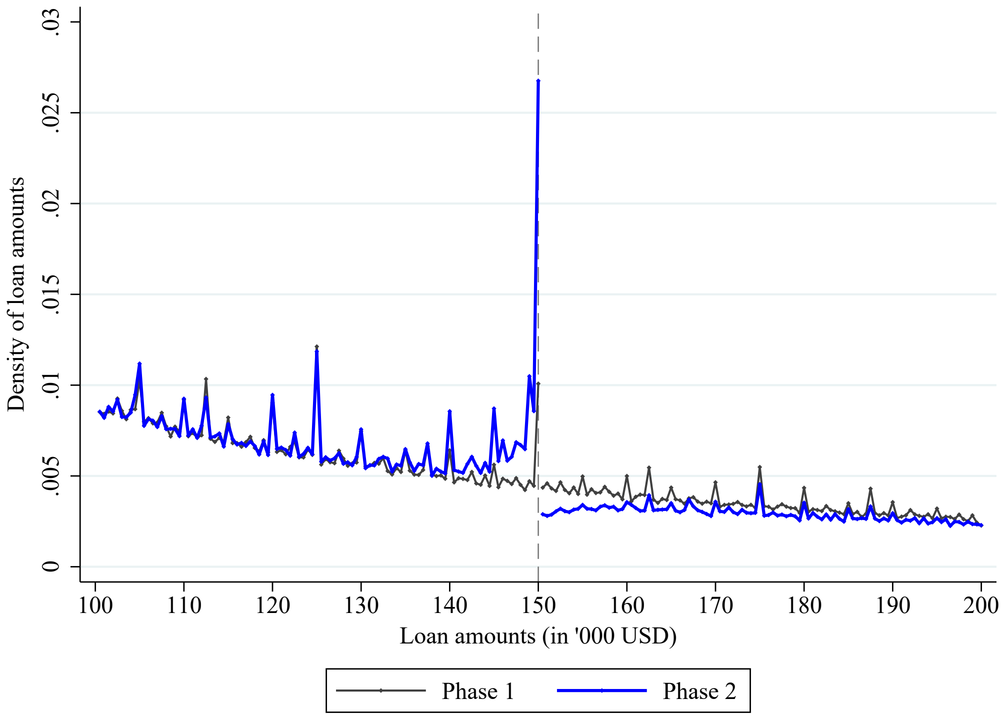
Notes. This graph was created by parameterizing the model as follows: $v(x) = \ln(x)$, $\pi(x) = x^2/2$, $\zeta = 0.99$, $\tau = 1$, $\phi = 1/500$, $\tilde{g} = 0.15$, $\bar{g} = 2.0$, and F_1, F_0 are uniform densities with support [1,70].

Figure 6: Model calibration exercise



Notes: The figure shows values of τ/ϕ most consistent with observed proportion of fraudulent firms among bunchers. The exogenously given quantities in the calibration were specified as follows: $v(x) = \ln(x)$, $\pi(x) = x^2/2$, $\zeta = 0.99$, $\tilde{g} = 0.15$, $\bar{g} = 2.0$, and F_1, F_0 are uniform densities with support [1,70]. The calibration proceeded by first setting τ to a value between 0.1 and 1, and then for each such value, calculating the ratio $\tau/\hat{\phi}$ that minimized the squared difference between the model-based prediction of the proportion of fraudulent firms among the bunchers and the corresponding proportion in the data (0.027).

Figure 7: Empirical density of loan amounts across the two phases of PPP program



Notes. This figure plots the empirical density of loan amounts separately for phase 1 and phase 2 of the PPP program. Phase 2 includes both first draw and second draw loans. The vertical dashed line marks the $150,000 threshold at which upfront documentation was required for second draw loans. Bins are $500 wide.

Online Appendix

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1 Appendix tables

Table A.1: 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.2: 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.3: 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 a 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 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 a 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 lender location 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:	**Firms with irregular loans**		
	Whether Overpaid in Phase 1	Overpayment Rate in Phase 1	Whether received Multiple Loans in Phase 1
	(1)	(2)	(3)
Bunching firms in Phase 2	0.017***	0.016***	0.00053
	(0.0021)	(0.0035)	(0.00051)
Constant	0.0095***	0.0058***	0.0018***
	(0.00053)	(0.00045)	(0.00028)
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 firms 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. "Whether 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. "Overpayment rate in Phase 1" classifies firms as irregular based on their overpayment rate, which is defined as the overpaid amount divided by the maximum payment that a firm was eligible for (as measured in phase 1). "Whether received 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. 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: Attrition around the bunching window

	Firms with no irregularity in phase 1	**Firms with irregularity in phase 1**	**Diff** (2)-(1)
	(1)	(2)	(3)
Phase 1 loans around \$150k cut-off (\$136K-\$170K)			
Exit	0.65	0.61	-0.04***
			(0.01)
Total loans (N)	135,379	2565	137,944
Phase 1 loans below \$150K cut-off (\$136K-\$150K)			
Exit	0.64	0.62	-0.02
			(0.015)
Total loans (N)	63,766	1,071	64,837
Phase 1 loans above \$150K cut-off (>\$150K-\$170K)			
Exit	0.66	0.60	-0.06***
			(0.013)
Total loans (N)	71,613	1,494	73,107

Notes. Exit 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. Irregular firms are those that had either a multiple loan or an overpayment on their phase 1 loans. Firms with no irregularity are those that took out a single loan in phase 1 and did not have any overpayment on their loans. In Column (3) standard error is in parenthesis. 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: Parameterizations used in the model calibration exercise

Expression	**Parameterization**
$v(x)$	$\ln(x)$
$\pi(x)$	$x^2/2$
F_1	$U(1,70)$
F_0	$U(1,70)$
ζ	0.99
$\tilde{g}$	0.15
$\bar{g}$	2.0
N (number of firms)	11500000

Notes. $v(x)$ represents firm utility from program support. $\pi(x)$ is the (continuous) cost of procuring additional support for fraudulent firms. F_1 and F_0 are the distributions of taste parameters for legitimate and illegitimate firms, respectively. ζ is the proportion of legitimate firms in the population. $\tilde{g}$ is the threshold of government support that triggers screening. $\bar{g}$ is the maximum amount of support permitted by the program.

Table A.8: Estimates of screening-induced compliance costs

Panel A: Compliance Costs I: Estimates based on bunching firms

Sample:	Bunching firms		
	No irregularity in phase 1	Irregularity in phase 1	Diff (2)-(1)
	(1)	(2)	(3)
Avg. change in loan amount	-43,093.2	-128,017.7	-84924.45**
(loan amount P2 - P1)			(33707.43)

Panel B: Compliance Costs II: Estimates based on OMB reports

Time taken to complete the paperwork = 8 min or 0.133 hrs/application

Per hour cost= $45.5/hr

USD compliance cost per application = $6.05

Panel C: how many additional minutes of work would the addition of screening needed to have caused in order for the amount of fraud reduction to be equal to compliance costs?

Compliance costs = Fraud reduction

210,296 borrowers $\times$ (M min/60) $\times$ $45.5 per hour= $831.9 million

Time needed to comply with screening= 86.9 hours

10.9 working days

Notes. The sample is restricted to firms that received loans in both phase 1 and phase 2. Bunching firms are 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. Irregular firms are those that had either a multiple loan or an overpayment on their phase 1 loans. Firms with no irregularity are those that took out a single loan in phase 1 and did not have any overpayment on their loans. In Column (3) standard error is in parenthesis and these are clustered at the firm-level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01. The estimates for time taken to complete the application are from the Office of Management and Budget Report accessed at: `https://omb.report/icr/202101-3245-004/doc/107676901`. Time spent to complete the paperwork include "The estimated time for each applicant to review the form, gather the necessary information and complete the application is 8 minutes. These estimates are based on a sample testing by 1 or more individuals who were not familiar with the form." Per hour cost is estimated by OMB based on the salary for a GS-11, Step 1 Federal employee's annual salary, including fringe benefits.

Table A.9: Estimates of the amount of screening-induced fraud reduction

Panel A: Overpayments			
Sample	Exposed firms		Diff
	Phase 1	Phase 2	(2)-(1)
	(1)	(2)	(3)
Avg. overpaid amounts	4549.5	998.2	-3551.3***
			(175.9)
N	209,911	209,911	209,911
Total overpayment amount (Avg. overpaid $\times$ N)	\$955 mil	\$209.5 mil	-\$745.5 mil
Panel B: Multiple loans			
Sample	Exposed firms		Diff
	Phase 1	Phase 2	(2)-(1)
	(1)	(2)	(3)
Avg. amounts on multiple loans (excluding first loan)	639.8	229	-410.8***
			(78.9)
N	210,296	210,296	210,296
Total amount on multiple loans (Avg. amount $\times$ N)	\$134.5 mil	\$48 mil	-\$86.4 mil

Notes. The sample is restricted to firms that received loans in both phases of the program. 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. The average loan amount across multiple loans is computed by excluding the first loan in each phase. This calculation considers only the amounts disbursed for additional loans. 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. Standard errors are clustered at the firm level. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.10: Studies examining screening mechanisms in public programs

	Country	**Program**	**Population Targeted**	**Changes to Screening Method**	**Compliance Cost**	**Targeting**
	(1)	(2)	(3)	(4)	(5)	(6)
Alatas et al. (2016)	Indonesia	Social assistance	Households (poorest 5–10%)	Increase in ordeals: Interested households were required to go to a central registration site to take an asset test administered by the statistics office	Rp. 17,000 (US $1.70 per HH)	Targeting improved: Substantially poorer beneficiaries relative to automatic screening select into the program
Dupas et al. (2016)	Kenya	Chlorine water treatment	Parents of infants aged 6–12 months	Increase in ordeals: Relative to free delivery (i) charging a partially subsidized price (ii) combining free provision with a non-monetary cost (voucher redemption at a local shop)	N/A	Targeting improved: Combining free provision with a voucher screened out 88% of non-users while adding few exclusion errors. Rates of positive residual chlorine tests were almost identical when comparing free distribution & voucher provision.
Ashraf et al. (2010)	Zambia	Chlorine water treatment	Low-income peri-urban households	Increase in prices: Chlorine was sold at an offer (transaction) price at or below market price	N/A	Targeting improved: Holding transaction price constant, higher willingness to pay for Chlorine predicts greater use.

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	Country	Program	Population Targeted	Changes to Screening Method	Compliance Cost	Targeting
	(1)	(2)	(3)	(4)	(5)	(6)
Finkelstein and Notowidigdo (2019)	USA	Supplemental Nutrition Assistance Program (SNAP)	Low-income families	Reduction in ordeals: Provided information and assistance with application	$4,775 for L types, $425 for H types	Targeting worsened: Higher net income & healthier individuals enrolled, relative to the average enrollee in the control group
Deshpande and Li (2019)	USA	Disability Insurance	Individuals with disabilities	Increase in ordeals: Closings of Social Security Administration field offices which provided assistance with filing disability applications	$1,230 per person (forgo $670 due to congestion, $50 due to driving distance, & $510 in other costs of switching offices)	Targeting worsened: People with moderately severe conditions, low education levels, & low pre-application earnings discouraged (based on current govt. standards of eligibility)
Homonoff and Somerville (2021)	USA	Supplemental Nutrition Assistance Program (SNAP)	Low-income families	Increase in ordeals: Based on timing of recertification interview	No direct estimate, can be proxied by lost benefits: between $35-2300 for a single person HH	Targeting worsened: Cases that fail to recertify are no less needy than the average SNAP recipient, suggesting inefficient screening

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	Country	Program	Population Targeted	Changes to Screening Method	Compliance Cost	Targeting
	(1)	(2)	(3)	(4)	(5)	(6)
Muralidharan et al. (2025)	India	Public Distribution System (PDS) Provides food security to the poor	Poor households	Increase in ordeals: Required beneficiaries to have linked their ration cards with their Aadhaar numbers & to authenticate themselves via an ePOS device at the Fair Price Shops	Rp. 48 (US $0.73 per HH)	Targeting worsened: Eligible unseeded beneficiaries excluded, tend to be less educated; wealthier and better-educated HHs received differentially more value
Bhargava and Manoli (2015)	USA	Earned Income Tax Credit (EITC)	Low- to moderate-income workers and families	Reduction in ordeals: By increasing the frequency, salience, and simplicity of information about provided benefits	Non-claimants forgo $1,096	Targeting improved: Simplification helped low earners, those with dependents, & single female filers (fraud was minimal irrespective of ordeals)
Gray (2019)	USA	Supplemental Nutrition Assistance Program (SNAP)	Low-income families	Reduction in ordeals: Online case management tool to simplify process	Non-claimants forgo $150–325 per month	No change in the share of clearly ineligible beneficiaries, but eligible childless adults and those with some earnings are more likely to benefit

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	Country	Program	Population Targeted	Changes to Screening Method	Compliance Cost	Targeting
	(1)	(2)	(3)	(4)	(5)	(6)
Rossin-Slater (2013)	USA	Special Supplemental Nutrition Program	Low-income pregnant and postpartum women and children under age five at nutritional risk	Reduction in ordeals: Based on geographical distance to the WIC clinics	N/A	Targeting improved: The effects are higher for mothers with a high school education or less, who are most likely to be eligible for WIC
Kopczuk and Pop-Eleches (2007)	USA	Earned Income Tax Credit (EITC)	Low- to moderate-income workers and families	Reduction in ordeals: Electronic tax filing programs	N/A	Does not provide analysis of differential effects by need. However, take-up increased i.e., electronic filing led to an increase in EITC participation
Cohen and Dupas (2010)	Kenya	Antimalarial insecticide-treated bed nets (ITNs)	Pregnant women most vulnerable to complications from malaria	Increase in prices: Cost-sharing was introduced i.e., women were charged a subsidized, positive price rather than getting the ITN for free	N/A	No effect on taregting: Prenatal clients who pay positive prices for an ITN were no sicker, at baseline, than the clients at the control clinics

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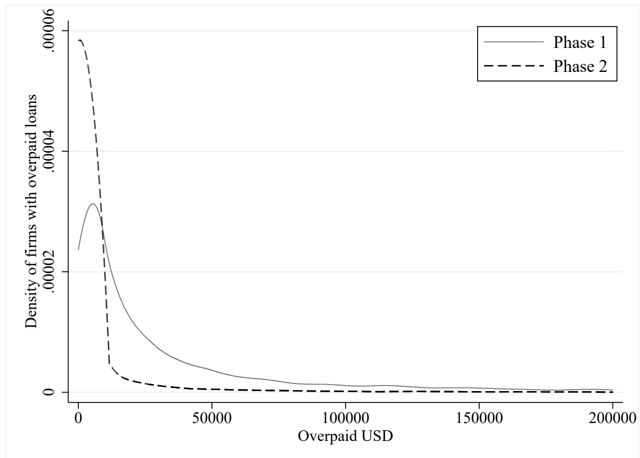
	Country	Program	Population Targeted	Changes to Screening Method	Compliance Cost	Targeting
	(1)	(2)	(3)	(4)	(5)	(6)
Bettinger et al. (2012)	USA	Free Application for Federal Student Aid (FAFSA)	Low-income individuals	Reduction in ordeals: Assistance and information to complete FAFSA for the individuals themselves or their children	Non-claimants forgo on avg $1,100 - $5,833	Does not provide analysis of differential effects by need. However, take-up increased i.e., assistance and information increased FAFSA submissions and ultimately the likelihood of college attendance, persistence, and aid receipt by low-income individuals

Notes: This table summarizes studies that examine how changes in ordeals or prices affect program targeting and take-up. "N/A" indicates that compliance costs were not reported.

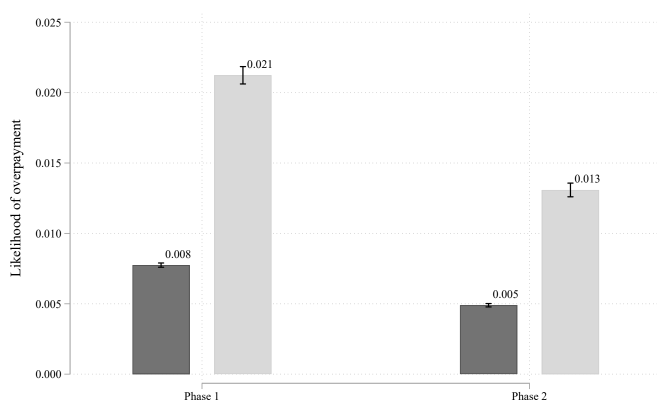
2 Appendix figures

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



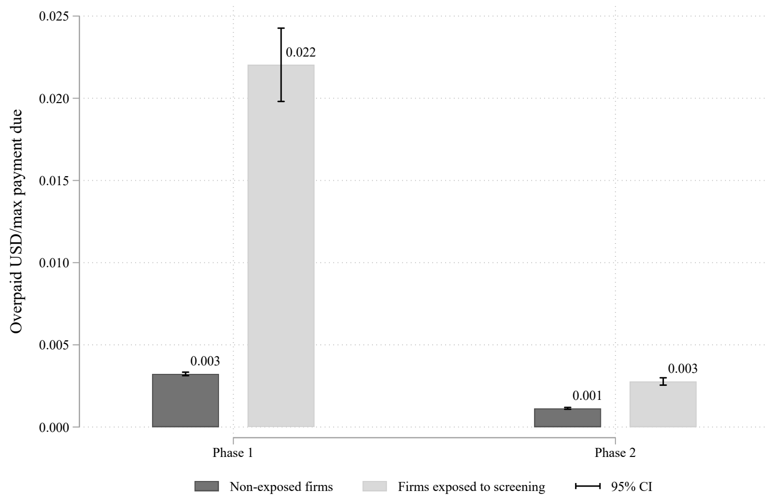
Notes. Data is at the firm-phase level, restricted to firms that applied in both phases and had at least one overpayment in phase 1. Overpaid amount on a loan is the approved dollar value less the maximum payment due. For each firm that took out more than one loan in a phase, the overpaid amount is the total overpaid value across all the loans the firm was approved for in that phase.

Figure B.2: Probability that a firm was overpaid by treatment status



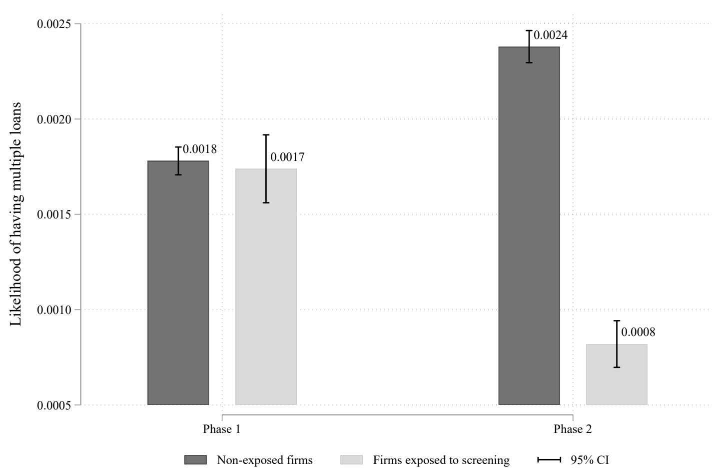
Notes. Data is at the firm-phase level. The sample is restricted to firms that took out loans in both phase 1 and phase 2.

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



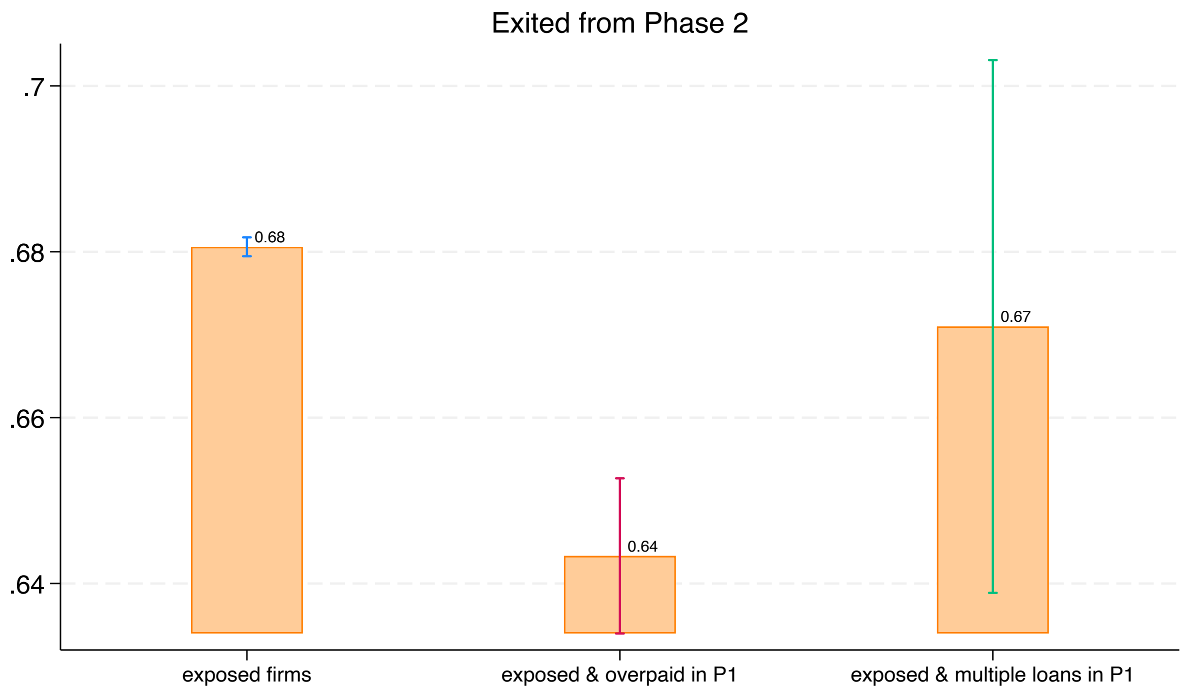
Notes. Data is at the firm-phase level. For firms with multiple overpaid loans in a phase, the overpayment rate plotted is the maximum rate among all those loans. The sample is restricted to firms that took out loans in both phase 1 and phase 2.

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



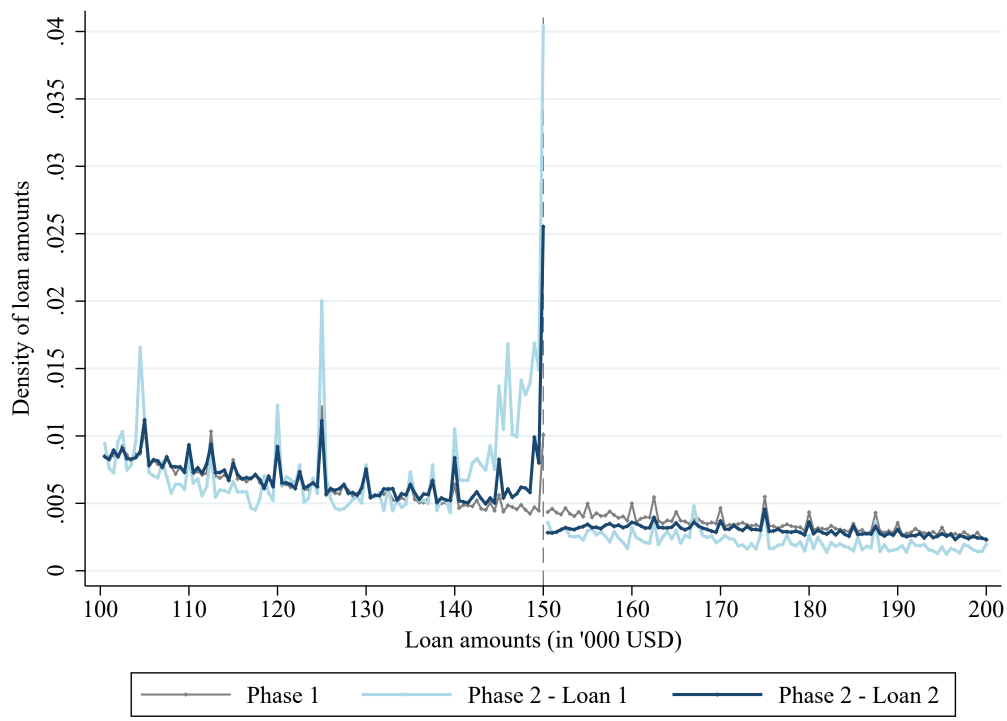
Notes. Data is at the firm-phase level. The sample is restricted to firms that took out loans in both phase 1 and phase 2.

Figure B.5: Did screening affect fraud through the extensive margin?



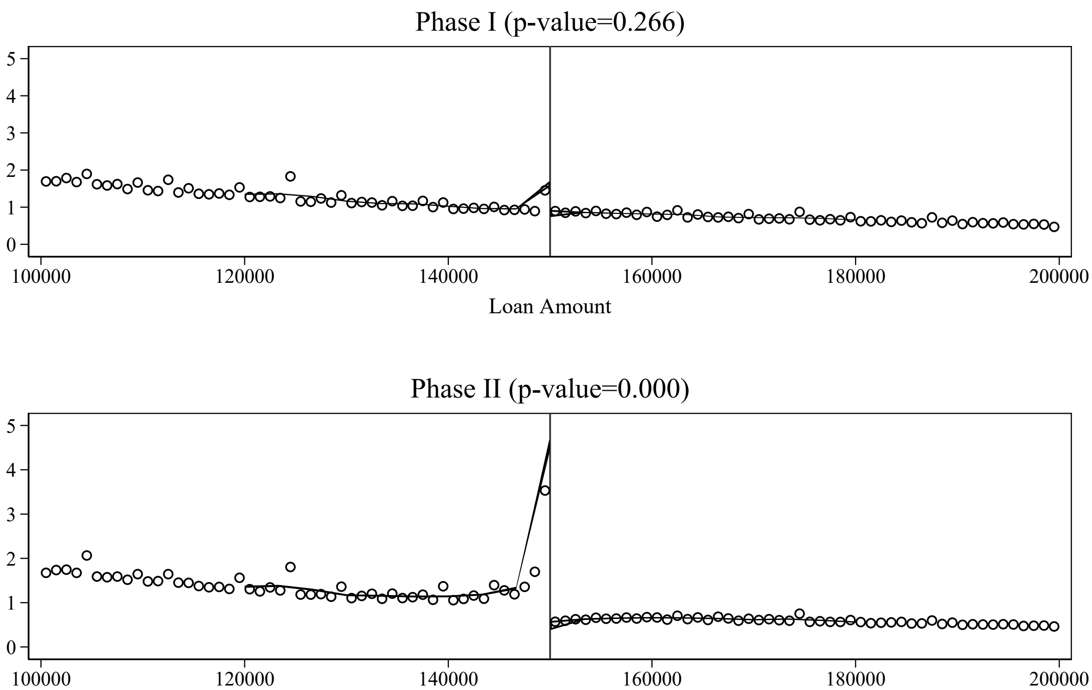
Notes. This graph visualizes column (1) from Table A.3a and Table A.3b. The unit of observation is the firm. The sample is restricted to those that received loans in both phase 1 and phase 2. Exited from phase 2 is a dummy variable that takes a value of 1 when a firm showed up only in phase 1, and 0 otherwise. Exposed firms are those with loans greater than $150,000 in phase 1 that were subject to the upfront documentation requirement. "Overpaid in P1" 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 P1" 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. 95% confidence intervals using standard errors clustered at the firm level are shown in blue.

Figure B.6: Empirical density of loan amounts across the two phases of the PPP



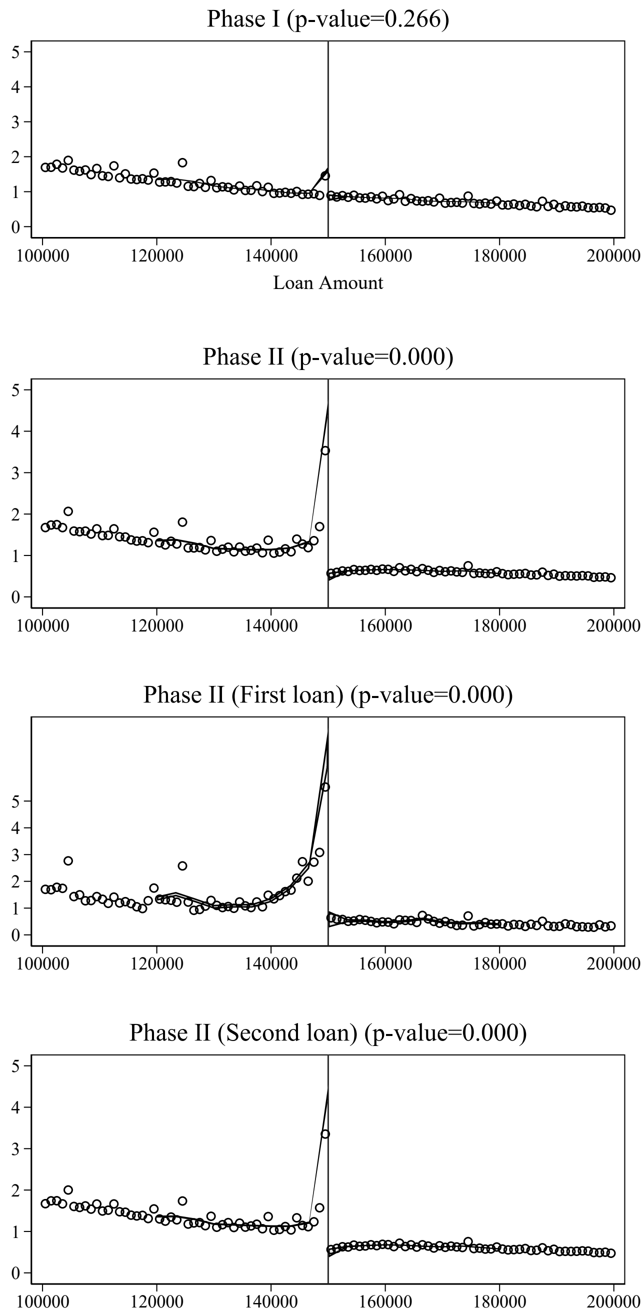
Notes. This figure plots the empirical density of loan amounts. Phase 2 loans are separated into First Draw loans ("Phase 2 – Loan 1") and Second Draw loans ("Phase 2 – Loan 2"). The vertical dashed line marks the $150,000 threshold at which upfront documentation was required. The bin width is $500.

Figure B.7: Distribution of approved loan amounts in phase I and phase II of PPP



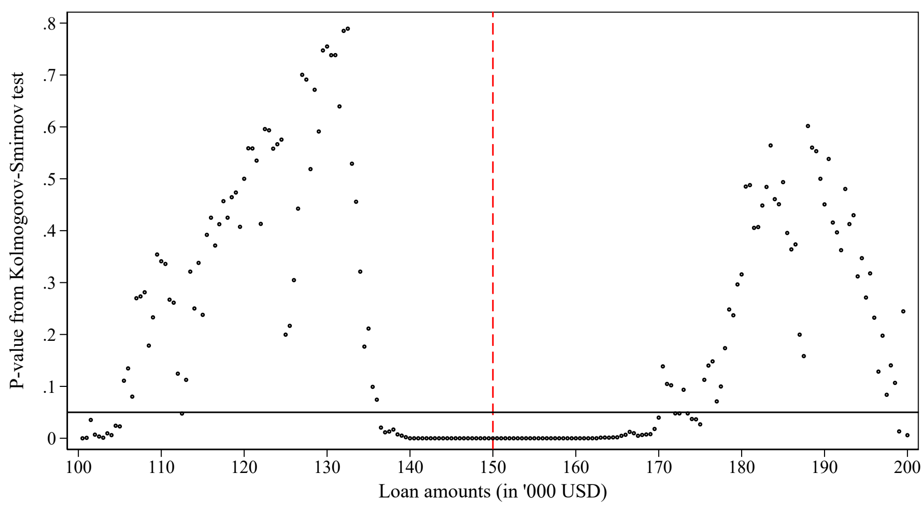
Notes. The *p*-value is from a McCrary Density Test (McCrary, 2008) of continuity of densities at the $150K cut-off

Figure B.8: Distribution of approved loan amounts in phase I and phase II of the PPP



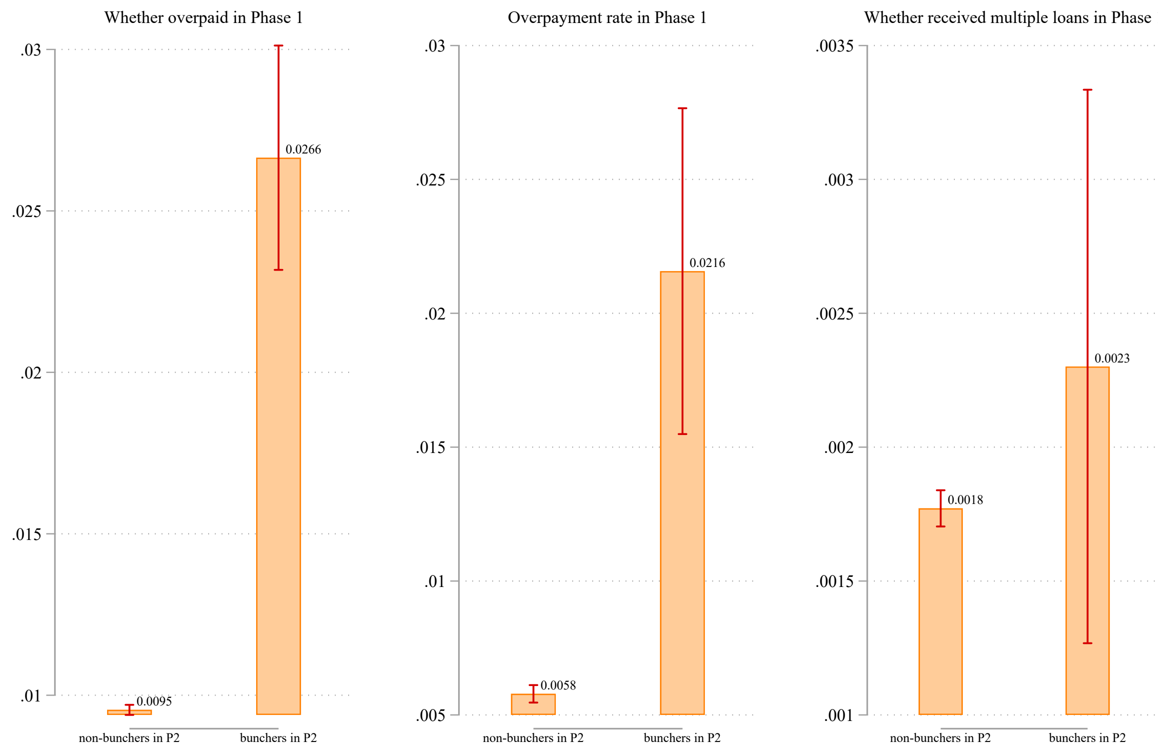
Notes. The p-value is from a McCrary Density Test (McCrary, 2008) of continuity of densities at the $150K cut-off

Figure B.9: 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.

Figure B.10: Are fraudulent firms more prevalent among bunchers than non-bunchers?



Notes. This figure visualizes Table 3. 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. "Bunchers in P2" 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 in P2" 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. "Whether 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. "Overpayment rate in Phase 1" classifies firms as irregular based on their overpayment rate, which is defined as the overpaid amount divided by the maximum payment that a firm was eligible for (as measured in phase 1). "Whether receoved 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. 95% confidence intervals using standard errors clustered at the firm level are shown in blue.

Figure B.11: The behavior of bunching and non-bunching firms across phases

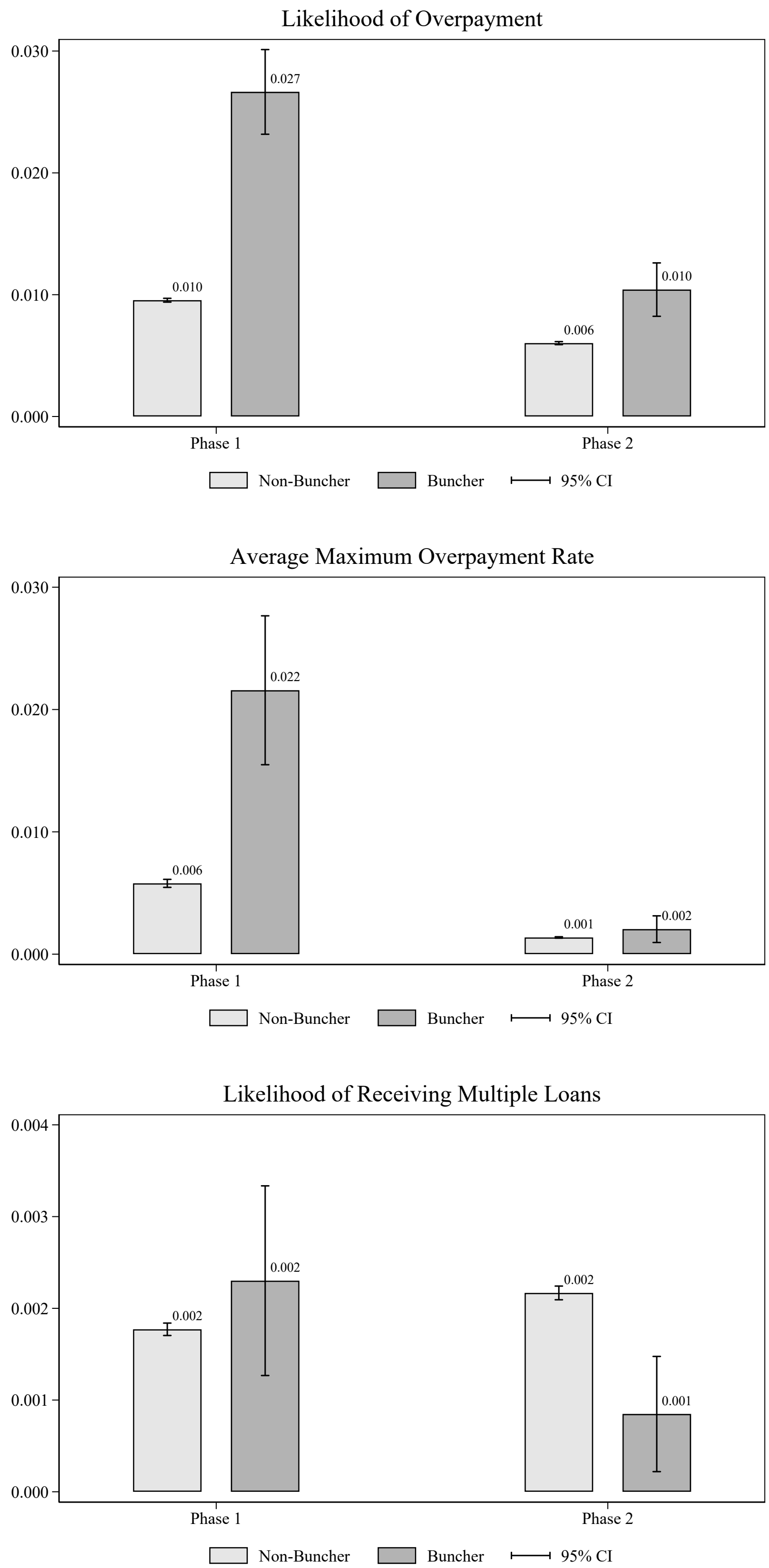
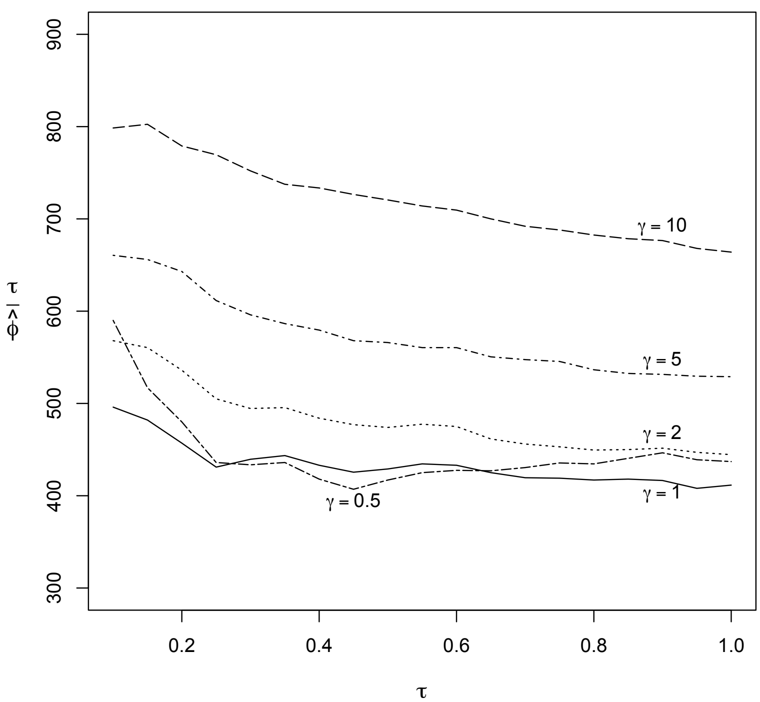


Figure B.12: Extensions of the model calibration exercise



Notes. The figure displays extensions of the model calibration exercise described in the text to scenarios in which the true proportion of illegitimate firms deviates from the observed proportion of overpaid firms in phase one. In each extension, the overall proportion of illegitimate firms is set to $\gamma \times 0.01$ (the proportion of overpaid firms in phase one) and the proportion of illegitimate firms among bunchers is set to $\gamma \times 0.027$ (the proportion of overpaid firms among bunchers).

3 Further Details on Firm Matching Across Phases

3.1 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.[33] We used a similarity score cut-off of 0.7.[34] 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.[35] To verify the accuracy of these matches, we randomly sampled 1000 firms out of 2078901 total firms.[36] 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.

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

[33] 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.

[34] 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.

[35] 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.

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

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[37] 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.

3.3 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 A.3a and Table A.3b "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 3.2, 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. Table C.1-Table C.3 present the results including this sample. The results are substantively identical to the main results presented in Tables 2-3.

We next followed the methodology suggested by Hausman et al. (1998) for consistently estimating effects when the dependent variable has misclassification error.[38] 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 $P(Exit = 0|Exit^* =$

[37]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/

[38]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).

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:

$$\begin{aligned} E(Exit|X) &= P(Exit|X) \\ &= P(Exit^* = 1|X)P(Exit = 1|Exit^* = 1) + P(Exit^* = 0|X)P(Exit = 1|Exit^* = 0) \\ &= F(X`\beta)(1 - \alpha_1) + (1 - F(X`\beta))\alpha_0 = \alpha_0 + (1 - \alpha_0 - \alpha_1)F(X`\beta) \end{aligned} \quad (7)$$

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 3. We can estimate Equation 7 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 A.3a and Table A.3b.

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 robustness of the main results 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 bunching firms disproportionately composed of bad actors?

Dependent Variable:	**Firms with irregular loans**		
	Whether Overpaid in Phase 1	Overpayment Rate in Phase 1	Whether received Multiple Loans in Phase 1
	(1)	(2)	(3)
Bunching firms in Phase 2	0.017***	0.016***	0.00052
	(0.0018)	(0.0031)	(0.00052)
Constant	0.0096***	0.0058***	0.0017***
	(0.000079)	(0.00017)	(0.000034)
Observations	1532289	1532289	1532289

Note: The table shows robustness of the main results after inclusion of an additional subsample of firms identified by the geocoding algorithm. 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 firms 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. "Whether 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. "Overpayment rate in Phase 1" classifies firms as irregular based on their overpayment rate, which is defined as the overpaid amount divided by the maximum payment that a firm was eligible for (as measured in phase 1). "Whether received 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. 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.

4 Other measures of fraud

In this section we examine alternative measures of fraud, including those presented in Griffin et al. (2023).

We begin by comparing our measures to two measures utilized therein: abnormally high implied compensation per employee relative to the industry benchmark[39] and large inconsistencies in jobs reported with another government program, i.e., the COVID-19 Economic Injury Disaster Loan (EIDL) Advance.[40]

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[41], treatment effects estimated using the aforementioned measures would provide little insight into the aggregate affects of screening.

Note that other red flags identified by Griffin et al. (2023) cannot be used for our application since they are aggregate measures or because they do not apply to the \$150,000 threshold. This is true of discontinuities around the \$100,000 compensation, or Suspicious Activity Reports (SARs) which are at the county level and not linked to specific PPP loan amounts, or over-representation of loans within industry-county pairs and loan clustering (which are geographic measures).

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

[39] 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.

[40] 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`.

[41] 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.

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.[42] 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.

We recognize that 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 these limitations, we donot 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.

[42] 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 compensation as 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.

5 Robustness Checks

Robustness using the "more credible approach". Following recent developments in differences-in-differences estimation (see Roth et al. (2023) for a review and Manski and Pepper (2018) for foundational work on bounded-variation restrictions and partial identification), we implement the approach to parallel trends proposed by Rambachan and Roth (2023), which relaxes exact parallel trends by allowing for bounded deviations.

Rambachan and Roth (2023) emphasize that restrictions on potential violations of parallel trends must be specified by the researcher and grounded in the economic context. In our setting, the most plausible threats arise from gradually evolving administrative behavior and reporting practices rather than from discrete shocks that differentially affect treatment and control units at specific points in time. This institutional context suggests that any violations are likely to evolve smoothly.

We then discipline this intuition using evidence from the pre-treatment data. Appendix Figure E.1 and Figure E.2 summarize the pre-period evolution of treated–control differences and inform our choice of the smoothness class used in the sensitivity analysis.[43] The linear projections shown in Appendix Figure E.1 and Figure E.2 summarize the magnitude and structure of pre-treatment deviations and are intended only to visualize pre-period dynamics, not to assume that the true counterfactual path is linear. Overlaying a linear projection estimated from the pre-treatment period shows that the treated–control difference evolves gradually and without discrete breaks. There is little evidence of discrete pre-period breaks, particularly for the overpayment dummy (Figure E.1, bottom panel) and multiple loans (Figure E.2).

We therefore implement the smoothness restriction $\Delta^{SD}(M)$ of Rambachan and Roth (2023), which formalizes the idea that deviations from parallel trends evolve gradually rather than through discrete shocks. The diagnostic evidence above suggests that the treated–control differential follows an approximately linear path in the pre-period. The sensitivity analysis treats this linear extrapolation as a benchmark and allows bounded departures from it.

Formally, we bound changes in the slope of the differential trend between treated and control firms as

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

where δ_t denotes the treated–control difference in trends at time t, and M controls the maximum curvature relative to a linear extrapolation of the pre-trend. When $M = 0$, the differential trend is exactly linear; values $M > 0$ relax exact linearity by permitting bounded deviations.

The estimates for the probability of overpayment and the overpayment rate are summarized in Figure E.3, while the estimates for multiple loans are shown in Figure E.4. The underlying data are at the loan–day level, whereas treatment is defined at the firm level. The figures show that the conclusions for overpayment outcomes remain robust for at least $M = 0.005$, and for the multiple loans outcome for at least $M = 0.0025$.

As emphasized by Rambachan and Roth (2023), the data alone cannot place an upper bound on M, so benchmarking requires additional structure. Following Dustmann et al. (2022), we construct a reference value for M using the observed dispersion of pre-period deviations from the linear trend used to summarize phase 1 dynamics. Specifically, we compute the median absolute deviation of the coefficients ρ in Figure E.1 and Figure E.2 relative to the phase 1 linear projection. This

[43] These figures plot the coefficient ρ from Equation 1 for each month in phase 1 and 2 of the program. The black line is a linear trend using only phase 1 data.

procedure yields benchmark values of $M = 0.0015$ for the overpayment dummy, $M = 0.0026$ for the overpayment rate, and $M = 0.001$ for the multiple loans outcome. These benchmarks are below the values of M at which the treatment effects lose significance, indicating that the results are robust to deviations at least as large as the median pre-period fluctuation.

We also report robustness using the relative-magnitude class of restrictions, $\Delta^{RM}(\overline{M})$, which provides a useful complementary stress test allowing for less smooth (shock-type) violations. Figure E.5 reports results under this alternative restriction. As expected, the bounds widen under this more permissive class. The overpayment effects remain robust over a wider range of admissible deviations than the multiple-loan outcome.

Other checks. In this section we present further checks that support a causal interpretation of our estimates. Appendix Table E.2 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*.[44] Among firms with Phase 1 loans between \$100K and \$200K, those just above and below the \$150K threshold are likely similar in fundamentals and baseline fraud risk. The results are consistent with those in Table 2, albeit of smaller magnitude. 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.[45]

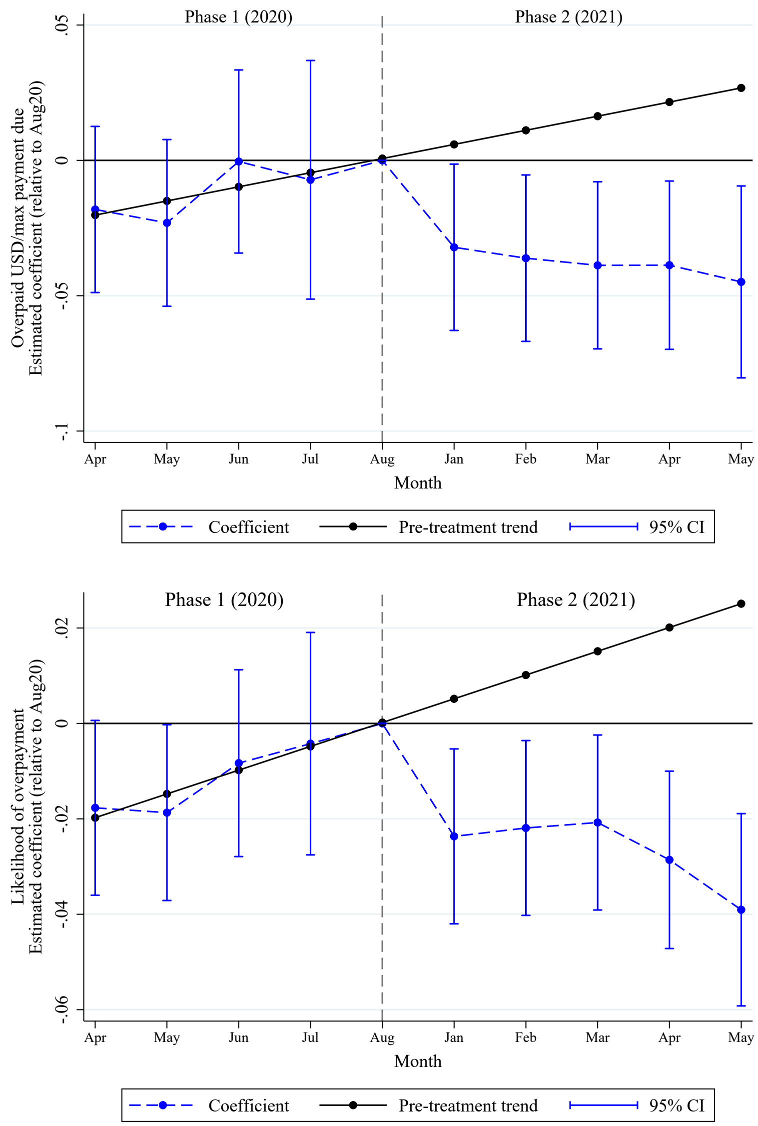
In this context one might also be worried about anticipation effects of an upfront documentation requirement based on loan size. However, the documentation requirement based on the \$150K threshold was not known during Phase 1; it was announced on January 6, 2022, right before the start of Phase 2 (January 14, Figure I). Accordingly, no anticipation effects are visible in Figure 2a, Figure 2b or Figure 2c.

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 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. Details of this analysis are in subsection 4.4 and subsection 6.1.

[44] 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.

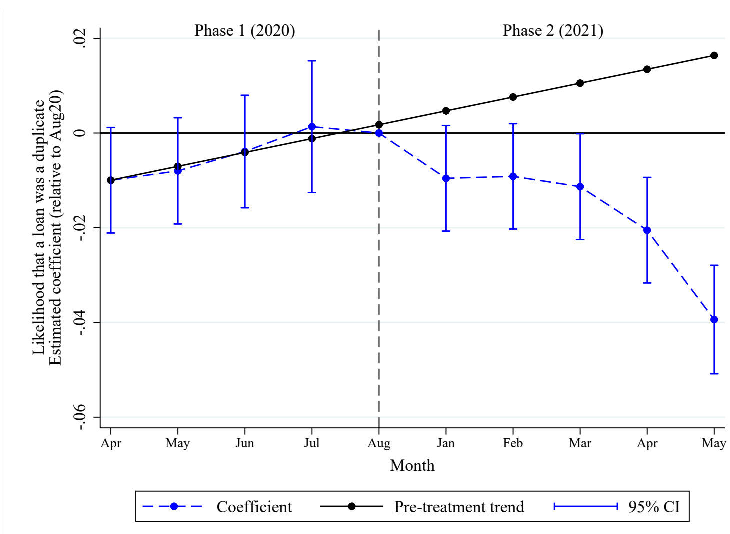
[45] For instance, because the overpaid loan calculations uses a maximum annual salary threshold of \$100,000, the loan overpayment variables might classify larger business with higher annual salaries as fraudulent rather than smaller ones.

Figure E.1: Event study plot for the overpaid amount (in USD) as a fraction of maximum payment due per firm (top panel) and the probability that a firm was overpaid (bottom panel)



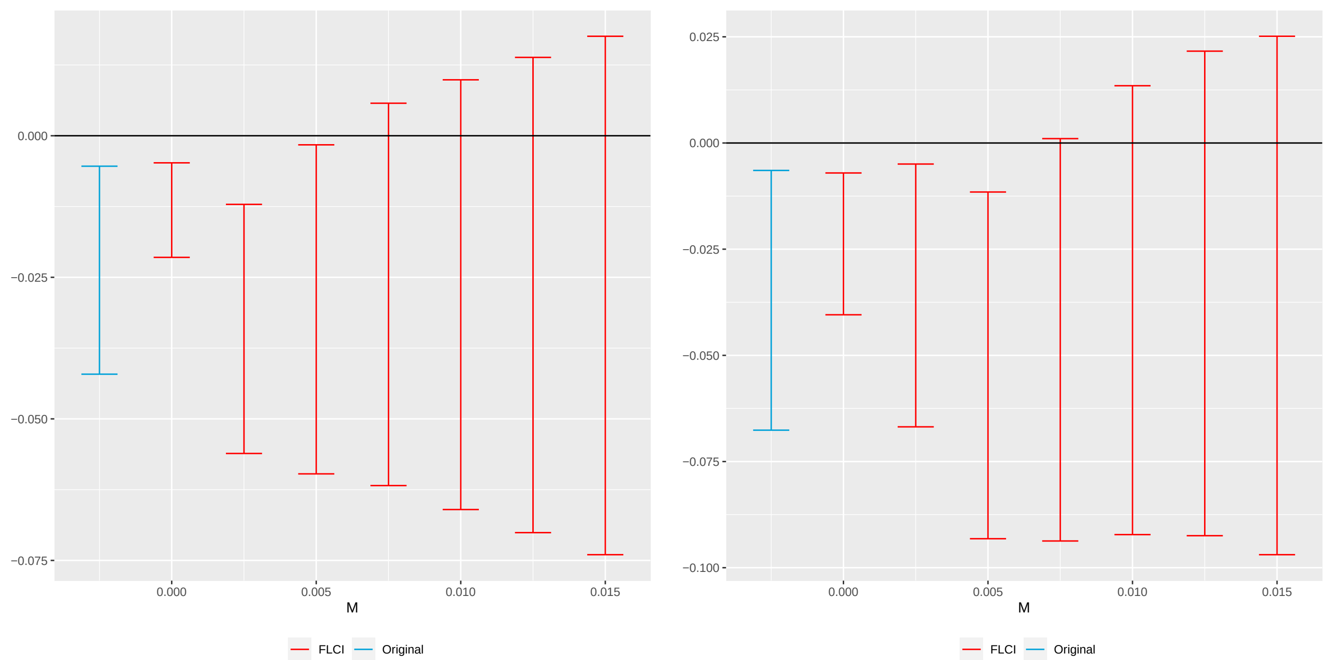
Notes. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown in Equation 1. The treatment group consists of those firms with at least one loan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

Figure E.2: Event study plot for the probability that a loan was a duplicate



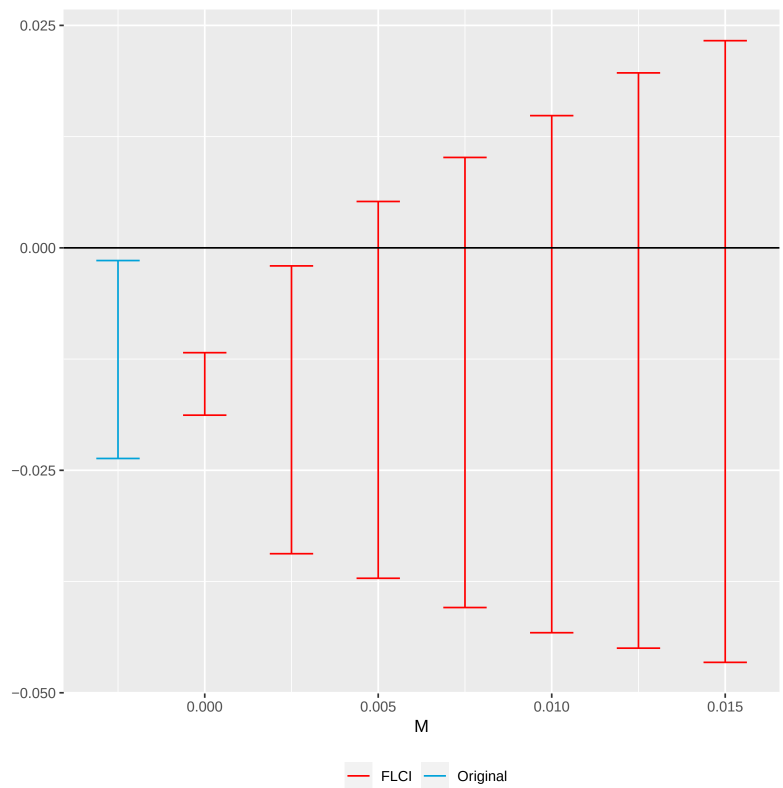
Notes. A duplicate loan is defined as any loan that is not the first loan issued to a firm. Data is at the loan-date level, restricted to firms that took out loans in both phase 1 and phase 2. Each coefficient is obtained from the interaction terms between treatment and the corresponding month as shown 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 E.3: Effects on the probability that a firm was overpaid (left panel) and the overpaid amount (in USD) as a fraction of maximum payment due per firm (right panel)- Robustness of the DID estimates using the "more credible approach" suggested by (Rambachan and Roth, 2023)



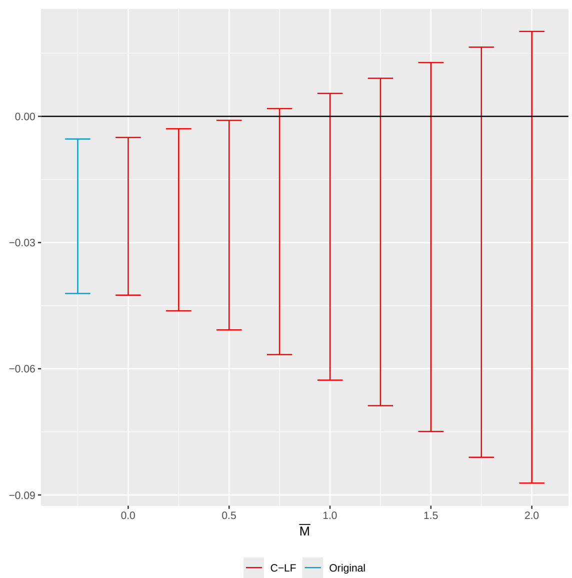
Notes. FLCI refers to fixed length confidence intervals. The blue line is the confidence intervals for the coefficient obtained from the interaction term between treatment and phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that 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 E.4: 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)

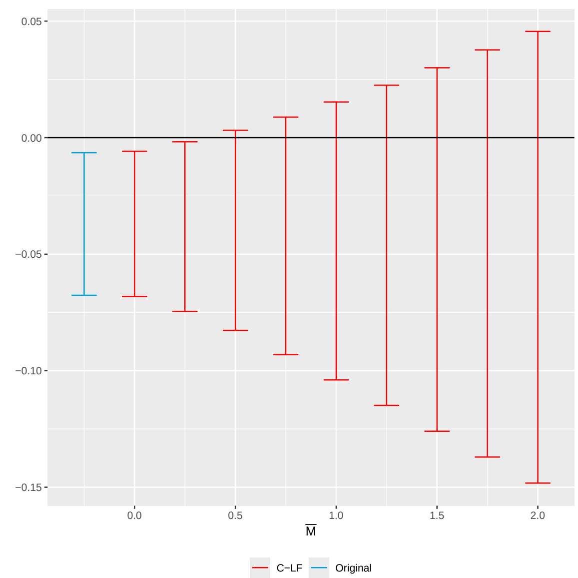


Notes. FLCI refers to fixed length confidence intervals. The blue line is the confidence intervals for the coefficient obtained from the interaction term between treatment and phase 2 in an event-study regression similar to Equation 1. The only difference between this regression and Equation 1 is that 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.

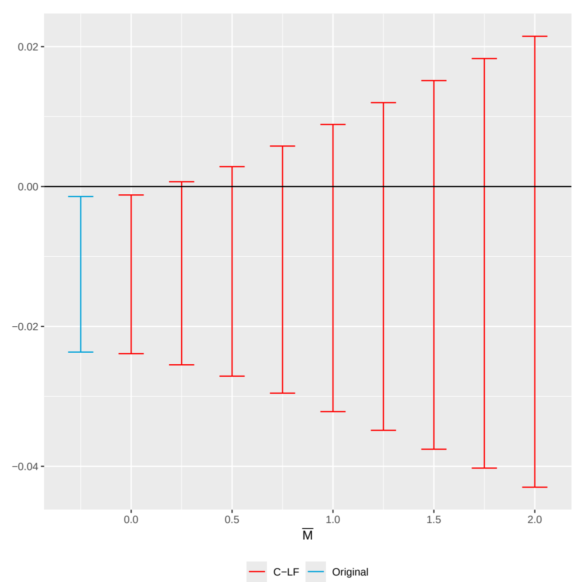
Figure E.5: Rambachan and Roth (2023) relative-magnitude sensitivity bounds under $\Delta^{RM}(\overline{M})$.



(a) Overpayment dummy



(b) Overpayment rate



(c) Multiple loans

Notes: C–LF are the conditional least favorable hybrid confidence sets constructed under the relative-magnitude restriction $\Delta^{RM}(M)$ following Rambachan and Roth (2023). The blue line shows the original confidence interval for the coefficient obtained from the interaction term between treatment and phase 2 in an event-study regression analogous to Equation 1. In this specification, the treatment indicator interacts with a phase 2 indicator rather than individual post-treatment months. Exposed firms are those with at least one loan greater than \$150,000 in phase 1 and that were subject to the advance documentation requirement in phase 2. The relative-magnitude restriction allows post-treatment deviations from parallel trends to be bounded relative to the largest deviation observed in the pre-treatment period, scaled by M.

Table E.2: 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	0.0029***		0.0025***		0.00026**	
	(0.00062)		(0.00093)		(0.00012)	
Phase 2	-0.0051***	-0.0051***	-0.0077***	-0.0077***	-0.000038	-0.000038
	(0.00037)	(0.00053)	(0.00046)	(0.00065)	(0.000073)	(0.00010)
Exposed firms × Phase 2	-0.0018***	-0.0018*	-0.0030***	-0.0030**	-0.00016	-0.00016
	(0.00069)	(0.00097)	(0.00094)	(0.0013)	(0.00013)	(0.00018)
Control mean of outcome	0.012	0.012	0.0099	0.0099	0.00038	0.00038
Observations	322752	322752	322752	322752	322752	322752
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. If they had multiple loans in phase 1, they are part of the sample if more than half of the loans in phase 1 were between $100K and $200K. Overpayment dummy is a dummy variable that takes a value of 1 when a firm received any overpayment in a phase, and 0 otherwise. Overpayment rate on each loan is the overpaid amount divided by the maximum payment that a firm was eligible for. For firms with multiple overpaid loans, the regression was run with the maximum overpayment rate. Multiple loans dummy is a dummy variable that takes a value of 1 when a firm is identified to have been approved for more than one loan, and 0 otherwise. 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.

6 Optimal Loan Requests in the Value-Based Screening Model

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:

$$g_i^{1*}(\eta_i) = \begin{cases} \widehat{g}_i \equiv g_i | v'(g_i) = \eta_i & \text{if } \widehat{g}_i \leq \overline{g} \\ \overline{g} & \text{if } \widehat{g}_i > \overline{g} \end{cases}, \tag{8}$$

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

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

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

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

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

$$\begin{aligned} \eta_U^1 &\equiv \eta_i | g_i^{1*} = \widetilde{g} \\ \eta_U^0 &\equiv \eta_i | g_i^{0*} = \widetilde{g}. \end{aligned} \tag{10}$$

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:

$$u_i(\widetilde{g}) = \begin{cases} v(\widetilde{g}) - \eta_i \widetilde{g} & \text{if } s_i = 1 \\ v(\widetilde{g}) - \pi(\widetilde{g}) - \eta_i \widetilde{g} & \text{if } s_i = 0 \end{cases} \tag{11}$$

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 (8). 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:

$$g_i^{0*}(\eta_i, \tau) = \begin{cases} g_i^{\ddagger} \equiv g_i | v'(g_i) - (1+\tau)\pi'(g_i) = \eta_i & \text{if } g_i^{\ddagger} \leq \overline{g} \\ \overline{g} & \text{if } g_i^{\ddagger} > \overline{g} \end{cases} \tag{12}$$

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

$$\begin{aligned} \eta_L^1 &\equiv \eta_i | u_i(g_i^{1*}(\eta_i)) = u_i(\widetilde{g}) \\ \eta_L^0 &\equiv \eta_i | u_i(g_i^{0*}(\eta_i, \tau)) = u_i(\widetilde{g}) \end{aligned} \tag{13}$$

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

$$\Delta = \zeta[F_1(\eta_U^1) - F_1(\eta_L^1)] + (1-\zeta)[F_0(\eta_U^0) - F_0(\eta_L^0)], \tag{14}$$

resulting in a concomitant reduction in the mass of requests above $\widetilde{g}$.

6.1 Proof of Proposition 1

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 (13), 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.

7 Extensive margin of exit

The analysis of extensive margin responses by firms in subsection 4.4 examines whether the imposition of the screening threshold of \$150,000 led firms that behaved fraudulently in Phase 1 to disproportionately exit in Phase 2. This analysis relies on our matching of firms across phases, which might have issues (discussed in Appendix 3). Moreover, one might independently want to know whether screening led to exit by *any* type of firms, not just whether fraudulent firms exited disproportionately when compared to non-fraudulent firms (see Table A.6).

To address these issues, we conduct a complementary exercise examining the extensive margin of firm response, based on the bunching framework from Cengiz et al. (2019). This paper examines the impact of state-level minimum wage law changes on employment as well as firm exit. In our context, the basic idea is to estimate the extensive margin effect (Δe) as the sum of the "excess mass" (Δa) of observations appearing at the threshold (in their case the new minimum wage, in our case the \$150,000 screening threshold) and the "missing mass" (Δb) disappearing from above the threshold. In our setting, a non-negative net effect would indicate that firms strategically changed loan sizes to avoid screening (pure intensive margin response), while a negative net effect would indicate that screening caused firms to exit the program.

While Cengiz et al. (2019) have multiple (138) changes in minimum wages across states over a long period to estimate impacts of minimum wages on firm exit, we only have one change. To implement this test while conducting inference, we adapted their binwise mass-based estimator to a loan-level probability model.

From the universe of all loans, we first drop first draw loans in the second phase, as these are not part of our main analysis. We then restrict analysis to loans between \$100,000 and \$200,000 in both phases; these are the loans most likely to be affected by the screening threshold. We define an "affected window" that encompasses both the bunching region (excess mass) and the missing region (missing mass). Based on the distribution visualized in Figure 7 and the formal test in Figure B.9, we define this window as \$136,000 to \$170,000. We then estimate the following linear probability model:

$$\mathbb{I}(Affected_{it}) = \alpha + \beta \text{Phase2}_t + \mathbf{X}_i \gamma + \epsilon_{it} \tag{15}$$

where $\mathbb{I}(Affected_{it})$ is an indicator that takes the value of 1 if a loan falls within the \$136,000–\$170,000 range. X_i denotes firm or lender fixed effects, in certain specifications. The coefficient β directly tests the net effect on firm exit.

The results are reported in Table G.1. We find that the coefficient β is very small and positive, with a point estimate of 0.012 (s.e. 0.001). We perform various types of robustness checks — adding in lender fixed effects/ clustering by lender, adding in firm fixed effects/ clustering by firm (this removes some loans from the analysis since we are not able to match all loans across phases), expanding the range of the potentially affected loans, making the "bins" around the screening threshold symmetric — but the coefficient remains very small in magnitude and positive in sign, suggesting that the screening threshold did not lead to large-scale exit.

Table G.1: Changes in loan distribution at screening threshold

	(1)	(2)	(3)	(4)	(5)	(6)
Phase 2	0.012***	0.052***	0.012***	0.019***	0.008***	0.010***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Control mean of outcome	0.302	0.302	0.302	0.257	0.111	0.095
Affected Range	136k-170k	136k-170k	136k-170k	136k-164k	136k-170k	136k-164k
Analysis Window	100k-200k	100k-200k	100k-200k	100k-200k	50k-250k	50k-250k
Firm FE	No	Yes	No	No	No	No
Lender FE	No	No	Yes	No	No	No
Observations	692476	333966	692474	692476	1862657	1862657

Notes: This table presents estimates of the extensive margin (program exit) response to the $150,000 screening threshold, adapting the bunching methodology of Cengiz et al. (2019) to a loan-level linear probability model. The dependent variable is an indicator equal to 1 if the loan amount falls within the "affected window" of $136,000 to $170,000, which encompasses both the observed bunching region (excess mass, Δa) and the hollowed-out region (missing mass, Δb). The primary independent variable is an indicator for Phase 2 of the program. The main specifications restrict the sample to loans between $100,000 and $200,000. Other specifications show robustness to both the "affected window" and sample range. The main specification presents robust standard errors; other specifications cluster at the firm and lender levels, as indicated. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.