What (Small) Firms Don't Do With Cash Windfalls*

Gabriel Chodorow-Reich
Harvard University

Adam Isen
Johns Hopkins University

Adi Sunderam
Harvard University

Eric Zwick
University of Chicago

March 2026

Abstract

During the COVID recession and recovery, forgivable loans and cash grants to firms became a large-scale business cycle policy. We study the effect of such programs on small firms using two programs that together disbursed $125 million in grants to 12,129 U.S. businesses via random lottery. Our data set links both program applicants and awardees to their business tax records and to their owners' credit bureau records. We find *negative* point estimates for the effect on firm employment, payroll, and investment and can reject with 95% confidence increases in payroll of more than $0.20 per dollar and in investment of more than $0.07 per dollar received. We also find a precise zero effect on firm survival. These results cast doubt on the notion that small and micro businesses are financially constrained and have high marginal propensities to invest or hire out of cash flow shocks. In contrast, we find evidence that some firm owners use the grants to acquire an automobile or to bolster personal finances.

*Manuscript date: March 26, 2026. We are indebted to Brent Carpenter, Thu-Mai Ho-Kim, and Brandon Toner of the Minnesota Department of Employment and Economic Development for their help in understanding the SBRG and MSCRG programs and data files and to Sage Belz and Henry Weiland for excellent research assistance. Financial support was provided by the Washington Center for Equitable Growth. Disclaimer: All data work for this project involving confidential taxpayer information was done at IRS facilities, on IRS computers, and at no time was confidential taxpayer data ever outside of the IRS computing environment. The views expressed herein are those of the authors and do not necessarily represent the views of the IRS or the U.S. Department of the Treasury. All results have been reviewed to ensure that no confidential information is disclosed.

1 Introduction

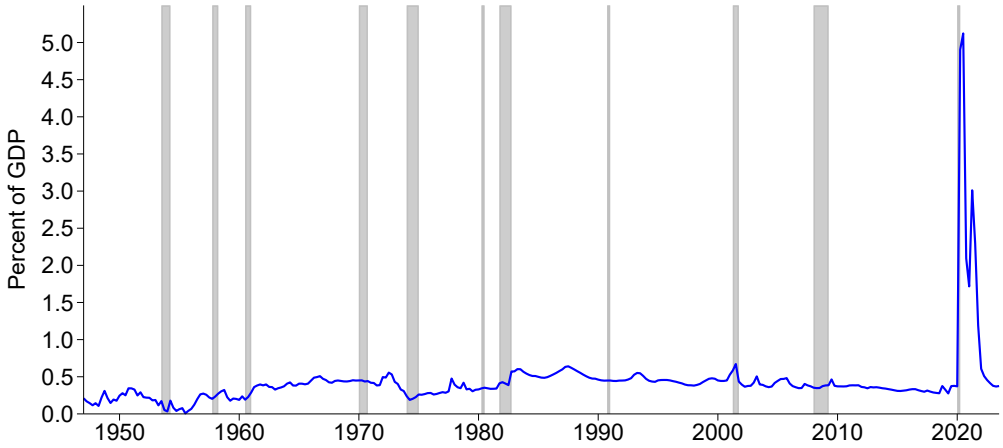
The 2020 recession introduced a new business cycle management tool: cash transfers to businesses. Figure 1 shows the unprecedented scale of this policy, which reached more than 5% of GDP after never exceeding 0.7% in the post-war history of the United States. Financial frictions were one key motivation for such subsidies. In frictionless capital markets, viable firms facing recession-induced declines in revenue can raise external financing to cover their fixed costs until the economy recovers. However, if firms are financially constrained, as empirical evidence starting with Fazzari, Hubbard and Petersen (1988) suggests, then aid to businesses may help avoid inefficient liquidations while also indirectly stimulating the macroeconomy by raising payroll and investment (Hanson, Stein, Sunderam and Zwick 2020).

This argument may apply with particular force to micro and small firms. Such firms are often thought to face the most severe financial constraints (e.g., Kaplan and Zingales 1997, Whited and Wu 2006, Chodorow-Reich 2014, Zwick and Mahon 2017), in part motivating the focus of pandemic-era business aid programs on relatively small firms. The largest of these programs was the federal government's Paycheck Protection Program (PPP), which provided more than 11 million forgivable loans totaling nearly $800 billion, reaching nearly the entire eligible population of businesses with fewer than 500 employees.[1] The PPP had the explicit aim of increasing payroll at recipient firms. Yet, the near-universal take-up makes it poorly suited for identifying the causal impact of the program, especially in the long run—apart from the recipients near the 500 employee cutoff, no control group exists.

In this paper, we provide causal evidence of the effect of business transfers to micro and small businesses in the context of two programs administered by the state of Minnesota. The Minnesota Small Business Relief Grants (SBRG) disbursed more than $60 million in grants of $10,000 over the summer, fall, and winter of 2020 to just under 6,000 businesses. The Main Street COVID Relief Grants (MSCRG) disbursed $64 million in grants of between $10,000 and $25,000 in the winter of 2021 and spring of 2022 to just over 6,000 businesses. Unlike the federal PPP, total demand for the Minnesota programs exceeded their funding authorizations. To solve this allocation problem, Minnesota selected recipients using a strat-

[1] According to Bureau of Economic Analysis (2024), $683 billion of forgiven loans went to businesses and hence count as subsidies in figure 1. Other large federal programs during the 2020-21 period include Economic Injury Disaster Loan Advance Grants ($27 billion), grants to air carriers ($42 billion), the employee retention credit ($58 billion), the restaurant revitalization fund ($29 billion), and the provider relief fund ($47 billion). See Chodorow-Reich, Iverson and Sunderam (2022) for an overview of these programs.

Figure 1: Government Subsidies in the United States



Notes: Government subsidies (NIPA table 3.1 line 30) are defined by Bureau of Economic Analysis (2023) as: "Payments from government agencies to private business and to government enterprises to support their current operations."

ified lottery, with minimum funding thresholds for certain firm or owner demographic groups perceived to face greater historical disadvantage. We obtained the full set of applicants and awardees via a public records request. To induce randomization in the stratified lottery, our research design compares lottery winners to losers with the same *ex ante* probability of winning.

We obtain data on firm outcomes by matching the applicants to their business tax returns using confidential data from the IRS. Due to statutory size maximums (50 employees for SBRG and 200 employees for MSCRG) and capped award sizes, across both programs three-quarters or more of applicants had 6 or fewer full-time-equivalent employees. Our main firm-level analysis consists of the subset of firms with federal Employment Identification Numbers (EINs), which allow a clean match to the IRS data.

We construct three main firm-level outcomes using the tax returns: quarterly employment, quarterly payroll, and annual depreciation (which we show also characterizes the response of investment under reasonable conditions). Because of attrition from lottery winners that do not ultimately receive awards, we estimate instrumental variable (IV) regressions in which the endogenous variable is the award amount and the excluded instrument is winning the lottery. We further condition on fixed effects for the *ex ante* probability of winning, which delivers identification via random assignment. We report both reduced-form dynamic effects and summary treatment effects scaled to "follow the money."

We do not find significant positive effects of the grants on any of the main outcomes. In most cases, the point estimates are negative. For payroll, the 95% confidence interval excludes an increase over the six quarters following the lottery of more than $0.20 per $1 of grant receipt. For depreciation, the 95% confidence interval excludes an increase in the subsequent year of more than $0.07 per $1 of grant receipt. These results are robust across alternative specification choices and hold across the distribution of firm sizes in our data. We also do not find significant positive responses for firms with observable characteristics traditionally associated with being credit constrained, such as those with positive interest payments or relatively high growth firms.

What else might firms have done with the money? Combining all deductions, the point estimate for the effect of a grant remains negative but with less precision than payroll or depreciation alone. Still, the 95% confidence interval rejects more than $0.26 of additional deductions per dollar of grant. The effect on firm survival is zero and precisely estimated, suggesting that owners did not engage in precautionary saving on behalf of their firms. We also do not find evidence that lottery losers were more likely to obtain other sources of business aid.

A remaining possibility is that owners used the grants for personal consumption or to bolster their savings. To shed light, we merge the applicant data with confidential credit bureau records of the firm owners. We examine three main outcomes: debit or credit card spending, registration of a car, and delinquency. We find no evidence of lottery winners increasing their debit and credit card spending. Some MSCRG winners, however, appear to have used the funds to acquire a car. Specifically, receipt of an MSCRG grant increases the probability of an auto registration in the six months after the lottery date by 3.3p.p. (s.e.= 1.05), relative to a base rate of 10%. Conversely, the share of SBRG recipients with a 30+ day delinquency in the past 12 months falls by about 3p.p. (s.e.=XX), relative to a base rate of 20%. In sum, the credit bureau data provide affirmative evidence that at least some recipients used the funds either for personal consumption or to bolster their personal finances.

If the program ultimately benefited firm owners, a salient question for program evaluation becomes the overall progressivity of the transfers. Despite the concentration of micro and very small firms, the owners of applicant firms are higher income than the population of all Minnesota taxpayers. For example, only about 30% of applicants have 2019 incomes in the bottom 50% of the distribution, while more than one quarter of applicants had income

in the top 10%. The distribution of recipients tilts slightly lower income relative to that of applicants, reflecting the programs' minimum funding thresholds. However, recipients remain much higher income than the general population.

These results have important implications beyond the specific Minnesota programs. We report below newly collected data documenting more than 800 state and local government business grant programs across more than 300 jurisdictions during 2020-2021. While implementation details differ, these programs appear to share broad similarities especially in their targeting population of micro or very small businesses. Likewise, our analysis of data from Small Business Administration (2024) shows 5.6 million federal PPP disbursements in the range of $5,000 to $25,000 (more than half of the total) and to firms of broadly similar size as in the Minnesota programs, while the federal Economic Injury Disaster Loan (EIDL) Advance program made 5.8 million grants of up to $10,000. Summing over the cost of the state and local programs, PPP grants in the range of $5,000 to $25,000, and the EIDL Advance grants gives a total cost of $125 billion. Thus, there appears to be broad policy interest in giving grants to micro or small businesses in economic environments such as prevailed in 2020-2021.

This interest likely reflects in part a belief that such firms face especially large and binding external financing premia. If they did, then a cash flow windfall should increase the firm's operations, since the internal rate of return to the firm exceeds the financial market discount rate available to the owner (Bernanke and Gertler 1989, Bolton, Chen and Wang 2011). Our evidence that the grants do not increase payroll, investment, or survival casts doubt on this presumption. One possible explanation may be that most such businesses do not actually grow or desire to grow large (Haltiwanger, Jarmin and Miranda 2013, Hurst and Pugsley 2011). In such cases, although the statutory incidence of these transfers may fall on workers or other input providers—for example, when the programs state that transfers must be spent on payroll—the economic incidence falls mainly on firm owners.

The magnitude of the grants, about 4% of median firm sales, roughly coincides with macroeconomic volatility, suggesting that these firms also may have small marginal propensities to invest (MPI) out of fluctuations in sales due to typical macroeconomic forces. Under reasonable assumptions that permit us to map the depreciation-based estimate to an MPI, the 95% confidence interval excludes an MPI of more than XXX.

Furthermore, the two programs operated in different macroeconomic environments. The SBRG was put in place in the immediate aftermath of COVID related shutdowns, as the

unemployment rate fell rapidly, while the MSCRG delivered aid after vaccinations were widely available and the labor market had recovered. Neither environment gave rise to large positive effects. At the same time, these programs have costs, both directly in total dollars and indirectly due to the administrative effort by state and local government officials and the constraints on verification and requirements for federal programs making millions of disbursements.[2]

We review related literature next. Section 2 details the Minnesota programs, lottery process, and our research design. We also present our newly collected data on other state and local business aid programs. Section 3 describes the IRS data, variable construction, and our analysis sample. Section 4 shows reduced form dynamic responses and our main IV estimates, including specification robustness and heterogeneity analysis. Section 6 provides a discussion of the results and concludes.

Related Literature. The literature studying pandemic-era aid to businesses has primarily focused on the Paycheck Protection Program. Yet, PPP's high takeup makes evaluating its causal effects difficult. The literature has taken two main approaches. The first exploits the 500 employee maximum cutoff and tends to find small causal effects of PPP on relatively large firms (e.g., Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar and Yildirmaz 2022*b*, Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar and Yildirmaz 2022*a*, Chetty, Friedman, Hendren and Stepner 2020).[3] The second exploits the implementation of the program through banks, which may have generated quasi-random variation in the timing of PPP receipt across firms (e.g., Bartlett and Morse 2020, Cole 2022, Denes, Lagaras and Tsoutsoura 2021, Doniger and Kay 2021, Granja, Makridis, Yannelis and Zwick 2022, Bartik, Cullen, Glaeser, Luca, Stanton and Sunderam 2020, Faulkender, Jackman and Miran 2020). These papers find mixed results, potentially because the timing of PPP receipt is not randomly assigned and the differences in timing they exploit are relatively small, with essentially the universe of firms treated after two months. Our results align with those studies that find modest effects of PPP on employment and business closures.

[2]See e.g. Griffin, Kruger and Mahajan (2023) for evidence of fraud in the PPP program tied to the high processing rate of fintech lenders. Indeed, well after the SBRG and MSCRG programs concluded, allegations emerged of severe fraud in other Minnesota aid programs, with a particular focus on the Somali-American community (Londoño 2025). In robustness exercises, we obtain similar effects on owner outcomes in strata with high and low concentrations of applicants of Somali heritage, suggesting that our findings do not reflect any quirk of the applicants to the SBRG and MSCRG programs.

[3]Hubbard and Strain (2020) finds somewhat larger effects using a similar strategy.

A smaller literature studies the Economic Injury Disaster Loan Program (e.g., Fairlie and Fossen 2022, Li 2021), which also had very high takeup. Our paper contributes to this literature by first documenting the size and scope of subnational business aid programs and then evaluating the efficacy of two such programs. By exploiting randomized allocation schemes, we are able to provide evidence on medium and long-term outcomes for micro and small firms.

More broadly, our paper belongs to the literature on financial constraints among firms, which were a key rationale for business aid grants during the Covid-19 pandemic (Hanson, Stein, Sunderam and Zwick 2020, Chodorow-Reich, Iverson and Sunderam 2022). Beginning with Fazzari, Hubbard and Petersen (1988), this literature has largely studied the differential response of investment and employment to various shocks for firms that are more financially constrained, as measured by proxies for such constraints like size, dividend payments, and leverage (e.g., Kaplan and Zingales 1997). It has taken this approach in part due to the lack of clean exogenous shocks that increase firm cash or net worth while holding fixed investment opportunities.[4] In a recent review, Winberry, Auclert, Rognlie and Straub (2025) emphasize the importance of MPIs out of cash flow shocks to business cycle transmission and note the lack of high quality quasi-experimental evidence on this margin. Instead, they calibrate a model with collateral constraints in which the unweighted MPI is \$0.50 per dollar, more than five times larger than the upper bound of the 95% confidence interval of the firms in the programs we study.

2 Program Background and Research Design

2.1 Programs and Macroeconomic Context

Like the rest of the United States, Minnesota experienced a sharp recession in 2020 due to the spread of COVID and associated Executive Orders (EOs), which temporarily closed businesses. In response, and amid the fallout from the haphazard roll-out of the federal PPP (Granja, Makridis, Yannelis and Zwick 2022), the state passed its own business grants program in 2020. It passed a second program in 2021 at a time when unemployment had already fallen below the January 2020 level.

[4]Blanchard, Lopez-de Silanes and Shleifer (1994) is one exception, studying a sample of 11 firms that received cash windfalls from the resolution of litigation.

Table 1: Round Eligibility and Target Size

Round	Eligibility	Award Minimum
1	Veteran-owned	$\$2.5M$
2	BIPOC-owned	$\$10M$
3	Micro business	$\$18M$
4	Women-owned	$\$2.5M$
5	Non-metro	Ensure half (SBRG) or $\$31.152M$ (MSCRG)
6	Metro	Ensure half (SBRG) or $\$32.952M$ (MSCRG)

The Minnesota Small Business Relief Grants (SBRG) were signed into law on June 16, 2020. The legislation authorized $60,300,000 in grants (later increased to a final disbursement of $61,500,000) of $10,000 each to businesses with 50 or fewer Full-time Equivalent Employees (FTEs) to be distributed via lottery. Essentially the entire program was funded through the federal CARES Act Coronavirus Relief Fund, which provided $150 billion to state and local governments. The SBRG legislation contained several pre-specified funding levels and priorities, summarized in table 1: half of the grants should go to businesses based in the Twin Cities metropolitan area (Metro) and half to businesses in the remainder of the state (non-Metro); minimums of $2,500,000 were to be granted to veteran-owned businesses, $2,500,000 to women-owned businesses, $10,000,000 to BIPOC-owned businesses, and $18,000,000 to micro businesses (6 or fewer FTEs); and priority for businesses directly affected by pandemic-related EOs in place on May 18, 2020.

The Minnesota Main Street COVID Relief Grants (MSCRG) were signed into law on June 30, 2021. The legislation authorized $64,104,000 in grants to businesses with 200 or fewer FTEs. The MSCRG legislation also contained pre-specified funding levels and priorities: $31,152,000 for Metro-area businesses and $32,952,000 for non-Metro businesses; minimums of $2,500,000 for veteran-owned businesses, $2,500,000 for women-owned businesses, $10,000,000 for BIPOC-owned businesses, and $18,000,000 for micro businesses; and priority for businesses without a previous state-funded grant. Finally, the awards ranged in size according to the number of FTEs: $10,000 if 0-6 FTEs, $15,000 if 7-49, $20,000 if 50-99, $25,000 if 100-200.

Table 2 shows that most applicants to both programs were micro firms. A slightly higher share of applicants with a federal Employment Identification Number (EIN) had 7 or more

Table 2: Award Size Distribution

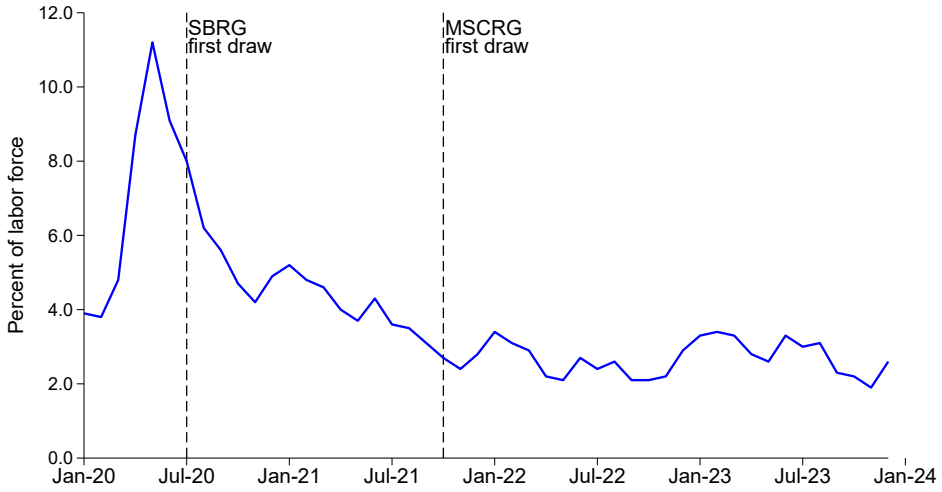
Program and FTE range	Award size	Applicants (%)			Recipients (%)		
		Share with EIN	Size distribution		Share with EIN	Size distribution	
			All	Has EIN		All	Has EIN
SBRG: 0-6	10,000	67.0	74.9	69.7	71.7	70.2	65.8
SBRG: 7-50	10,000	86.9	25.1	30.3	87.7	29.8	34.2
MSCRG: 0-6	10,000	60.3	82.0	74.8	56.7	85.2	78.0
MSCRG: 7-49	15,000	92.5	17.0	23.8	91.5	13.8	20.4
MSCRG: 50-99	20,000	95.2	0.7	1.0	95.5	0.7	1.1
MSCRG: 100-200	25,000	97.9	0.3	0.4	100.0	0.3	0.4

Notes: the table shows the share of firms with an EIN and the size distribution of applicants and recipients according to the self-reported FTE count used to determine eligibility for the Micro round and award size.

FTEs, with 30% of SBRG and 25% of MSCRG applicants with an EIN in this category. Our firm-level analysis below focuses on firms with an EIN, approximately 70% of the applicant pool, as we use this identifier to match to tax returns. Not shown in the table, about 80% of firms with an EIN are incorporated, while about 65% of those without an EIN are sole proprietorships. Thus, our firm-level analysis overwhelmingly covers incorporated firms, while in the owner-level analysis about one-third of the owners operate sole proprietorships.

Figure 2 provides macroeconomic context for the programs. By the first draw of SBRG, the state had rescinded EOs closing any businesses (Walz 2020), and the statewide unemployment rate had fallen from a peak of 11.2% in May 2020 to 7.9% in July. The unemployment rate continued to fall in the months immediately following the first SBRG lottery, reaching 6.1% in September and 4.8% in December 2020. Thus, the SBRG grants arrived in a period of falling unemployment. By the time of the MSCRG lottery, vaccinations were widely available and the unemployment rate in Minnesota had fallen further to below 4%; by December 2021 it had fallen below 3%. Thus, the MSCRG grants arrived at a time of exceptionally low unemployment.

Figure 2: Minnesota Unemployment Rate



2.2 Lottery Design

Both Minnesota programs used a stratified lottery. At the start of each lottery, each of N applicants was assigned a random number drawn from a uniform distribution. Without loss of generality, let $n = 1, 2, \ldots, N$ index applicants and also denote the applicant's random number rank. Let $a(n)$ denote the award size for applicant n if selected, with $a(n) = \$10k$ for SBRG and $a(n)$ determined by table 2 for MSCRG. Each program selected recipients over six sequential rounds, with each round restricted to applicants satisfying the criteria in table 1.

In round r, winners were selected as follows. Among businesses (a) eligible for the round; (b) with overall priority (i.e., those subject to an EO (SBRG) or those without a prior state award (MSCRG)); and (c) that had not yet been selected, choose all applicants with $n \leq \bar{n}_r$ s.t. $\sum_{n \leq \bar{n}_r} a(n) \leq A_r$, where A_r is the target award size (table 1).[5] If all eligible businesses with overall priority had been selected without exhausting A_r, allotments continued to applicants without overall priority. Because the random number assignment occurred once-and-for-all at the start, a business eligible for k rounds (e.g., a micro business in the metro area owned by a female person of color would have eligibility in rounds 2,3,4, and 6) had a lower probability of selection in the later rounds than a business eligible for

[5] As we discuss further below, because of anticipation of ineligible recipients, the target award sizes for rounds 1 and 2 of the initial draw of MSCRG were increased by 25% and 30%, respectively.

the first time.[6]

Lottery winners were assigned to a non-profit partner, typically a Community Development Finance Institution, that administered the grant, including reviewing the winners' eligibility. A substantial share (21% in SBRG and 33% in MSCRG) of lottery winners failed this review, necessitating additional draws.[7] The additional draws used the same random number assignment.

The first run of the SBRG lottery occurred on July 16, 2020. Redraws occurred on September 29, October 28, November 23, and December 30, 2020.[8] The first run of the MSCRG lottery occurred on October 14, 2021. Redraws occurred on January 22, February 25, March 11, March 31, April 22, May 17, and June 7, 2022.[9] For both programs, lottery winners were typically notified within about two weeks of the lottery draw. The administrating organization then reviewed winners for eligibility and provided a final list of awardees to the state, which then provided funds for disbursement. Thus, fund receipt occurred on a staggered basis over the course of the redraw rounds. Table A.2 reports the number of lottery winners and those ultimately deemed eligible by draw date.[10]

2.3 Award Probabilities

A business's probability of selection depended on eligibility for each round and whether it had overall program priority. Let $g \in G$ denote a unique group. For SBRG, the set G consists of Veteran×BIPOC×Micro×Women×Metro×EO priority. For MSCRG, the set G

[6]For example, suppose that the overall distribution of random numbers is $U[0,1]$ and there are E_r applicants eligible for round r. In round 1, the distribution of eligible random numbers also obeys a $U[0,1]$ distribution. In round 2, the distribution of random numbers of applicants also eligible but not chosen in round 1 obeys a $U[\bar{n}_1/E_1, 1]$ distribution while the distribution of random numbers of newly eligible applicants still obeys a $U[0,1]$ distribution. This feature was not intentional, but resulted from the decision to draw random numbers once-and-for-all to ease the process across rounds and re-draw dates.

[7]Reasons for failing the eligibility review could include not having a Minnesota resident as the primary owner or not qualifying for the priority round, among others. Londoño (2025) discusses fraud in other pandemic-era Minnesota aid programs, which we address in robustness exercises.

[8]The initial draw was revised on July 24 to correct the geographic distribution requirement for mis-coding of the county variable. The November 23, 2020 redraw incorporated an additional authorization of $4,500,000 for the program.

[9]We found fourteen cases in MSCRG that appear to violate the randomization algorithm, all in favor of providing awards. Thirteen arise in date 0 round 6 and 1 in date 4 round 6. We drop these observations from the analysis.

[10]We lack full information on the number of winners and recipients by draw date for SBRG because the program administrators did not keep a full longitudinal history of observations whose eligibility was called into question. In some cases, these winners were deemed eligible after initially being deemed ineligible.

consists of Veteran×BIPOC×Micro×Women×Metro×no prior award. By construction, all businesses in g have the same *ex ante* probability of selection: $p(n) = p_g$, where p denotes the *ex ante* selection probability.

Figure 3 plots the distribution of recipients in each program by p, rounded to the nearest 0.01. The probabilities for SBRG mostly lie in the range 0.75 to 0.85. The small number with probability 1 are all veteran-owned businesses subject to an executive order, for which the minimum award threshold was binding. In total, 10,112 applicants to the SBRG program and 6,125 recipients had interior probabilities for which a valid control group exists.

The range of interior probabilities in MSCRG clusters around two subsets, businesses in the metro area without a prior grant, with a lottery win probability of between 0.6 and 0.7, and businesses in the non-metro area with a prior grant, with a win probability in the range of 0.2-0.4. In MSCRG, the geographic allocation to businesses in non-metro areas was binding for businesses without a prior state grant; these are the businesses with probability 1. In total, there are 10,254 applicants to the MSCRG program and 3,752 recipients with interior probabilities.

2.4 Research Design

Let $W_{i,g;\mathcal{P}}$ denote an indicator equal to one if firm i in group g is selected in the lottery for program $\mathcal{P} \in \{SBRG, MSCRG\}$, $R_{i,g;\mathcal{P}}$ denote the award amount (if any), and $\text{Post}_{\mathcal{P}}$ an indicator for the period of lottery $\mathcal{P}$ or later. For an outcome $Y_{i,g,t}$ observed over some period t, we implement variants of the following difference-in-difference system:

$$\text{First stage: } R_{i,g;\mathcal{P}} \times \text{Post}_{\mathcal{P},t} = \gamma W_{i,g;\mathcal{P}} \times \text{Post}_{\mathcal{P},t} + \delta_{i,\mathcal{P}} + \xi_t + \alpha_{g,\mathcal{P}} \times \text{Post}_{\mathcal{P},t} + \nu_{i,g,t;\mathcal{P}}, \quad (1)$$

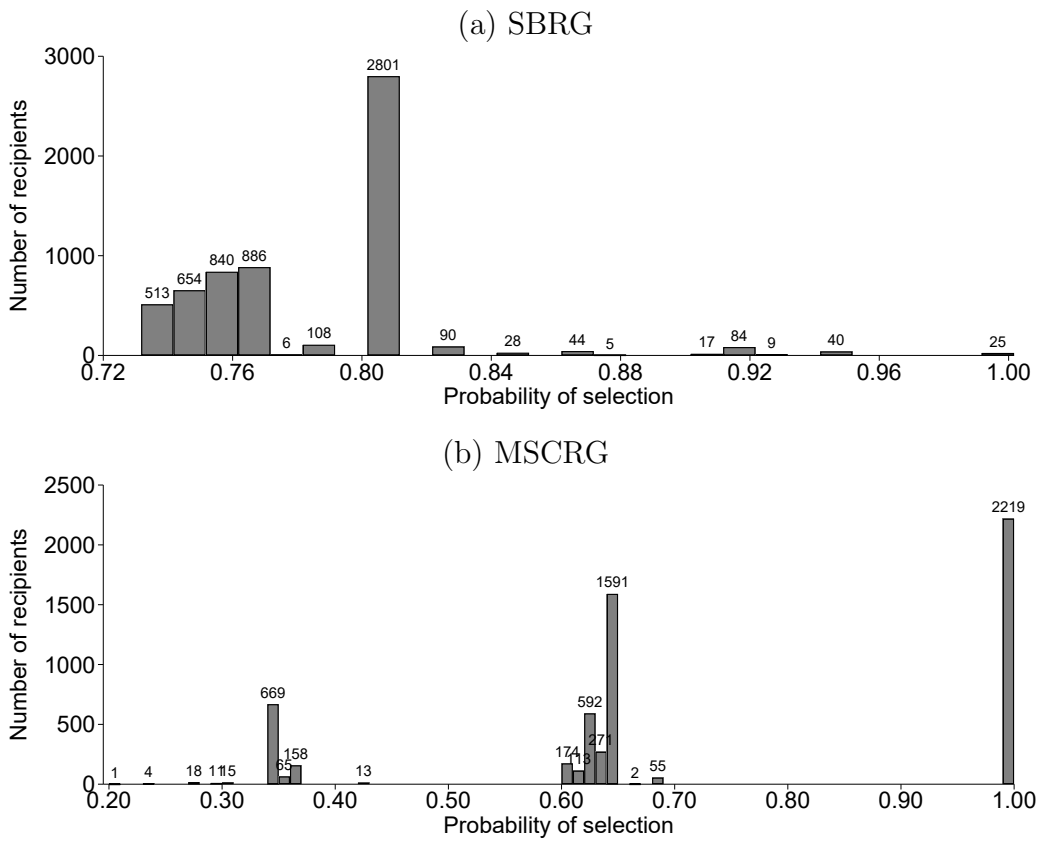
$$\text{Reduced form: } Y_{i,g,t} = \beta W_{i,g;\mathcal{P}} \times \text{Post}_{\mathcal{P},t} + \delta_{i,\mathcal{P}} + \xi_t + \alpha_{g,\mathcal{P}} \times \text{Post}_{\mathcal{P},t} + e_{i,g,t;\mathcal{P}}, \quad (2)$$

$$\text{Second stage: } Y_{i,g,t} = \tau \widehat{R_{i,g;\mathcal{P}} \times \text{Post}}_{\mathcal{P},t} + \delta_{i,\mathcal{P}} + \xi_t + \alpha_{g,\mathcal{P}} \times \text{Post}_{\mathcal{P},t} + \epsilon_{i,g,\mathcal{P}}. \quad (3)$$

This specification includes both pre and post-treatment periods and fixed effects for Firm×Program, time period, and treatment probability group $g\times$ post-treatment. The inclusion of the group g fixed effects in $\alpha_{g,\mathcal{P}}$ addresses the lottery stratification; because all applicants within a group have the same *ex ante* probability of selection, these fixed effects induce randomization among lottery winners and losers.[11] The two-stage design addresses

[11] In particular, the control group of lottery losers for MSCRG includes firms that received grants under

Figure 3: *Ex ante* Selection Probabilities



Notes: The figure shows the distribution of probabilities of winning each lottery.

the attrition from lottery winners to recipients due to failures of the eligibility reviews.[12] The notational dependence on program $\mathcal{P}$ anticipates specifications in which we pool the outcomes from both programs. The inclusion of pre-period outcomes in the difference-in-difference specification is not strictly necessary given the lottery randomization but reduces standard errors. We also report versions of equation (2) in reduced form distributed lag specification with β and $W_{i,g;\mathcal{P}}$ indexed by treatment lead/lag ℓ.

SBRG. This poses no problem even with heterogenous dynamic treatment effects because the fixed effect definitions account for priority in MSCRG and hence whether a firm received a prior grant. That is, the fixed effects absorb an indicator for prior receipt. The control group of lottery losers for SBRG includes firms that subsequently received MSCRG grants. However, the additional likelihood of receiving an MSCRG grant is small and the overlap period for outcomes for SBRG that occur after the MSCRG lottery is small, so we address this issue in robustness.

[12]It is straightforward to show that with fixed grant sizes, the first stage coefficient γ equals the share of lottery winners that eventually receive an award multiplied by the grant size.

2.5 Comparison to Other Programs

We hand collected information on other state and local government business grant programs during 2020 and 2021 using a stratified random sample with certainty sampling of all states and large cities and counties.[13] Table 3 shows the results. Forty-nine states had at least one such program; combined these states implemented 265 programs with total dollar authorization of $14.7 billion. More than three-quarters of large ($> 200,000$ population) cities and large ($> 500,000$ population) counties had at least one such program, with smaller cities and counties relatively less likely to have a program. In our data, these programs authorized an additional $3 billion of aid. While the details of these other programs vary, many share features with the Minnesota programs: at least three-quarters of all such programs had some federal funding and 60% had maximum grant sizes of $25,000 or less.[14]

The Minnesota programs also share several similarities with PPP. Appendix table A.1 shows the distribution of forgiven loans (i.e., grants) by size. Two-thirds of all PPP disbursements were below $25,000, with more than half in the range from $5,000-25,000. Unlike the PPP, however, the Minnesota programs did not specify numeric maintenance of payroll thresholds. Instead, they provided general guidance that the funds be used to support the regular course of business. Of course, any earmarked grant smaller than the targeted expense may be viewed as simply providing fungible funds to the firm, and the Minnesota programs did not require the recipients to report how they used the funds.

3 Data and Summary Statistics

We made a public records request to the state of Minnesota for the full set of grant applications to SBRG and MSCRG, which returned 41,794 applicants. The files include all of the variables necessary to determine eligibility for each round of the lottery, the lottery number, whether the firm won the lottery, and whether the firm received a grant and the amount if

[13] Specifically, we start from the 2017 Census of Governments Master Address File, which is a master list of all governments in the United States. We sample with certainty all states, the District of Columbia, all cities and townships with a population of 100,000 or more, and all counties with a population of 250,000 or more. We then probabilistically sample cities/towns and counties within the remaining strata shown in table 3 using a simple random sample within each stratum. Each sampled unit was assigned to a research assistant who searched for business aid programs administered by that unit.

[14] These totals are conservative in that in some cases we were unable to determine a funding source or whether there was a maximum award size. Minnesota also was not unique in using a lottery to distribute funds. Other entities used scoring systems or first-come, first-served allocation mechanisms.

Table 3: State and Local Business Grant Programs

	Sample size	Sampling rate (%)	Has any grant program?	Number of programs	Estimated dollars (millions)
States	51	100.0	49	265	14,720
Cities and towns total	1,446		133	248	914
Pop. 200,000+	123	100.0	93	187	746
Pop. 100,000-199,999	228	100.0	21	33	131
Pop. 50,000-99,999	365	63.9	10	15	8
Pop. 10,000-49,999	365	10.7	6	7	16
Pop. 2,500-9,999	365	5.5	3	6	14
Counties total	647		122	311	2,300
Pop. 500,000+	119	100.0	101	272	2,160
Pop. 250,000-499,999	123	100.0	11	24	86
Pop. 100,000-249,999	135	44.6	3	5	23
Pop. 50,000-99,999	135	35.4	2	2	5
Pop. 20,000-49,999	135	16.3	5	8	26
Total			304	824	17,934

Notes: the table reports statistics on business grant or tax relief programs administered by state and local governments. The final column reports estimated dollars using sampling weights.

it did so.

3.1 Tax Return Analysis Sample

Starting from the full set of applicants, we remove those without an EIN (i.e., self-employed individuals who use a Social Security Number for their business, which under Minnesota's Data Privacy Act the state could not provide us) and drop a small number of duplicates, leaving 29,594 firm applications. We merge applicant firm EINs to the universe of tax records from our analysis period.

We draw information from two types of tax forms. An employer's quarterly tax return (primarily form 941) contains information on quarterly employment and payroll used to determine the employer's share of payroll taxes. Annual income tax returns (form 1120 for C-corporations, 1120-S for S-corporations, 1065 for partnerships, and 1040 Schedule C for sole proprietorships) contain information on annual revenue and deductions used to determine income tax. We match 89% (26,261 firms) of the applicants with an EIN to at least one of these forms in our sample period. Restricting to firms with a match in the pre-treatment

period, the match rate is 64% (18,873 firms) for the quarterly tax return and 80% (23,589 firms) for the annual return. The higher match rate for the annual return in part reflects the inclusion of firms without any employees, which do not need to file quarterly returns. Finally, our main analysis restricts to firms with 30 or fewer employees, approximately the 90th percentile of the sample employee size distribution.[15]

3.2 Credit Bureau Analysis Sample

We contracted with the credit bureau TransUnion (TU) to merge applicant owners with their semi-annual credit histories over the period December 2018 to June 2024. TU requires a name and residential address to merge. The application data contain the owner's name and an address, but in many cases the provided address corresponds to the business rather than the owner's residence. We therefore first performed a match of applicants with their residential address histories using the proprietary data set Infutor. In this step, we kept only residential histories from records with a matching name and address (meaning the application address or the address listed on the business's secretary of state filing was a residential address), records with a matching name and phone number, or records with a unique name match. We then provided the applicant data augmented with residential address histories to TU, who performed the match with the credit histories and returned to us an anonymized data set. The merged TU data set contains just under 26,000 applicants, with the precise count varying by month.

3.3 Outcome Variables

We measure quarterly *employment* and *payroll* using the employer's quarterly tax return. Employment means the "number of employees who received wages, tips, or other compensation" for the pay period including the twelfth day of the third month of the quarter. Payroll means total quarterly "wages, tips, and other compensation."

We measure annual *depreciation* using the annual income tax forms. In the United States, firms can deduct a portion of past and current investment in equipment and structures from

[15]The employment size restriction helps to guard against type-II errors in the matching process. We show robustness to this cutoff. For firms matched to an income tax return but not an employment tax return, we impute employee size using the wage salary deduction and assuming an annual salary of fifty thousand. Appendix figure A.1 tests for balance by treatment for each step of sample winnowing as well as the TransUnion sample described next. Consistent with the randomization of the lotteries, we find no evidence of sample selection bias within the *ex ante* probability groups.

their taxable income according to formulas that vary by asset and firm type and the year the investment occurred. The depreciation line on the "front page" of the annual tax return does not distinguish between depreciation claimed for current or past investment and does not show future depreciation due to current investment. Nonetheless, we interpret the response of depreciation to the grants as also giving the response of current investment, for two reasons. First, because many firms in our sample have investment below the Section 179 threshold for claiming full deprecation of equipment investment in the initial year and because of the 100% bonus depreciation for equipment investment throughout 2018-2022 under the Tax Cut and Jobs Act, for many firms in the sample claimed depreciation exactly equals investment. Second, the difference between depreciation and current investment arises only because of depreciation due to past investment and to current investment not fully expensable in the first year such as investment in real property. Under random assignment of treatment, winning a program lottery should be uncorrelated with all lagged variables, including past investment.[16] We address the remaining difference due to investment during the treatment period but not immediately fully expensable in a robustness exercise where we also include depreciation in future years.

We also measure gross receipts ("*revenue*"), cost of goods sold ("*COGS*"), and *deductions* using the annual income tax forms. Finally, we measure *survival* based on whether a firm continues to file tax returns.

Turning to the credit bureau outcomes, we measure monthly *card spending* using a proprietary variable from TransUnion that takes as inputs balances and payments on revolving cards. We aggregate to 6 month frequency, keeping only observations with non-missing monthly spending in all months. We observe the registration date of autos at the Registry of Motor Vehicles ("*autos*"), which reflect the acquisition of a vehicle (used or new). We also aggregate this variable to a 6 month horizon and define an indicator for a new registration over the 6 month period. We measure whether an individual has any delinquencies of 30 days or more over the past year ("*delinquency*").

In our baseline analysis of the tax return data we define the treatment period for quarterly variables as the six calendar quarters following the quarter of the first run of the lottery and the pre-treatment period as the two quarters prior to the first run of the lottery. We define the treatment period for annual variables as the year following the lottery and the pre-

[16] Appendix XX [makes us feel comfortable in mapping the depreciation effects to investment effects in this setting.]

treatment period as the year preceding the lottery. In our analysis of the credit bureau data we examine both 12 and 6 month responses.

We winsorize non-binary weakly positive variables at the 95th percentile and variables that can take negative values symmetrically at the 2.5th and 97.5th quantiles.

3.4 Sample Summary Statistics

Table 4 reports summary statistics for the outcome variables. Before computing percentiles, we average each non-binary variable over the full sample period or the sample periods for SBRG or MSCRG.[17] Median quarterly payroll for firms filing form 941 is about $27,000, with the 25th percentile of firms having payroll around $8,000 and the 75th having payroll around $68,000. Median employment for these firms is 4 employees. The median firm with an income tax form has annual depreciation of about $1,000 and total deductions and revenue in the range of $200,000 to $300,000.

The firms in our analysis sample are small, reflecting the size restriction of the programs. They are nevertheless important to study for several reasons. First, such firms account for a meaningful share of economic activity. Per the Census Business Dynamics Statistics in 2021, firms with fewer than 20 employees account for 17% of aggregate employment, and firms with fewer than 5 employees account for 6%. Second, small firms were the prime targets for the business transfer programs during the pandemic recession. For example, the PPP disbursed $120 billion to firms with fewer than 20 employees and $51 billion to firms with fewer than 5 employees. Thus, assessing the impact of the Minnesota programs on small firms is instructive about a macro- and budget-relevant policy.

The median firm owner has about $5700 of card spending over a 6 month period. About 11% of the sample registers a car in each 6 month period and 24% has a delinquency over the past 12 months.

4 The Effect of a Windfall on Firms

This section presents the firm-level outcomes from firm tax returns. Section 4.1 shows the time path of treatment effects for employment, payroll, and depreciation for both programs.

[17] To avoid disclosing data for an individual firm, the table reports means of firm-level variables within 0.01 standard deviation of each percentile.

Table 4: Outcome Variable Summary Statistics

Source	Percentile:	P5	P25	P50	P75	P95
Tax	Payroll	15	7,945	26,584	67,678	197,431
Tax	Payroll: SBRG	18	7,946	26,241	67,422	204,853
Tax	Payroll: MSCRG	11	8,105	26,974	68,038	187,741
Tax	Employment	0	1	4	10	22
Tax	Employment: SBRG	0	1	4	10	21
Tax	Employment: MSCRG	0	1	4	10	22
Tax	Depreciation	0	5	1,482	10,425	61,000
Tax	Depreciation: SBRG	0	6	1,831	11,132	61,000
Tax	Depreciation: MSCRG	0	4	979	9,554	59,399
Tax	Deductions	156	43,110	154,021	407,231	1,247,343
Tax	Revenue	364	66,641	244,549	672,896	2,359,617
TU	Spending	319	2,288	5,716	12,819	36,735
TU	Spending: SBRG	347	2,358	5,921	13,190	36,948
TU	Spending: MSCRG	290	2,196	5,441	12,381	36,160
TU	Auto	0	0	0	0	1
TU	Auto: SBRG	0	0	0	0	1
TU	Auto: MSCRG	0	0	0	0	1
TU	Delinquency	0	0	0	0	1
TU	Delinquency: SBRG	0	0	0	0	1
TU	Delinquency: MSCRG	0	0	0	1	1

Notes: Each row reports firm-level summary statistics for the variable indicated averaged over the sample period indicated.

Section 4.2 reports the instrumental variable estimates. Section 4.3 considers treatment heterogeneity. Section 4.4 shows other outcomes including total deductions and firm survival.

4.1 Reduced-Form Dynamic Treatment Effects

Figure 4 plots the time path of reduced form treatment effects $\{\beta_\ell\}$, where ℓ indexes the period since the lottery, using the specification in event time:

$$Y_{i,g,\ell} = \sum_{\ell \neq 0} \beta_\ell W_{i,g,\ell;\mathcal{P}} + \delta_{i,\mathcal{P}} + \alpha_{g,\ell,\mathcal{P}} + e_{i,g,\ell}. \tag{4}$$

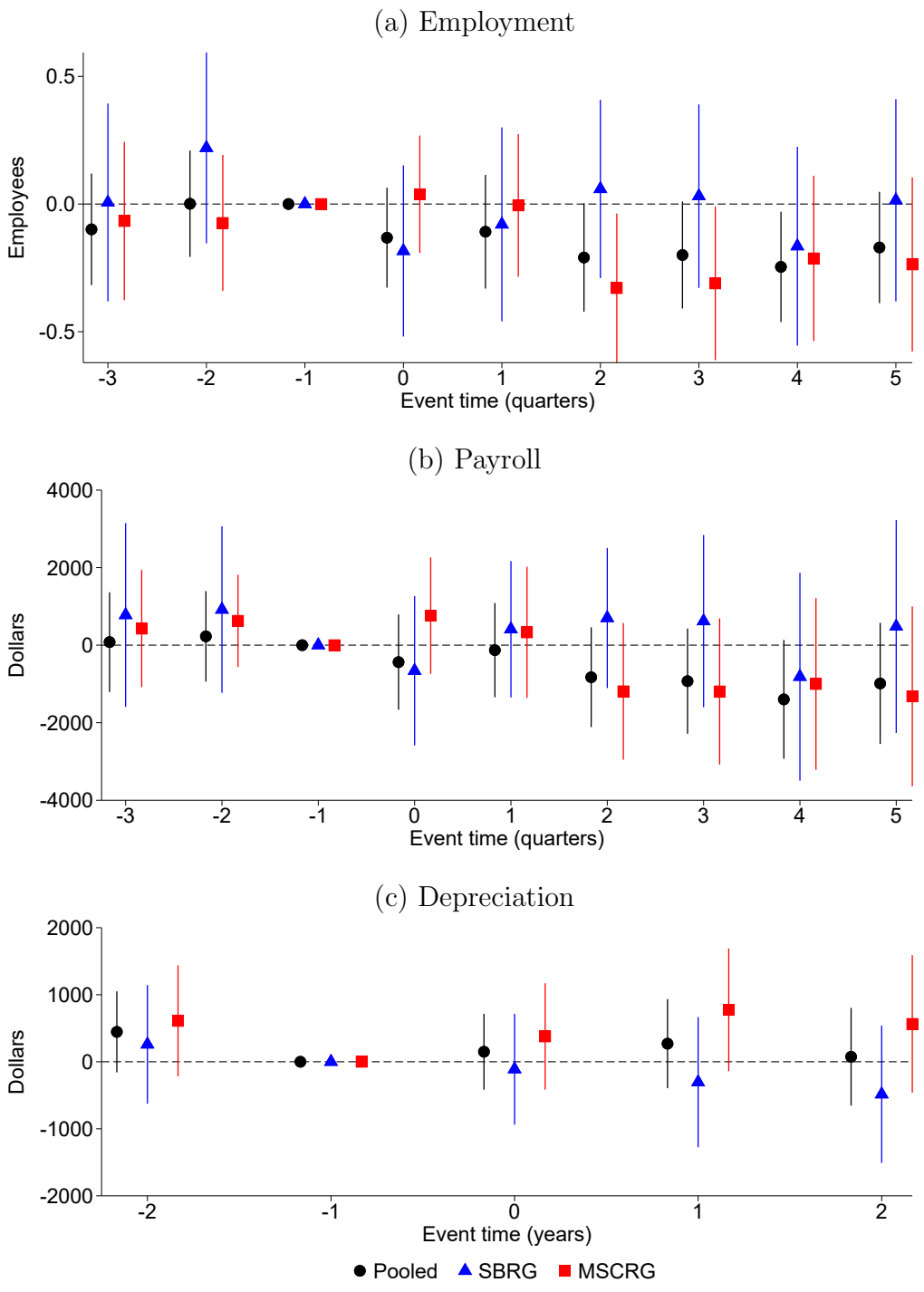
In particular, the group×lag fixed effects $\alpha_{g,\ell,\mathcal{P}}$ restrict the identifying variation to firms facing the same *ex ante* probability of lottery selection. We cluster standard errors at the firm level.

In each panel, the blue triangles show treatment effects estimated using only the 2020 SBRG program, the red squares show treatment effects estimated using only the 2021 MSCRG program, and the black circles show the treatment effects from pooling the programs by stacking them together. Panels (a) and (b) show the quarterly response for employment and payroll, respectively, where $\ell = 0$ for 2020Q3 (SBRG) or 2021Q4 (MSCRG) while panel (c) shows annual depreciation, where $\ell = 0$ for 2020 (SBRG) or 2021 (MSCRG). For both figure 4 and table 5, we set the outcome $Y_{i,g,\ell}$ to zero in post-treatment periods where the firm did not file a tax return. Thus, the treatment effect coefficients $\{\beta_\ell\}$ incorporate any extensive margin effects on firm survival.

We do not find a statistically or economically significant effect of winning the lottery on any variable at any horizon. The pooled point estimates for employment and payroll are always negative. In fact, in quarter 3 the pooled employment response is significantly negative at the 95% confidence level. The MSCRG coefficients generally lie below the SBRG coefficients for labor market responses, but the confidence intervals always overlap substantially. The pooled investment point estimates are close to zero and the confidence intervals exclude a response above $1,000 in the year following the lottery win.

The pre-treatment coefficients do not show evidence of differential trends.

Figure 4: Reduced Form Dynamic Firm Responses



Notes: The dots plot the coefficients β_ℓ from the reduced form regression $Y_{i,g,\ell} = \sum_{\ell \neq 0} \beta_\ell W_{i,g,\ell;\mathcal{P}} + \delta_{i,\mathcal{P}} + \alpha_{g,\ell,\mathcal{P}} + e_{i,g,\ell}$, where ℓ indexes the time since the treatment period. The bars show the 95% confidence intervals based on standard errors clustered by firm.

Table 5: Baseline Payroll and Depreciation Results

Dep. var.:	Award per quarter	Quarterly Payroll		Award	Annual Depreciation	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	1464**	−738		8465**	47	
	(11)	(525)		(61)	(282)	
	[1442,1486]	[-1767,290]		[8345,8585]	[-506,600]	
Award			−0.50			0.01
			(0.36)			(0.03)
			[-1.21,0.20]			[-0.06,0.07]
Firm × lottery FE	✓	✓	✓	✓	✓	✓
Time period FE	✓	✓	✓	✓	✓	✓
Post × group g FE	✓	✓	✓	✓	✓	✓
Control mean		56,436	56,436		9,638	9,638
Observations	155,292	155,292	155,292	66,123	66,123	66,123

Notes: Columns (1) and (3) report first stage regressions of the form $R_{i,g;\mathcal{P}} = \gamma W_{i,g;\mathcal{P}} \times \text{Post} + \delta_{i,\mathcal{P}} + \xi_t + \alpha_{g,\mathcal{P}} \times \text{Post} + \nu_{i,g,t;\mathcal{P}}$. Columns (2) and (4) report reduced form regressions of the form $Y_{i,g,t} = \beta W_{i,g;\mathcal{P}} \times \text{Post} + \delta_{i,\mathcal{P}} + \xi_t + \alpha_{g,\mathcal{P}} \times \text{Post} + e_{i,g,t;\mathcal{P}}$. Columns (3) and (6) report the corresponding IV regressions. The row labeled Control mean gives the mean dependent variable in the group of lottery losers. Standard errors clustered by firm in parentheses and 95% confidence interval in brackets.

4.2 Spending per Dollar of Award

Table 5 shows regression outcomes for payroll and depreciation. We focus on these outcomes for the next several exhibits because they easily translate into firm spending per dollar received. The specifications, given in equations (1) to (3), mirror equation (4) but with the full set of dynamic treatment effects $\sum_{\ell \neq 0} \beta_\ell W_{i,g,\ell;\mathcal{P}}$ replaced by an interaction of $W_{i,g;\mathcal{P}}$ (first stage or reduced form specifications) or $R_{i,g;\mathcal{P}}$ (IV specification) and an indicator Post that equals one starting in the treatment period. For payroll, the treated period extends to six quarters after the lottery. For investment, the treated period is the year following the lottery. We report results from the pooled specifications for brevity and to maximize power.

The IV regressions confirm the conclusion from figure 4 that the awards did not meaningfully increase payroll or investment. Columns (1) and (4) show the first stage: on average a lottery winner received about $1400 per quarter over six quarters (column 1) for a total of about $8500 (column 4). Columns (2) and (5) show the reduced form: compared to lottery losers, lottery winners reduced payroll by an average of $738 per quarter over the first six quarters (relative to a quarterly average for lottery losers of $56,436) and increased depre-

ciation by $47 in the year after receipt (relative to an average for lottery losers of $9,638). Columns (3) and (6) show the corresponding IV coefficients: on average award recipients reduced their payroll by $0.50 (s.e.=$0.36) per dollar received and increased their depreciation by $0.01 (s.e.=$0.03) per dollar received. The 95% confidence interval excludes an increase in payroll of more than $0.20 per dollar and an increase in depreciation of more than $0.07.[18]

Table 6 reports robustness of the IV coefficients to several specification changes. The first row repeats the baseline results for convenience. Rows 2 and 3 consider alternative post-treatment periods. In general, shortening the post-treatment period reduces standard errors (because in the presence of idiosyncratic shocks the variance of the dependent variable increases with the horizon) but risks bias if treatment effects persist. In both rows, we rescale the first stage as well, so the coefficient τ continues to have the interpretation of the total change in the dependent variable per dollar of grant. Row 2 shortens the horizon for payroll to four quarters and extends the horizon for investment to include the year of the lottery. The four quarter effect on payroll is smaller than the 6 quarter effect but the standard error also shrinks, with the consequence that the upper bound of the confidence interval changes little. Row 3 extends the horizon for the post-treatment period through 2023, the most recent available period of data. While the additional periods of data increase the standard errors, the point estimates fall. Thus, we find no evidence of delayed positive effects on payroll and depreciation. In particular, the decline in the treatment effect for depreciation when including additional years supports our interpretation of the impact effect of depreciation as also giving the effect on investment, since we do not find additional depreciation claimed in future years.

Row 4 winsorizes outcomes at the 99th percentile rather than the 95th percentile. Row 5 excludes missing observations rather than setting them to zero. Neither of these changes affects the main conclusion.

Row 6 addresses the small overlap in the treatment periods for SBRG and MSCRG. This overlap creates "forbidden comparisons" insofar as some of the control group for the SBRG lottery become treated by winning the MSCRG lottery. In practice, however, SBRG losers are only 2 percentage points more likely to receive an MSCRG award than SBRG winners, which along with the small overlap period suggests any such contamination is

[18]In interpreting these IV coefficients, we do not adjust for income taxes owed on the SBRG and MSCRG awards because the outcomes considered are tax deductible. Thus, a hypothetical firm using the entire award on payroll or depreciation would not owe any additional tax due to the award. We also do not have information on whether firms actually included the award amounts in their reporting of gross income.

Table 6: Robustness

	Payroll		Depreciation	
	τ	SE	τ	SE
Specification:				
Baseline	−0.50	0.36	0.01	0.03
Alternative horizon	−0.23	0.22	−0.00	0.03
Through 2023	−0.95	0.80	−0.01	0.07
Winsorize 99th per.	−0.85	0.44	−0.04	0.06
Drop missing	−0.53	0.33	0.00	0.04
Scale SBRG by SBRG+MSCRG	−0.51	0.36	0.01	0.03

Notes: Each cell reports the coefficient or standard error from a separate IV regression. Row 1 repeats the coefficients from columns (3) and (6) of table 5. Row 2 changes the treatment horizon for payroll from the first six to the first four quarters and for investment to include the lottery year in addition to the following year. Row 3 extends the post period to end in 2023, the last period of data available. Row 4 winsorizes the outcome variables at the 99th rather than 95th percentile. Row 5 drops missing post-treatment period observations rather than balancing the panel by setting missing observations to zero. Row 6 replaces the SBRG award with the sum of the SBRG and MSCRG awards. Standard errors clustered by firm.

small.[19] To address this issue formally, we replace the SBRG award in the first stage with the combined SBRG and MSCRG award. Consistent with the small overlap and MSCRG treatment likelihood difference, this modification barely changes the results.

4.3 Do Some Firms Respond More than Others?

Figure 5 reports the IV coefficients for different initial firm employment size bins. We find similar treatment effects when: including firms with more than 30 employees, removing firms with 0 employees, and splitting the sample above and below median employment.

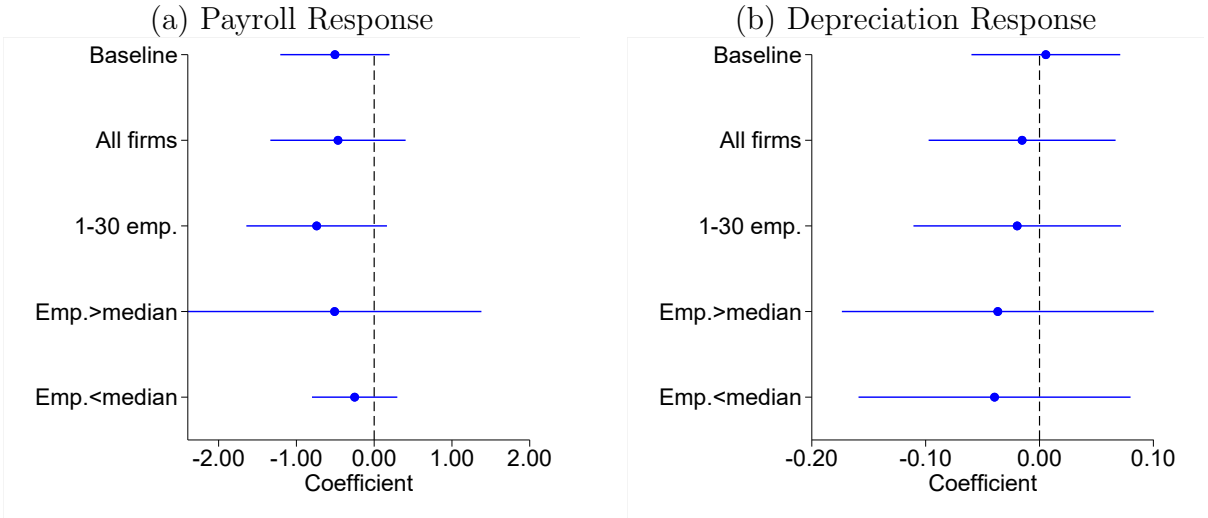
Figure 6 reports several other interactions. The data reject large positive effects for any of the subsets of firms we examine, including firms with characteristics evocative of financial constraints such as having positive interest or being high growth.

4.4 Effects on Survival, Revenue, and Other Outcomes

Table 7 reports the IV coefficients for employment, revenue, COGS, and total deductions and the reduced form coefficient for firm survival. For employment, we rescale the first stage so that the coefficient has the interpretation of the number of job-years (i.e. one job-year is

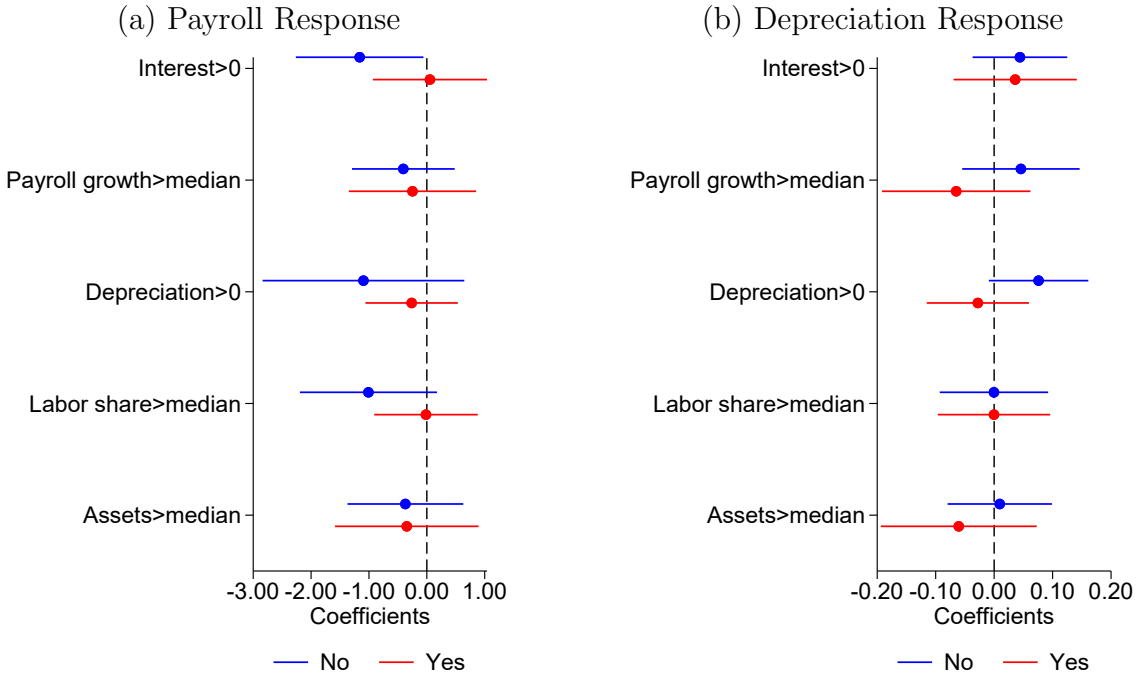
[19] In this vein, note that the first four quarters of the SBRG response shown in figure 4 do not contain any overlapping treatment periods.

Figure 5: Heterogeneity by Employment Size



Notes: The figure reports the coefficient and 95% confidence interval for the IV regression (3) in different samples defined by initial employment. Baseline shows the results for columns (3) and (6) of table 5. All firms includes firms with more than 30 employees. 1-30 emp. removes firms with 0 employees. Emp.¿median and Emp.¡median restrict to firms with above and below median employment, respectively.

Figure 6: Other Heterogeneity



Notes: The figure reports the coefficient and 95% confidence interval for the IV regression (3) where the excluded instrument and endogenous variable are interacted with the variable shown in the vertical axis.

Table 7: Other Outcomes

Dep. var.:	Annual Employment per 100K	Annual Revenue	Annual COGS	Annual Deductions	Survival
	(1)	(2)	(3)	(4)	(5)
Award	−2.21	−0.96	−0.23	−0.73	
	(1.35)	(0.89)	(0.40)	(0.50)	
	[-4.86,0.43]	[-2.71,0.79]	[-1.01,0.55]	[-1.71,0.26]	
Winner					−0.00
					(0.01)
					[-0.02,0.01]
Firm × lottery FE	✓	✓	✓	✓	✓
Time period FE	✓	✓	✓	✓	✓
Post × group g FE	✓	✓	✓	✓	✓
Control mean	6.86	518236.6	160423.2	299422	0.91
Observations	155,292	66,123	66,123	66,123	66,123

Notes: Columns (1)-(4) report IV regressions following the specifications in columns (3) and (6) of table 5. Column (5) reports the reduced form regression of firm survival on winning the lottery. Standard errors clustered by firm in parentheses and 95% confidence interval in brackets.

an additional employee for four quarters) per $100K. The point estimate is negative and the 95% confidence interval rejects an increase in employment of 0.43, which corresponds to a cost-per-job-year of at least $230K.

The estimated coefficients for revenue, COGS, and deductions are all negative. The confidence intervals for these variables are somewhat wide, reflecting the volatility in business outcomes during the 2020-2022 period. The upper bound of the 95% confidence interval for deductions suggests a maximum of $0.26 spent on factors of production out of each $1 of grants. Subtracting the sum of the coefficients on COGS and deductions from the revenue coefficient, the estimated effect on business net income is about zero.[20]

Column (5) shows a precisely estimated zero effect of the grants on firm survival. The 95% confidence interval rejects an increase in survival probability of more than 1%, compared to a baseline survival rate of 91% in the control group.

[20]Because of winsorization, this difference is not exact. Appendix table A.3 reports a coefficient of 0.05 for net income as well as a breakdown of total deductions into all components listed on the "front page" of tax returns.

4.5 Did Lottery Losers Find Funds Elsewhere?

One possible reason that lottery winners may not have experienced better business outcomes than lottery losers is that losers may have substituted toward other business aid programs. We do not find evidence favoring this interpretation. Table 8 reports use of the two main federal subsidy programs for small firms, PPP and EIDL grants.[21] The recipients of both programs were made public by the Small Business Administration (SBA); we perform a "fuzzy" match of business names in the SBA data with our applicant data.

The PPP was originally enacted in the federal CARES Act of March 26, 2020, extended by subsequent legislation until August 8, 2020, expired, and re-opened in January 2021. Thus, 2020 PPP receipt almost entirely pre-dated the SBRG program. In fact, most SBRG recipients received a 2020 PPP loan (column 1), with firms with EINs more likely to receive loans. Not shown in the table, the median loan amount was roughly $20,000. Interestingly, winners of both the SBRG and MSCRG lotteries were less likely to have 2020 PPP loans, consistent with the program minimum funding thresholds effectively targeting firms less likely to have access to the initial rounds of PPP through a bank. However, controlling for group g fixed effects as in our main research design renders these differences much smaller and statistically insignificant (column 2).

The EIDL advance grant program provided disbursements of up to $10,000 ($1000 per employee) to 5.8 million firms during the summer of 2020. To qualify, firms should be able to demonstrate hardship from COVID-19. Since SBRG prioritized firms in sectors subject to lockdown orders, SBRG winners were more likely to have received EIDL grants (column 3). Conversely, MSCRG de-prioritized SBRG winners, making MSCRG winners less likely to have received EIDL grants. Once again, controlling for group g fixed effects renders these differences smaller and statistically insignificant (column 4).

If losing the lottery made firms more likely to obtain other sources of business aid, we would expect to see SBRG losers more likely to subsequently obtain PPP loans in 2021. Column (5) shows that SBRG winners were unconditionally no more or less likely to obtain 2021 PPP loans (MSCRG winners were somewhat more likely) and controlling for group g fixed effects does not alter this result. Thus, SBRG losers did not substitute by obtaining 2021 PPP loans. We conclude that substitution toward the main federal programs cannot explain our results.

[21] For this table we include all program applicants including those not matched to their tax returns.

Table 8: Federal Program Receipt

Dep. var.:	2020 PPP loan		2020 EIDL grant		2021 PPP loan	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: SBRG			
Lottery winner	−6.2**	−1.9	3.4**	1.4	−0.5	1.4
	(0.7)	(1.1)	(0.7)	(1.2)	(0.5)	(0.9)
Has EIN	26.2**	23.6**	9.3**	9.1**	−11.4**	−10.8**
	(0.7)	(0.7)	(0.7)	(0.7)	(0.6)	(0.6)
Constant	31.8**	32.1**	38.6**	39.5**	26.1**	25.0**
	(0.6)	(0.7)	(0.6)	(0.7)	(0.6)	(0.6)
Group g FE		✓		✓		✓
Observations	23,223	23,223	23,223	23,223	23,223	23,223
			Panel B: MSCRG			
Lottery winner	−7.4**	0.1	−8.3**	0.7	3.1**	0.0
	(0.7)	(0.9)	(0.7)	(1.0)	(0.6)	(0.9)
Has EIN	28.5**	20.8**	8.3**	6.1**	−19.7**	−14.5**
	(0.7)	(0.7)	(0.8)	(0.8)	(0.7)	(0.8)
Constant	21.6**	23.0**	40.2**	37.4**	38.3**	36.3**
	(0.6)	(0.7)	(0.7)	(0.8)	(0.7)	(0.8)
Group g FE		✓		✓		✓
Observations	18,571	18,557	18,571	18,557	18,571	18,557

Notes: The dependent variable in columns (1) and (2) equals 100 if the firm received a PPP loan on or before August 8, 2020, when the initial PPP expired, and 0 otherwise. The dependent variable in columns (3) and (4) equals 100 if the firm received a Targeted EIDL Advance grant and 0 otherwise. The dependent variable in columns (5) and (6) equals 100 if the firm received a PPP loan between January 15, 2021, when the program restarted, and June 30, 2021 and 0 otherwise. Heteroskedastic robust standard errors in parentheses.

5 The Effect of a Windfall on Business Owners

This section presents the owner-level outcomes from the credit bureau data. Section 5.1 shows the time path of treatment effects for spending, autos, and delinquency for both programs. We focus on these outcomes because they broadly summarize the information available in the credit bureau data. Section 5.2 reports the instrumental variable estimates. We find no evidence of higher spending on cards, but affirmative evidence of auto acquisition by MSCRG recipients and lower future delinquency rates among SBRG recipients.

5.1 Reduced-Form Dynamic Treatment Effects

Figure 7 plots the time path of reduced form treatment effects $\{\beta_\ell\}$ for card spending, auto registrations, and delinquencies using equation (4). Since we observe monthly card spending and the exact date of auto registrations, for these outcomes we define event time as starting with the month of the first lottery draw (July 2020 for SBRG and October 2021 for MSCRG) and set the omitted period as the six months before the lottery. We observe the delinquency flag only every 6 months and hence define event time for this variable relative to the 6 month period including the first lottery.

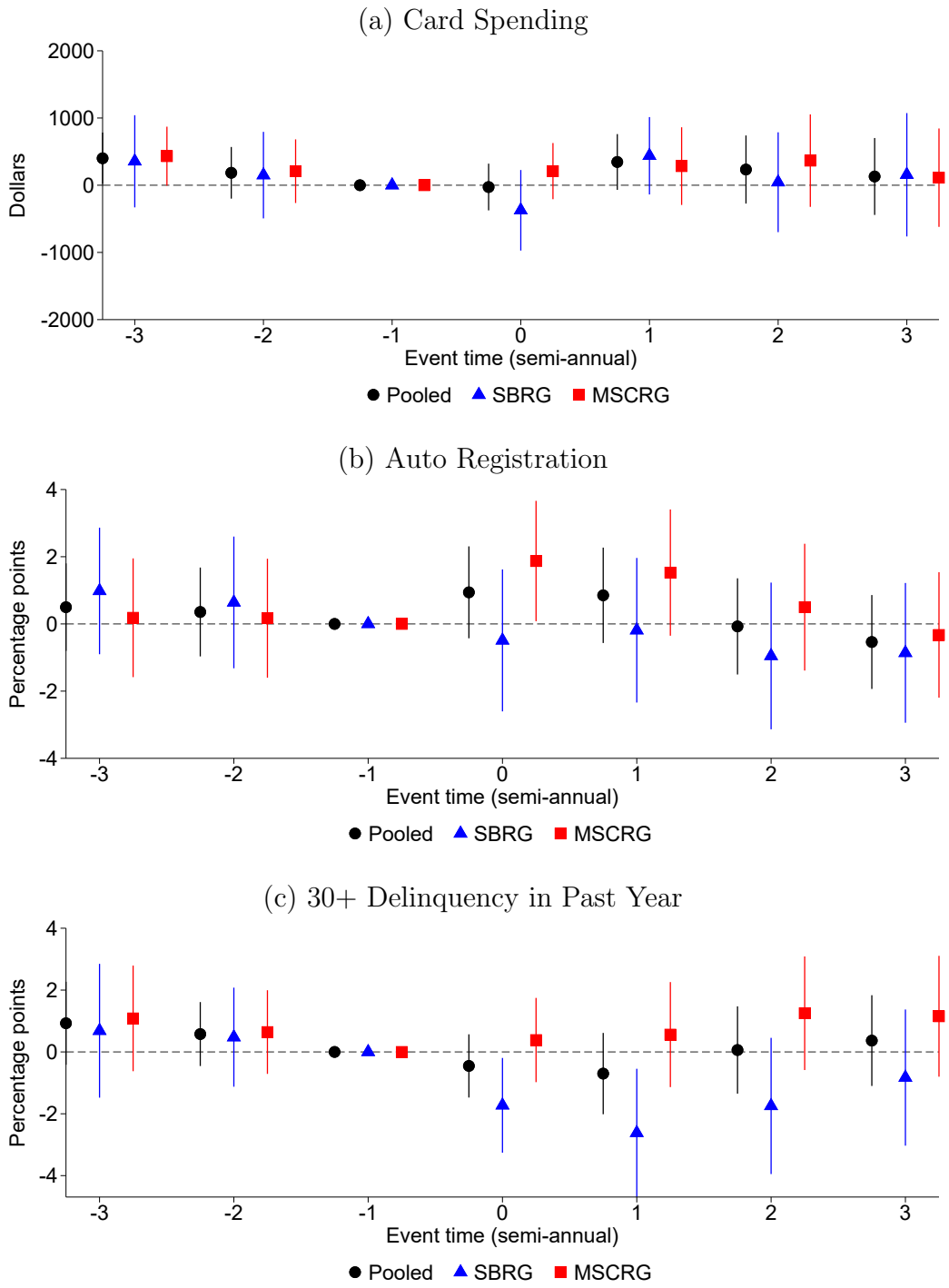
The coefficients from the spending specification are uniformly small, across both programs and all treatment horizons. In contrast, acquisition of autos and delinquencies both exhibit seeming heterogeneity across programs. The auto coefficients for winners of the SBRG lottery are close to zero and indeed slightly negative. Winners of the MSCRG lottery appear to have been more likely to acquire a car in the months after the lottery. The 95% confidence interval for the 6 months after the initial lottery excludes zero, while the response for the next 6 months is significant at the 10% level.[22] For delinquencies, only winners of the SBRG lottery exhibit an economically meaningful and statistically significant response, with a decline of about 2p.p. in the 30+ day delinquency rate following a lottery win.

5.2 Credit Card Spending, Car Purchases, and Delinquency

Table 9 shows regression outcomes for card spending, auto registrations, and delinquency. For compactness, we report second stage coefficients only. Card spending and auto registration outcomes in columns (1)-(3) define event time as starting with the month of the first lottery draw. For MSCRG we also know in which draw a firm was selected (recall this information was not tracked by the program administrators for SBRG). We exploit this information in column (4), which defines event time relative to the draw date on which a firm won and stacks all of the draw dates into a single regression, with the never-winners serving as the control set for each draw date. We also simplify the specification in column (4) to include a single cross-section for each draw date, with the dependent variable in Panel (A) the change in spending from 6 months before to 6 months after and in Panels (B) and

[22] Recall that about two-thirds of the MSCRG winners were selected in the initial lottery draw, with the remainder spread over subsequent draws through June 2022. Thus, the positive coefficients at horizons 0 and 1 reflect winners of the initial lottery who acquired a car in the next 12 months and winners of later lotteries who acquired a car more quickly.

Figure 7: Reduced Form Dynamic Owner Responses



Notes: The dots plot the coefficients β_ℓ from the reduced form regression $Y_{i,g,\ell} = \sum_{\ell \neq 0} \beta_\ell W_{i,g,\ell;\mathcal{P}} + \delta_{i,\mathcal{P}} + \alpha_{g,\ell,\mathcal{P}} + e_{i,g,\ell}$, where ℓ indexes the time since the treatment period. The bars show the 95% confidence intervals based on standard errors clustered by firm.

(C) an auto registration or deliquency in the 6 months after the draw date.[23] Event time for delinquency is relative to the 6 month period including the first lottery. The endogenous variable for spending is award amount, while for the categorical outcomes of auto registration and delinquency it is an indicator for award receipt.

The IV results confirm and quantify the impression given by the dynamic reduced form plots. The point estimates for card spending are close to 0 and the confidence intervals reject increases in spending of more than \$0.03 per dollar of receipt in columns (1)-(3) and \$0.06 per dollar of receipt in column (4). Such low spending responses may reflect the large size of the payments relative to typical consumer stimulus amounts (Andre, Flynn, Nikolakoudis and Sastry 2025) and the fact that business owners tend to have relatively high incomes. Alternatively, they come with the caveat that card spending in the credit bureau data may omit a substantial amount of actual expenditure.

In contrast, Panel (B) shows evidence of additional spending on autos among recipients of MSCRG grants. Column (2) shows a negative point estimate for SBRG recipients, although the confidence interval cannot rule out a small positive effect. Column (3) shows that receipt of an MSCRG grant increases the probability of a new auto registration in the year after the first lottery by 2.3 p.p., relative to a base rate of 10p.p. in the control group. The difference between the SBRG and MSCRG coefficients could reflect the presence of other cash transfers available to all businesses around the rollout of SBRG (e.g., PPP and EIDL advances), or statistical noise. Column (4) shows an even larger increase of 3.3p.p. in the 6 months after the lottery date, with a t-statistic of 3.2. The larger value in column (4) may reflect attenuation bias in column (3) stemming from the (non-classical) misclassification of event dates for winners of later draws. The positive response of car-buying and non-response of card spending aligns with the analysis by Orchard, Ramey and Wieland (2025) of consumer stimulus programs.

It is possible the auto purchases we observe are for business use rather than personal use. In smaller privately-held firms, the line between how vehicles are used is often blurred, as consuming through allowable business purchases is a well-documented tax preference (Kopczuk and Zwick 2020, Leite 2024). We do not have access to detailed investment items

[23]That is, the second stage specification in column (4) is $Y_{i,g,t} = \tau \hat{R}_{i,t} + \alpha_{g,t} + \epsilon_{i,g,t}$, where t indexes draw dates, the set of observations at each t includes all lottery winners on that date and all applicants that never won the lottery, and $Y_{i,g,t}$ denotes the 6 month change in spending (Panel (A)), registration of a new auto in the 6 months after the lottery (Panel (B)), or a 30+ day delinquency in the first full six month period ending in June or December after the lottery (Panel (C)).

Table 9: Instrumental Variable Owner Outcomes

Treatment horizon:	12 months after first draw			6 months after draw date
	(1)	(2)	(3)	(4)
	Panel A dep. var.: card spending			
Award	−0.02	−0.02	−0.01	0.01
	(0.02)	(0.03)	(0.03)	(0.03)
	[-0.05,0.02]	[-0.08,0.03]	[-0.06,0.03]	[-0.04,0.06]
Specification	Panel	Panel	Panel	First-difference
Control mean	12076	11606	12887	824
Observations	110,100	61,643	48,457	43,458
	Panel B dep. var.: new auto registration			
Recipient	0.84	−1.14	2.28*	3.33**
	(0.74)	(1.10)	(1.01)	(1.05)
	[-0.62,2.29]	[-3.29,1.01]	[0.31,4.26]	[1.28,5.38]
Specification	Panel	Panel	Panel	Cross-section
Control mean	12	13	10	10
Observations	128,622	70,842	57,780	51,205
	Panel C dep. var.: 30+ day delinquency			
Recipient	−1.49+	−3.21**	−0.22	0.85
	(0.88)	(1.24)	(1.23)	(1.56)
	[-3.22,0.24]	[-5.64,-0.78]	[-2.63,2.19]	[-2.21,3.92]
Specification	Panel	Panel	Panel	Cross-section
Control mean	21	20	23	19
Observations	127,211	70,297	56,914	51,284
Program	Pooled	SBRG	MSCRG	MSCRG
Firm × lottery FE	✓	✓	✓	
Post × group g FE	✓	✓	✓	
Group $g\times$ draw date FE				✓
Cluster	Firm	Firm	Firm	Firm

Notes: The table reports IV regressions of the form $Y_{i,g,t} = \tau R_{i,g;\mathcal{P}} + \alpha_{g,\mathcal{P}} + e_{i,g;\mathcal{P}}$, where $R_{i,g;\mathcal{P}}$ denotes the award amount (Panel A) or an indicator for award recipiency (Panels B and C) and the instrument is winning the program lottery. The first draw for SBRG is July 2020 for SBRG and October 2021 for MSCRG. Column (1) pools all observations. Column (2) pools all SBRG participants. Column (3) pools all MSCRG participants. Column (4) stacks the MSCRG award winners by draw date, including the full set of never-winners as control observations on each draw date. Column (4) reports single cross-sections, with the dependent variable in Panel A in first-differences. Standard errors in parentheses clustered by firm and 95% confidence interval in brackets.

for the firms in our administrative tax data. However, focusing on unicorporated firms, we can ask whether recipients of the grants report increases in car and truck expenses (Appendix

Table A.3). We find no evidence of such increases, which suggests that the cars we observe in the credit bureau data are not being used for business purposes.

Panel (C) instead shows a statistically significant decline in future delinquency among SBRG, but not MSCRG, recipients. This decline appears consistent with SBRG recipients saving the funds and hence the absence of any positive responses for firm or personal spending for this group.

We address a specific and unusual source of heterogeneity in tables A.4 and A.5. Well after the programs concluded, allegations emerged of severe fraud in other Minnesota aid programs, with a particular focus on the Somali-American community (Londoño 2025). The SBRG and MSCRG application data contain an optional, self-reported variable for Somalian ethnicity.[24] Because both programs allocated a minimum funding amount to businesses owned by persons of color (see table 2), the applicants of Somali heritage concentrate in a few priority groups. We therefore restrict the sample to groups g with a share of Somali applicants below (table A.4) or above (table A.5) 10%. Both sub-samples produce similar results, suggesting that our findings do not reflect any quirk of the applicants to the SBRG and MSCRG programs.

5.3 Who Benefits from These Programs?

If the programs ultimately benefited firm owners, a salient question for program evaluation becomes the overall progressivity of the transfers. Table 10 compares the owners of applicant firms to the population of all Minnesota taxpayers. Despite the programs catering to micro and very small firms, their owners still have relatively high incomes. For example, only 31% of applicants had 2019 incomes in the bottom 50% of the distribution, while more than one quarter of applicants had income in the top 10%. The distribution of recipients tilts slightly lower income, reflecting the minimum funding thresholds for groups with slightly lower income. However, it remains similarly much higher income than the general population. On the other hand, applicants are lower income than the population of all Minnesota business owners, or even than owners of firms with 30 or fewer employees.

[24] Approximately 4% of SBRG and 13% of MSCRG applicants identify as Somali, Somali-American, or another mixed-Somali identity. We also applied a prediction algorithm to classifying all applicants based on first and last name and found similar shares.

Table 10: Business Owner Income Distribution

Income segment	Percent of owners in segment of 2019 income distribution			
	Applicants	Recipients	All MN businesses	MN businesses < 31 employees
Bottom 50	31.1	31.9	19.5	18.6
50-75	23.3	23.4	18.3	19.4
75-90	18.9	19.3	17.8	20.6
90-99	21.7	20.8	27.4	30.0
99+	5.0	4.7	17.0	11.4

Notes: Each column shows the percent distribution of a group of business owners in the 2019 income distribution of all Minnesota tax filers. In column (1) the group is SBRG and MSCRG recipients. In column (2) the group is SBRG and MSCRG applicants. In column (3) the group is all Minnesota business owners. In column (4) the group is all Minnesota business owners of firms with fewer than 31 employees. This table excludes businesses that are C-corporations.

6 Discussion and Conclusion

In this paper, we provide causal evidence of the effect of business transfers to micro and small businesses. Studying two programs that administered $125 million in grants to over 12,000 firms via randomized lottery, we do not find significant positive effects of the grants on employment or investment. Most of our point estimates are negative, and our 95% confidence intervals generally reject economically meaningful positive effects. Instead, we find evidence that some owners used the funds to finance acquisition of a car, while others bolstered personal finances.

These results have important implications beyond the specific programs we study. Business grant programs were ubiquitous at federal, state, and local levels of government during the COVID recession and subsequent recovery. These programs were largely targeted towards small and micro firms, in part due to a belief that such firms face large financing frictions. Our evidence casts doubt on that belief.

References

Andre, Peter, Joel P Flynn, Georgios Nikolakoudis, and Karthik Sastry. 2025. "Quick-Fixing: Near-Rationality in Consumption and Savings Behavior." National Bureau of Economic Research Working Paper 33464.

Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz. 2022*a*. "The $800 billion paycheck protection program: where did the money go and why did it go there?" *Journal of Economic Perspectives*, 36(2): 55–80.

Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz. 2022*b*. "An evaluation of the paycheck protection program using administrative payroll microdata." *Journal of Public Economics*, 211.

Bartik, Alexander, Zoe Cullen, Edward L Glaeser, Michael Luca, Christopher Stanton, and Adi Sunderam. 2020. "The targeting and impact of paycheck protection program loans to small businesses." *NBER Working Paper*, , (w27623).

Bartlett, Robert, and Adair Morse. 2020. "Small Business Survival Capabilities and Policy Effectiveness: Evidence from Oakland." National Bureau of Economic Research w27629.

Bernanke, Ben, and Mark Gertler. 1989. "Agency Costs, Net Worth, and Business Fluctuations." *The American Economic Review*, 79(1): 14–31.

Blanchard, Olivier Jean, Florencio Lopez-de Silanes, and Andrei Shleifer. 1994. "What do firms do with cash windfalls?" *Journal of financial economics*, 36(3): 337–360.

Bolton, Patrick, Hui Chen, and Neng Wang. 2011. "A Unified Theory of Tobin's q, Corporate Investment, Financing, and Risk Management." *The Journal of Finance*, 66(5): 1545–1578.

Bureau of Economic Analysis. 2023. "NIPA Handbook: Glossary." Accessed: 2024-07-18.

Bureau of Economic Analysis. 2024. "Federal Recovery Programs and BEA Statistics: COVID-19 Recovery." *https://www.bea.gov/federal-recovery-programs-and-bea-statistics/covid-19-recovery*, Accessed: 2024-07-18.

Chetty, Raj, John N. Friedman, Nathan Hendren, and Michael Stepner. 2020. "How did covid-19 and stabilization policies affect spending and employment? a new real-time economic tracker based on private sector data." National Bureau of Economic Research w27431.

Chodorow-Reich, Gabriel. 2014. "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-9 Financial Crisis." *The Quarterly Journal of Economics*, 129(1): 1–59.

Chodorow-Reich, Gabriel, Ben Iverson, and Adi Sunderam. 2022. "Lessons Learned from Support to Business during COVID-19." *Recession Remedies: Lessons Learned from the U.S. Economic Policy Response to COVID-19*, , ed. Wendy Edelberg, Louise Sheiner and David Wessel, 153–179.

Cole, Allison. 2022. "The Impact of the Paycheck Protection Program on (Really) Small Businesses." *Available at SSRN 3730268*.

Denes, Matthew, Spyridon Lagaras, and Margarita Tsoutsoura. 2021. "First Come, First Served: The Timing of Government Support and Its Impact on Firms." SSRN working paper.

Doniger, Cynthia, and Benjamin Kay. 2021. "Ten Days Late and Billions of Dollars Short: The Employment Effects of Delays in Paycheck Protection Program Financing." Federal Reserve Working Paper.

Fairlie, Robert, and Frank M Fossen. 2022. "Did the Paycheck Protection Program and Economic Injury Disaster Loan Program get disbursed to minority communities in the early stages of COVID-19?" *Small Business Economics*, 58(2): 829–842.

Faulkender, Michael, Robert Jackman, and Stephen Miran. 2020. "The Job Preservation Effects of Paycheck Protection Program Loans." US Department of the Treasury, Office of Economic Policy Working Paper 2020-01.

Fazzari, Steven M., R. Glenn Hubbard, and Bruce C. Petersen. 1988. "Financing Constraints and Corporate Investment." *Brookings Papers on Economic Activity*, 141–195.

Granja, João, Christos Makridis, Constantine Yannelis, and Eric Zwick. 2022. "Did the Paycheck Protection Program Hit the Target?" *Journal of Financial Economics*.

Griffin, John M., Samuel Kruger, and Prateek Mahajan. 2023. "Did FinTech Lenders Facilitate PPP Fraud?" *The Journal of Finance*, 78(3): 1777–1827.

Haltiwanger, John, Ron S. Jarmin, and Javier Miranda. 2013. "Who Creates Jobs? Small versus Large versus Young." *The Review of Economics and Statistics*, 95(2): 347–361.

Hanson, Samuel, Jeremy Stein, Adi Sunderam, and Eric Zwick. 2020. "Business Credit Programs in the Pandemic Era." *Brookings Papers on Economic Activity*.

Hubbard, R. Glenn, and Michael Strain. 2020. "Has the Paycheck Protection Program Succeeded?" National Bureau of Economics Working Paper 28032.

Hurst, Erik, and Benjamin Wild Pugsley. 2011. "What Do Small Businesses Do?" *Brookings Papers on Economic Activity*. Accessed: [Access Date].

Kaplan, Steven N., and Luigi Zingales. 1997. "Do investment-cash flow sensitivities provide useful measures of financing constraints?" *Quarterly Journal of Economics*, 112: 159–216.

Kopczuk, Wojciech, and Eric Zwick. 2020. "Business Incomes at the Top." *Journal of Economic Perspectives*, 34(4): 27–51.

Leite, David. 2024. "The Firm as Tax Shelter: Micro Evidence and Aggregate Implications of Consumption Through the Firm." World Inequality Lab Working Paper 2024/25.

Li, Meng. 2021. "Did the small business administration's COVID-19 assistance go to the hard hit firms and bring the desired relief?" *Journal of Economics and Business*, 115: 105969.

Londoño, Ernesto. 2025. "How Fraud Swamped Minnesota's Social Services System on Tim Walz's Watch." *The New York Times*. Updated 7:08 a.m. ET.

Orchard, Jacob D, Valerie A Ramey, and Johannes F Wieland. 2025. "Micro MPCs and Macro Counterfactuals: The Case of the 2008 Rebates." *The Quarterly Journal of Economics*, 140(3): 2001–2052.

Small Business Administration. 2024. "Paycheck Protection Program FOIA Data." Accessed: 2024-07-18.

Walz, Tim. 2020. "Emergency Executive Order 20-74." Accessed: 2024-07-18.

Whited, Toni M., and Guojun Wu. 2006. "Financial Constraints Risk." *The Review of Financial Studies*, 19(2): 531–559.

Winberry, Thomas, Adrien Auclert, Matthew Rognlie, and Ludwig Straub. 2025. "New Keynesian Economics with Household and Firm Heterogeneity." National Bureau of Economic Research Working Paper 34611.

Zwick, Eric, and James Mahon. 2017. "Tax Policy and Heterogeneous Investment Behavior." *American Economic Review*, 107(1): 217–48.

What (Small) Firms Don't Do With Cash Windfalls

Online Appendix

Gabriel Chodorow-Reich Adam Isen
Adi Sunderam Eric Zwick

March 2026

A Additional Tables and Figures

Table A.1: PPP Forgiven Loans Size Distribution

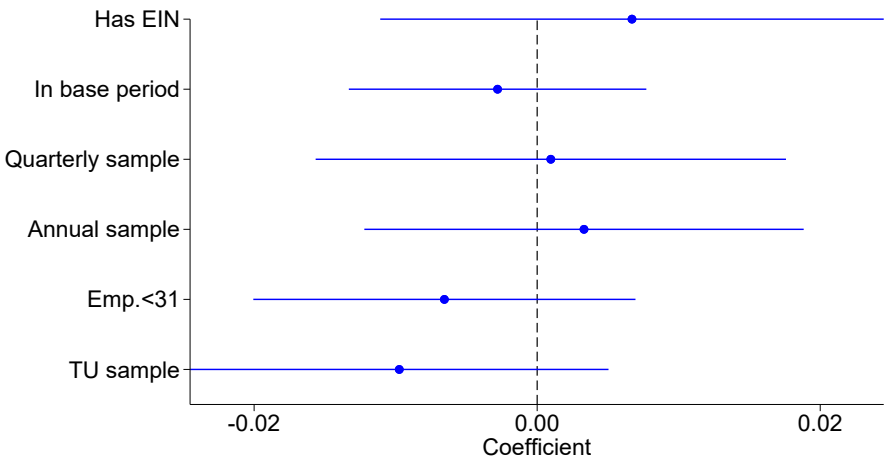
Grant size	Count	Dollars
0-4,999	1,405,693	3,761,910,493
5,000-9,999	1,570,596	10,807,890,650
10,000-14,999	1,232,193	14,163,991,538
15,000-19,999	1,099,920	17,306,347,174
20,000-24,999	2,349,157	41,606,758,998
25,000-49,999	1,315,111	45,723,570,035
50,000-99,999	1,053,991	73,340,500,972
100,000-10,000,000	1,441,619	555,735,296,527
Total	11,468,285	762,446,346,768

Table A.2: Award Timing

Draw date	Winners	Recipients
	SBRG	
July 16, 2020		4,166
September 29, 2020		479
October 28, 2020		278
Other		1,227
Total	7,819	6,150
	MSCRG	
October 14, 2021	6,000	3,900
January 22, 2022	744	414
February 25, 2022	978	694
March 11, 2022	315	210
March 31, 2022	239	150
April 22, 2022	211	145
May 17, 2022	275	245
June 7, 2022	221	221
Total	8,983	5,979

Notes: the table shows the timing of lottery winners and recipients, by draw date. This information is not available for some of the SBRG draws.

Figure A.1: Balancing of Sample Restrictions



Notes: The figure reports the coefficient and 95% confidence interval pertaining to the reduced form regression with the dependent variable an indicator for belonging to the sample indicated on the vertical axis and the right hand side variable an indicator for winning the lottery. The regressions also control for group g fixed effects.

Table A.3: All Annual Return Front Page Items

	τ	SE	$P_{0.025}$	$P_{0.975}$	Control mean
Revenue	−0.96	0.89	−2.71	0.79	518,237
COGS	−0.23	0.40	−1.01	0.55	160,423
All deductions	−0.73	0.50	−1.71	0.26	299,242
Net income	0.05	0.20	−0.35	0.45	30,057
Deductions reported for all filers					
Interest	−0.00	0.01	−0.03	0.02	3,784
Salaries, pensions, benefits	−0.36	0.23	−0.80	0.09	108,365
Repairs and maintenance	−0.03	0.02	−0.06	0.01	5,272
Deductions reported for non-Schedule C filers					
Compensation of officers	−0.02	0.09	−0.19	0.16	41,066
Rents	−0.05	0.08	−0.21	0.10	34,057
Taxes and licenses	−0.02	0.05	−0.12	0.08	21,149
Bad debts	0.00	0.00	−0.00	0.00	19
Charitable contributions	−0.00	0.00	−0.00	0.00	14
Depletion	−0.00	0.00	−0.00	0.00	2
Advertising	0.01	0.02	−0.03	0.04	4,921
Other	−0.26	0.23	−0.70	0.18	113,500
Deductions reported for Schedule C filers					
Car and truck expenses	−0.01	0.02	−0.05	0.02	2,626
Meals	0.00	0.01	−0.01	0.02	544
Travel	0.00	0.01	−0.01	0.01	499
Insurance	0.00	0.02	−0.03	0.03	2,583
Legal and professional services	0.01	0.01	−0.01	0.02	909
Office expense	0.01	0.01	−0.01	0.03	1,180
Home office	0.01	0.00	0.00	0.02	407
Other	−0.09	0.09	−0.26	0.08	13,481

Notes: Each row reports the coefficient, standard error, 95% confidence interval, and control mean for the variable indicated using the specification in equation (3). Standard errors clustered by firm.

Table A.4: Instrumental Variable Owner Outcomes With Low Somali Share

Treatment horizon:	12 months after first draw			6 months after draw date
	(1)	(2)	(3)	(4)
	Panel A dep. var.: card spending			
Award	−0.01	−0.04	−0.00	0.03
	(0.02)	(0.02)	(0.03)	(0.03)
	[-0.05,0.02]	[-0.09,0.01]	[-0.05,0.05]	[-0.03,0.09]
Specification	Panel	Panel	Panel	First-difference
Control mean	11270	10716	12642	748
Observations	96,848	58,129	35,205	34,040
	Panel B dep. var.: new auto registration			
Recipient	0.34	−1.35	2.32^{+}	3.01^{*}
	(0.84)	(1.19)	(1.31)	(1.39)
	[-1.31,1.99]	[-3.69,0.98]	[-0.26,4.89]	[0.29,5.73]
Specification	Panel	Panel	Panel	Cross-section
Control mean	12	13	10	10
Observations	111,328	66,193	40,486	38,797
	Panel C dep. var.: 30+ day delinquency			
Recipient	-2.19^{*}	-2.49^{+}	−0.84	0.81
	(0.92)	(1.31)	(1.35)	(1.64)
	[-3.99,-0.39]	[-5.06,0.09]	[-3.49,1.81]	[-2.40,4.02]
Specification	Panel	Panel	Panel	Cross-section
Control mean	20	20	19	16
Observations	110,377	65,756	40,080	38,830
Program	Pooled	SBRG	MSCRG	MSCRG
Firm × lottery FE	✓	✓	✓	
Post × group g FE	✓	✓	✓	
Group g× draw date FE				✓
Cluster	Firm	Firm	Firm	Firm

Notes: The table reports IV regressions of the form $Y_{i,g,t} = \tau R_{i,g;\mathcal{P}} + \alpha_{g,\mathcal{P}} + e_{i,g;\mathcal{P}}$, where $R_{i,g;\mathcal{P}}$ denotes denotes the award amount (Panel A) or an indicator for award recipiency (Panels B and C) and the instrument is winning the program lottery. The first draw for SBRG is July 2020 for SBRG and October 2021 for MSCRG. Column (1) pools all observations. Column (2) pools all SBRG participants. Column (3) pools all MSCRG participants. Column (4) stacks the MSCRG award winners by draw date, including the full set of never-winners as control observations on each draw date. Column (4) reports single cross-sections, with the dependent variable in Panel A in first-differences. Standard errors in parentheses clustered by firm and 95% confidence interval in brackets. The sample restricts to groups g with a share of applicants of Somalian ethnicity of less than 10%.

Table A.5: Instrumental Variable Owner Outcomes With High Somali Share

Treatment horizon:	12 months after first draw			6 months after draw date
	(1)	(2)	(3)	(4)
	Panel A dep. var.: card spending			
Award	−0.02	0.05	−0.02	−0.01
	(0.02)	(0.06)	(0.03)	(0.03)
	[-0.06,0.01]	[-0.07,0.18]	[-0.08,0.05]	[-0.08,0.06]
Specification	Panel	Panel	Panel	First-difference
Control mean	10348	6651	7914	779
Observations	74,895	3,514	13,252	9,418
	Panel B dep. var.: new auto registration			
Recipient	0.14	0.10	2.24	3.66*
	(0.90)	(2.81)	(1.56)	(1.57)
	[-1.62,1.91]	[-5.41,5.62]	[-0.82,5.30]	[0.58,6.74]
Specification	Panel	Panel	Panel	Cross-section
Control mean	12	8	9	9
Observations	88,136	4,649	17,294	12,408
	Panel C dep. var.: 30+ day delinquency			
Recipient	−1.79	−7.61*	0.55	0.90
	(1.13)	(3.69)	(2.18)	(2.69)
	[-4.01,0.42]	[-14.85,-0.37]	[-3.73,4.84]	[-4.37,6.16]
Specification	Panel	Panel	Panel	Cross-section
Control mean	22	34	36	30
Observations	87,131	4,541	16,834	12,454
Program	Pooled	SBRG	MSCRG	MSCRG
Firm × lottery FE	✓	✓	✓	
Post × group g FE	✓	✓	✓	
Group g× draw date FE				✓
Cluster	Firm	Firm	Firm	Firm

Notes: The table reports IV regressions of the form $Y_{i,g,t} = \tau R_{i,g;\mathcal{P}} + \alpha_{g,\mathcal{P}} + e_{i,g;\mathcal{P}}$, where $R_{i,g;\mathcal{P}}$ denotes denotes the award amount (Panel A) or an indicator for award recipiency (Panels B and C) and the instrument is winning the program lottery. The first draw for SBRG is July 2020 for SBRG and October 2021 for MSCRG. Column (1) pools all observations. Column (2) pools all SBRG participants. Column (3) pools all MSCRG participants. Column (4) stacks the MSCRG award winners by draw date, including the full set of never-winners as control observations on each draw date. Column (4) reports single cross-sections, with the dependent variable in Panel A in first-differences. Standard errors in parentheses clustered by firm and 95% confidence interval in brackets. The sample restricts to groups g with a share of applicants of Somalian ethnicity of more than 10%.