The One-Dollar Company: Filing Fees and What Business Registrations Measure

Draft prepared for Ethan Mollick by Claude, an AI research agent[1]

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Abstract

Counts of new business registrations are used to measure entrepreneurship, and governments change the fees that registrants pay. We study three changes in a formation fee of about $50 and follow companies formed around each into records that operating businesses leave. When Colorado twice charged $1 instead of $50, registrations rose by 17%; when the United Kingdom raised its fee from £12 to £50, they fell by 17%. In pre-specified tests, registrations that responded to the fee had such records less often than others. Their relative rate is 0.07 for Paycheck Protection Program loans (95% confidence interval −0.57 to 0.60; second reduction, 0.21), −0.02 for any of three lending records (−0.65 to 0.41; second reduction, 0.20), and 0.00 for non-dormant U.K. accounts (confidence set −2.5 to 0.42). Alternative comparison periods give pooled estimates from −0.41 to 0.81 for the loans and at most 0.34 for the other two outcomes. These rates describe records. They describe businesses only if businesses among such registrations are recorded as often as others, and they do not measure a registration's value to its filer. Changes in registrations after fee changes should not be read one-for-one as changes in the number of operating businesses.

Keywords: entry costs; business registration; measurement of entrepreneurship; filing fees; selection

[1]This draft, its analyses, and its code were produced by Claude, an AI model developed by Anthropic, working as a research agent at Ethan Mollick's request; "we" in the text denotes Claude. No person has verified the results, rerun the code, or audited the links between records. Before any submission, a named author would need to rerun the code from the archived raw files, recompute the main estimates independently, audit a sample of the record links and the sources cited, and take responsibility for the paper; decisions about authorship, submission, and any public deposit of the analysis plans rest with Ethan Mollick. The plans were written by Claude at most a few hours before the tabulations that they govern and were not deposited outside the project (Section 4.6; Appendix B). A separate research log, archived with the code and the result files, records the rounds of review and the corrections that followed them; the reports of the reviews and the responses to them are in the same archive.

1. Introduction

Governments set the fee to form a company, and they and researchers then count the companies that are formed. Several U.S. states have recently reduced or waived the fee in order to encourage entrepreneurship. Colorado, California, and Montana did so in 2022 (California Legislative Analyst's Office, 2022; Colorado General Assembly, 2022; Montana Secretary of State, 2022), Kansas announced a reduction of its fee to $90 in 2026 (Kansas Secretary of State, 2026), a proposal to reduce the Massachusetts fee from $500 to $100 was before the legislature in the same year (Banker & Tradesman, 2026), and a model act circulated to state legislatures provides that a state "shall encourage the elimination of all first-year business fees" (American Legislative Exchange Council, 2022). The United Kingdom has moved in the opposite direction, raising its incorporation fee from £12 to £50 in 2024 and to £100 in 2026 to fund the policing of its register. The evidence cited in these debates is the number of new registrations. The Colorado Secretary of State's office reported a "record increase in new business filings caused by filing fee reduction" (Colorado Secretary of State, 2014d), and the U.K. government states that it will monitor "register size and incorporations" (Department for Business and Trade, 2025). Researchers likewise construct the timeliest measures of entrepreneurship from registrations and from applications for Employer Identification Numbers (Guzman and Stern, 2020; Haltiwanger, 2022). Whether a change in such a count is a change in the number of businesses depends on which registrations respond to the fee.

We study three changes in a fee of about $50. Colorado charged $1 instead of $50 to form a limited liability company (LLC) for three months from December 2012 and for four months from July 2014, and the United Kingdom raised its fee to incorporate a company from £12 to £50 in May 2024. In each case forming a company already took minutes, and no procedure changed. Both jurisdictions publish their registers, so that every company formed around each change can be counted and followed into records that operating businesses leave. In Colorado these are lending records created by third parties: loans under the Paycheck Protection Program (PPP) of 2020 and 2021, loans guaranteed by the Small Business Administration (SBA), and lenders' financing statements under the Uniform Commercial Code (UCC). In the United Kingdom the record is a set of accounts other than dormant accounts, which a company that has traded is required to file.

Our measure is the rate at which the registrations that respond to the fee have a given record, relative to the rate among other registrations, which we denote by ρ. It can be estimated without observing which registrations respond. If a fee reduction adds a share s of a cohort, and the added registrations have the record ρ times as often as the others, the share of the cohort with the record falls by the proportion $s(1-\rho)$. The relative rate is also the response of the number of companies with the record relative to the response of registrations: if ρ is one, that number changes in proportion to registrations, and if ρ is zero, it does not change.

Registrations respond strongly to the fee (Figure 1). The two Colorado fee holidays raised registrations of LLCs by 17% (95% confidence interval 13 to 22), and the U.K. increase reduced registrations by 17% (4 to 29). The registrations that responded were less likely than other registrations to leave the records that we observe. In the tests that we specified before tabulating the outcomes, the estimate of ρ is 0.07 for the PPP loan, with a 95% confidence interval from −0.57 to 0.60, and −0.02 for any of the three lending records, which we also call the broader lending record (−0.65 to 0.41). For the second holiday alone, which reduced

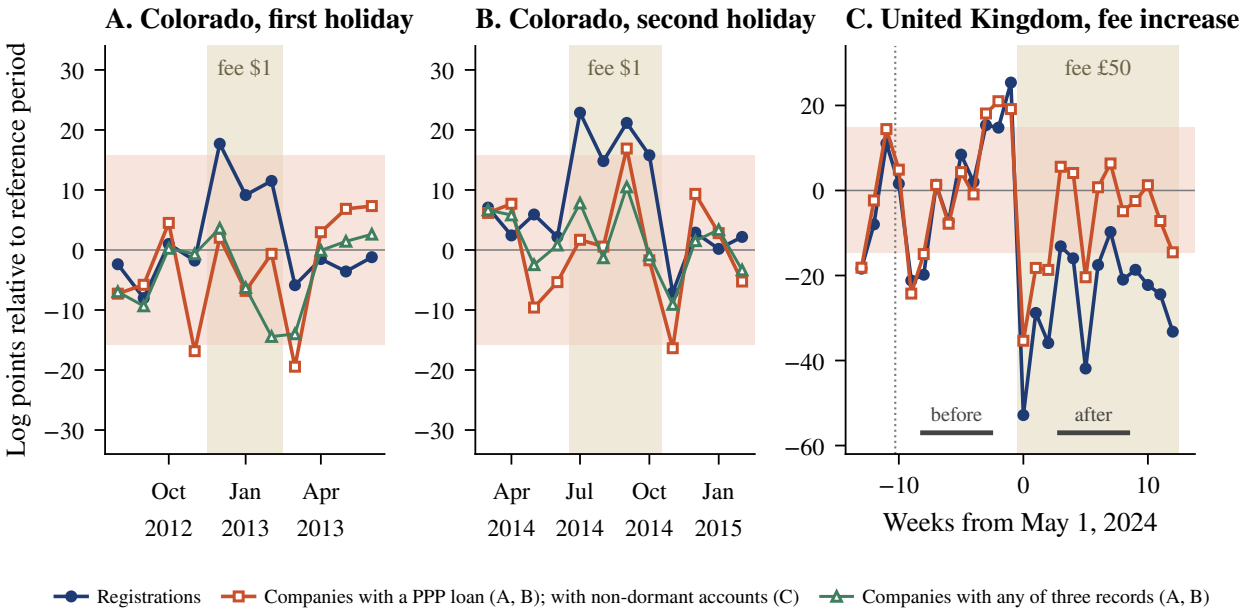


Figure 1. Registrations and the Number of Companies with a Record of Business Activity around Three Fee Changes

Notes. Panels A and B: Colorado, by month of formation. Filings to form a limited liability company, excluding bulk filings (Section 4.2), the number of those companies that later received a Paycheck Protection Program (PPP) loan (the primary outcome of the pre-specified test), and the number with any of three lending records (a PPP loan, a loan guaranteed by the Small Business Administration, or a lender's financing statement), each in log points (one log point is one-hundredth of a unit of the natural logarithm) relative to the mean of the same month one year earlier and one year later. Shading marks the months in which the fee was $1; the band is two standard errors of the single-month statistic for companies with a PPP loan. In both panels, the number with a PPP loan is below its reference level in the months flanking the fee holiday, a pattern that Section 6.2 examines. Panel C: United Kingdom, by week of incorporation around the fee increase of May 1, 2024. Registrations per working day, and the number of those companies with non-dormant accounts on file in October 2026, each in log points relative to the mean of the same week in the reference years and normalized to average zero over weeks −8 to −3; the bars mark the before and after periods of the pre-specified test, the band is two standard deviations of one week in the reference years, and the dotted line marks the announcement of February 19. Appendix Figure A4 shows the series for any of the three records with its own band. The pre-specified test for Colorado concerns the shares of companies with each record, whereas the numbers plotted in Panels A and B were constructed afterward [E]; in Panel C, the weekly series are those that Addendum 4 specified for a figure [A4]. Section 4.6 defines the bracketed labels.

no other fee, the two estimates are 0.21 (−0.55 to 0.81) and 0.20 (−0.53 to 0.67). Among new filers in the two holidays together, who had formed no company in the preceding year, they are −0.22 (−1.64 to 0.60) and −0.25 (−1.23 to 0.45). For non-dormant accounts in the United Kingdom the estimate is 0.00, with a confidence set from −2.5 to 0.42. We had derived from the Colorado results the prediction that the share of new U.K. companies with such accounts would rise, and had recorded it before tabulating any outcome of a company formed around the increase. Each of these intervals, and the U.K. set, excludes one. Under the same comparisons, the number of companies with the record changed by +1% (−7 to +11) for the PPP loan, 0% (−7 to +7) for any of the three lending records, and 0% (−12 to +14) for U.K. accounts, beside changes of about one-sixth in registrations. These estimates are near zero, but the intervals admit changes in the number of such companies that are smaller than the change in registrations and are not negligible.

The strength of this evidence differs by outcome, and Section 8 reports each estimate under alternative reference periods, reference years, definitions of the outcome, and methods of joint inference (Table 5). For the PPP loan, the primary outcome in Colorado, the estimate depends on the reference period. Companies

formed in the month before and the month after each holiday, at the regular fee, were also less likely than their reference cohorts to have a PPP loan. When the holiday months are pooled with those months the estimate is −0.41, and when they are compared with those months it is 0.67 to 0.81, with a confidence interval that includes one; for the second holiday alone, the corresponding estimates are −0.05 and 0.58 to 0.73. The data do not determine which comparison is appropriate. For any of the three lending records, the estimate for the two holidays together lies between −0.23 and 0.34 under six reference periods, and every confidence interval excludes one; for the second holiday alone the estimate lies between 0.11 and 0.54, and two of the six intervals include one. For U.K. accounts, the estimate is 0.28 when the base period precedes the announcement of the increase, and the set then extends to 0.95; the set includes one only in a few comparisons that rest on two or three reference years (Sections 7.3 and 7.4; Appendix D.6). Under the pre-specified comparison, the upper end of the set by test inversion is 0.42 under the error model of Addendum 4 and lies between 0.32 and 0.58 under the other assumptions that Section 7.2 examines about the errors of the two estimated changes, in registrations and in the share with non-dormant accounts. It is below one-half at any within-year correlation of these errors of −0.45 or stronger, a condition that each of the three estimates of that correlation meets, and the exclusion of one does not depend on those assumptions. We therefore report the evidence by outcome.

These estimates measure the probability of leaving a record. A relative rate of records equals a relative rate of operating businesses only if the businesses among the registrations that respond to the fee are as likely as other businesses to leave the record, a condition that Section 3 states formally. Equal rates of operating businesses in the two groups would be consistent with the pre-specified intervals and the U.K. set only if the businesses among the registrations that respond to the fee left the record at no more than 0.60, 0.41, and 0.42 times the rate of other businesses for the three outcomes. The U.K. record is a statutory filing, and the estimate is similar when accounts ever filed are counted, whether or not the company has since left the register. In Colorado, a business may borrow in its owner's name. We therefore link the PPP loans made to persons to the companies for which the same person is the registered agent. With such loans counted, the estimate for the PPP loan under the pre-specified comparison is 0.18 when a person's loan is attributed to every company of which the person is the agent and 0.04 when only the loans that match a single company are counted, compared with 0.07 without them, and both confidence intervals exclude one. Under the first attribution, the interval also excludes one when such loans are weighted to allow for links that were not found, at every weight up to five, so that borrowing in the agent's name in place of the company's does not account for the result under the pre-specified comparison unless the link finds fewer than about one such loan in five; for the second holiday alone, the interval reaches one at a weight of three.

The registrations that respond to a fee of this size are less likely than other registrations to leave records of lending or of non-dormant accounts. The tests show this directly; for the PPP loan the conclusion depends on the reference period, and for the broader lending record in the two holidays together and for U.K. accounts it does not. If the businesses among them are recorded about as often as other businesses, such registrations are also less likely to be the entities of operating businesses, and a change in registrations that follows a change in the fee then overstates the change in the number of operating businesses. The results do not show that such registrations lack value; they are silent on uses of a company that leave none of these records.

The paper contributes to two literatures. Research on entry costs studies reforms that bundle fees with

procedures, delays, and capital requirements (Branstetter et al., 2014; Djankov et al., 2002; Klapper et al., 2006); we isolate a price, in settings where registration already took minutes, and examine both a reduction and an increase. Research that measures entrepreneurship from registrations and applications (Andrews et al., 2022; Bayard et al., 2018; Guzman and Stern, 2020) emphasizes that the average registration is not a growth-oriented business, and earlier work cautions that incorporation should not be equated with entrepreneurship (Massenz, 2025). We add estimates for the registration at the margin of a fee, in two registers in which every company can be followed into administrative records. For the registrars and legislatures that set fees, the results imply that a change in the fee is a break in the series of registrations, and that the number of registrations that later show a first record of activity should be reported with it.

The analyses were specified in five dated documents written at most a few hours before the tabulations that they govern and not deposited outside the project, and we treat them as a record of intentions (Section 4.6); analyses that were not so specified are marked [E]. Among the latter are the confidence sets by test inversion, the direct test of the U.K. ratio, the numbers of Colorado companies with each lending record, every analysis of what the records capture, and the comparison of outcomes across reference periods, on which the weight that this paper gives to the broader lending record rests. No person has rerun the code or audited the links between records. The evidence comes from two registers and from fees of about $50 or £50, and it does not extend to the fees of $90 to $500 that are the subject of current proposals in other states.

Section 2 reviews the related literature, and Section 3 sets out the framework. Section 4 describes the fee changes, the data, and the methods. Section 5 reports the response of registrations, Section 6 the Colorado tests, and Section 7 the U.K. test. Section 8 collects the estimates under alternative specifications, and Section 9 concludes.

2. Related Literature

Registrations as a measure of entrepreneurship. Applications for Employer Identification Numbers provide the timeliest count of prospective businesses in the United States (Bayard et al., 2018; Haltiwanger, 2022), and state business registrations are used to measure the quantity and the quality of entrepreneurship across locations and in near real time (Andrews et al., 2022; Guzman and Stern, 2015, 2020). This literature emphasizes that quantity is distinct from quality. Most small businesses neither grow nor intend to grow (Hurst and Pugsley, 2011), incorporation distinguishes entrepreneurs from other self-employed individuals (Levine and Rubinstein, 2017), outcomes can be predicted from characteristics at founding (Guzman and Stern, 2020), and only a minority of applications lead to employer businesses (Bayard et al., 2018). Recent work treats the application and its transition to an employer business as distinct margins (Dinlersoz et al., 2026) and finds that applicants overestimate their employment "mainly because many expect to enter but do not" (Dinlersoz and Ma, 2026). State policies have been evaluated by the quantity and the quality-adjusted quantity of the registrations that follow them (Fazio et al., 2020). Evidence is lacking on how the number of registrations responds to the price of registering and on whether the registrations induced by a lower price leave the records that businesses leave. The closest result is from the Netherlands, where the abolition of the minimum capital requirement led self-employed individuals near a kink in the income tax schedule to incorporate more often, with "no significant impact on total business activity" in the short term (Massenz, 2025). A further consideration is that entry is an experiment (Kerr et al., 2014; Manso, 2016) and that less

costly experiments are undertaken with lower probabilities of success (Ewens et al., 2018). If a $1 fee admits experiments, a few should succeed, and we therefore report the number of companies with each record in addition to the rate.

Entry costs and the composition of entrants. Djankov et al. (2002) framed the debate: the regulation of entry may be a tollbooth or a screen that protects the public from undesirable entrants, and the cross-country evidence favored the tollbooth view. Higher entry costs are associated with less entry and with larger entrants (Klapper et al., 2006), and within-country reforms show that a large reduction in the cost of forming a company increases the number of firms (Appendix Table A13). One-stop shops raised the number of new firms by about 5% in Mexico (Bruhn, 2011; Kaplan et al., 2011) and by 17% in Portugal, where the marginal firms were small and less likely to survive (Branstetter et al., 2014). Reductions of minimum capital were followed by more incorporations in Europe (Braun et al., 2013), in Norway "with no deterioration in survival, profitability, productivity" (Bacher et al., 2026), and in China (Barwick et al., 2025; Cui and Wei, 2022). Where founders gained credit, wealth, or insurance against failure, the marginal entrants performed worse in some settings (Andersen and Nielsen, 2012; Kerr and Nanda, 2009) and not in others (Hombert et al., 2020). In lower-income countries, a lower cost of registration does little by itself to formalize existing firms (Asuming et al., 2025; Benhassine et al., 2018; Bruhn and McKenzie, 2014; de Mel et al., 2013; Galiani et al., 2017). The reforms that increased the number of firms removed thousands of dollars of required capital or weeks of procedure. We study the other end of this range, a fee of about $50 that changed while little else did, and we change the unit of analysis from entrants to entities, because one actor may register several. Studies of where and in what legal form firms organize find responses to fee differences across U.S. states (Häusermann, 2012), to set-up costs across European countries (Becht et al., 2008), and to substantive law and courts (Dammann and Schündeln, 2012; Kobayashi and Ribstein, 2011). In the United Kingdom, owners who shifted income by incorporating saved £2,000 to £3,000 a year in tax (Tazhitdinova, 2020), and a survey conducted for the government implied that an annual fee of £50 would reduce the number of small companies by about 1% (Department for Business, Energy and Industrial Strategy, 2021). Relative to tax savings of that size and to a predicted response of 1%, a response of one-sixth of registrations to $50 requires explanation.

Small prices as screens. Whether a small price or an ordeal selects the intended recipients has long been studied in research on transfers and health products. Ordeals can improve targeting (Alatas et al., 2016; Nichols and Zeckhauser, 1982) or worsen it by deterring those least able to bear them (Deshpande and Li, 2019). A charge of $0.60 for a bed net in Kenya reduced uptake by 60 percentage points without selecting the women who would use it (Cohen and Dupas, 2010), whereas higher prices for a water disinfectant in Zambia did screen for use (Ashraf et al., 2010). Small costs change consequential choices of low-income individuals (Abebe et al., 2021; Pallais, 2015). Where an act is nearly costless, in contrast, volume is determined by those whose payoff per act is very small (Rao and Reiley, 2012); an increase in U.S. patent fees in 1982 eliminated about one-tenth of the applications of the lowest quality (de Rassenfosse and Jaffe, 2018), and a crowdfunding platform that removed its screening received an influx of weak projects (Wessel et al., 2017). A business registry has the same margin, because an anonymous company is an inexpensive instrument for uses that the rules do not intend (Findley et al., 2015; Griffin et al., 2023); in the Paycheck Protection Program, the ordeal found to reduce fraud was a documentation requirement and not a price (Aman-Rana et al., 2023). Our

results bear on which of these cases a formation fee resembles.

3. Framework: Entities, Businesses, and Records

In models of entry costs, each founder has one project and the fee is the price of starting a business. A registry, in contrast, charges for each legal entity, and one person, firm, or formation service may register none, one, or thousands. We therefore treat the *actor* as the decision maker and the number of entities as the choice variable. The kth entity that an actor holds has an incremental value $v(k)$ to the actor, with $v(1) \geq v(2) \geq \cdots \geq 0$. For a first company, this is what a company adds relative to having none. For a subsequent company, it is what a further company adds to those already held: the separation of the liabilities of one property from those of another (Hansmann and Kraakman, 2000), a second name, or a company held ready for an idea not yet tried. Each entity costs the fee F and a non-price cost c, and the actor registers every entity with $v(k) \geq F + c$. The number of registrations is then

$$N(F) = B + E(F), \tag{1}$$

where B is the number of first entities of activities worth operating and $E(F)$ is the number of all other entities whose incremental value covers the fee. A change in a small fee alters E. Whether it also alters B is an empirical question, because a founder at the margin may have spent far more than the fee on time and advice, so that nothing ties the value of a marginal project to the fee. Appendix A gives the formal statements and proofs, and Appendix Table A1 lists the types of additional registration that the framework distinguishes, with the evidence on each.

Two properties of this account matter for the interpretation of the estimates. The first is a bound on private value. An actor who registers an entity at a reduced fee F_1 and would not have registered it at the regular fee F_0 expects from it a surplus, net of the fee and of all other costs, of less than $F_0 - F_1$ (Proposition 1 gives the corresponding bounds on the value of the entity). The actor's own loss when the higher fee deters such an entity is therefore smaller than the difference between the fees. The bound concerns the value that the actor expects on the day of filing, and it says nothing about value to third parties. The second property is dilution. When an owner divides the same activity among more companies, the acts that follow from the activity (a payroll, a loan) do not increase, and their rate per registration falls with no change in any business (Proposition 2). A lower rate of an outcome among the companies formed at a low fee therefore does not show that the founders who entered were of lower quality, and we report the *number* of companies with each record alongside every rate.

The relative rate. Consider the companies formed at the lower of two fees. A share s of them would not have been formed at the higher fee, and these have a given record ρ times as often as the others. Let r_L and r_H denote the shares of companies with the record among those formed at the lower and at the higher fee. If the two cohorts do not differ for other reasons, then

$$\frac{r_L}{r_H} = 1 - s(1 - \rho). \tag{2}$$

If ρ is one, the share with the record is the same at both fees and the number of companies with the record changes in proportion to registrations. If ρ is zero, the number of companies with the record is the same at

both fees, and the share is lower at the lower fee by the proportion s, whatever the record and however common it is. The relation holds in either direction, so that a fee increase that removes a share s of registrations raises the share with a record by the factor $1/(1-s)$ when ρ is zero. In general, the number of companies with the record is higher at the lower fee by the proportion $\rho s/(1-s)$ and the number of registrations by the proportion $s/(1-s)$. The relative rate is therefore also the proportional response of the number of companies with the record relative to that of registrations.

Records and businesses. The relative rate concerns a record, and the question of interest concerns businesses. Write M for the registrations that respond to the fee and I for the others. Let b_g be the probability that a registration of group g is the entity of an operating business, and κ_g the probability that such an entity leaves the record. If the record never arises without an operating business, the rate of the record in group g is $b_g\kappa_g$, and

$$\rho = \frac{b_M}{b_I} \times \frac{\kappa_M}{\kappa_I}. \tag{3}$$

The tests estimate the product. They identify the first factor, the relative probability of being an operating business, only if the second factor, the relative capture of businesses by the record, is known. We accordingly distinguish three statements. That the registrations that respond to the fee are less likely than others to leave a given record is what an estimate of ρ below one shows. That they are less likely to be operating businesses follows if capture is comparable, that is, if κ_M/κ_I is close to one. That they have little value, or that deterring them entails little loss, does not follow from either statement, because an entity can have value to its owner or to others without operating a business that borrows or files accounts.

Equation (3) also gives the capture ratio at which the evidence would be consistent with equal business rates. If the upper end of a confidence set for ρ is $\bar{\rho}$, the hypothesis $b_M = b_I$ is consistent with the data only if $\kappa_M/\kappa_I \leq \bar{\rho}$: the businesses among the registrations that respond to the fee would have to leave the record at no more than $\bar{\rho}$ times the rate of other businesses. We report this value for each record, and Sections 6.3 and 7.4 examine what can be learned about capture from loans made in a person's own name, from records dated near formation, and from accounts filed by companies that later left the register. Capture may be lower among the businesses that respond to a fee if such businesses are smaller, shorter-lived, or less likely to borrow in the company's name. A record can also arise without an operating business, as when a loan is obtained in the name of a dormant company or a company with minimal activity declares its accounts non-dormant. Such records raise the rate among registrations that are not businesses and move ρ toward one.

Timing. Filings can be retimed around a dated fee change. Registrations that would be filed at either fee may be delayed until a reduction begins, or advanced before a reduction ends or before an announced increase takes effect. Counted among the registrations that respond to the fee, they move the estimate of ρ toward one if they have the record about as often as other registrations filed at either fee. Registrations that the higher fee would deter may also be advanced ahead of an increase or into a reduction; these enlarge both the response of registrations and the difference in the share with a record, and they leave the estimate unbiased if they resemble the other registrations of that kind. Retiming therefore does not by itself lower the estimate of ρ, but it changes the composition of the months adjacent to a fee change, which matters for the choice of reference period (Section 6.2).

4. Setting, Data, and Methods

4.1. The Fee Changes

In Colorado, an LLC is formed by filing articles of organization online with the Secretary of State. The filing takes a few minutes, is recorded as submitted, and carries a regular fee of $50. LLCs accounted for 80% of new entities in 2021–22; the remainder are corporations, also formed for $50, and entities organized elsewhere that register to do business in Colorado (*foreign entities*), at a fee of $100. Each entity must file a periodic report every year, for a fee that was $10 until July 2024, and an entity that does not file becomes *delinquent*. Anyone may file on behalf of a company, and the registry identifies the party responsible for a filing only through the principal address and the name and address of the registered agent.

Colorado has reduced the fee to form an LLC to $1 three times (Table 1, Panel A). In the two *fee holidays*, from December 1, 2012 to February 28, 2013 and from July 1 to October 31, 2014, the Secretary of State's office reduced fees under its own authority and at the expense of its own cash fund (Colorado Secretary of State, 2012a, 2014c). The first holiday reduced every online filing fee, including the fees for a periodic report and for a UCC financing statement, whereas the second reduced the fee to form a domestic entity and no other fee. The third reduction was the 2022–23 fee-reduction *program*, for which the legislature transferred $8.435 million so that filers paid $1 to form an LLC from July 1, 2022 until the funds were exhausted on May 21, 2023 (Colorado General Assembly, 2022; Colorado Secretary of State, 2022b, 2023). The tests of this paper rest on the two holidays, because the cohorts of the program were formed after the PPP loans had been made; Sections 5 and 6.5 report what the program shows.

A U.K. company is incorporated by an application to Companies House, submitted online or through the software used by formation services. From June 2016 the fee was £12 online and £10 through software. Both fees increased to £50 on May 1, 2024 and to £100 on February 1, 2026 (Table 1, Panel B). The first increase followed the Economic Crime and Corporate Transparency Act 2023, which expanded the registrar's power to set fees so that the cost of policing the register could be recovered from them (United Kingdom, 2023), and it was announced on February 19, 2024. To maintain its registration, a company must file a confirmation statement every year and annual accounts, the first of which are due 21 months after incorporation. A company that has not traded may file dormant accounts, and a company that files nothing is eventually struck off the register. The incorporation fee was not the only change in the spring of 2024. The fee for the confirmation statement rose on the same day; the first measures of the 2023 Act took effect on March 4 (Companies House, 2024a); national insurance rates and the dividend allowance changed on April 6; and a general election was called on May 22. The base period of the U.K. test begins on March 4, and Section 7.3 examines the other events.

4.2. Colorado: Registry and Lending Records

The Secretary of State publishes a register of entities, with 3.1 million records, and the history of every document filed for those entities since January 1, 2010, with 16.0 million filings (Colorado Secretary of State, 2026a). We downloaded both files on October 4, 2026. For each entity, we identify the filing that created it, the date on which that filing was received, and every subsequent periodic report, dissolution, and reinstatement. A few filers form companies in bulk. A filing belongs to a *bulk group* if its principal address,

Table 1. Five Changes in the Fee to Form a Company

A. Colorado: reductions of the fee to form an LLC from $50 to $1

	First fee holiday	Second fee holiday	Program
Period at $1	Dec. 1, 2012 to Feb. 28, 2013	July 1 to Oct. 31, 2014	July 1, 2022 to May 21, 2023
Fees reduced to $1	Every online business filing: all formations ($50; nonprofit $20; foreign $100), periodic report ($10), trade name ($20), reinstatement ($100), dissolution ($25). UCC financing statements and amendments were reduced from $8 to $4 ($1 and a $3 surcharge)	Formation of any domestic entity ($50): LLC, profit or nonprofit corporation, limited partnership	Formation of an LLC ($50) and trade name of a business entity ($20)
Fees not reduced	Late fee; cure of delinquency	Foreign entities, trade names, periodic report, UCC filings	Corporations, foreign entities, periodic report
Start date made public	Nov. 19, 2012 (12 days in advance)	June 9, 2014 (22 days in advance)	Proposed Nov. 1, 2021; act signed May 16, 2022; reduced fees announced July 1, 2022
End date made public	Feb. 20, 2013 (8 days in advance)	Sept. 30, 2014 (31 days in advance)	Friday, May 19, 2023 (fee reinstated on Monday)
Source of funding	Department of State Cash Fund	Department of State Cash Fund	$8.435 million from the state's general fund

B. United Kingdom: increases in the fee to incorporate a company

	First increase	Second increase
Date	May 1, 2024	February 1, 2026
Incorporation fee	Online £12 to £50; through formation services' software £10 to £50	£50 to £100
Other fees changed on the same day	Annual confirmation statement £13 to £34; voluntary striking-off £8 to £33	Confirmation statement £34 to £50; striking-off £33 to £13
Date announced	Feb. 19, 2024 (72 days in advance); ministers had referred in 2023 to fees of "no more than around £50"	Oct. 30, 2025 (94 days in advance)
Other changes around the same time	March 4: new rules on registered offices and new powers for the registrar. April 6: national insurance rates reduced; dividend allowance halved. May 22: general election called	Nov. 18, 2025: identity verification compulsory for directors of new companies

Notes. Sources for Panel A: Colorado Secretary of State (2012a, 2013a, 2014b,c, 2021b, 2022b, 2023); Colorado Department of State (2013); Colorado General Assembly (2022). Sources for Panel B: Companies House (2024a,b, 2025a,b); United Kingdom (2016, 2024, 2025). The U.K. fee is the fee in force on the day the application is delivered. Appendix C reports Colorado's fees since 2008 and other dates relevant to the outcomes, with the source of each.

its agent's address, or its agent's name appeared on at least 50 LLC filings in one week and on at least ten times its own usual weekly number. Every other filing is an *ordinary filing*, and all counts refer to ordinary filings unless otherwise stated. We also classify ordinary filings by type of filer: *formation-service filings*, whose registered agent is an organization; *new filers*, persons with no earlier ordinary LLC filing under the same agent name or principal address in the preceding 365 days; and *repeat filers*, persons with such a filing (Appendix C.5).

A registry records the filings by which a registration is maintained, and not whether the company operates a business. We therefore use three *lending records*, created by lenders and federal agencies (U.S. Small

Business Administration, 2024, 2026); the appendix tables refer to them as external records. The first is a *PPP loan*, from the loan-level file of the Paycheck Protection Program, which lists every loan approved from April 2020 to May 2021. A borrower had to be in operation on February 15, 2020 and borrowed against its payroll or, without employees, against the owner's income from the business; 192,267 loans name a Colorado borrower. The second is an *SBA loan*, guaranteed under the 7(a) or 504 program since October 2009 (26,927 loans in Colorado). The third is a *UCC financing statement*, by which a lender records a security interest in a company's assets, filed with the Secretary of State's office at any time up to October 2026. We link loans to entities by name, using a rule that relies on neither the status nor any outcome of the entity and that was specified and run before the outcomes were tabulated. The rule links 68% of PPP loans, 88% of those whose borrower is described as an LLC, and 88% of SBA loans. Section 6.4 reports what is known about the accuracy of the links.

The primary outcome is an indicator that the company has a PPP loan, which shows that it still had a payroll or an owner's income in 2020 or 2021, six to eight years after the holidays; 5.3% of LLCs formed from 2010 to 2018 have a PPP loan, and 10.9% have any of the three lending records. Take-up among employers was widespread: according to the Census Bureau's linked data, which the Bureau describes as a lower bound, 66% of firms with fewer than 500 workers received a loan (Beem and Stinson, 2025), and an earlier estimate, weighted by employment, was 94% (Autor et al., 2022). The PPP loan does not capture a business that had closed by 2020 or an owner who borrowed in the owner's own name, and a loan obtained fraudulently in the name of a dormant company is counted as a record (on the extent of such loans, see Griffin et al., 2023). The financing statement can be filed at any age of the company, and the file of financing statements retains the statements that have lapsed since May 2010, which include every lapsed statement that could name a company of the sample (Appendix C.7) [E]. Section 6.3 examines what these records capture.

4.3. United Kingdom: Register Snapshots

Companies House publishes each month a file of every company on the register at that date (Companies House, 2026). We assembled the 45 files dated from October 2019 to October 2026, which are monthly from October 2023. Because a file lists only live companies, we count registrations from company numbers, which are issued sequentially, so that a company dissolved before it appeared in any file is still counted. Outcomes are measured in the October file of a later year. The primary outcome is an indicator that the company has accounts on file other than dormant accounts (*non-dormant accounts*) in the October that falls 27 to 31 months after incorporation, by which time first accounts are overdue. The company itself declares this category, and a company with minimal activity may declare it. The other outcomes are a charge registered by a lender, which is the only U.K. record that a third party creates, a first confirmation statement on file after 15 to 19 months, and continued presence on the register.

4.4. Estimation

A *log point* is one-hundredth of a unit of the natural logarithm, and confidence intervals and sets are at the 95% level.

Colorado. For a window W of consecutive calendar months, let $W - 12$ and $W + 12$ denote the same months one year earlier and one year later. For the number of filings N and for the share r of a window's

companies with a given outcome, we define

$$T_N(W) = \log N(W) - \tfrac{1}{2}\big[\log N(W-12) + \log N(W+12)\big], \qquad T_r(W) = r(W) - \tfrac{1}{2}\big[r(W-12) + r(W+12)\big]. \quad (4)$$

We compute the same statistic for every run of the same number of months such that neither the run nor the same months one year earlier and one year later contain a month affected by a holiday. We call these the *reference runs*, and we use the standard deviation of the statistic over them as the standard error. The sample period is January 2010 to December 2019 for filing counts and January 2010 to December 2018 for the loan outcomes. Confidence intervals use Student's t distribution with degrees of freedom equal to the number of non-overlapping runs minus one (10 and 7 for the loan outcomes in the two holidays), and pooled estimates weight each holiday by its number of months and use the smaller number of degrees of freedom. A *marginal registration* is a registration that responds to the fee (group M of Section 3), one that would not have been filed at the regular fee, and an *inframarginal registration* (group I) is one that would have been filed at either fee. If a share s of a window's filings is marginal and the window's rate differs from that of its reference period by d, the rate among marginal registrations relative to that among inframarginal registrations is

$$\rho = 1 + \frac{d}{s\,\mu_0}, \quad (5)$$

where $\mu_0 = r(W) - d$ is the *inframarginal rate*, the rate that the window would have had in the absence of marginal registrations. This is equation (2) solved for ρ, and it is the argument that Gruber et al. (1999) used to describe the "marginal child." It assumes that the reference period gives the rate that the same months would otherwise have had and that the fee reduction changed the cohort only by adding filings. With s of 14.7% and μ_0 of 5.2%, as in the pre-specified test, an error of one-tenth of a percentage point in μ_0 changes ρ by 0.11; the estimate is less sensitive for a more common outcome (0.05 for any of the three records). For the two holidays together, ρ is computed from the pooled values of d, s, and μ_0.

United Kingdom. For the fee increase of May 1, 2024, the *before* period, which is the base period of the comparison, runs from March 4 to April 14 and the *after* period from May 20 to June 30. The five intervening weeks, which contain the bunching of registrations before the increase and the temporary decline after it, are excluded and reported separately. Let N denote the after-minus-before difference in the log of registrations per working day, B the same difference for the number of companies with an outcome, and $R = B - N$ the log change in the rate of the outcome; here N and B denote these differences and not the counts of equation (1). The estimate of each quantity (E_N, E_B, E_R) is its value in 2024 minus its mean over the reference years at the same dates: nine years for registrations and, for the primary outcome, the four years for which the later files exist. The standard error is the standard deviation over the reference years multiplied by $\sqrt{1 + 1/n}$, where n is the number of reference years. The rate of the outcome among *deterred registrations*, those that the fee increase removed, relative to the rate among the remaining registrations, is

$$\rho = \frac{e^{-E_B} - 1}{e^{-E_N} - 1}, \quad (6)$$

which equals zero if the number of companies with the outcome did not change and one if that number fell in

proportion to registrations.

4.5. Confidence Sets for the Relative Rate

The relative rate is a ratio whose denominator, the share of registrations that respond to the fee, is estimated. The analysis plans specified that its confidence interval come from joint draws of the errors of the numerator and the denominator, and the scripts locked with them take percentiles of the ratio over the draws. A ratio is undefined at a denominator of zero and changes sign with the denominator, and the scripts set aside the draws in which the denominator is near zero or has the opposite sign before taking percentiles: draws with a marginal share of 0.01 or less in Colorado, and draws in which registrations decline by less than half a log point in the United Kingdom. Addendum 3 states this step for the Colorado test. Addendum 4 provides that an interval for the U.K. ratio is reported only if at most 2.5% of the draws are of this kind, and does not state that they are set aside. An interval that conditions on a subset of the draws need not cover at its nominal rate. For each ratio we therefore also form a confidence set that uses every draw, by the approach of Fieller (1954): a value ρ_0 belongs to the set unless a two-sided test at the 5% level rejects the hypothesis that the numerator equals ρ_0 times the denominator, a hypothesis that involves no division. For the Colorado lending records, the test statistic has Student's t distribution under the error model of the plans and the set has a closed form. For the other ratios, among them every U.K. ratio, a value is retained if the share of the joint draws in which the numerator exceeds ρ_0 times the denominator lies between 2.5% and 97.5%; Section 7.2 and Appendix D.1 examine the coverage of this rule. For each of the 22 Colorado estimates in Appendix Table A9, at most 0.13% of the draws were set aside, and the set agrees with the percentile interval to within 0.03 at each end. We therefore report the Colorado intervals as pre-specified. Where the marginal share is less precisely estimated, as for the first holiday alone under some reference periods and for filings through formation services, the two constructions differ, mainly at the lower end, and Appendix D.1 reports those cases. In the U.K. test, 1.15% of the draws were set aside, and 87% of those imply a ratio above one-half; the two constructions nevertheless have the same upper end and differ at the lower end. We report sets by test inversion for every U.K. ratio that the text cites, other than the 315 specifications of Section 7.3 and the comparison without reference years of Section 7.5, which has no standard error, and Section 7.2 compares the constructions [E].

4.6. Pre-specified and Exploratory Analyses

The analyses were specified in five documents, each committed to the project's version history before the new tabulations that it governs were run. The plan [P] concerned the 2022–23 program; Addendum 1 [A1] added analyses of the program; Addendum 2 [A2] specified the registry-based outcomes for the two holidays; Addendum 3 [A3] specified the test on the lending records, with the PPP loan as the primary outcome; and Addendum 4 [A4] specified the U.K. test. Appendix Table A3 lists when each document was recorded and what was known when it was written, Appendix Table A5 lists the pre-specified interpretations with their outcomes, and Appendix B reports every provision with its result and every consequential departure. We wrote all five on October 4 and 5, 2026, with the data in hand, each at most a few hours before the tabulations that it governs. Nothing was deposited outside the project, and the account of what was known at each point relies on our own records. With data that already exist there is, as Olken (2015) writes, "no obvious before-and-after date" (see also Burlig, 2018), and we treat the documents as a dated record of intentions and

not as preregistration.

The two tests differed in what was known when they were specified. When Addendum 3 was written, we knew that companies formed in the holidays were less likely to file periodic reports and less likely to have a UCC financing statement within two years, so that the direction of the loan result could be anticipated. When Addendum 4 was written, we knew every Colorado result and the monthly totals of U.K. registrations, which had declined; we knew nothing about the outcomes of companies incorporated from 2024. In the U.K. test, the prediction could therefore have failed, and by the addendum's own calculation the test would have been inconclusive if the deterred registrations had filed accounts half as often as the remaining registrations. Analyses that no document specified are marked [E]. They include the framework, the counts of Colorado companies with each lending record, every estimate of ρ under an alternative reference period or base period, the analyses of what the records capture, the confidence sets by test inversion, and the direct test of the U.K. ratio. Where the interpretation that a plan specified says more than the evidence supports, we report the narrower statement (Sections 6.1 and 7.1).

5. The Response of Registrations to Filing Fees

The two fee holidays. In the three months of the first holiday, the Secretary of State's office received 13% more ordinary filings to form an LLC than in the same months of the adjacent years, and in the four months of the second holiday, 20% more (Table 2, Figure 1) [A2]. Each estimate exceeds the statistic in every one of the approximately forty reference runs. Pooled over the two holidays, the increase is 17% (95% confidence interval 13 to 22; Appendix Table A14) [A2]. Filings to form profit corporations, whose fee was reduced by the same $49, increased by 24.6 log points (s.e. 3.7) [A2], or by 16.2 (s.e. 4.8) when two episodes of bulk filing are excluded [E]. Filings by foreign entities, whose fee was not reduced in the second holiday, showed no detectable change in that holiday (+0.8 log points, s.e. 4.8).

Because the dates of each holiday were announced, some filings were retimed. Filings bunched at the end of each holiday, and in the month after each holiday they were 6 and 7 log points below their reference level. Net of the month before and the month after, additional filings were 9% and 16% of the filings in the holiday months, compared with 12% and 17% when only those months are counted, and 11% and 17% when every window is measured per weekday [E]. Most of the response therefore consists of additional registrations and not of retimed ones. In the months that followed the month after each holiday, which we had examined before Addendum 2 was written, filings were at their reference level (−2.5 and +0.1 log points, with standard errors of 2.9 and 2.5) [A2].

The 2022–23 program. Between July 1, 2022 and May 21, 2023, Colorado received 137,008 ordinary filings, compared with 109,705 on the same dates one year earlier, and a further 19,789 filings in bulk groups. Growth over a single year is a weak comparison, and the end of the program provides a sharper one. Relative to the same days of May in thirteen other years, ordinary filings per business day declined by 18 log points (s.e. 9) when the $50 fee was reinstated, or by 16 (s.e. 5) if May 2020 is excluded from the reference years [A1]. Addendum 1 specified this measure after the decline had been observed under several windows.

The U.K. fee increase. In 2024, registrations per working day were 33 log points lower in the six weeks from May 20 than in the six weeks from March 4. Between the same weeks of nine other years they declined by 13 log points on average, because the U.K. tax year ends in early April. The difference of 19.2 log points

Table 2. The Response of Registrations to the Fee

	Fee	Change in registrations		
		log points	percent	95% CI, percent
Colorado, first holiday, Dec. 2012 to Feb. 2013 [A2]	\$50 to \$1	+12.4 (2.6)	+13	+7 to +20
Colorado, second holiday, July to Oct. 2014 [A2]	\$50 to \$1	+18.6 (2.5)	+20	+14 to +27
Colorado, program relative to the preceding year [E]	\$50 to \$1	+14.5 [8.6]	+16	
Colorado, end of the program, May 2023 [A1]	\$1 to \$50	−18.1 (8.9)	−17	−31 to +1
United Kingdom, May 2024 [A4]	£12 to £50	−19.2 (6.7)	−17	−29 to −4

Notes. Ordinary filings to form an LLC in Colorado; registrations of companies per working day in the United Kingdom. Standard errors are in parentheses; confidence intervals use the t distribution with the degrees of freedom of each reference set (Section 4.4). The holidays: equation (4). The program: the change from the same dates one year earlier less the mean change over the preceding ten years; the number in square brackets is the standard deviation of that change over the ten years and not a standard error, and we report no confidence interval. The end of the program: filings per business day from May 22 to 31, 2023 relative to May 1 to 21, compared with the same ratio in the thirteen years from 2010 to 2022; excluding 2020 the estimate is −16.1 (5.5), or −15% (−25 to −4). The United Kingdom: Section 4.4; the fee increase was £38 for an application made online and £40 through formation services' software, or \$47 and \$50 at the exchange rate of May 1, 2024 (\$1.2488 per pound; Federal Reserve Board, statistical release H.10). The bracketed letters identify the document under which each estimate was specified (Section 4.6).

(s.e. 6.7, $p = 0.021$) is the estimated effect of the fee increase, a reduction of 17% in registrations [A4]. Registrations in the last sixteen days at £12 were 19 log points above their level in the before period.

Magnitude. In each of the five estimates of Table 2, a fee change of about \$50 changed the number of registrations by between one-eighth and one-fifth. The estimates cover two countries, fee reductions and fee increases, notice periods from one business day to ten weeks, and changes made by a state office and by a national government. The response is large relative to the fee: a Colorado company that files its periodic report each year pays the state as much as the formation fee within five years.

Composition by filer type. The registry's fields give a first indication of who filed the additional registrations (Appendix Table A14). Pooled over the two holidays, filings by repeat filers increased by 26%, filings by new filers by 13%, and filings through formation services by 16% [A2]. Because new filers are the larger group, they contribute about as much to the additional filings as repeat filers do: 41% of the additional filings came from repeat filers, 40% from new filers, 14% through formation services, and 5% from addresses with twenty or more filings in the preceding year, a fourth type that Appendix C.5 defines [E]. These shares depend on how addresses are matched (Appendix D.5) [E].

Filers in the program. The program shows who files when a fee of \$1 is maintained for almost eleven months [E]. Bulk filers are concentrated in it: the rule of Section 4.2 assigns 57 filings to bulk groups in the first holiday, 418 in the second, and 19,789 in the program. The program year had 47 thousand more registrations than the preceding year. Bulk filings account for 42% of the increase, eight formation services whose combined volume more than doubled for 15%, and repeat filers and new filers for 19% and 17% (Appendix Table A42). One filer formed 15,527 companies on 95 days, which is one-tenth of the LLCs registered in Colorado during the program and accounts for 9% of the appropriation. The dates do not establish that the fee caused this wave: nearly all of its filings had been made by the sixth day before the reinstatement of the regular fee was announced, and 1,625 companies had been formed from the same address in 2020, at \$50 each. Bulk filing has since resumed at the regular fee (Appendix D.7).

Business applications and characteristics at founding. Applications for Employer Identification Numbers,

which the Census Bureau publishes as business applications (U.S. Census Bureau, 2026a), increased by less than registrations. In the second holiday they rose by 9.8 log points (s.e. 3.3) relative to a weighted combination of other states (Arkhangelsky et al., 2021), which corresponds to 0.35 applications per additional registration [A2]. Applications with planned wages show no increase that is statistically distinguishable from zero in any episode, although none of these estimates excludes an increase of several percent. An index of founding characteristics of the type used to adjust registration counts for quality (Guzman and Stern, 2020), fitted to what the registry records at founding, increased by as much as the unadjusted number of registrations when summed over the filings of the holiday months (Appendix Table A23) [A3].

Fee relief. The two holidays returned $2.2 million to those who formed ordinary LLCs, 87% of it on filings that would have been made at $50 in the same or the adjacent months, and the relief amounts to $365 for each additional registration. Fee relief on the 156,797 LLCs formed at $1 in the program totals $7.7 million, of which 70% went to a number of filings equal to the number made at $50 in the preceding year and 13% to bulk groups [E].

The registry does not identify which filers have businesses, and the next two sections examine records of business activity.

6. Lending Records of Marginal Registrations: Colorado

6.1. The Pre-specified Test

Of the LLCs formed during the first fee holiday, 4.62% later had a PPP loan, compared with 5.08% and 5.63% of those formed in the same months a year earlier and a year later. For the second holiday the corresponding shares are 4.38%, compared with 5.03% and 5.11%. The statistic of equation (4) is −0.74 percentage points (s.e. 0.26) for the first holiday and −0.68 (0.22) for the second, and the pooled estimate is −0.71 (0.17), with 7 degrees of freedom (Table 3, Panel A) [A3]. The marginal share is 14.7%, the inframarginal rate is 5.2%, and equation (5) gives $\rho = 0.07$, with a 95% confidence interval from −0.57 to 0.60. For the second holiday alone, which reduced no fee other than the formation fee, ρ is 0.21 (−0.55 to 0.81), and for the first holiday alone it is −0.18 (−1.61 to 0.75).

Addendum 3 specified that an interval below one would be read as showing that "the added companies were less often businesses with a payroll or an owner's income in 2020-21 than other companies formed in the same months." That sentence treats a company without a linked loan as a company without such a business. The test establishes the narrower statement that marginal registrations less often had a PPP loan in the company's name, and Section 6.3 examines the step from the record to the business.

Other lending records. The share is also lower among the holiday cohorts for an SBA loan and for a UCC financing statement filed at any time up to October 2026 (Panel A). As a proportion of its inframarginal rate, the decline is 14% for the PPP loan, 15% for the SBA loan, 15% for the financing statement, and 15% for any of the three records, and the marginal share is 14.7% [E]. Equation (2) implies this pattern if marginal registrations rarely have a record of any type, because each share then declines by the marginal share whatever its level. For any of the three records (the broader lending record), the share declines by 1.72 percentage points (s.e. 0.26) from an inframarginal rate of 11.5%, and ρ is −0.02, with a confidence interval from −0.65 to 0.41; in the second holiday alone it is 0.20 (−0.53 to 0.67). This record was a secondary outcome, for

Table 3. Colorado, Pre-specified Test: Lending Records among Companies Formed in the Two Fee Holidays

A. Share of companies with each record [A3]

	Inframarginal, per 100	Change in the share, percentage points			Marginal relative to inframarginal, ρ	Marginal, per 100
		First holiday	Second holiday	Pooled		
PPP loan, 2020–21 (primary)	5.2	−0.74 (0.26)	−0.68 (0.22)	−0.71 (0.17)	0.07 [−0.57, 0.60]	0.4 [0.0, 3.1]
reporting two or more jobs	3.3	−0.63 (0.18)	−0.43 (0.15)	−0.52 (0.11)	−0.08 [−0.89, 0.52]	0.0 [0.0, 1.7]
SBA loan, to June 2026	0.9	−0.26 (0.08)	−0.04 (0.07)	−0.13 (0.06)	0.00 [−1.28, 1.00]	0.0 [0.0, 0.9]
Either federal loan	5.7	−0.95 (0.23)	−0.72 (0.19)	−0.82 (0.15)	0.02 [−0.56, 0.47]	0.1 [0.0, 2.7]
UCC financing statement, to Oct. 2026	8.3	−1.31 (0.42)	−1.20 (0.39)	−1.25 (0.29)	−0.02 [−0.83, 0.58]	0.0 [0.0, 4.8]
Any of the three records	11.5	−1.95 (0.39)	−1.55 (0.36)	−1.72 (0.26)	−0.02 [−0.65, 0.41]	0.0 [0.0, 4.7]
UCC financing statement within 24 months [A2]	4.1	−0.70 (0.24)	−0.70 (0.20)	−0.70 (0.15)		

B. By type of filer, and corporations [A3]

	Marginal share	Inframarginal, per 100	PPP loan		Any of the three records: change, pct. points
			change, pct. points	marginal, per 100	
All ordinary LLCs	0.15	5.2	−0.71 (0.17)	0.4 [0.0, 3.1]	−1.72 (0.26)
Formation service	0.14	5.4	−0.42 (0.29)	2.3 [0.0, 9.2]	−2.13 (0.64)
Repeat filer	0.21	5.1	−0.79 (0.28)	1.3 [0.0, 4.7]	−1.97 (0.34)
New filer	0.11	5.2	−0.72 (0.17)	0.0 [0.0, 3.2]	−1.45 (0.31)
Profit corporations	0.22	11.9	−1.90 (0.39)	3.2 [0.0, 7.4]	−3.15 (0.50)
outside bulk groups [E]	0.15	11.9	−1.01 (0.26)	5.1 [0.0, 9.6]	−1.75 (0.57)

Notes. Ordinary LLCs formed from 2010 to 2018, by month of formation; the holiday months are December 2012 to February 2013 and July to October 2014. Panel A: T of equation (4) in percentage points, with the standard deviation of the same statistic over unaffected reference runs in parentheses; *Pooled* weights the holidays by their months. *Inframarginal, per 100*: the rate that the holiday cohorts would have had in the absence of marginal registrations. *Marginal relative to inframarginal,* ρ and *Marginal, per 100*: equation (5) with the marginal share counted in the holiday months, and 95% confidence intervals from joint draws (Section 4.5); a negative estimate of ρ is shown as zero per hundred. The last row is the early record that Addendum 2 had specified, with that addendum's reference runs, which begin in June 2011; no ρ was specified for it. Panel B: each type of filer with its own marginal share; types are defined in Section 4.2. The last row excludes the corporations assigned to bulk groups when the rule that identifies such groups is applied to corporations as it was to LLCs [E]. Appendix Tables A19 to A24 report every statistic that Addendum 3 specified.

which Addendum 3 specified no interpretation. The first holiday also reduced the fee for a financing statement from $8 to $4. When only the statements filed in 2020 or later are counted, which the lenders of every cohort filed at the same fee, ρ for any of the three records is 0.08 (−0.47 to 0.47) (Appendix Table A37) [E].

Numbers of companies. The number of companies formed in the holiday months that have a PPP loan differed from its reference level by +1% (confidence interval −7 to +11), and the number with any of the three records by 0% (−7 to +7), while registrations increased by 17% [E]. The intervals exclude an increase in proportion to registrations. They do not exclude increases of 11% and 7%, which are 61% and 40% of the increase in registrations, so that the estimates do not show the effect on the number of such companies to be negligible.

Filers and corporations. Among new filers, the change in the loan share is as large as among repeat filers, at −0.72 percentage points (0.17) compared with −0.79 (0.28); for new filers ρ is −0.22 (−1.64 to 0.60) for the PPP loan and −0.25 (−1.23 to 0.45) for any of the three records. Filings through formation services are the least conclusive case (Panel B) [A3]. Profit corporations, whose fee was reduced by the same $49, are twice as likely as LLCs to have a PPP loan. When two episodes of bulk filing are excluded, their loan share declined by 1.01 percentage points (0.26), and ρ is 0.43 (−0.64 to 0.80); with the threshold of the rule that identifies bulk filings halved, it is 0.44 (−0.92 to 1.02), an interval that reaches one [E].

6.2. The Reference Period

Equation (5) recovers the rate among marginal registrations only if the reference period provides the rate that the holiday months would otherwise have had. The pre-specified reference period is the same months of the two adjacent years. That it was specified first gives it no claim to be the correct counterfactual, and one feature of the data bears on the choice. Companies formed in the *flanking months*, defined as the month in which each holiday was announced and the month after it ended, when the fee was $50, were also less likely than their reference cohorts to have a PPP loan. The deficit is 0.56 percentage points (s.e. 0.17), four-fifths of the deficit in the holiday months (Figure 2; Addendum 3 had specified the statistic for these months [A3]). For any of the three records, the deficit in the flanking months is 0.31 percentage points (0.25), less than one-fifth of the deficit of 1.72 in the holiday months. The analyses in the remainder of this section were chosen after the results of the pre-specified test were known [E].

The appropriate comparison depends on the source of the deficit in the flanking months. If it arose by chance, the pre-specified comparison is appropriate. If it reflects retiming, in which owners of operating businesses delayed a filing until a holiday began or advanced one before it ended, the holiday cohorts include businesses that the fee did not create, the pre-specified estimate is too high, and the appropriate comparison is over the whole window of the holiday and flanking months. If it reflects a disturbance unrelated to the fee that lowered the loan rate of the companies formed in the flanking and the holiday months alike, the pre-specified estimate is too low, and the holiday months should be compared with the flanking months. For the PPP loan, ρ is −0.41 (−1.22 to 0.32) over the whole window, 0.07 as pre-specified, and 0.81 (−0.19 to 1.78) or 0.67 (0.24 to 1.17) when the holiday months are compared with the flanking months, relative to the adjacent years or to every other unaffected year. With all unaffected months of 2010 to 2018 as the reference period and a linear or a quadratic trend, it is 0.19 or 0.07, with intervals that exclude one. When the statistic for the flanking months is subtracted at the weight that blocks of unaffected months imply, it is 0.13, with an interval from −0.83 to 1.15 when the sampling error of the weight is included (Table 5, Panel A; Appendix Table A11).

The evidence does not identify the source. Chance is unlikely, because in none of the 239 pairs of unaffected blocks with the structure of the two holidays is the statistic for the outer months as far from zero. Retiming occurred around the first holiday, but if retimed filings accounted for the whole deficit, they would need to have had PPP loans at a rate of 24 per hundred, compared with 5.4 for all ordinary LLCs formed from 2011 to 2018. Several findings weigh against a common disturbance. The deficit in the flanking months is confined to the PPP loans of LLCs. Among the companies formed in those months, the share with a UCC financing statement differs from the reference level by −0.09 percentage points (0.29); the share with a financing statement filed in 2020 or later, a lending record of the same years as the loan program, by +0.12 (0.14); and the share that had become delinquent before the loans began by +0.2 (1.0). Profit corporations formed in those months, outside bulk groups, show no deficit in PPP loans (+0.46 percentage points, s.e. 0.59). The deficit is absent from all but one of the eight months formed by the two months before and the two months after each whole window. In blocks of unaffected months, the slope of the statistic for the inner months on the statistic for the two outer months is 0.06 (s.e. 0.40), whereas the comparison with the flanking months presumes a slope of one; a slope of one is rejected at the 10% level and not at the 5% level ($p = 0.058$). Other findings are consistent with a common disturbance. Loan rates were below the reference level in the month

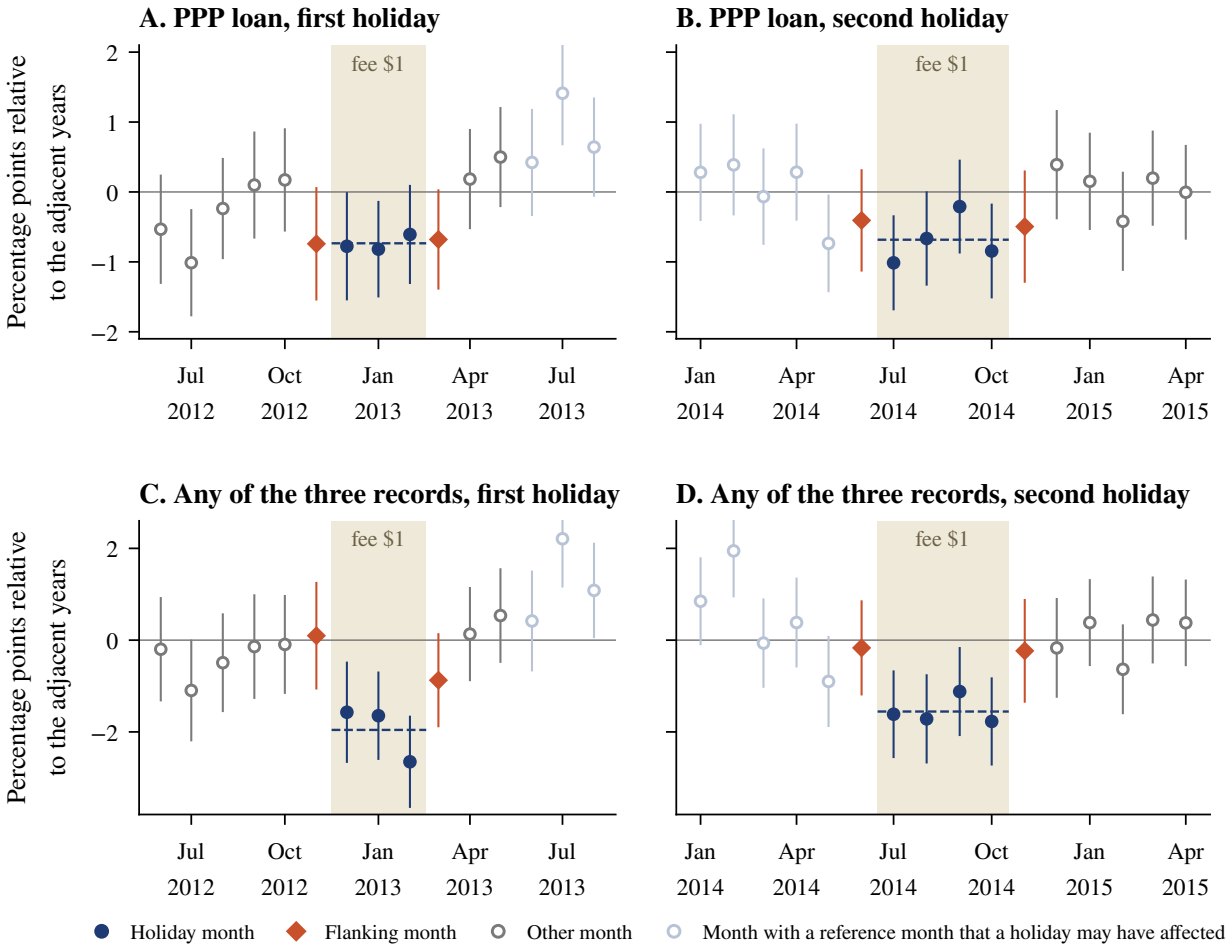


Figure 2. Colorado: Two Lending Records by Month of Formation around the Fee Holidays

Notes. The statistic of equation (4) for single months: the share of the ordinary LLCs formed in a month that have the record, less the mean share among those formed in the same month one year earlier and one year later, in percentage points. Panels A and B: a PPP loan. Panels C and D: any of the three lending records. Bars extend two standard deviations of binomial sampling error above and below each estimate. Shading marks the months in which the fee was $1, and the dashed line is the mean over those months. The flanking months are the month in which each holiday was announced and the month after it ended. Light markers are months with a reference month in a period that a holiday may have affected. The tabulation by single months is not pre-specified [E]; Addendum 3 specified the statistic for the four flanking months and for the first month of each holiday (Appendix Tables A20 and A21) [A3].

before the second announcement (−0.74 percentage points, s.e. 0.33), less clearly before the first (−0.69, s.e. 0.49), although not in the last eight days before the second announcement (+0.46, s.e. 0.73). Over the whole first window, fewer companies had a PPP loan than in the reference period (−8.3 log points, s.e. 3.8), which no account of a fee reduction predicts. Among registrations that had not become delinquent, the deficit is as large in the flanking months as in the holiday months. None of the findings excludes a disturbance specific to participation in the loan program by the LLCs formed in those months, because such a disturbance need not appear in secured borrowing, in SBA lending, or among corporations. Appendix D.3 reports this evidence in detail.

We therefore state the Colorado evidence separately for the two outcomes. For the PPP loan, the primary outcome, the conclusion depends on the reference period. The estimate ranges from −0.41 to 0.81 for the two

holidays together. The interval excludes one under the pre-specified comparison, over the whole window, and with all unaffected months as the reference period, and it includes one under both comparisons with the flanking months and at the estimated weight. For the second holiday alone, the estimate ranges from −0.05 to 0.73 under the six reference periods, and the interval includes one under one of them. Under the comparison with the flanking months relative to the adjacent years, the number of companies with a PPP loan rose by 17% (+1 to +35) and registrations by 21%. The data do not determine which comparison is correct, and for this outcome considered alone they are consistent with a relative rate near zero and with a relative rate near one.

For any of the three records, the conclusion for the two holidays together does not depend on the reference period. The estimate lies between −0.23 and 0.34 under the six reference periods, and every interval excludes one, with upper ends between 0.41 and 0.91. In the second holiday alone, the estimate is between 0.11 and 0.54, and the interval includes one under two of the six reference periods (Appendix Tables A25 and A33). The two outcomes differ because the flanking months show a deficit in the PPP loan and not in the other records: for any of the three records, the statistic for the flanking months is within the range found in unaffected blocks (52 of the 239 pairs are as far from zero). The stability of the broader record reflects its largest component, the financing statement, which 7.7% of the LLCs in the sample have and for which ρ under the comparison with the flanking months is 0.18 (−0.72 to 0.95). The broader record was a secondary outcome, and the weight that we give it rests on a comparison of the two outcomes made after the results were known.

6.3. Measurement Validity of the Lending Records

By equation (3), the estimates describe businesses only if the businesses among marginal registrations leave these records as often as other businesses do. The upper ends of the pre-specified intervals give the capture ratios at which equal business rates would fit the data. The businesses among marginal registrations would have to hold a PPP loan in the company's name at no more than 0.60 times the rate of other businesses, or any of the three records at no more than 0.41 times that rate. Capture could be lower among them for three reasons, and the data bear on two [E].

First, a business may borrow in its owner's name. Of the 192,267 PPP loans with a Colorado borrower, 60,904 are not linked to an entity by the rule of Addendum 3, and 47,364 of these name the borrower in the form of a person's name; most describe the borrower as a sole proprietorship, a self-employed individual, or an independent contractor. We linked these loans to the entities whose registered agent is a person with the same name and ZIP code, under a rule that we wrote before any tabulation by cohort and did not record in the version history beforehand (Appendix D.4). The rule links 13,036 loans, of which an estimated 3.3% are chance matches, and 1.2% of ordinary LLCs have an agent with such a loan, compared with 5.3% with a loan in the company's name. The estimate for these loans depends on how a person's loan is attributed. When the loan is counted for every company of which the person is the agent, the share of companies with one differs from the reference level by −0.02 percentage points (s.e. 0.07), and the relative rate is 0.85, with an interval from −0.09 to 2.02. This measure carries a person's loan to each further company that the person forms, including a company formed because the fee was $1. When only the loans that match a single company are counted, the share is lower in the holiday cohorts by 0.09 percentage points (0.03), and the relative rate is −0.26 (−1.16 to 0.68); this measure omits every person who is the agent of more than one company, and with it every further company that such a person formed during a holiday. The first measure attributes too

many of the agents' loans to marginal registrations, unless a marginal registration is more likely than another company of a borrowing agent to be the business for which the agent borrowed, and the second attributes too few. With a loan in either name counted, ρ under the pre-specified comparison is 0.18 (−0.34 to 0.63) by the first measure and 0.04 (−0.56 to 0.54) by the second (Table 5, Panel C), compared with 0.07 for the loan in the company's name alone.

These estimates bear on whether marginal registrations borrowed in the agent's name in place of the company's. If they had borrowed as often as other registrations, in the agent's name where not in the company's, ρ would equal one for an outcome that counts a company with an agent's loan and no loan in its own name at a weight equal to the inverse of the proportion of such loans that the rule finds. At a weight of 2.08, the inverse of the share of loans in a person's name that the rule links where the borrower states that the business is an LLC (48.1%), the estimate under the pre-specified comparison is 0.26 (−0.21 to 0.70), and the interval excludes one at every weight up to five (Appendix D.4). Under the pre-specified comparison, borrowing in the agent's name in place of the company's therefore does not account for the deficit unless the rule finds fewer than about one such loan in five. For the second holiday alone, the interval excludes one at the weight of 2.08 and reaches one at a weight of three. Under the comparison with the flanking months, ρ with a loan in either name counted is 0.75 and 0.73 by the two measures, with intervals that include one, as for the loan in the company's name.

A loan in an agent's name is weak evidence that the company operates a business. Among the LLCs whose registered agent is a person, such loans were about half as common among companies that had become delinquent before February 15, 2020 as among other companies (0.9% and 1.8%), whereas loans in the company's name were about one-twentieth as common (0.4% and 7.9%). The results do not determine whether the agents of marginal registrations borrowed as individuals as often as other agents: by the first measure the relative rate of an agent's loan is 0.85, with an interval that includes one.

Second, a business may have closed before 2020. The share of registrations that had become delinquent before the loans began is 3.2 percentage points higher in the holiday cohorts (s.e. 1.3). Given the loan rates of delinquent and other registrations, that difference accounts for 34% of the decline in the loan share, and among registrations that had not become delinquent the loan rate remains lower by 0.76 percentage points (0.24). A record dated near formation gives the same result: the share with a UCC financing statement within two years of formation is lower by 0.70 percentage points (0.15), about one-sixth of its rate [A2]. Earlier closure therefore accounts for about one-third of the deficit and not for the remainder, although delinquency records the lapse of a registration and not the closure of a business.

Third, a business may never borrow. The three records identify companies that borrowed from a lender or under a federal program, and 10.9% of the LLCs formed from 2010 to 2018 have any of them. A business without employees or secured debt appears only if it took a PPP loan against its owner's income. These data do not measure such businesses, and if they are more common among the registrations that respond to a fee than among other registrations, the relative rate of businesses exceeds the relative rate of records. The Colorado results therefore establish the first statement of Section 3, that marginal registrations were less likely than others to leave a lending record, for the PPP loan under the pre-specified comparison and for the broader record, in the two holidays together, under every reference period. They support the second statement for

businesses of the kind that borrow, and they do not bear on entities held for other purposes.

6.4. Record Linkage and Inference

Linkage. Three properties of the links matter, and they are measured to different degrees [E]. The first is precision. Of the links that the rule made, two sets of AI readers judged 96 to 98% to join the same company in each of four cohorts (the holiday months, the same months of the adjacent years, the flanking months, and the other months of 2010 to 2018), and the difference between the first two cohorts is +0.6 percentage points (s.e. 1.2). The second is recall within the set of candidates. Among pairs of a company without a linked loan and an unlinked loan whose borrower's name is close to the company's, the readers judged 3.3 to 4.2 links to have been missed per hundred links made. With the share of each cohort corrected for both, ρ is 0.16 (-0.47 to 0.68). The third is the frequency of true links outside the candidate set, which a search among similar names cannot measure. Loans in the name of the registered agent (Section 6.3) are one such category. Loans under an unrelated trade name, or in the name of an owner who is not the agent, are not measured; 13,540 of the unlinked loans are not in the form of a person's name. Under eight other linking rules, the estimate for the PPP loan under the pre-specified comparison is between 0.06 and 0.30, with every interval below one, and the estimate under the comparison with the flanking months is between 0.66 and 1.07 (Appendix Table A38). The pairs were read by AI readers only. On 100 pairs with a known answer, constructed to be difficult, the readers were correct in 91% of the cases in which the two records are of the same company and in 89% of the others. No person has audited a sample of the links, and the precision and recall reported here are subject to that limitation.

Inference. In simulated data in which no holiday has an effect, a test of nominal size 5% rejects in 3.8 to 5.6% of replications across ten designs, and in simulations with a true ρ of zero, one-half, or one, the interval contains the true value in 94 to 98% of replications. At the largest critical value that the simulations require, the interval runs from -0.60 to 0.61 for the PPP loan and from -0.67 to 0.43 for any of the three records. Of 567 pairs of reference runs with disjoint months, none gives a statistic as far from zero as the estimate for either record. The simulations concern the pre-specified statistic and do not cover the alternative reference periods (Appendix D.5) [E].

6.5. Registration Maintenance

A registry's own measure of whether a company survives is whether it files its periodic report. Under the measure that Addendum 2 specified as primary, companies formed in the holiday months filed a first report on time 3.6 percentage points less often than companies formed in the same months of the adjacent years (s.e. 0.7), and a marginal registration was 0.44 times as likely as an inframarginal registration to file (-0.10 to 0.74) [A2]. That measure subtracts the filing rate of established companies whose reports fell due in the same month, and the estimate is sensitive to this adjustment. With the adjustment at the weight observed in unaffected months, ρ is 0.48, and with no adjustment it is 0.63, with an interval that extends to one; other measures of maintenance give values between 0.35 and 0.58 (Appendix Table A40) [E]. In every version, the holiday cohorts maintained their registrations less often than the reference cohorts, and the relative rate is higher than the pre-specified estimate for the PPP loan. A higher rate would be expected if a registration can be maintained without a business, since the report cost $10 and took a few minutes online; under the

comparison with the flanking months the estimate for the PPP loan is as high, so that the difference is not established.

The 2022–23 program cannot be used for the tests of Section 6.1, because its cohorts were formed after the PPP loans had been made, and its only pre-specified outcome is the maintenance of the registration. The plan [P] compared ordinary LLCs formed from July 2022 to March 2023 with those formed on the same dates a year earlier. Net of the same change among corporations and foreign entities, the share that filed a first report on time declined by 7.0 percentage points (s.e. 1.0), which the plan interprets as a marginal registration filing at 0.22 times the inframarginal rate [P]. We do not adopt that interpretation. Cohorts formed at $50 in the two years after the program also filed less often than the 2021–22 cohort, by 5.8 and 6.4 percentage points net of established companies, compared with 8.6 for the program cohort, and a deficit among companies that paid the regular fee does not reflect selection on the reduced fee (Appendix E.8) [E].

7. Non-Dormant Accounts of Deterred Registrations: The United Kingdom

If the fee reductions in Colorado added registrations that seldom left a lending record, a fee increase should remove registrations of the same type. Registrations should decline, and among the companies that still register, the share with a record of activity should increase in the proportion by which registrations decline (equation (2) with ρ equal to zero). The U.K. fee increase of May 1, 2024 provides this test in a second country, a decade later, with a fee change of the opposite sign and a different type of record.

7.1. Main Results

Registrations declined by 17% (Section 5), and the share of new companies with non-dormant accounts increased. Among the companies registered in the six weeks of the before period, 35.7% had non-dormant accounts on file in October 2026, compared with 42.2% of those registered in the six weeks of the after period (Table 4, Panel A). Between the same weeks of the four reference years, the share changed by −2.5 log points on average and in no year by more than 6.5. The estimated increase in the share is 19.2 log points (s.e. 3.3, 3 degrees of freedom, $p = 0.010$), or 7.5 percentage points [A4].

The share increased by as much as registrations declined. The estimated change in the number of new companies with such accounts is 0%, with a 95% confidence interval from −12% to +14%, and the rate of such accounts among deterred registrations relative to the remaining registrations is $\rho = 0.00$ [A4], with a 95% confidence set by test inversion from −2.5 to 0.42 [E]. The direct estimate that the addendum also specified, in which the changes in registrations and in the share are both measured against the four years in which the outcome is observed, is −0.8% for the number of such companies (−14.2 to +14.7) [A4]. The hypothesis that this number declined in proportion to registrations is the hypothesis that the share did not change, and it is rejected. The estimate of the change in the number is near zero. Its interval admits a decline of 12%, and the confidence set for ρ admits a response of that number of up to 0.42 times the response of registrations, so that the test does not show the effect on the number of such companies to be negligible.

Addendum 4 specified the reading that the fee increase "removed registrations, and those it removed were less often businesses, by this measure, than those still made," and it permitted the statement that most of the removed registrations were not businesses if the interval for ρ lay wholly below one-half. We state the result for the record and not for businesses: deterred registrations would have had non-dormant accounts on file less

Table 4. United Kingdom: The Fee Increase of May 1, 2024

A. Registrations and subsequent outcomes [A4]

	Age, months	Before the increase	After the increase	Change in the share, log points	Change in the number, percent	Deterred relative to remaining, ρ
Registrations per working day		4,084	2,948		−17 [−29, −4]	
Percent of registrations with:						
Non-dormant accounts (primary)	27–31	35.7	42.2	+19.2 (3.3)	0 [−12, +14]	0.00 [−2.5, 0.42]‡
A charge registered by a lender	27–31	3.9	5.0	+26.9 (2.4)	+8 [−9, +28]	−0.35 [−5.3, 0.25]‡
Either of the two	27–31	36.1	42.8	+19.3 (3.3)	0 [−12, +14]	−0.01 [−2.6, 0.42]‡
First confirmation statement filed	15–19	59.9	66.5	+14.7 (1.9)	−4 [−18, +12]	0.22 [−2.5, 0.59]†
On the register and active	15–19	59.3	66.2	+14.5 (2.0)	−5 [−19, +12]	0.22 [−2.5, 0.61]†

B. Other outcomes in October 2026 [E]

	Before the increase	After the increase	Change in the share, log points	Change in the number, percent	Deterred relative to remaining, ρ
Still on the register	47.3	57.0	+17.2 (2.2)	−2 [−15, +13]	0.09 [−2.6, 0.46]‡
Accounts of any type on file	46.4	55.2	+17.1 (2.2)	−2 [−15, +12]	0.10 [−2.6, 0.47]‡
Dormant accounts on file	10.8	13.1	+10.5 (2.4)	−8 [−24, +10]	0.43 [−2.2, 0.86]†
Micro-entity accounts on file[b]	23.0	26.9	+16.6 (4.1)	−3 [−18, +15]	0.12 [−2.0, 1.08]
Total exemption full accounts on file[b]	10.7	12.8	+24.6 (4.7)	+6 [−11, +26]	−0.25 [−3.2, 0.64]†

Notes. Before the increase: companies incorporated from March 4 to April 14, 2024; *after the increase*: companies incorporated from May 20 to June 30. Panel A: the first row reports registrations per working day; the remaining rows report the share of each period's registrations with the outcome, measured in the register snapshot of October 2026 (ages 27 to 31 months) or October 2025 (15 to 19 months). *Change in the share*: the log change from before to after in 2024 minus its mean over the reference years, in log points, with the standard error in parentheses. *Change in the number*: the sum of the changes in registrations and in the share, in percent, with a 95% confidence interval from joint draws of the two errors. ρ: equation (6), with the 95% confidence set by test inversion (Section 4.5) [E]; the lower end is given to one decimal because it carries simulation error. † The set excludes one. ‡ The set excludes every value from one-half to one. Panel B: the sets are formed at each outcome's own correlation between the two errors, which the addendum did not specify; the addendum had excluded the share still on the register because the fee for a confirmation statement changed between the two periods in some of its reference years. Reference years: 2015 to 2019, 2021 to 2023, and 2025 for registrations; 2017 and 2021 to 2023 for outcomes at 27 to 31 months; 2018, 2021, 2022, and 2025 for outcomes at 15 to 19 months. [b] Categories of accounts are recorded for three reference years, 2021 to 2023, which leaves two degrees of freedom. Appendix Tables A26 to A31 report every statistic that the addendum specified, with the percentile intervals over the draws retained, and Appendix Table A9 compares those intervals with the sets shown here.

often than the remaining registrations. Section 7.2 examines whether the relative rate is below one-half, and Section 7.4 what the record captures.

Few new companies have a charge registered by a lender (3.9% of registrations in the before period). The share with a charge increased by 27 log points (s.e. 2), the number of companies with a charge changed by +8% (−9 to +28), and ρ is −0.35 [A4], with a confidence set by test inversion from −5.3 to 0.25 [E].

7.2. The Confidence Set for the Relative Rate

The percentile interval for ρ over the draws retained, from −1.62 to 0.42, uses the joint draws in which registrations decline by at least half a log point (Section 4.5) [A4]. The draws set aside are 1.15% of the total, and 87% of them imply a ratio above one-half. The remainder of this section is exploratory [E]. From the same draws, percentiles of the ratio over all draws run from −1.68 to 0.51, and the set by test inversion runs from −2.5 to 0.42 (Appendix Table A10, Panel A). The set by test inversion, which uses every draw, thus has the same upper end as the percentile interval over the draws retained, and setting draws aside changed only the lower end. The percentile interval over all draws reaches above one-half because it counts the 1.0% of draws

in which registrations rise among the largest values of ρ, and it is not the inversion of a test (Appendix D.1). We report the set by test inversion, which applies to equation (6) the approach of Fieller (1954) for a ratio.

At $\rho_0 = 1$ the test is the t test of no change in the share of registrations with non-dormant accounts ($t = 5.79$ with three degrees of freedom, $p = 0.010$), which involves neither the error of the change in registrations nor its correlation with the error of the change in the share. At other values the set is formed from draws centered on the estimates and not from a distribution simulated under each hypothesis, and two checks support it. In simulated years with the assumed error model, the set contains the true value in 95.1 to 98.8% of samples (Appendix Table A10, Panel E). A second test, whose null distribution is simulated from the same error model, retains values up to 0.44, or up to 0.46 when its p-value is maximized over the change in registrations, and rejects one-half ($p = 0.034$ and 0.037; Appendix D.1).

The set depends on the correlation between the errors of the change in registrations and of the change in the share, which Addendum 4 fixed at -0.72 within a year from 21 dates in data that precede the test. With independent errors the upper end is 0.58. At the correlation pooled over 36 placebo dates in the four reference years, which is -0.55 and is negative at 94% of the dates, the upper end is 0.47, and at the correlation of the four reference years at the date of the test (-0.98) it is 0.32 (Appendix Table A10, Panel B). One-half is excluded at any within-year correlation of -0.45 or stronger. The second test retains values 0.01 to 0.04 higher at each correlation. It rejects one-half at a correlation of -0.50 or stronger with the change in registrations at its estimate and of -0.58 or stronger with its p-value maximized, and at the pooled correlation its two forms retain values up to 0.49 and 0.51.

A direct test requires no assumption about the correlation. Let N and B be the log changes in registrations and in the number of companies with the outcome between the two periods of a year, both observed in the four reference years and in 2024. The hypothesis that the fee changed B by λ_0 times its effect on N is tested by comparing the value of $B - \lambda_0 N$ in 2024 with its values in the four reference years, a t test with three degrees of freedom. The test rejects every λ_0 between 0.35 and 2.89 ($p = 0.002$ at one-half and 0.010 at one). Because ρ is no greater than the ratio of the two log changes when that ratio lies between zero and one, the test rejects every ρ from 0.35 to one (Appendix Table A10, Panel C). The four reference years vary little in this contrast at the date of the test; with the spread pooled over the placebo dates in its place, one-half remains beyond the critical value for three degrees of freedom (Appendix D.1).

The test by inversion and the direct test were applied at the 36 placebo dates, taking each reference year in turn against the other three. Tests of ρ_0 equal to zero, one-half, and one reject in 1.4, 1.4, and 6.9% of the 144 cases by test inversion and in 4.2, 4.2, and 6.9% by the direct test (Appendix Table A10, Panel E). In simulated years in which the true correlation is zero and not -0.72, the set by test inversion contains the true value in as few as 89.5% of samples, and the direct test contains it in 93.4 to 95.2% of samples in every design. When the true value is one-half or more, either set lies wholly below one-half in at most 1.2% of samples under the assumed correlation, and in 2.4% when the true correlation is zero and the errors have heavy tails (Appendix Table A10, Panel F).

The upper end of the set by test inversion for the pre-specified comparison is therefore between 0.32 and 0.58, depending on what is assumed about the two errors. One-half is excluded by the direct test and, under the error model of the addendum, at any within-year correlation of -0.45 or stronger by test inversion and, by

the second test, at −0.50 or stronger with the change in registrations at its estimate and at −0.58 or stronger with its p-value maximized; the three estimates of that correlation are −0.55, −0.72, and −0.98. At weaker correlations only the direct test excludes one-half. Every construction excludes one.

7.3. Base Period and Reference Years

Base period. The before period of the pre-specified test falls after the announcement of February 19. Under the comparison that the addendum specified as a check, which takes the six weeks before the announcement as the base period, the decline in registrations is 21.8 log points (s.e. 5.8) and the increase in the share is 15.3 log points (4.6) [A4]. The number of companies with such accounts then changes by −6% (−18 to +7), and ρ is 0.28, with a set from −0.4 to 0.95 that excludes one and includes one-half [E]. The data do not determine which base period is correct. Registrations in the pre-specified base period were not higher than in the weeks before the announcement (−2.6 log points, s.e. 4.9), so that the addendum's criterion for a base period inflated by anticipation was not met. The share with such accounts in that period was nevertheless 4.0 log points lower (s.e. 1.5, $p = 0.083$). The earlier base period precedes March 4, 2024, when the first measures of the 2023 Act took effect, so that a comparison with it includes any effect of those measures. Filings advanced ahead of the increase do not bias the pre-specified estimate downward (Section 3).

Reference years. The fee for the annual confirmation statement also rose on May 1, 2024. The companies of the two periods of 2024 owed their statements at the same fees, but in two of the four reference years, 2022 and 2023, a statement fell due at £13 for companies of the before period and at £34 for those of the after period, which would raise the estimate for 2024 if the higher fee lowered the share in those years. Addendum 4 noted this exposure and retained both years. Without 2022, the one year in which an effect of the statement fee on survival is detected, ρ is 0.01 (−2.6 to 0.41). With 2017 and 2021 as the only reference years, the increase in the share is 17.2 log points and ρ is 0.10. Two reference years leave one degree of freedom, and with their own standard error the set runs from −2.8 to 1.33 and includes one. With a standard error computed from the same contrast at 36 placebo dates in the two years (3 degrees of freedom), the set runs from −2.5 to 0.45. The second procedure takes its variance from other dates of the year. The point estimate thus does not depend on the two exposed years, whereas the inference does: with their own standard error, the two unexposed years do not bound ρ below one. When any one of the nine reference years is omitted, the estimate is between −0.06 and 0.04 and the upper end of the set between 0.39 and 0.89. With the earlier base period and without 2022, ρ is 0.34 (−0.5 to 0.77) (Table 5, Panel B) [E].

Windows, placebo dates, and timing. Across 315 combinations of the length of the two periods, the days excluded around May 1, and the reference years, all with a base period after the announcement, the decline in registrations ranges from 8 to 29 log points, the largest value of ρ is 0.28, and the confidence interval for the change in the number of companies with such accounts includes zero in every specification (Appendix Figure A1). At placebo dates on which no fee changed, the largest of the 144 placebo estimates for the share is 7.3 log points in absolute value, compared with 19.2 at the fee increase. By week of incorporation, the share with non-dormant accounts is between 32 and 38% in every week from January to April 2024 and between 41 and 43% in each of the nine weeks that follow the first full week at £50. The share does not change in the week of March 4 or in the week of April 6, and it had increased before the general election was called, so that the timing of the change matches the fee increase and not the other events of that spring. Appendix D.6

reports these analyses and those of the age at which outcomes are measured [E].

7.4. Measurement Validity of the Accounts Outcome

The outcome indicates a company that is on the register at 27 to 31 months and has filed accounts that it does not declare dormant. Filing accounts is a statutory duty, so that a trading company that remains on the register is recorded: at that age, nearly every company still on the register has accounts of some type on file (47.3% of the registrations of the before period are still on the register, and 46.4% have accounts on file). The record misses two kinds of business. The first is a company that traded and left the register before October 2026. From the monthly files we reconstruct whether a company ever had non-dormant accounts on file, whether or not it was later removed, for the cohorts of 2022 to 2024 [E]. For this outcome ρ is −0.16, with a set from −3.7 to 0.28 when the standard error is computed from 36 placebo dates; its two reference years alone give a set from −3.6 to 3.18, which is uninformative. The second is a business that operated for a short time and never filed accounts, the first of which are due 21 months after incorporation. The register cannot show such a business. In the other direction, a company with minimal activity may declare its accounts non-dormant. By equation (3), equal business rates among deterred and remaining registrations would require that the trading companies among the deterred have such accounts on file at no more than 0.42 times the rate of other trading companies.

Most of the deterred registrations were of companies that would have left the register within thirty months. At 27 to 31 months, a deterred registration would have been on the register 0.09 times as often as a remaining registration (−2.6 to 0.46; Table 4, Panel B), a comparison that the addendum had excluded because of the statement fee. At the upper end of that set, 26% of the deterred registrations would have remained on the register, compared with 57% of the remaining registrations. The point estimates indicate that most would have left without filing accounts. When every company of the two periods of 2022 to 2024 is assigned to one of nine classes by what it had filed and whether it remained on the register, 76% of the deterred registrations were of companies that would have left the register with nothing on file, compared with 41% of the registrations expected in the absence of the increase, and 4.5% would at some time have filed non-dormant accounts, compared with 35.5%. This classification has no standard error. For dormant accounts, the indicator of a company maintained without trading, ρ is 0.43 (−2.2 to 0.86), and the confidence sets do not distinguish companies with dormant accounts from companies with non-dormant accounts. Registrations at addresses where a hundred or more companies had been incorporated within a year and registrations in same-day batches of ten or more account for 19% of the decline in registrations per working day, so that most of the deterred registrations were single registrations at other addresses (Appendix D.6) [E].

7.5. Persistence and Two Predictions for the 2026 Fee Increase

The decline in registrations persisted. In each of the seven six-week periods to April 2025, registrations were 16 to 19 log points lower under the same comparison, and in each period for which outcomes can be measured, the change in the number of companies with non-dormant accounts, estimated directly, was +0.5 to +3.2 log points and was not statistically distinguishable from zero (Appendix Table A32) [E]. A coarser comparison that uses every month and has no reference years gives a ρ of 0.20, close to the estimate with the earlier base period (Figure 3) [E].

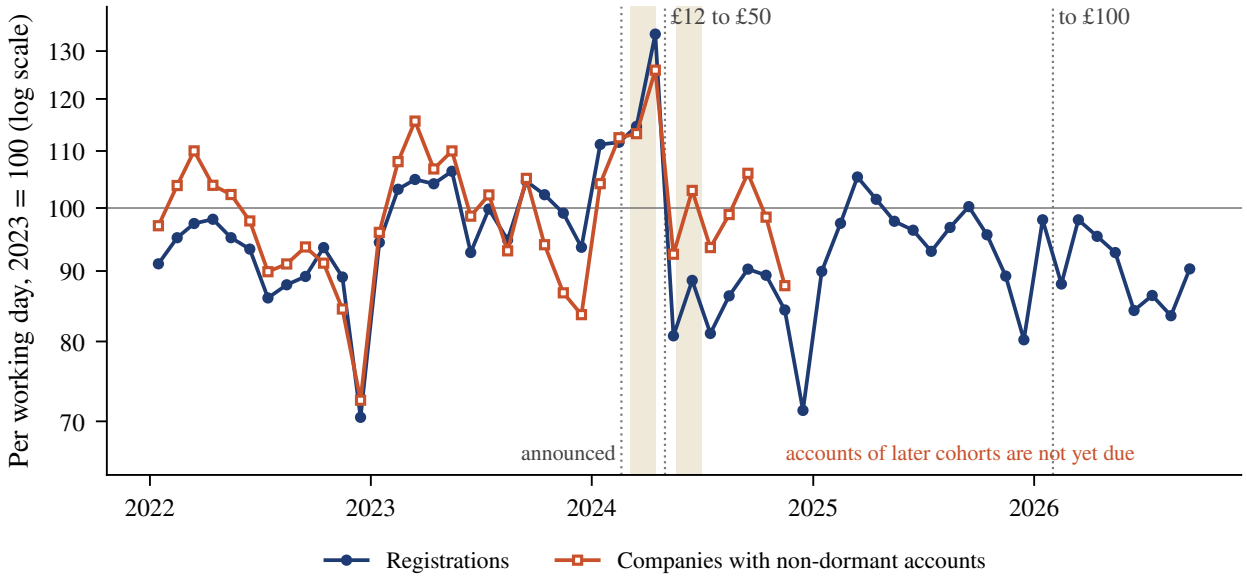


Figure 3. United Kingdom: Registrations and Companies with Non-Dormant Accounts, by Month of Incorporation

Notes. Both series are per working day and are shown as an index on a logarithmic scale with the mean of 2023 set to 100. Registrations are counted from company numbers. Companies with accounts are those with non-dormant accounts on file in the October that falls two years after the cohort year; the series ends with companies incorporated in November 2024, the last for which the outcome can be measured. Dotted lines mark the announcement of February 19, 2024 and the fee increase to £100 on February 1, 2026; shading marks the before and after periods of the pre-specified test, on either side of May 1, 2024. Not pre-specified [E].

The second fee increase, to £100 in February 2026, is too recent for any outcome to be measured, and the change in registrations around it is confounded with compulsory identity verification for directors, which began in November 2025. The addendum records two predictions for data that do not yet exist, to be computed with the same code. When the register is observed in October 2027, the share of companies with a first confirmation statement will have increased at February 1, 2026, and when it is observed in October 2028, the share with non-dormant accounts will have increased; Appendix B gives the reference years and the smallest estimates whose intervals would exclude zero. Both are weaker tests than the one reported here, because the second increase doubled a fee that had already removed the registrations most easily deterred.

8. Robustness to Alternative Specifications

Table 5 collects, for the three outcomes, the estimate of the relative rate under each choice that changes it materially: the reference period, the episodes and reference years, the definition of the outcome, and the method of joint inference. Panel E reports the corresponding changes in registrations and in the number of companies with each record. The estimates in the first row and in the rows marked [A3] or [A4] were specified in advance, as were three U.K. entries of Panel E, namely the two changes in registrations and, under the pre-specified comparison, the change in the number of companies [A4]; the U.K. sets by test inversion and all other entries are exploratory [E].

For the PPP loan, the primary outcome in Colorado, the conclusion depends on the reference period. The interval excludes one under the pre-specified comparison, with all unaffected months as the reference period, over the whole window, in each holiday, and under each alternative linking rule, and it includes one in

Table 5. The Relative Rate ρ under Alternative Specifications

	Colorado: fee reduced from $50 to $1		U.K.: fee raised from £12 to £50
	PPP loan	Any of three lending records	Non-dormant accounts
Pre-specified comparison [A3, A4]	0.07 [−0.57, 0.60]†	−0.02 [−0.65, 0.41]‡	0.00 [−2.5, 0.42]‡
A. Reference period			
All unaffected months, linear trend	0.19 [−0.27, 0.68]†	0.31 [−0.24, 0.91]†	
All unaffected months, quadratic trend	0.07 [−0.34, 0.42]‡	0.12 [−0.30, 0.43]‡	
Whole window: holiday and flanking months together	−0.41 [−1.22, 0.32]‡	−0.23 [−1.17, 0.46]‡	
Holiday months less flanking months, adjacent years	0.81 [−0.19, 1.78]	0.26 [−0.38, 0.78]†	
Holiday months less flanking months, every other unaffected year	0.67 [0.24, 1.17]	0.34 [−0.04, 0.67]†	
Flanking months at the weight estimated from unaffected years	0.13 [−0.83, 1.15]	0.05 [−0.58, 0.61]†	
Base period: the six weeks before the announcement			0.28 [−0.4, 0.95]†
B. Episodes and reference years			
First holiday alone [A3]	−0.18 [−1.61, 0.75]†	−0.44 [−2.03, 0.33]‡	
Second holiday alone [A3]	0.21 [−0.55, 0.81]†	0.20 [−0.53, 0.67]†	
Without the reference year 2022			0.01 [−2.6, 0.41]‡
Reference years with equal maintenance fees, their own s.e.			0.10 [−2.8, 1.33]
The same two years, s.e. from 36 placebo dates			0.10 [−2.5, 0.45]‡
Leaving out one reference year: range of estimates			−0.06 to 0.04
range of the upper ends			0.39 to 0.89†
Earlier base period and without 2022			0.34 [−0.5, 0.77]†
C. Definition of the outcome			
Loan linked only by an identical full name and ZIP code [A3]	0.13 [−0.76, 0.89]†		
Loan linked to every company with the name	0.30 [−0.23, 0.74]†		
Loans in the name of the company's agent counted	0.18 [−0.34, 0.63]†		
only such loans that match a single company	0.04 [−0.56, 0.54]†		
Financing statements filed from 2020 only		0.08 [−0.47, 0.47]‡	
Accounts ever filed, whether or not still on the register[a]			−0.16 [−3.7, 0.28]‡
D. Joint inference			
Set by test inversion in place of percentiles	[−0.57, 0.60]†	[−0.65, 0.41]‡	as in the first row
Critical value required by dependent reference runs	[−0.60, 0.61]†	[−0.67, 0.43]‡	
Percentiles over the draws retained [A3, A4]	as in the first row	as in the first row	[−1.62, 0.42]‡
Percentiles over all draws			[−1.68, 0.51]†
Second test, null distribution simulated[b]			[0, 0.44]‡
its p-value maximized over the change in registrations			[0, 0.46]‡
Errors of the two estimates independent			[−3.8, 0.58]†
Error correlation as pooled over 36 placebo dates			[−2.8, 0.47]‡
second test at that correlation			[0, 0.49]‡
its p-value maximized			[0, 0.51]†
One scale for both errors, three degrees of freedom			≤0.41 or ≥6.27‡
Four common reference years, no correlation assumed[c]			≤0.35 or ≥2.89‡
E. Change in the number of registrations and of companies with the record, percent			
Pre-specified comparison: registrations	+17 [+14, +20]		−17 [−29, −4]
companies with the record	+1 [−7, +11]	0 [−7, +7]	0 [−12, +14]
Whole window (Colorado); earlier base period (U.K.): registrations	+10 [+7, +13]		−20 [−30, −8]
companies with the record	−4 [−11, +3]	−2 [−9, +5]	−6 [−18, +7]
Holiday months less flanking months, adjacent years: registrations	+21 [+14, +28]		
companies with the record	+17 [+1, +35]	+6 [−3, +14]	

Notes. Each cell reports the estimate of ρ and, in brackets, a 95% confidence interval (Colorado) or a 95% confidence set by test inversion (United Kingdom); Section 4.5 describes both and shows that they agree for Colorado. † The interval or set excludes one. ‡ It excludes every value from one-half to one. Colorado: ordinary LLCs formed in the two fee holidays, pooled; the flanking months are the month in which each holiday was announced and the month after it ended. Panel A: Section 6.2 and Appendix Table A11 describe the reference periods; in the sixth row the statistic for the flanking months is subtracted at the weight estimated across blocks of unaffected months, and the interval includes the sampling error of that weight. Panel B: the reference years with equal maintenance fees are 2017 and 2021 (Section 7.3); the range of the upper ends is over the nine sets obtained when one reference year is omitted. Panel C: Sections 6.1, 6.3, 6.4, and 7.4; a loan in the name of a company's agent is attributed to every company of which the borrower is the registered agent or, in the following row, only where it matches a single company. [a] With the standard error computed from 36 placebo dates; with the standard error of its two reference years, the set runs from −3.6 to 3.18. Panel D: the second row uses the largest critical value that the simulations of Section 6.4 require. *Percentiles over the draws retained*: the interval of the locked scripts, taken over the draws in which registrations decline by at least half a log point. [b] The second test of Section 7.2, whose null distribution is simulated; it was applied to values from zero to one. *One scale for both errors*: the two errors are drawn with a single scale and three degrees of freedom, and the set is then the union of two rays. [c] The values of the ratio of the log change in the number of companies with the outcome to the log change in registrations that the direct test of Section 7.2 does not reject; between zero and one, that ratio is an upper bound on ρ. Panel E: percent change in registrations and in the number of companies with each record, with 95% confidence intervals. The Colorado intervals of Panel E are computed in the sample of the loan test, with seven degrees of freedom for the pre-specified comparison and three for the other two comparisons; with the reference runs of Addendum 2, which extend to 2019 and give ten, the pre-specified interval for registrations is 13 to 22 (Section 5).

the two rows that compare the holiday months with the flanking months, in which the estimate is 0.81 and 0.67, and in the row that subtracts the flanking months at the estimated weight, in which it is 0.13. For any of the three lending records, the estimate is at most 0.34 and the interval excludes one in every row: under every reference period for the two holidays together, in each holiday under the pre-specified comparison, and under both methods of inference. For the second holiday alone, which the table shows under the pre-specified comparison only, the estimate under the six reference periods of Section 6.2 ranges from −0.05 to 0.73 for the PPP loan and from 0.11 to 0.54 for any of the three records, and the interval includes one under one of the six periods for the PPP loan and under two for any of the three records (Appendix Tables A25 and A33). For non-dormant accounts in the United Kingdom, the estimate is at most 0.34 and the set excludes one in every row but one. The exception takes the two reference years with equal maintenance fees and their own standard error, which leaves one degree of freedom; the estimate in that row is 0.10.

The evidence that the relative rate is below one is stronger than the evidence that it is below one-half. The pre-specified comparisons do not exclude one-half for the PPP loan, and they exclude it for the broader Colorado record and for the U.K. outcome. For the broader record, the interval includes one-half under the linear trend, under the comparisons with the flanking months, and in the second holiday alone. For the U.K. outcome, the set includes one-half with the earlier base period, with the two equal-fee years and their own standard error, when any one of the reference years 2017, 2021, and 2023 is omitted, when the after period is extended to include the last 16 days at the old fee (Appendix D.6), and if the correlation of the two errors within a year is weaker than −0.45, or than −0.50 and −0.58 under the two forms of the second test of Section 7.2; the percentile interval over all draws, which is not the inversion of a test, also reaches above one-half. In simulated samples in which the true value is one-half or more, the set by test inversion or that of the direct test lies wholly below one-half in at most 1.2% of samples under the assumed correlation and in 2.4% in the least favorable design (Section 7.2; Appendix Table A10, Panel F).

In Panel E, the estimated changes in the number of companies with each record are near zero under the pre-specified comparisons, and the ends of their intervals lie 7 to 14% from zero. Under the comparison with the flanking months, the number of Colorado companies with a PPP loan rose by 17% (+1 to +35), while registrations under the same comparison rose by 21%.

9. Discussion and Conclusion

Summary of findings. A change of about $50 in a formation fee changed the number of registrations by about one-sixth, in Colorado's two fee holidays taken together and in the United Kingdom, and it changed the composition of the registered cohorts. In the two pre-specified tests, the registrations that the lower fee added in Colorado and those that the higher fee deterred in the United Kingdom were less likely than other registrations to leave the records examined. The relative rate is 0.07 for a PPP loan in the two holidays together (0.21 in the second holiday alone, and −0.22 among new filers), −0.02 for any of three lending records (0.20 and −0.25), and 0.00 for non-dormant accounts, and the interval or set for each pooled estimate excludes one. The strength of this conclusion differs by outcome (Table 5). For the PPP loan, the primary outcome in Colorado, the estimate ranges from −0.41 to 0.81 with the reference period (−0.05 to 0.73 in the second holiday alone), and its interval includes one when the holiday months are compared with the months that flank them, a comparison that the data neither support nor exclude. For the broader lending record in the two holidays together and

for the U.K. accounts, the estimate is at most 0.34 and the interval or set excludes one under every reference period, base period, and method of inference that we examine, with the exception of a few U.K. comparisons that rest on two or three reference years. For the second holiday alone, the estimate for the broader record is between 0.11 and 0.54 under the six reference periods of Section 6.2, and two of the intervals include one. The relative rate is below one-half in the pre-specified comparisons for these two outcomes and not under several of the alternatives (Section 8). The number of companies with each record changed by +1%, 0%, and 0% for the three outcomes under the pre-specified comparisons, with intervals whose ends lie 7 to 14% from zero, and with the earlier U.K. base period the estimate is a decline of 6% (−18 to +7). The estimates therefore do not show that a fee change leaves the number of companies with these records unchanged.

Records and businesses. These estimates are relative rates of records. They are relative rates of operating businesses only if the businesses among the registrations that respond to a fee leave the records as often as other businesses do (equation (3)). The evidence on that condition is partial (Sections 6.3 and 7.4). In the United Kingdom, filing accounts is a statutory duty, and the estimate is no higher for accounts ever filed. In Colorado, a lending record dated near formation shows the same deficit, and borrowing in the agent's name in place of the company's does not account for the result for the PPP loan under the pre-specified comparison unless the link finds fewer than about one such loan in five, or no more than about one in three for the second holiday alone. A business that never borrows, or that trades briefly and files no accounts, appears in none of these records. We therefore conclude that the registrations that respond to a fee of this size leave these records less often than other registrations do. They include a smaller share of businesses of the kind that borrow or file accounts unless the businesses among them leave the records at no more than 0.60, 0.41, and 0.42 times the rate of other businesses for the three outcomes, the values at the upper ends of the pre-specified intervals and of the U.K. set. We do not conclude that they include few businesses of any kind.

The results do not show that such registrations lack value. The framework gives one bound: a filer who registers only at $1 expects a surplus from the entity smaller than the $49 difference between the fees, so that the filer's own loss from the higher fee is smaller than that amount. The bound concerns the filer's expectation on the day of filing, and it does not cover value to others. The estimates are silent on uses of an entity that leave none of these records, and with a few thousand marginal registrations we cannot detect a thin right tail.

Implications for measurement. Counts of new registrations are used as a measure of entrepreneurship across places and over time (Andrews et al., 2022; Guzman and Stern, 2020; Klapper and Love, 2016), and governments cite them as evidence that their policies work. Our results distinguish the average registration from the registration at the margin of the fee. First, a change in a filing fee should be treated as a break in a series of registrations: at fees of this level, a change of $50 in either direction shifts the series by about one-sixth. Second, a registrar or statistical office that reports new registrations should also report the number that show a first indication of activity within a fixed period, such as a lender's filing or non-dormant accounts, because at the point estimates that number responds to the fee by less than registrations do in every comparison of Table 5. Third, an index built from the characteristics recorded at founding did not register the change in composition: it increased by as much as the unadjusted number of registrations (Section 5). The registry's own measure of maintenance did register it, at a relative rate of 0.44 for a first periodic report; that this rate exceeds those for the lending records is not established (Section 6.5).

Implications for policy. A reduction in a fee of this size is mainly a transfer to filers who would have registered at the regular fee: 87% of the relief in the two holidays went to filings that would have been made at $50 in the same or the adjacent months (Section 5). The relief is a transfer and not an estimate of a welfare cost, which would depend on the value of the additional registrations to their filers and to others. Conversely, the U.K. fee increase reduced new registrations by about one-sixth, and the number of new companies with non-dormant accounts did not change detectably, within an interval that does not exclude a decline of 12%. The analysis published with the 2024 regulations had expected "the impact of a fee in this range on the number of legitimate businesses to be limited" (Department for Business and Trade, 2024). Whether a small fee deters the filings that a registrar most wants to prevent is less clear. In Colorado, bulk filing occurred at $50 before the program and has resumed at $50 since, and the U.K. registrar judged, six months after its first fee increase, that it is "highly likely that this cost is not a barrier to individuals seeking to undertake mass incorporations" (Companies House, 2024c).

Limitations. The evidence comes from two registers and from fees of about $50 or £50, whereas the decisions currently before U.S. states concern fees of $90 to $500; for fees of that size, the only evidence in this paper is the reduction of the Illinois fee from $500 to $150 in 2017, in years in which concurrent changes preclude a clear interpretation (Appendix E.9). The outcomes are limited to activity that leaves a record. Two of the three fee changes altered other fees: the first Colorado holiday reduced every online filing fee, and the U.K. increase raised the fee for the annual confirmation statement. The Colorado inference rests on two episodes and seven degrees of freedom, and the source of the deficit in the months that flank the holidays is unresolved. The U.K. test has one event, four reference years for the outcome, a base period that falls after the announcement, and a confidence set by test inversion whose upper end lies between 0.32 and 0.58 according to what is assumed about the two errors. We wrote the analysis plans with the data in hand, at most a few hours before the tabulations that they govern, and did not deposit them outside the project; the Colorado test was specified after the direction of its result could be anticipated; and the weight that we give to the broader lending record was assigned after the results were known. No person has rerun the code. The links between loans and companies were reviewed by AI readers only, and those of agents' loans by no reader. Before the results are relied upon, a person would need to rerun the code from the archived raw files, recompute the main estimates independently, and audit a stratified sample of the record links and of the sources cited. Section 7.5 records two predictions for the second U.K. fee increase, which others can check when the register files of October 2027 and October 2028 are published.

Conclusion. A fee of $50 or £50 is a small share of the cost of a business and a large share of the cost of a registration. Changes in such a fee changed registrations by about one-sixth, and the registrations that responded were less likely than other registrations to appear in lending records in Colorado and in non-dormant accounts in the United Kingdom; for the PPP loan considered alone, this conclusion depends on a choice of reference period that the data do not settle. The findings caution against reading a change in registrations that follows a fee change one-for-one as a change in the number of operating businesses, and against presuming that evidence on the regulation of entry (Djankov et al., 2002; Klapper et al., 2006) extends to a filing fee of this size. They do not establish that the registrations that respond to such a fee lack value to their filers or to others.

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Online Appendix

A. Conceptual Framework: Formal Statements and Proofs

Section 3 presents the framework in brief. This appendix specifies the model, states each result together with the conditions that it requires, and provides the proofs. It also identifies the statements on which the paper does not rely. The main text uses two results, the upper bounds, or ceilings, of Proposition 1 and the accounting identity of Proposition 2; the ordering of Proposition 3 enters only through Table A1. Section A.6 presents two extensions, on sunk set-up costs and on formation services. They were developed to explain the filings that persist after a long fee reduction, and the paper does not rely on them, because the sharpest of their pre-specified implications failed when tabulated, a second held but does not discriminate, and the other checks specified for the extensions at the same time failed or were uninformative. The mathematics is not new: the results consist of an argument from revealed preference, Markov's inequality, an accounting identity, a preservation result for stochastic orders, and the deterministic case of a model of sunk entry costs. We use them to determine what can and cannot be inferred from each estimate in the paper.

The statements and proofs were checked by AI reviewers in three rounds, and each correction appears in the statement to which it applies; the research log records them. A script in the replication files (`code/60_theory_checks.py`) checks Lemma 1 and Proposition 1 against direct computation on random cases and Proposition 3 on 77 fixed combinations of distributions for the shock and for costs, enumerates set-up dates for the remark on the length of a fee reduction, and produces the numerical examples below. For Propositions 4 and 5, the script only evaluates the closed forms; independent checks of these two propositions by enumeration are in the replication files with the reviewers' reports.

A.1. Actors and Entities

An actor a (a person, a household that owns property, an operating firm, a service that files for clients) can hold $n \in \{0, 1, \ldots, \bar{n}_a\}$ entities, worth $\sum_{k \le n} v_a(k)$ before costs, with $v_a(1) \ge v_a(2) \ge \cdots \ge 0$. The term $v_a(k)$ is the incremental value of the kth entity, that is, the value of the separate entity and not the value of the activity placed in it. Actors are risk neutral, and $v_a(k) = \delta_a \mathbb{E}[\max\{X_{ak}, 0\}]$, where X_{ak} is a payoff that the actor can decline at no cost and $\delta_a \in (0, 1]$ discounts it to the day of filing. Each entity costs the fee F and a non-price cost $c_a \ge 0$. An actor that has never filed also bears a set-up cost $h_a \ge 0$, incurred once, and observes all of its potential entities before paying it. Values and costs do not depend on the fee, and ties are broken in favor of filing.

Lemma 1. *Let $\hat{n}_a(F) = \#\{k : v_a(k) \ge F + c_a\}$ and $S_a(F) = \sum_k \max\{v_a(k) - F - c_a, 0\}$. The actor files $\hat{n}_a(F)$ entities if $S_a(F) \ge h_a$ and none otherwise.*

Proof. Because $v_a(k)$ is non-increasing in k, so is the net gain $v_a(k) - F - c_a$ from the kth entity. Among portfolios with $n \ge 1$ the objective $\sum_{k \le n}[v_a(k) - F - c_a] - h_a$ is therefore maximized by taking every k with $v_a(k) \ge F + c_a$, that is $n = \hat{n}_a(F)$, provided $\hat{n}_a(F) \ge 1$, and its value is then $S_a(F) - h_a$. Not filing is worth zero. If $\hat{n}_a(F) = 0$ then $S_a(F) = 0$ and every $n \ge 1$ gives less than zero, because $v_a(1) < F + c_a$; the actor files none. Both $\hat{n}_a$ and S_a are non-increasing in F, and S_a is continuous. □

The condition that involves the fee is therefore applied to each entity separately, whereas the condition that involves the set-up cost is applied once, to the surplus of the entire portfolio. A fee reduction from F_0 to $F_1 < F_0$ changes the number of filings by

$$\Delta N = \sum_{a:\, S_a(F_0) \ge h_a} \left[\hat{n}_a(F_1) - \hat{n}_a(F_0)\right] + \sum_{a:\, S_a(F_0) < h_a \le S_a(F_1)} \hat{n}_a(F_1). \tag{7}$$

The first sum is taken over actors that are set up at the initial fee, and the second over actors that the fee reduction induces to set up. An actor with no set-up cost and no entity worth at least the initial fee plus the non-price cost per entity belongs to the first sum although it files, if at all, only at the lower fee. If $v_a(\cdot)$ declines steeply, an actor adds one or two entities; if it is flat up to a capacity $\bar{n}_a$, the actor files nothing above a threshold fee and $\bar{n}_a$ entities below it.

A.2. Upper Bounds on the Value of Marginal Entities

Proposition 1 (Ceilings by margin)**.** *Let the fee fall from F_0 to F_1.*

(a) An actor with $S_a(F_0) \geq h_a$ adds exactly its entities with $F_1 + c_a \leq v_a(k) < F_0 + c_a$. For each of these marginal entities and every $w > 0$, $\Pr(X_{ak} \geq w) < (F_0 + c_a)/(\delta_a w)$.

(b) An actor with $S_a(F_0) < h_a \leq S_a(F_1)$ files $n = \hat{n}_a(F_1) \geq 1$ entities whose mean incremental value $\bar{v}_a$ satisfies $F_1 + c_a + h_a/n \leq \bar{v}_a < F_0 + c_a + h_a/n$. For every $w > 0$, the mean over those entities of $\Pr(X_{ak} \geq w)$ *is below* $(F_0 + c_a + h_a/n)/(\delta_a w)$.

Proof. (a) Since S_a is non-increasing, $S_a(F_1) \geq S_a(F_0) \geq h_a$, so by Lemma 1 the actor files at fee F exactly the entities with $v_a(k) \geq F + c_a$, for $F = F_0$ and for $F = F_1$. The entities filed at F_1 and not at F_0 are those with $F_1 + c_a \leq v_a(k) < F_0 + c_a$. For $w > 0$ we have $w\mathbf{1}\{X \geq w\} \leq \max\{X, 0\}$, so

$$\Pr(X_{ak} \geq w) \;\leq\; \frac{\mathbb{E}[\max\{X_{ak}, 0\}]}{w} \;=\; \frac{v_a(k)}{\delta_a w} \;<\; \frac{F_0 + c_a}{\delta_a w}.$$

(b) Let $n = \hat{n}_a(F_1)$; $n \geq 1$ because $S_a(F_1) \geq h_a > S_a(F_0) \geq 0$. The terms of $S_a(F_1)$ are zero for $k > n$, so $h_a \leq S_a(F_1) = \sum_{k \leq n}[v_a(k) - F_1 - c_a]$, which is the lower bound on $\bar{v}_a$. Each term of $S_a(F_0)$ is non-negative and no smaller than $v_a(k) - F_0 - c_a$, so $\sum_{k \leq n}[v_a(k) - F_0 - c_a] \leq S_a(F_0) < h_a$, which is the upper bound. Averaging the first inequality of the display over $k \leq n$ gives the bound on the mean probability. □

Limitations of the ceilings. Part (a) does not involve the set-up cost. It does involve the cost per entity and the discount factor, which, like the set-up cost, are not observed. With c_a = \$10 and no discounting, a marginal entity filed at \$1 by an actor that is set up at the initial fee has a probability below 6% of being worth at least \$1,000 as a separate entity, and for an owner who files through a lawyer at a cost of \$200 per entity the probability is below 25%. Part (b) is weaker still, because it bounds a mean and no single entity, and because the bound includes the set-up cost per entity, which is not observed and may be large. Five further limitations apply to both parts. (i) The bounds are ceilings and not predictions. They are at least one, and are therefore uninformative, when $w \leq (F_0 + c_a)/\delta_a$. For an act that costs less than the initial fee, such as a \$10 report, they are vacuous whatever the actor's costs and discount factor, and they depend on the level of the initial fee and not on the size of the reduction. (ii) The argument relies on the option to decline. If declining costs $d > 0$, the value is $\delta_a \mathbb{E}[\max\{X_{ak}, -d\}]$ and the same step gives the weaker bound $[(F_0 + c_a)/\delta_a + d]/(w + d)$. If holding the entity until its payoff is known costs d whether or not the payoff is taken, that bound holds for the payoff net of the cost, and for the payoff gross of the cost the bound is $[(F_0 + c_a)/\delta_a + d]/w$. (iii) The probabilities are those held by the actor on the day of filing. If the actor's decision is based on perceived values $\tilde{v} \geq v$ (optimism about the level of payoffs), then $v \leq \tilde{v} < F_0 + c_a$ and the bounds hold for the true probabilities. If the actor underestimates dispersion, $\tilde{v}$ can be below v and the bounds can fail. For example, if X is normal with mean -500 and standard deviation 2,000 but is believed to have standard deviation 600, the perceived value is \$68, which lies between $F_1 + c_a$ and $F_0 + c_a$ when c_a = \$50; the true value is \$573 and the true probability that $X \geq 1{,}000$ is 0.23, compared with a bound of 0.10. Under risk aversion the decision depends on a certainty equivalent that can be far below $\mathbb{E}[\max\{X, 0\}]$, and the bounds can fail in the same way. (iv) For a group of marginal entities, the mean of their ceilings bounds the *expected* share that reaches w; a realized share can exceed it. (v) Both parts bound only the value of the separate entity. An owner to whom a separate company for a second building is worth \$30 holds both buildings in one company when the fee is \$50, places the second building in a company of its own when the fee is \$1, and may borrow against it. None of these results bounds the number of ventures with large payoffs that a fee deters, and for that reason the paper addresses that question empirically.

A.3. Registrations and Activity

Proposition 2 (Dilution)**.** *Let an actor's activities give rise to A_j acts of type j (an application for an Employer Identification Number, a payroll, a secured loan), and suppose that A_j does not depend on the number of entities among which the activities are divided. If a fee reduction moves the actor from $n_0 \geq 1$ to $n_1 > n_0$ entities, then additional acts*

of type j per additional entity are zero, and the actor's acts per entity fall from A_j/n_0 to A_j/n_1, whatever the activities are worth. An act that every entity owes for itself is the exception: if the inframarginal entities, which would have existed at either fee, are maintained as before, the number of periodic reports rises by the number of marginal entities that are maintained, which lies between zero and $n_1 - n_0$.

Proof. Total acts of type j are A_j at both fees, so their change is zero while the number of entities changes by $n_1 - n_0 > 0$; acts per entity are A_j/n_0 before and A_j/n_1 after. Each entity that is maintained owes one report, so with the inframarginal entities maintained as before, the number of reports changes by the number of marginal entities that are maintained. □

The proposition is an identity about an actor's total number of acts over all of its entities. This feature has two implications for the application of the proposition, one for rates and the other for numbers of companies. The rates in this paper are computed over the companies formed in a period. The proposition implies a decline in such a rate only if the act remains with the inframarginal company. If the act moves to the marginal company, as when the owner in the example above borrows against the second building, the rate among new companies can rise, and so can the number of new companies with the act. The identity holds constant the actor's number of acts over all of its entities, old and new. The number of companies formed in a period that show evidence of business activity is therefore not an invariant of the model. We report this number alongside every rate for two reasons. First, marginal companies with no activity leave the number unchanged, whereas they lower a rate. Second, acts that move to a marginal company, or are divided among several companies, raise the number after a fee reduction and lower it after a fee increase, which works against a finding that the number did not change. The proposition also has no implication for the share of companies whose registration is maintained, which is the outcome that the registry itself measures.

A.4. Selection

Let payoffs take the form $X = \theta\varepsilon$, where the scale $\theta > 0$ is known to the actor and the shock $\varepsilon \geq 0$ is independent of scale and of costs, with finite mean m and $\Pr(\varepsilon > 0) > 0$; then $v = \delta m\theta$. Suppose that an act is performed when $X \geq w$. Write $\rho(w) = \Pr_M(X \geq w)/\Pr_0(X \geq w)$ for the marginal entities (M), which are added by the fee reduction, relative to the inframarginal entities (0), which are filed at either fee, wherever the denominator is positive.

Proposition 3 (Selection)**.**

(a) Marginal entities of continuing actors. *Among actors that are set up at the initial fee and share a per-entity cost c and a discount factor, every marginal entity has a smaller scale than every inframarginal entity. Hence $\rho(w) \leq 1$ at every w, whatever the distributions of scale and of the shock. If $\Pr(\varepsilon \geq z)$ is log-concave in $\log z$, ρ is non-increasing in w.*

(b) First-time founders at the margin of the price. *Let each founder have one project, a total non-price cost $\kappa = c + h$ and the same discount factor, let $x(\theta) = \delta m\theta - F_0$ and $\Delta = F_0 - F_1$, and suppose that for all $\theta < \theta'$*

$$\Pr\big(x < \kappa \leq x + \Delta \mid \theta\big)\Pr\big(\kappa \leq x' \mid \theta'\big) \;\geq\; \Pr\big(x' < \kappa \leq x' + \Delta \mid \theta'\big)\Pr\big(\kappa \leq x \mid \theta\big), \tag{8}$$

with $x = x(\theta)$ and $x' = x(\theta')$; that is, relative to inframarginal entrants, marginal entrants are at least as common at smaller scales as at larger scales. Then the two conclusions of (a) hold. Condition (8) *holds if κ is independent of θ with a log-concave distribution function, which includes a common cost.*

(c) Founders attracted by publicity. *Suppose instead that publicity removes the non-price cost of a random subset of those who had not entered, that founders share a discount factor, and that κ is independent of θ. Whatever the distribution of κ, the entrants so added satisfy the two conclusions of (a).*

Proof. Let P be the distribution of scale among potential entities, $\bar{Q}(z) = \Pr(\varepsilon \geq z)$ and $K(w, \theta) = \Pr(\theta\varepsilon \geq w) = \bar{Q}(w/\theta)$. Write $\lambda_M(\theta)$ and $\lambda_0(\theta)$ for functions proportional to the densities, with respect to P, of scale among marginal and among inframarginal entities, and $S_g(w) = \int K(w, \theta)\lambda_g(\theta)\, dP(\theta)$, so that $\rho(w) = [S_M(w)/S_M(0^+)] \,/\, [S_0(w)/S_0(0^+)]$.

Step 1 (ordering of scale). In each of the three cases, for all $\theta < \theta'$,

$$\lambda_M(\theta)\lambda_0(\theta') \;\geq\; \lambda_M(\theta')\lambda_0(\theta). \tag{9}$$

(a) An entity of a continuing actor is marginal if $F_1 + c \leq \delta m\theta < F_0 + c$ and is filed at either fee if $\delta m\theta \geq F_0 + c$. If $\delta m\theta' \geq F_0 + c$ then $\lambda_M(\theta') = 0$; otherwise $\delta m\theta < F_0 + c$ and $\lambda_0(\theta) = 0$. Either way the right side of (9) is zero. (b) A founder with one project enters at fee F if $\delta m\theta \geq F + \kappa$. Those who enter at either fee have $\kappa \leq x(\theta)$ and those added by the fee reduction have $x(\theta) < \kappa \leq x(\theta) + \Delta$, so $\lambda_0(\theta) = \Pr(\kappa \leq x \mid \theta)$ and $\lambda_M(\theta) = \Pr(x < \kappa \leq x + \Delta \mid \theta)$, and (9) is condition (8). If κ is independent of θ with distribution function H, (8) reads $[H(x+\Delta) - H(x)]H(x') \geq [H(x'+\Delta) - H(x')]H(x)$ for $x < x'$, that is $H(x+\Delta)H(x') \geq H(x'+\Delta)H(x)$. This holds trivially if $H(x) = 0$, and otherwise because the increments of the concave function $\log H$ over a step of length Δ are non-increasing. (c) Given θ, a founder had not entered with probability $1 - H(x(\theta))$; one whose non-price cost is removed enters at the fee $F' \in \{F_1, F_0\}$ then in force if $\delta m\theta \geq F'$. So $\lambda_M(\theta) = \phi[1 - H(x(\theta))]\mathbf{1}\{\delta m\theta \geq F'\}$, where ϕ is the share reached, and $\lambda_0(\theta) = H(x(\theta))$. If $\lambda_0(\theta) = 0$ the right side of (9) is zero. If $\lambda_0(\theta) > 0$ then $x(\theta) \geq 0$ because $\kappa \geq 0$, so $\delta m\theta \geq F_0 \geq F'$ and the indicator equals one at θ and at $\theta' > \theta$; (9) is then $[1 - H(x)]H(x') \geq [1 - H(x')]H(x)$, that is $H(x') \geq H(x)$, which holds for every distribution function.

Step 2 ($\rho \leq 1$). Inequality (9) is the likelihood-ratio order, which implies that scale among marginal entities is smaller than among inframarginal entities in the usual stochastic order. For every w, $K(w, \theta)$ is non-decreasing in θ, whatever the distribution of the shock. Hence the probability of reaching w is no larger among marginal entities than among inframarginal entities. No condition on the shock has been used.

Step 3 (monotonicity). Let $\psi(u) = \log \bar{Q}(e^u)$ be concave. For $w < w'$ and $\theta < \theta'$ put $a = \log w$, $a' = \log w'$, $b = \log \theta$, $b' = \log \theta'$. The points $a - b$ and $a' - b'$ lie between $a - b'$ and $a' - b$ and have the same sum, so concavity gives $\psi(a - b) + \psi(a' - b') \geq \psi(a - b') + \psi(a' - b)$, that is

$$K(w, \theta)K(w', \theta') \;\geq\; K(w, \theta')K(w', \theta),$$

which also covers $K = 0$. By the basic composition formula (Karlin, 1968),

$$S_M(w)S_0(w') - S_M(w')S_0(w) = \iint_{\theta < \theta'} \big[K(w, \theta)K(w', \theta') - K(w', \theta)K(w, \theta')\big]\big[\lambda_M(\theta)\lambda_0(\theta') - \lambda_M(\theta')\lambda_0(\theta)\big]\, dP(\theta)\, dP(\theta'),$$

because the first bracket is antisymmetric in (θ, θ') and vanishes on the diagonal. Both brackets are non-negative, so $S_M(w)/S_0(w) \geq S_M(w')/S_0(w')$ wherever $S_0(w') > 0$: ρ is non-increasing. □

Relation to known results. The proposition is a standard preservation result and is not new. Step 3 is the closure of the hazard-rate order under mixtures (Shaked and Shanthikumar, 2007, Theorem 1.B.14), and the sufficient condition in part (b), a log-concave distribution function, is a decreasing reversed hazard rate (Bagnoli and Bergstrom, 2005). The condition on the shock, that ψ be concave, states that the generalized failure rate $g(z) = zq(z)/\bar{Q}(z)$ is non-decreasing, where q is the density of the shock, because $\psi'(\log z) = -g(z)$ (Lariviere, 2006; Lariviere and Porteus, 2001). It holds for a lognormal shock; for a Weibull shock with shape k and scale λ, for which $g(z) = k(z/\lambda)^k$; for a gamma shock with shape a and unit scale, for which $1/g(z) = \int_1^\infty u^{a-1}e^{-z(u-1)}\, du$ decreases in z; and, as the boundary case, for a Pareto shock. Mixtures of such distributions need not satisfy it. Condition (8) is stronger than the conclusions require. The first conclusion requires only that scale be smaller among marginal entities in the usual stochastic order, and the second requires only that it be smaller in the hazard-rate order (Shaked and Shanthikumar, 2007, Theorem 1.B.14). The inequalities are weak and can hold with equality at every w: if $\log H$ is linear over the relevant range, marginal entrants are a random draw with respect to scale.

Conditions required for the result. Part (a) requires no assumption about set-up costs or about the distribution of

scale. It does require that the actors compared share a per-entity cost and a discount factor, as the entities of one actor do, and that the distribution of the shock not vary with scale. Each of these conditions is needed for the result. A difference in discount factors has the same effect as a difference in per-entity costs, because both enter only through the threshold $(F + c)/(\delta m)$ that scale must reach for an entity to be filed. The replication files contain an example with two types of continuing actor, one that files for itself at no cost and one that files through a lawyer at a cost of $200 per entity, in which ρ exceeds one over a range of stakes (the payoff that an act requires) although it is below one within each type, and an example with a common cost in which small entities are safe and large ones risky, in which ρ exceeds one at the stakes of a $10 report. The types of filer used in the paper are also broader than the classes of the proposition. Filings of repeat filers are filings whose agent or address had another filing within the previous year, and they include second companies of people who first filed at the low fee, to whom part (a) does not apply. For these filings, as for those of new filers, the sign of selection is an empirical finding and not an implication of the framework. The version of part (a) that requires no assumption on costs compares the companies that one actor forms at the two fees; Section E.7 reports this comparison.

Condition (8) can fail in cases of practical relevance, and the conclusions of part (b) can fail with it. If founders are either attentive, with κ near zero, or inattentive, with κ large, the fee deters inattentive founders of large projects and ρ can exceed one. With $\kappa \in \{0,\ 1{,}000\}$ in equal proportions and independent of scale, values equal to $60 plus $200 times a standard lognormal variable (so that no potential founder has a value below the initial fee), and a standard lognormal shock, ρ reaches 3.2 at stakes near $1.4 thousand. The height of that peak depends on the distribution of values. The same result obtains if non-price costs rise more than one for one with scale (an example is in the replication files). The second conclusion, that the proportional deficit among marginal entities is non-decreasing in the payoff that an outcome requires, can fail when payoffs take the form of a small probability of a large success, which is how the returns to new ventures are usually described. With values that are lognormal with median $100 and a log standard deviation of 1.5, a shock that is exponential with mean 1 with probability nine in ten and with mean 1,000 otherwise, and exponential non-price costs with mean $50, ρ rises from 0.35 at $w = 1$ to 0.78 at $w = 100$ before falling to 0.24 at $w = 1{,}000$. A comparison of ρ across outcomes is therefore a joint test of selection and of the shape of payoffs, and the paper reports the ratio for several outcomes without treating their order as a test of selection. If, in addition, an act of type j is needed only with a probability π_{jg} that differs between the groups and is independent of the payoff within each, the observed ratio is $(\pi_{jM}/\pi_{j0})\,\rho(w_j)$; for example, an owner's second company owes a report of its own but may need no Employer Identification Number of its own.

Part (c) requires non-price costs to be unrelated to scale. Under that condition, those who stayed out are negatively selected on value, so publicity that reaches them at random attracts projects that are no larger, as a lower price does under condition (8). The pattern that the plan treated as evidence of screening by price therefore does not distinguish a price from publicity about it.

A.5. Timing

Remark (timing). Suppose that filers know the fee in force and cannot retime a filing. By Lemma 1, given which actors are set up, who files depends on the fee in force alone. A difference between companies formed at F_1 and companies formed at F_0 that is due to who files at each price therefore begins with the first company formed at F_1 and ends with the last. A difference that persists among companies formed at F_0 after the fee has been reinstated is not selection on the price that those companies paid. It may still be a consequence of the fee reduction, through actors that the reduction induced to set up (Section A.6).

The remark is narrow in three respects. First, publicity about a price has the same dates as the price, so timing separates an effect on the companies formed at the low fee, through either channel, from a change that does not coincide with the fee; it does not separate the price from publicity about it. Second, filers can retime a filing: filings in the month after each fee holiday were below the level of the reference period, and net of the month before and the month after, 80% and 95% of the holidays' additional filings remain. Third, in the static model every response begins on the first day, whereas the response of ordinary filers to the 2022–23 fee-reduction program increased gradually over the course of the program and its largest bulk filer arrived in the fifth month; this gradual increase is outside the model. Two further

channels through the price were examined without the checks having been specified in advance. (i) If a lower fee led founders to register an idea at an earlier stage, as a lower trigger does in models of investment under uncertainty (Dixit and Pindyck, 1994), filings would fall below their initial level when the fee was reinstated and would recover later. The deficits in filings after the holidays bound this channel at about one-fifth of the holidays' increase in filings, there was no deficit after the program, and the check has no power if those who registered early would have reached the original trigger slowly. (ii) If the fee is the cost of replacing an entity, then while a new company cost $1 and a report $10, allowing a company to lapse and forming it again was the less costly course. The share of new filings that took the name of a lapsed company was 6.6% in the year before the program, 6.6% during it and 6.7% after it; this check detects only replacement under the same name. A second check uses the periodic reports of established companies, which would have been filed less often during the program if owners had allowed companies to lapse in order to form them again. Among the established companies used for the adjustment (Appendix C), the on-time rate of reports that fell due in the months of the program was 1.5 percentage points below the mean of the same months in the two adjacent years. This difference is the second lowest among 13 years and 1.3 times the root mean square of the same difference in the other years. Much of it arises in February 2023: the mean monthly difference is 1.4 percentage points over the eleven months of the program and 0.6 when that month is excluded. The second check therefore neither establishes nor excludes replacement under a different name [E].

A.6. Two Extensions Not Supported by the Evidence

The two propositions of this section are correct under their assumptions. As explanations of the filings that persisted after the program, however, they failed the one pre-specified test that had power.

Sunk set-up costs. If every actor draws its entities once, filings return to their initial level when the fee is reinstated. A sunk set-up cost can change this result (Dixit, 1989), as sunk costs of entry do for exporters after a temporary movement of the exchange rate (Baldwin and Krugman, 1989). Let time be discrete with discount factor $\beta \in (0, 1)$. In each period an actor that is set up receives a new draw of potential entities; $\hat{n}_a(F)$ is now the expected number it files in a period at fee F and $s_a(F)$ its expected surplus in that period, both non-increasing in F. The set-up cost is paid once and cannot be recovered, and the decision to pay it is based on expected surplus before the period's draw is observed. No actor exits, a set-up never lapses, and a draw cannot be shifted from one period to another. The fee is F_0 in every period but one, in which it is $F_1 < F_0$; the reduction is not anticipated, and during the reduction the fee is known to revert.

Proposition 4 (Hysteresis)**.** *The fee reduction sets up the actors in* $\mathcal{A} = \{a : s_a(F_0)/(1-\beta) < h_a \leq s_a(F_1) + \beta s_a(F_0)/(1-\beta)\}$*. Actors that were set up before the reduction file after it as they did before it. In every period after the fee is reinstated, expected filings exceed their level before the reduction by* $\sum_{a\in\mathcal{A}} \hat{n}_a(F_0)$*, all of it from actors that were not set up before the reduction; the decrease when the fee is reinstated is smaller, by the same amount, than the increase when the fee was reduced, and in no period are expected filings below their initial level. Each of these later filings is worth at least* $F_0 + c_a$ *to its actor. An actor with* $\hat{n}_a(F_0) = 0$ *adds nothing to the excess.*

Proof. Before the reduction the fee is expected to stay at F_0, so setting up is worth $s_a(F_0)/(1-\beta) - h_a$ and the actors set up are those with $h_a \leq s_a(F_0)/(1-\beta)$. For any other actor, setting up in the period of the reduction is worth $s_a(F_1) + \beta s_a(F_0)/(1-\beta) - h_a$, and setting up in any later period is worth $s_a(F_0)/(1-\beta) - h_a < 0$; it therefore sets up during the reduction if and only if it belongs to $\mathcal{A}$. Once paid, h_a is sunk, and by Lemma 1 with a set-up cost of zero an actor in $\mathcal{A}$ files in each later period the entities worth at least $F_0 + c_a$, an expected $\hat{n}_a(F_0)$ of them. Actors set up before the reduction file as they did before it, and all other actors file nothing, as before. Writing $\mathcal{B}$ for the actors set up before the reduction, expected filings are $\sum_{\mathcal{B}} \hat{n}_a(F_0)$ before, $\sum_{\mathcal{B}\cup\mathcal{A}} \hat{n}_a(F_1)$ during and $\sum_{\mathcal{B}\cup\mathcal{A}} \hat{n}_a(F_0)$ after, which gives the statements about levels. □

The proposition depends materially on its assumptions. A founder with a single project is not an actor of this section, because such a founder draws once, files during the reduction or not at all, and adds nothing afterward. If set-up can be decided after a period's draw is observed, as in Lemma 1, an actor outside $\mathcal{B}$ sets up at the initial fee whenever a draw is sufficiently favorable, say with probability p_0 per period compared with $p_1 > p_0$ during the reduction; with

an exit hazard η that applies to an actor whether or not it is set up, the excess that a one-period reduction leaves after t periods is then $(p_1 - p_0)[(1-\eta)(1-p_0)]^t$ of such actors and decays to zero. A permanent level shift is the case $p_0 = \eta = 0$. Finally, if draws can be shifted across periods, filings can fall below their initial level after the fee is reinstated, as they did in the month after each holiday.

Remark (length of the fee reduction). Let the fee be F_1 for $L \geq 1$ consecutive periods and F_0 otherwise, with L known once the reduction begins. For $0 \leq \ell \leq L$ put $\sigma_a(\ell) = [(1-\beta^\ell)s_a(F_1) + \beta^\ell s_a(F_0)]/(1-\beta)$, the value of being set up with ℓ periods of the reduction remaining; it is non-decreasing in ℓ because $s_a(F_1) \geq s_a(F_0)$. For an actor not set up before the reduction, setting up when j periods of it have passed is worth $\sigma_a(L-j) - h_a$ at that date, and setting up after it is worth $\sigma_a(0) - h_a < 0$. Since $\sigma_a(L) = s_a(F_1)(1-\beta^j)/(1-\beta) + \beta^j \sigma_a(L-j)$, setting up at once rather than j periods later gains $(1-\beta^j)[s_a(F_1)/(1-\beta) - h_a]$ in present value, which is non-negative whenever $h_a \leq \sigma_a(L)$, because $\sigma_a(L) \leq s_a(F_1)/(1-\beta)$. If $h_a > \sigma_a(L)$, setting up is worth less than zero at every date. The reduction therefore sets up, in its first period, the actors in $\mathcal{A}_L = \{a : s_a(F_0)/(1-\beta) < h_a \leq \sigma_a(L)\}$. These sets are nested and increasing in L, so the level shift $\sum_{a \in \mathcal{A}_L} \hat{n}_a(F_0)$ that remains is non-decreasing in L: a shorter reduction sets up weakly fewer actors and leaves a weakly smaller level shift. With $\beta = 0.9$, of 4,182 simulated actors not set up before the reduction, 892 are set up by a reduction of three periods and 1,402 by a reduction of eleven periods. The remark establishes no more than this weak ordering by the length of the reduction; in particular, the level shift can be large at $L = 1$, and an actor with $\hat{n}_a(F_0) = 0$ leaves nothing at any L. In the model, every actor that sets up does so in the first period, so the late arrival of the program's bulk filings is not evidence for the remark; it is evidence of delay in learning of the reduction, which the model does not include. If the length of the reduction is not known, an actor may also wait to learn whether it lasts, and waiting can then be preferable over a wide range of set-up costs. The remark was added after the results for the two holidays were known.

Formation services. For the client of a formation service the alternative to a company in one state at $50 is often a company in another state, and the location in which companies are formed is known to respond to a jurisdiction's set-up costs and capital requirements (Becht et al., 2008) and to its substantive law (Dammann and Schündeln, 2012). Let an intermediary m serve, each period, D_m identical clients who need an entity and are indifferent to where it is formed. That they need an entity whatever the fee is an assumption, under which demand through the intermediary is inelastic. Forming a client's entity in state j costs the intermediary $T_{mj}(F_j) = F_j + a_{mj} + R_j$: the fee, its own cost of handling a filing there, and the present value of the state's recurring charges. Offering a state at all requires a set-up cost K_{mj}, which is sunk once paid. Let J_m be the states in which m is set up.

Proposition 5 (Routing)**.**

(a) The intermediary forms all D_m entities in the state of J_m with the lowest T_{mj}. As a function of state j's own fee, the volume it receives from m is a step function: D_m when $T_{mj}(F_j)$ is below the cost in every other state of J_m, and zero when it is above the lowest of them.

(b) An entity moved to state j by a reduction in its fee would have been formed elsewhere at that other state's cost. It is not an additional entity for the country, and neither the ceilings of Proposition 1 nor the ordering of Proposition 3 applies to it.

(c) Suppose m is set up in at least one state and not in state j, let T_m^* be the lowest cost among the states in which it is, and let ties be broken in favor of the states used before the reduction. If the fee in j is reduced for one period from F_0 to F_1, then m either does not enter; or enters for the period and leaves entirely, which requires $T_{mj}(F_1) < T_m^* \leq T_{mj}(F_0)$; or enters and stays entirely, which requires $T_{mj}(F_0) < T_m^*$ and

$$\frac{D_m\,[T_m^* - T_{mj}(F_0)]}{1-\beta} \;<\; K_{mj} \;\leq\; D_m\,[T_m^* - T_{mj}(F_1)] + \frac{\beta D_m\,[T_m^* - T_{mj}(F_0)]}{1-\beta}.$$

(d) Suppose m is set up in state j before the reduction. The volume that j receives from m in any period depends on the fee then in force alone, so its volume after the fee is reinstated equals its volume before the reduction. This holds whether or not clients are identical. If they are, and m files in j before the reduction, it already forms all D_m entities

there and the reduction adds nothing: the volume of such a service rises during the reduction only if its clients differ in what each state costs them.

Proof. (a) Clients are identical and indifferent among states, so the intermediary minimizes its cost per client over J_m; state j receives D_m if $T_{mj}(F_j) < T_{mk}$ for every other $k \in J_m$ and nothing if $T_{mj}(F_j) > \min_{k \neq j} T_{mk}$. (b) A client whose entity is moved to j was served at cost $\min_{k \neq j} T_{mk}$ before the reduction; by the assumption that clients need an entity whatever the fee, the entity existed, in another state, at the initial fee. (c) Before the reduction m is not set up in j, so $K_{mj} > D_m[T^*_m - T_{mj}(F_0)]^+/(1-\beta)$. Setting up during the reduction is worth $D_m[T^*_m - T_{mj}(F_1)]^+ + \beta D_m[T^*_m - T_{mj}(F_0)]^+/(1-\beta) - K_{mj}$, and setting up later is worth less than zero. If $T_{mj}(F_1) \geq T^*_m$ this is negative and m does not enter. If $T_{mj}(F_1) < T^*_m \leq T_{mj}(F_0)$, m enters when $K_{mj} \leq D_m[T^*_m - T_{mj}(F_1)]$ and, by (a), routes nothing to j once the fee is reinstated. If $T_{mj}(F_0) < T^*_m$, m enters when K_{mj} is no larger than the right side of the display, and by (a) continues to route all D_m clients to j; the left inequality restates that it was not set up before. (d) With J_m unchanged by the reduction, each client is routed to the state of J_m that is cheapest for that client at the fees in force, which depends on those fees alone. With identical clients, part (a) gives j all D_m clients or none at the initial fee; if all, a lower fee in j cannot add to them. □

Part (b) restates the assumption of inelastic demand and fails without it, because a service that passes the fee on to its clients, or advertises a company for $1, creates clients. Part (a) is a property of identical clients; if clients differ, for instance in how long they will maintain the entity and therefore in what a state's recurring charges cost them, the share routed to a state falls smoothly as its fee increases. For a reduction of L periods the argument of the remark above replaces the right side of the display in part (c) by $D_m[T^*_m - T_{mj}(F_1)](1-\beta^L)/(1-\beta) + \beta^L D_m[T^*_m - T_{mj}(F_0)]/(1-\beta)$.

Evidence on the two extensions. Both extensions imply a persistent increase in filings only through a filer or a service that was not set up in the state before the fee reduction: Proposition 4 states this directly, and Proposition 5(d) implies that a service already set up in the state reverts in full. With identical clients, the latter proposition also implies that such a service, if it files in the state before the reduction, gains nothing from the reduction even while it lasts. The eight formation services already established in Colorado more than doubled their combined filings in the program year (Section 5). The increase itself, and not only the part of it that persisted, is therefore inconsistent with a constant number of identical clients; it suggests clients that differ or clients that a $1 offer created. After the first review, and before the registry had been tabulated by the date on which each filer first appears, we specified what the two extensions imply for the program, namely that the filings that remained after the fee was reinstated come from filers first observed during the fee reduction, and the alternative, namely that they come from intermediaries active before it (Section B.5, item N1). The tabulation supported the alternative. Of the increase that remained, 62% is accounted for by formation services first observed more than two years before the year of the filing, 3% by services first observed in the program year, and 4% by addresses first observed in the program year [A1]; the services first observed before July 2021, taken as a fixed group, account for 58% [E]. When each service is dated only from companies whose agent information has not been changed by a later statement, the first share is 52%; it is 29% when companies covered by a statement that an agent filed for many companies on one day are also excluded, and services first observed in the program year account for 3 and 4% in the two cases (Section E.8) [E]. None of the other checks specified at that time lends support to the extensions. The classification of agents by their own volumes (N2) held as written but does not discriminate, because agents on a rising trend satisfy it in every span of years; the share of filings at an agent's own address (N3) shows no break; the counts of addresses (N4) move in the same direction under both of the interpretations that they were intended to separate; and the rule for linking later bulk and marked filings to the program's filers (N8) is not met over the period as a whole (Section D.7). A second sunk cost, paid per product or per client and not per state (a default state on an order form, a page that advertises a $1 company, clients who return), would generate the pattern observed among the agents. We did not specify this explanation in advance, we cannot distinguish it from clients created by advertising, and the level shift is diminishing over time. The two extensions have therefore passed no test that they could have failed.

A.7. Status of Each Implication

Table A1 lists six types of additional registration that the framework distinguishes, with what the framework implies for each and the evidence. Table A2 lists, for the same six types, each implication used in the paper, the result on which it rests, and the conditions that it requires. D denotes an implication derived from the result named, and C denotes a conjecture that is not implied by the model.

Table A1. Types of Additional Registration: Implications of the Framework and Evidence

Type	Implication of the framework	Evidence
1. New founder at the margin of the price	Enters at the low fee and stops when the regular fee is reinstated. Whether the project is a business is not determined.	Filings by new filers increased during the holidays and then reverted. Their marginal registrations have no more lending records than those of the other types (Section D.2).
2. New founder attracted by publicity	Follows the publicity and not the price (conjecture). Projects are no larger than those of other founders, if costs are unrelated to scale and publicity reaches, at random, those who stayed out.	Not separable from type 1 in Colorado, where each fee reduction was announced. The U.K. fee increase reduced registrations without any publicity campaign (Section 7).
3. Owner adding an entity	Increases at the fee reduction and reverts in full. Marginal entities are maintained no more often than the owner's other entities, and generate no additional acts if the owner's activity is unchanged.	Filings by repeat filers, who had formed a company within the preceding year, increased and then reverted; they account for one-third to two-fifths of the holidays' additional registrations, depending on the address key (Section 5; Section D.5). Within filer, companies formed at $1 filed a first report about as often as the same filers' companies formed in adjacent years (Section E.7).
4. Client of a formation service	A service new to the state may enter and remain if setting up there is a sunk cost. A service that already files in the state gains nothing if its clients are identical; if they differ, its filings can increase discretely, and any increase reverts in full.	Established services more than doubled their combined filings and accounted for 62% of the increase that remained after the program, or 29 to 52% when a service is dated only from companies whose agent information no later statement changed. Both implications for established services fail (Section 5; Section A.6).
5. Bulk filer whose use is worth less than the regular fee	Goes from nothing to capacity when the fee falls below the value per entity net of costs; ends with the low fee.	One filer formed 15,527 companies at $1. This filer began in the fifth month of the program and had almost ceased before the end of the program was announced, and the same address had formed companies in bulk at $50 in 2020. The evidence matches neither "from nothing" nor the implied dates (Section 5; Section D.7).
6. Bulk filer whose use is worth more than the regular fee, including fraud	Insensitive to the fee once set up. Later bulk filings, and filings later marked as unauthorized or fraudulent, are linked to filers first observed during the fee reduction. Appears in entity types whose fee did not change (conjecture).	Bulk filing continued at $50 after the program, among LLCs and corporations. The pre-specified rule for the link to the program's filers was not met over the period as a whole (Section D.7).

Notes. Unmarked clauses in the middle column are derived from the model under the conditions that Table A2 gives for each; a clause marked as a conjecture is not implied by the model. The right-hand column cites the sections that report each finding with its uncertainty. The framework was written after the first results were known, so that the table organizes the evidence and does not report tests of predictions, except where a section cited says so.

Table A2. Status of Each Implication of the Framework

Type	Implication	Status and conditions
1. New founder at the margin of the price	Enters at the low fee and stops when the regular fee is reinstated.	D, Lemma 1, if filers know the fee in force and cannot retime a filing.

Table A2. (continued)

Type	Implication	Status and conditions
	Performs acts that require a given payoff no more often than other founders.	D, Proposition 3(b), if condition (8) holds, founders share a discount factor and payoffs differ only in scale. Otherwise not determined.
	Value of the project.	D, Proposition 1(b): the ceiling includes the set-up cost and is weak when that cost is large.
2. New founder attracted by publicity	Follows the publicity, which need not end when the regular fee is reinstated.	C.
	Projects are no larger than those of other founders.	D, Proposition 3(c), if costs are unrelated to scale, founders share a discount factor and publicity reaches those who stayed out at random.
	Value of the project is not bounded above by the fee.	D, from the condition for entry; Proposition 1(b) need not apply.
3. Owner adding an entity	Increases at the fee reduction and reverts in full when the regular fee is reinstated.	D, Lemma 1, under the conditions of the first row.
	Marginal entities are maintained no more often than the owner's other entities.	D, Proposition 3(a), with a shock whose distribution does not vary with scale. Within one owner it requires no assumption on costs.
	The separate entity is worth less than the regular fee plus the non-price cost per entity (minutes of time for an owner who files directly); no additional acts by the owner.	D, Proposition 1(a); D, Proposition 2, if the owner's activity is unchanged. For the companies of one period it also requires each act to remain with the inframarginal company.
4. Client of a formation service	The filings of a service new to the state increase in a step when the state becomes its cheapest, and then remain in full or revert in full. A service that already files in the state gains nothing if its clients are identical; if they differ, its filings can increase in a step, and any increase reverts in full.	D, Proposition 5(c) for identical clients, and (d).
	The entity is diverted and not created.	Rests on the assumption of inelastic demand (Proposition 5(b)).
	An organization as agent; few services account for the response.	C.
5. Bulk filer whose use is worth less than the regular fee	Goes from nothing to capacity when the fee falls below the value per entity net of costs, including the set-up cost per entity; ends with the low fee.	D, Lemma 1, for a flat schedule of values and under the conditions of the first row.
	Maintains all of its entities or none; performs no act that costs more than the fee and its own costs per entity.	D only if the value per entity is certain and is not discounted. Otherwise only the ceiling of Proposition 1 applies.
6. Bulk filer whose use is worth more than the regular fee, including fraud	Insensitive to the fee once set up; may begin during the fee reduction and continue after it.	D, Proposition 4, under its assumptions.
	Linked to filers first observed during the fee reduction.	D, Proposition 4, as a prediction.
	Appears in entity types whose fee did not change.	C.

B. Pre-specified Analyses, Their Outcomes, and Departures

This appendix reports each provision of the plan and of the four addenda together with its outcome. It also documents what was known when each document was written, lists the departures from the documents that bear on a reported result (Section B.9), and lists the analyses that were not pre-specified (Section B.10). The five documents, the code, the result files, the reports of the reviews with the responses to them, and a research log that records the departures, the

corrections, and the rounds of review are in the project archive.

B.1. The Five Documents

Table A3. The Plan and the Four Addenda

Document	Hash recorded (UTC)	Known when written	Provisions
The plan [P]	Oct. 4, 2026, 18:32	Counts of filings for all years and the outcomes of companies formed before July 2019 had been examined. No outcome of a later cohort had been computed, but five facts had become known unintentionally (Section B.3).	Seven hypotheses about the 2022–23 fee-reduction program (H1 to H7): a primary test with its decision rule and the interpretation of its magnitude, nine secondary estimators, a state-level analysis of business applications, descriptive analyses, and an auxiliary study of the report fee.
Addendum 1 [A1]	Oct. 4, 2026, 23:27	A first draft had been written, and five reports of the first internal review had been read.	Nine further analyses of the program (N1 to N9) and, for most of them, the interpretation of each possible result.
Addendum 2 [A2]	Oct. 5, 2026, 04:11	Every analysis of the program had been run. The two fee holidays had been identified in the press releases of the Secretary of State's office. For the purposes of the addendum, the registry had not been tabulated for the holiday months, although series computed earlier include those months (Section B.3).	Nine analyses of the two holidays (C1 to C9) based on registry outcomes, the interpretation of each result relative to the results for the program, and the implications of the discovery of the holidays for the plan's own test.
Addendum 3 [A3]	Oct. 5, 2026, 10:06	A second draft had been written, and five reports of the second internal review had been read. Loans had been linked to companies. No loan outcome had been computed for companies formed in the months around the holidays, apart from the lapse described in row 8 of Table A6.	Six outcomes constructed from three lending records created by lenders or federal agencies, for the companies formed in the two holidays; the method of inference; and the interpretation of every possible result of the main test.
Addendum 4 [A4]	Oct. 5, 2026, 11:32	Every Colorado result was known, as were monthly totals of U.K. registrations. Nothing had been tabulated by date of incorporation for U.K. companies incorporated from January 2024, which were held in a sealed file.	The test of the U.K. fee increase of May 2024: periods, reference years, five outcomes, seven checks and four interpretations; the same comparison for the fee increase of February 2026; and two predictions for data that do not yet exist.

File	SHA-256 hash
The plan	`7865d3829da809d5041d197a27fa74102a211071504bc7ce33dca9177ad24242`
Addendum 1	`e2a5c95942b1c502ff4ed6da0c5477f999a3d1dd7ed6bbf9678ee7ac63f7d3d2`
Addendum 2	`2e033fcc84ac94679a42993bf2556d341a414f489e0fe002002de680556901e8`
Addendum 3	`30ef3429a277ba076c8986a9183e6658bab871f4f03f3dadcd7270fe2478fcf7`
Addendum 4	`99e319d0cd0a54440f0fb517a15818a1e24da8baa6975e43465e958cec99ceab`

Notes. Each hash was recorded in the project's version history at the time shown in the upper panel; for Addendum 2 the time is that at which the hash was written to the file of hashes, and the commit followed 46 seconds later. The files frozen with each document are listed with their hashes in `prereg/SHA256SUMS.txt`: the counts and shares that the plan specifies, the definitions of the episodes, the configuration files of Addenda 3 and 4, and the scripts and libraries that run Addenda 2 to 4. The files that link loans to companies are hashed in Addendum 3 itself. The build verifies every hash.

None of the five documents was deposited with an external registry. That decision rests with the person for whom this draft was prepared, and a public registration cannot be withdrawn. The evidence on the timing of the documents is therefore internal to the project. The hash of each file was recorded in the project's version history at the time shown in Table A3, before the scripts that depend on it were run, and the history is distributed with the paper. Because a reader who does not trust that history has no independent means of verifying it, we describe the documents as a dated record of our intentions and not as preregistration. Results are marked [P] for what the plan specified, [A1] to [A4] for what an addendum specified before the new tabulations that it governs and after the results that preceded it, and [E] for everything else. The first addendum also specified the form of one analysis for which most of the results had already been seen, as Section B.5 records. No analysis or result under an addendum is described as preregistered.

Terminology. The plan and the addenda were written before the terminology of the paper was settled, and the passages quoted from them in this appendix are reproduced verbatim. Table A4 lists the terms of those documents that differ from the terms of the paper. The difference is one of wording only: each pair refers to the same object, as the document concerned defines it. In the replication files, the scripts and the result files use the terms of the documents.

Table A4. Terms of the Plan and the Addenda and the Corresponding Terms of the Paper

Term in the plan or an addendum	Term in the paper
Added companies (Addendum 3)	Marginal registrations
Marginal entrants, for the filings that a fee reduction adds (the plan)	Marginal registrations
The others; other companies (Addenda 3 and 4)	Inframarginal registrations
Registrations that the rise removed; removed registrations (Addendum 4)	Deterred registrations
Registrations still made (Addendum 4)	Remaining registrations
Additional share (Addendum 3)	Marginal share, s
Payroll loan (Addenda 3 and 4)	PPP loan
Guaranteed loan (Addendum 3)	SBA loan
Financing statement (the plan; Addenda 3 and 4)	UCC financing statement
Announcement month and month after (Addenda 2 and 3)	Flanking months
Share net of the months around (Addenda 2 and 3)	Share net of the flanking months
Months set aside (Addenda 2 and 3)	Held-out months
Touched; untouched, of months, years or runs (Addenda 2 to 4)	Affected; unaffected
Mass filings; the burst rule (the plan; Addenda 1 and 2)	Bulk filings; the rule that identifies bulk groups
Organization agent (Addenda 1 to 3)	Formation service as registered agent
Person agent (Addenda 1 to 3)	Person as registered agent
Additional company, as a type of filer (Addenda 2 and 3)	Repeat filer
First company in twelve months (Addenda 2 and 3)	New filer
Stepped up and stayed; stepped up and reverted; between; no step (Addenda 1 and 2)	Persistent increase; increase, fully reversed; increase, partly reversed; no increase
Kind of filer or of registration (Addenda 1 to 4)	Type
Accounts that are not dormant (Addendum 4)	Non-dormant accounts
Shortfall (the plan; Addenda 2 and 3)	Deficit, decline or difference
Reading of a result (Addenda 1 to 4)	Interpretation
The rise (Addendum 4)	The fee increase
The fee's return (Addenda 1 to 3)	The reinstatement of the $50 fee
The programme (the plan)	The program
Fixed; planned (all five documents)	Specified; pre-specified

B.2. The Pre-specified Tests in Summary

Table A5. The Pre-specified Tests: Interpretations, Outcomes, and What the Paper States

Document and test	Pre-specified interpretation	Outcome	What the paper states
[P] The program: first reports of its cohort compared with the preceding year	Rate among marginal registrations relative to the inframarginal rate: 0.8 or more indicates little screening; below 0.5, strong screening	ρ = 0.22 (−0.02 to 0.46), in the band of strong screening	The paper does not adopt the interpretation, because later cohorts formed at $50 also show a deficit (Section 6.5; row 2 of Table A6)
[A1] Nine further analyses of the program	Which filers account for a persistent increase in filings, and other interpretations	Mixed; the implication for filers new to the state did not hold	Two extensions of the framework are not supported (Section A.6)
[A2] The two holidays: registry outcomes	Which values of ρ for the first periodic report would support selection by the price	Filings increased by 13% and 20%; ρ = 0.44 (−0.10 to 0.74) for the first report	The result is not presented as the addendum prescribes, because it depends on an adjustment whose series had been seen (Section 6.5; row 5)
[A3] The two holidays: lending records, with the PPP loan as the primary outcome	An interval for ρ wholly below one: marginal registrations "were less often businesses with a payroll or an owner's income in 2020-21"; no interpretation for the other five outcomes	PPP loan: ρ = 0.07 (−0.57 to 0.60); second holiday alone, 0.21 (−0.55 to 0.81); new filers, −0.22 (−1.64 to 0.60). Any of the three records: −0.02 (−0.65 to 0.41); second holiday alone, 0.20 (−0.53 to 0.67)	The result is stated for the record and not for businesses (row 13). For the PPP loan it depends on the reference period, with estimates from −0.41 to 0.81; for any of the three records in the two holidays together it does not, with estimates from −0.23 to 0.34 (Section 6.2) [E]
[A4] The U.K. fee increase of 2024: non-dormant accounts	Registrations fall and the share with the outcome rises: "The Colorado finding holds with the sign reversed"; if the interval for ρ lies wholly below one-half, "the paper may say that most of the removed registrations were not businesses"	Both occurred; ρ = 0.00, with a percentile interval over the draws retained from −1.62 to 0.42 and a confidence set by test inversion from −2.5 to 0.42 [E]	The result is stated for the record, and the permitted statement is not made (row 21). The upper end of the set is between 0.32 and 0.58 according to what is assumed about the two errors, and with the earlier base period the estimate is 0.28 (Sections 7.2 and 7.3) [E]
[E] Exploratory	None		Marked where reported (Section B.10)

Notes. Table A3 gives the time at which each document was recorded and what was known when it was written. The entries for [A3] and [A4] are the two tests on which the paper rests; Table 5 reports their estimates under every consequential alternative.

B.3. Information Available When Each Document Was Written

The plan. When the plan was locked, we knew the public record of the 2022–23 fee-reduction program: the totals published by the Secretary of State's office, the lawsuit against the largest bulk filer, and press reports of fraudulent filings. We had counted filings by entity type, day, address and agent in every year, because the rule that identifies bulk groups and the shares of additional filings that the plan specifies are constructed from those counts. The plan accordingly states its first hypothesis, concerning quantity, as known. We had also computed outcomes for companies formed before July 2019. In addition, five facts had become known unintentionally, through tabulations that assisting agents made while scoping the literature and the data. The plan records them: (a) the share of LLCs in good standing was 39.5% for the cohort of 2021–22, 35.5% for the program cohort excluding the largest bulk filer and 43.6% for the cohort of 2023–24; (b) Colorado's business applications from July 2022 to May 2023 were 12.5% above the year before, compared with 2.4% for the median state, and increased by a further 13.3% in the following year; (c) weekday LLC filings excluding the largest bulk filer fell by 21% when the $50 fee was reinstated; (d) filings of corporations fell by 15% and filings of foreign LLCs by 12% during the program; and (e) formations did not bunch before January 1, 2024.

Facts (a) and (b) bear on the direction of H2 and of H4, and neither direction can be regarded as having been predicted blind. Given that knowledge, the elements that the plan specified were the estimator, the comparison, the method of inference and the interpretation of magnitudes.

Addendum 1. When this addendum was locked, every result of the first draft was known. The addendum lists the additional results that had been seen during the first review: the plan's primary estimator applied to the two cohorts formed after the program; on-time rates by quarter and by month of formation; the numbers of new LLCs with and without a first report, by cohort year; the contrast at the reinstatement of the fee for six outcomes and for credit and fraud marks; the decline in filings at the reinstatement, estimated at between 12 and 20 log points under several windows and sets of reference years; the shares of formation services and on-time rates by type of agent; the five largest formation services taken together and a few single services by rank; rates for Connecticut; and one internal reviewer's statements about the data. It also lists quantities that had not been examined, each of which was subsequently tabulated under one of the items N1 to N4 and N6 to N9: filers by the date on which they first appear, agent-by-agent volumes beyond those just named, the share of filings at the agent's own address, counts of addresses, the third-year report, a prediction from recorded characteristics, links from later marked or bulk filings to the program's filers, the dates of the court-dissolved companies of the largest bulk filer outside the program, and every series for Illinois. Neither list mentions the tabulation of N5, the fee reinstatement by type of filer; the decline in filings at the reinstatement, which that tabulation divides by type of filer, had been seen. A sixth report, the replication audit, was read after the lock.

Addendum 2. When this addendum was locked, everything about the program was known. For the holidays, we knew what the Secretary of State's office and the legislature's budget staff had published: the dates and fees, and totals such as 27,640 new entities in the third quarter of 2014, 32.2% above a year earlier, with LLCs 34.9% above. Series that contain the holiday months had been computed earlier for other purposes. The reference values of the plan's primary test for the five pairs of years affected by the holidays had been examined (+1.2, +0.1, −0.1, −0.6 and −0.9 percentage points). The first draft's figure of on-time rates by quarter included the holiday quarters, and nothing in them had been remarked upon; because a deviation of several percentage points could have been visible there, the absence of any remark is weak information against a large effect and should be regarded as known. The comparison was calibrated on months other than those that the addendum held out. In that calibration the months that follow each holiday (April and May 2013; December 2014 to March 2015) were seen, and the estimates for them are labeled accordingly wherever they appear. One further series had been seen (Table A6, row 5). The on-time rate of established companies, which the addendum subtracts from that of new companies to form its primary outcome, had been printed in the calibration for every month in which reports fell due other than November 2012 to March 2013. The months for which it was printed include those in which the first reports of both holiday cohorts fell due, so the amount that the adjustment adds to the estimate could have been read from the printed series before the adjusted outcome was specified as primary.

Addendum 3. When this addendum was locked, everything in the second draft was known, including every registry outcome of the companies formed in the holidays, and five reports of the second review had been read. Those reports had shown that the deficit in first reports depends on the adjustment for established companies, that for other registry outcomes the deficit is present without any adjustment, and that it is absent among companies formed in the first month of each holiday. The expected direction of the loan outcomes was therefore not a blind prediction. From the loan files we had seen numbers of Colorado loans by characteristics of the loan and of the borrower and, from the links, the shares linked by type of business, by program and by year of approval, but never by the date on which the linked company was formed. The calibration used only companies formed outside the months that the addendum holds out, November 2012 to May 2013 and June 2014 to March 2015. It reported the rate of each outcome by calendar year of formation over the remaining months; the number, mean, standard deviation and range of the statistic over reference runs, overall and, for the PPP loan, by type of filer; and, for an SBA loan within 24 months, the contrast at the calendar position of the fee reinstatement in each reference year. A trial run of the analysis script, with the holidays moved forward by 48 months and the held-out months removed, had also been made. The months that serve as the holidays' own reference windows entered those summaries, but no value was printed for them individually. There was one lapse, on an outcome that the addendum does not use, which the addendum itself records (Table A6, row 8). No rate of any loan outcome, and no

count of UCC financing statements beyond 24 months, had been computed for companies formed in the held-out months.

Addendum 4. When this addendum was locked, every Colorado result was known, including those under Addendum 3 and the follow-up of Section 6.2, so the expected direction was that of the Colorado finding with the sign reversed. Monthly totals of U.K. registrations were known from three sources: the registrar's official statistics as quoted in the report of an AI reviewer, the numbers of companies in each monthly file as printed when the files were downloaded, and the same numbers as recorded in the manifest of the download. It was therefore known that registrations fell after the first fee increase. From companies incorporated before 2024 we had computed registrations by month and by week around May 1 of each year, the outcomes by month of incorporation for 2020 to 2023, every statistic of the test at May 1 and at February 1 of each year to 2023, and the dispersion of those statistics across years. From the same companies we had also tabulated the categories of accounts of the cohorts of 2021 to 2023, the correlations used in the inference, registrations on bank holidays and on weekends, the change in the rate of non-dormant accounts when it is measured at later snapshots, and, for companies incorporated from August to December 2023, outcome rates by the number of earlier incorporations at the address, by industry code and by category. Two trial runs of the analysis script, at May 1 and February 1, 2023, when no fee changed, had also been made on these companies. Four measures of the level of registrations and five choices of period were compared by the dispersion of the test statistics across years; none was clearly preferable, and the first measure and the first period tried were retained. The outcome rates by month of incorporation also revealed the approximate result of a test of the fee for a confirmation statement, which increased on the same days. We would otherwise have specified that test, and the addendum therefore does not specify it. Nothing had been seen by day or week around either fee increase, and nothing had been seen about the outcomes or types of companies incorporated from 2024. Those companies were held in a separate file, of which we knew the number of rows and the hash. A first version of the script that built the files had also written, for the sealed days, the highest company number reached on each day; the part of that output for days that were not sealed revealed a fault, the method was replaced, and the sealed part of the output was deleted without being read.

B.4. Provisions and Outcomes: The Plan

H1, quantity (stated as known). The plan recorded that LLC filings from July 1, 2022 to May 21, 2023 were 42.9% above their level on the same dates a year earlier, and it specified no test. *Outcome:* Tables 2, A42 and A16.

H2, the primary test. *Specified:* for ordinary LLCs formed from July 1 to March 31, the change in the on-time rate of first reports from 2021–22 to 2022–23, less the mean change among profit corporations, nonprofit corporations and foreign entities; a standard error equal to the root mean square of the same contrast over eleven earlier pairs of years; a t distribution with eleven degrees of freedom; and the rule that "H2 is supported if the primary RF is negative and significant at 5% on the 11-pair placebo test." *Outcome:* −7.0 percentage points (s.e. 0.98; $t = -7.2$; permutation $p = 0.083$, the smallest attainable with twelve values). H2 is supported. The standard error from sampling variation alone, clustered by week of formation, is 0.82. With the eight pre-pandemic reference pairs, which the plan also named, the standard error is 1.02 and the permutation p-value is 0.111. Table A43, Panel A; Table A44, Panel A; Figure A5.

H2, the interpretation of its magnitude. *Specified:* with s the share of the cohort's filings that were additional, ρ of 0.8 or more is "little screening," 0.5 to 0.8 "moderate" and below 0.5 "strong," and "a category is claimed only if the 95% interval lies inside it." The share was specified as 0.189, based on the change in filings since 2021–22, and as 0.150 if the level of 2023–24 is taken as the trend, with both to be reported. The alternative hypothesis that the program attracted founders similar to other founders "is supported if the 95% interval for ρ lies above 0.8." *Outcome:* $\rho = 0.22$ [−0.02, 0.46] with the first share and 0.01 [−0.29, 0.32] with the second. Both confidence intervals lie below 0.5, so the plan's category is strong screening, and the alternative is not supported. We do not adopt that interpretation, for two reasons (Section E.8). First, the plan treated the share as known; when the uncertainty in the share is included in the draws, the first interval runs from −1.49 to 0.60 and lies inside no category [E]. Second, cohorts formed at $50 in the two following years also show deficits, between about half and three-quarters as large, relative to every comparison group except the plan's own, so that the part of the estimate attributable to the fee cannot

be identified from these cohorts. The bands themselves are a convention and have no basis in the framework.

H3, secured credit ("confirmatory, low power"). *Specified:* the same contrast for the share of ordinary LLCs formed from July 1 to May 21 that appear as debtor on a UCC financing statement within 24 months, with eight reference pairs; and a test that the proportional decline is larger for credit than for reports. The plan added that the test of credit "cannot reject equality even if marginal entrants obtain no secured credit at all (power about one half)," and that H3 "is therefore reported as an estimate with its interval, not as a decisive test." *Outcome:* −1.02 percentage points (s.e. 0.34; $p = 0.016$; permutation $p = 0.111$) on a base of 3.2%. The implied ρ is −0.59 [−1.81, 0.62]. Taken at face value, credit fell by more than a complete absence of credit among marginal registrations could explain, although the confidence interval includes positive ratios. The difference between the proportional declines is −0.150 (s.e. 0.089), with a one-sided p-value of 0.065, as the plan framed the test, 0.131 two-sided, and a permutation p-value of 0.222. The two reasons for which we do not adopt the interpretation of H2 apply here as well: the implied ρ treats the share as known, and the part of the estimate attributable to the fee cannot be identified from these cohorts. Table A44, Panel B.

Secondary estimators 1 to 9 ("all reported, whatever they show"). *Outcome:* Table A44. The seven estimators that retain the comparison of cohort years (each comparison type alone; no comparison type; other definitions of the report and of the window; all filings and other thresholds for bulk groups; a control from established LLCs; the two years before as the reference period; no delayed effective dates) give declines between 6.2 and 9.3 percentage points. All differ from zero at the 5% level except the simple change from the year before, which uses no comparison type ($p = 0.08$). The two estimators that use timing within the program year do not agree with these estimates in magnitude. The weekly dose-response among ordinary filings implies a gap between marginal and inframarginal registrations of −0.076 (s.e. 0.133), compared with −0.372 from the primary estimate, and the contrast at the reinstatement of the fee, scaled by the constant of 0.097 that the plan specified, implies −0.165 (0.127). Neither differs from zero, and they differ from the primary estimate by 2.2 and 1.6 of their own standard errors. The disagreement was the first indication, within the plan's own estimators, of the conclusion reached in Section E.8.

H4, business applications. *Specified:* synthetic difference-in-differences on log monthly business applications, comparing Colorado with 46 states, with January 2015 to June 2022 as the pre-period and July 2022 to May 2023 as the post-period, and with inference from the same estimate for each comparison state; the ratio θ of additional applications to additional LLCs, "well below one" if the upper end of its confidence interval for all filings is below 0.5; and a gradient in stakes, supported if the estimates for business applications with planned wages and for business formations within four quarters are each smaller than the estimate for all applications and the latter is positive and significant. *Outcome:* 0.098 (s.e. 0.035; permutation $p = 0.021$) for all applications, and $\theta = 0.23$ [0.06, 0.38], or 0.39 [0.11, 0.65] per additional ordinary LLC. Both rules are met as written. Because fact (b) above was known, we report these estimates as measurements. Three qualifications apply. First, the plan's interval for θ reflects the error of the numerator alone; when the error in the number of additional filings is included, the interval runs from 0.06 to 0.53, and its upper end then exceeds the plan's threshold [E]. Second, with standard difference-in-differences or synthetic control, which the plan required in addition to the primary estimator, the estimate for all applications is 0.038 (0.086) or 0.086 (0.070), so the finding that applications increased depends on the estimator that the plan named as primary. Finally, the increase persisted after the fee was reinstated (Section E.9). The weekly comparison across the reinstatement of the fee, with placebo years 2018, 2019, 2022 and 2024, gives −0.006 (0.029) over eight weeks on either side; one of those placebo years contains days at $1 in its later weeks, and the version with four weeks on either side, which contains none, gives −0.017 (0.027) [E]. The plan also required the same estimates for the following year and estimates for California and Montana, which Table A52 reports. Section E.9; Table A51.

H5, characteristics of additional filers (descriptive). *Specified:* six characteristics of ordinary filings, with the share among additional filings inferred from the change in each share, and with earlier year-to-year changes as reference values. *Outcome:* Table A48. Additional filings were more likely than other filings to come from ZIP codes in the lowest third of income and from addresses with several new LLCs in the year. Two further differences, a higher share

of filings through a formation service and a lower share of names with a property word, do not hold among filings with no later change of agent or in the comparison with the year after the program, and we make neither claim. The fields are the registry's values in October 2026 (Table A6, row 1).

H6, bulk filings. *Specified:* counts and outcome rates of filings in bulk groups, the number of distinct addresses and names that meet the rule that identifies them, and the share of the appropriation that these filings absorbed. *Outcome:* 19,789 filings under 22 addresses and names (five principal addresses, ten agent addresses and seven agent names), which reduce to nine linked groups; 78.1% were dissolved by court order, 0.16% carry a statement of correction and 0.46% carry a correction by the filing officer. At $49 per filing, these filings account for 11.5% of the appropriation, and the largest group alone accounts for 9.0%. Table A47; Section 5; Section D.7.

H7, other entity types (exploratory in the plan). *Outcome:* filings of profit corporations and of foreign entities fell by 17.3 and 14.5 log points in the program year, compared with usual changes of +2.6 and +8.0. Only the second decline is outside the usual variation of those changes, and filings of foreign entities remained lower after the fee was reinstated, so we do not interpret their decline as an effect of the price. Table A16.

Cost per additional unit. *Specified:* the appropriation divided by the number of additional LLCs, by the number of additional LLCs that file a first report, and by the number of additional applications; and the shares of spending received by filers who would have filed in the absence of the program and by bulk filers. *Outcome:* $179 per additional LLC, $791 per additional application, and $1,714 per additional LLC with a first report on time if every additional filing reports at the rate that the primary estimate implies for additional ordinary filings ($2,956 if only ordinary filings are counted). Of the fee relief on LLC filings, 70% went to filings that would have been made at $50, under the assumption that the number of filings in the preceding year is the number that would have been filed, and 13% went to bulk groups. Section 5 uses fee relief in place of the appropriation because fee relief can also be computed for the holidays.

Auxiliary study: the increase in the report fee from $10 to $25. *Specified:* among LLCs formed before July 1, 2021 that had filed their previous report on time, the on-time rate for anniversary months September 2024 to February 2025 less that for the same months a year earlier, less the same change between January to March 2023 and 2024; the same double difference in earlier years as reference values; and the share of companies with anniversary months July and August that filed between the announcement on June 17, 2024 and the increase on July 1. The plan noted that the federal deadline for beneficial ownership reports fell in the treated months. *Outcome:* a double difference of −0.058 (s.e. 0.034; $p = 0.134$), and no bunching, with 19.1% filing in those two weeks compared with 18.6% a year earlier. The point estimate arises from the comparison months, whose rate increased by 5.8 percentage points while that of the treated months did not change. One of the comparison months, February 2023, had an unusually low rate, 71.9% compared with 79.2% and 78.4% in the adjacent months, because its deadline of April 30, 2023 fell on a Sunday, and reports of every entity type were displaced to the following days. We conclude that the increase in the report fee to $25 had no detectable effect on the reports of established companies, and the paper reports the estimate only in support of that conclusion. Table A53; Figure A13.

B.5. Provisions and Outcomes: Addendum 1

N1 to N4 and N7 to N9 were specified before the tabulations that bear on them. For N6, the addendum specified the form of an analysis for which most of the results had been seen; the addendum's lists of what had and had not been examined do not mention the tabulation of N5 (Section B.3). N1 and N2 are implications of the two extensions of the framework (Section A.6); N3 is a conjecture, and N4 is not implied by the framework. A list of every statistic that the addendum specified, 703 rows in the addendum's order, is in the replication files (Table A6, row 4).

N1, filers accounting for the persistent increase. *Specified:* every agent and address is dated by the year in which it first appears in the registry, and the change in ordinary filings from 2021–22 to 2023–24 is decomposed by those dates. Three interpretations were specified. *Sunk set-up by new actors*: agents or addresses first seen in the program year file more in 2023–24 than those first seen a year earlier do in any reference year, and account for a larger share of

the change than in any reference span. *Established intermediaries*: organization agents (formation services) first seen before 2021–22 account for more than half of the change. *A larger flow of first-time filers*: agents first seen in the year itself account for most of the change. *Outcome:* Table A49. In the tabulation as specified, formation services first seen more than two years before the year of the filing account for 6.26 of the 10.14 percentage points, or 62%, whereas that class accounts for 17 to 26% in the reference spans other than the one ending in 2019–20, in which filings changed little. The membership of that class differs between the two years of a span. Formation services first seen before 2021–22, taken as the fixed group that the interpretation names, account for 58% of the change, compared with 14 to 21% for the corresponding groups in those reference spans [E]. The second interpretation holds on either basis, and the first holds in part. When each service is dated only from companies whose agent information has not been changed by a later statement, formation services first seen more than two years before the year of the filing account for 52% of the change; when companies covered by a statement that an agent filed for many companies on one day are also excluded, they account for 29%, which is less than the one-half that the second interpretation names (Section E.8) [E]. Formation services first seen in the program year made 564 filings in 2023–24, about twice the largest earlier number, and their companies filed a first report on time 10% of the time, but they account for 3% of the change, less than the same class accounted for in the span ending in 2019–20, when filings changed little. Addresses first seen in the program year meet both conditions and account for 4% of the change. The third interpretation does not hold, because agents first seen in the year itself account for 35% of the change, whereas agents of that class who are persons account by themselves for 45 to 52% in the reference spans other than the one ending in 2019–20. This result is a failure of the framework, whose two extensions imply a persistent increase only among filers and services new to the state (Section A.6). The agent is the one on record in October 2026; Section E.8 reports the volumes of the eight formation services with a persistent increase (item N2) among companies with no later statement of a change [E].

N2, agent-level classification. *Specified:* organization agents (formation services) with at least 50 filings in one of the three years are classified by their own volumes as new, stepped up and stayed, stepped up and reverted, between, or no step (the rows *New in the program year*, *Persistent increase*, *Increase, fully reversed*, *Increase, partly reversed* and *No increase* of the table cited under *Outcome*), with the same classification for six earlier spans. According to the addendum, routing by intermediaries predicts "that agents that stepped up lie mostly at the two poles and that those that stayed carry most of the organization-agent excess in B," where B is the year after the program. Agents are reported by rank only. *Outcome:* Table A50, Panel A. Eight agents show a persistent increase, one an increase that was fully reversed and one an increase that was partly reversed, and the eight account for 95% of the increase among formation services. The prediction holds as written but does not discriminate, because in each of the six earlier spans four to six agents showed a persistent increase and none showed an increase that was fully reversed, which is the pattern expected of a growing service. The agents with a persistent increase in the program differ from those of the earlier spans in their size: together they made 5,997 filings in 2021–22, whereas the agents so classified in each earlier span made between 250 and 1,205 filings in the first year of the span (Section D.7).

N3, the agent's own address. *Specified:* routing predicts that the share of ordinary filings whose principal address is that of the organization agent (formation service) increases in July 2022 and remains higher. *Outcome:* the share increased by 0.8 percentage points in the program year and was 0.8 percentage points above its level of 2021–22 in the year after, compared with a standard deviation of 0.5 percentage points for its usual yearly change. The share had increased by more in the two years before the program, and we do not regard the increase during the program as a break.

N4, addresses. *Specified:* if owners divide the same activity among more companies, the number of addresses with a reporting company remains constant while the number of companies per address increases; if the fee selects among new actors, the number of addresses increases and fewer of them have a reporting company. *Outcome:* an increase in the number of addresses accounts for 78% of the growth in filings at addresses with fewer than twenty filings, and the number of addresses with a reporting company grew by less than in a usual year. Taken at face value, this result

is the second case. A simulation by an AI reviewer showed, however, that when counted among a year's new filings both statistics move in the same way if owners merely divide existing activity, because an owner who would have formed no company in the year and forms one at $1 appears as a new address with one company. The tabulation that we specified therefore does not distinguish between the two interpretations, and we draw no conclusion from it.

N5, the fee reinstatement by type of filer ("no test is declared"). *Specified:* three types defined in the addendum (an organization as agent, that is, a formation service; a repeat filer or a shared address; a first filing by the sole filer at its address); for each type, the decline in filings at the reinstatement, filings on the three days after the announcement relative to the same weekdays of the four weeks before, and the contrast in three outcomes. *Outcome:* Table A45, Panel C. Filings in the three days after the announcement were 1.01 times their usual number, so there was no anticipatory filing. The differences in outcomes appear among repeat filers and shared addresses and not among first filings by the sole filer at the address, but the two types do not differ reliably from each other. We draw no conclusion about types of filer from this tabulation.

N6, later outcomes at the fee reinstatement. *Specified:* the contrast between companies formed May 1–21 and May 22–31, 2023, less the same difference in 2010 to 2022, for eight outcomes, with the standard deviation across those years times $\sqrt{1 + 1/13}$ as the standard error; the share of early-May filings that were marginal, estimated from filings per day, with stated variants; and stated checks. The third-year report and a trade name registered within twelve months were the outcomes that had not yet been examined. *Outcome:* Table A43, Panel B; Table A45, Panels A and B; Table A46, Panel A; Section E.8. The trade-name outcome could not be computed (Table A6, row 3). In the version with eight weeks on either side, two reference years, 2014 and 2022, contain days at $1 after the cutoff date, and in the version with six weeks 2022 contains three such days; excluding those two years changes the estimates little [E].

N7, prediction from recorded characteristics. *Specified:* a logit of the on-time report on the registry's fields for companies formed in 2017 to 2019, applied to later cohorts; the characteristics "capture" the decline between 2021–22 and 2022–23 if the model predicts at least half of it and "miss" it if the model predicts a quarter or less. *Outcome:* the model predicts 15% of the decline (-0.9 of -6.5 percentage points), which the addendum's rule classifies as a miss. For the contrast at the fee reinstatement, the model predicts -0.07 percentage points, whereas -1.6 are observed. The characteristics that the registry records at formation therefore do not account for the lower reporting rate of the program cohort.

N8, links between later filings and the program's filers. *Specified:* filings marked as unauthorized or fraudulent, or assigned to bulk groups, that were made after the reinstatement of the fee come from "the same hands" as the program's if they are linked to the program at least twice as often as other filings are, and in at least one-fifth of cases. The addendum named two links: an address, an agent's address or an agent's name first seen during the program; and, separately, one that met the rule that identifies bulk groups during the program. *Outcome:* on the first link, 7.0% of marked LLCs are linked, compared with 3.7% of other LLCs, and neither condition is met. The addendum's flagged set also includes LLCs in bulk groups (3.3% linked) and marked profit corporations (1.1% compared with 2.4% of other corporations), for which neither condition is met. On the second link, the shares are 5.9% and 0.7%, so the condition on the ratio is met and the condition of one-fifth is not. Of the marked filings, 22% have no usable address or agent name. By period of formation, which the addendum did not specify, both conditions are met on both links for the 1,714 marked or bulk LLCs formed in the thirteen months after the reinstatement, and on neither link for those formed later [E]. Of the companies in the largest bulk filer's cluster that a court dissolved, 42 were formed before the program, 15,461 during it and 2 after it. The number for the program counts entities registered at the filer's principal address and includes 18 profit corporations; of the 15,527 LLCs of the linked group described in Section 5, 15,455 have been dissolved by court order. Section D.7.

N9, Illinois. *Specified:* the plan's estimator for Illinois relative to the other states, with January 2013 to June 2017 as the pre-period, July to December 2017 omitted, and 2018 and 2019 as the post-period, together and separately, for six series, with standard difference-in-differences and synthetic control reported alongside. Three statements

on interpretation were specified: (a) an increase in applications accompanied by an increase in high-propensity applications or in applications with planned wages is what the framework predicts for a fee reduction of this size; (b) no detectable change is uninformative about stakes; (c) the income-tax increase of July 2017 and the end of the budget impasse are concurrent shocks that the design cannot remove. *Outcome:* 0.007 (0.045) for all applications, so interpretation (b) applies. Interpretation (a) overstates the framework, because a ceiling permits a response but does not predict it, and the ceiling depends on the level of the previous fee and not on the size of the reduction. Table A52, Panel B; Section E.9.

B.6. Provisions and Outcomes: Addendum 2

The addendum first stated four implications of the discovery of the holidays, each to be reported regardless of the results of its analyses. Statements made before the holidays were identified, that the fee had been $50 without interruption, that a fee of $1 was new, and that the report fee had been $10 since 2006, were incorrect, and the paper makes none of them (the report fee was $10 from July 1, 2005). The plan's primary test is reported as pre-specified and, in addition, with the six reference pairs that are unaffected by a holiday (item C7 below). Benchmarks drawn from "earlier years" include years with a holiday, which the paper notes wherever it uses them. Finally, the cohort year 2014–15, a reference year in several analyses of the program, begins with four months at $1. A list of every statistic that Part C of the addendum specified, 502 entries, is in the replication files (Table A6, row 4).

C1, filings. *Specified:* T_N of equation (4) for the holiday months, with four predictions and interpretations: (1) a positive estimate for ordinary LLCs in each holiday, where for the second holiday the sign was already known from the reports of the Secretary of State's office, so that the estimate is "a measurement by a method fixed in advance and not a test"; (2) the response is "mostly creation" if at least two-thirds of the additional filings of the holiday months remain net of the month before and the month after; (3) a positive estimate for profit corporations, whose fee and fee reduction were the same as those of LLCs, with no prediction for nonprofit corporations and with foreign entities in the second holiday, whose fee was not reduced, serving as a check on the comparison, a check that is passed if their confidence interval includes zero; (4) bulk groups are counted, with no direction predicted. *Outcome:* +0.124 (0.026) and +0.186 (0.025), each the largest of about forty reference values; 80% and 95% remain net of the flanking months; filings of profit corporations increased by 0.252 and 0.242, as predicted, and filings of foreign entities in the second holiday changed by +0.008 (0.048), with a confidence interval that includes zero, so the check is passed; 57 and 418 filings were in bulk groups. For the first holiday the second interpretation is less secure than the share net of the flanking months suggests, because relative to the year after alone 59% remains, which the addendum's rule would classify as mixed [E]. When one filing is counted per entity, the snapshot's counts of new entities are 0.4% to 2.5% below the totals that the Secretary of State's office published at the time (Appendix C). Table A14; Section 5.

C2, retiming. *Specified:* five statistics on filings per business day around the announcement, the start and the end of each holiday, net of the same statistics in other years. *Outcome:* Table A17, Panel A. Filings bunched in the last week of each holiday. Deferral of filings between the announcement and the start is not found in the first holiday, where the estimate has the wrong sign, and is not reliable in the second.

C3, types of filer. *Specified:* three expectations based on the program and one interpretation with no predicted direction: (1) names new to the registry increase, and by at least as much as filings by names seen before; (2) filings by repeat filers increase by at least as much as filings by new filers; (3) the share of new names with two or more filings in their first month increases; (4) for organization agents (formation services) no direction was predicted: if their filings increased by less than those of person agents, the paper was to state that "the agent response of 2022–23 was not reproduced by short cuts," and if by as much or more, that agents also respond to short fee reductions. *Outcome:* (1) holds in its first part and fails in its second, because new names increased by 0.125 and filings by names seen before by 0.205. (2) and (3) hold. For (4), the estimates are 0.114 compared with 0.124 and 0.172 compared with 0.183; the increase for formation services is smaller by a margin well within the standard errors (0.048), and we report the sentence that the addendum prescribed. Table A14, Panel B; Section 5.

C4, first reports: the main test. *Specified:* T_r for the first report on time, net of established companies, with four

interpretations of ρ computed from the marginal share net of the flanking months. (1) The plan's bands apply. (2) A confidence interval wholly above 0.22 rejects the plan's interpretation of 2022–23; one that contains 0.22 and excludes one "support[s] that reading: the paper will then present selection by the price as what the earlier holidays show, and will say that its argument from later cohorts, that the gap of 2022-23 was mostly something else, is in doubt"; one that contains both 0.22 and one does not discriminate. (3) Whether the interval contains 0.74, the estimate from the fee reinstatement. (4) A deficit that persists in the months after a holiday is not an effect of the price alone. *Outcome:* −3.3 (1.1) and −3.8 (0.9) percentage points, −3.6 (0.7) pooled; $\rho = 0.44$ [−0.10, 0.74]. Applying the addendum literally: (1) between strong and moderate; (2) the interval contains 0.22 and excludes one; (3) its upper end is 0.74, so the estimate from the fee reinstatement lies at the boundary of the interval, on one side or the other depending on the random draws; (4) the later months are +1.0 (1.2) and +1.0 (0.9) percentage points from their reference period, estimates that were computed on months already seen in the calibration. We do not present the result as the second interpretation prescribes. More than a third of the estimate is due to the adjustment, whose series had been seen (Table A6, row 5). Without the adjustment the pooled estimate is −2.3 percentage points (1.0), and the confidence interval for ρ, from 0.08 to 1.04, contains both 0.22 and one, which under the addendum's own rule does not discriminate [E]. The standard error depends on which reference runs are admitted and ranges from 0.58 to 0.95 percentage points. Section 6.5 and Section E.7 report these versions together, and the paper's conclusions about marginal registrations rest on the lending records of Addendum 3. Tables A40 and A41.

C5, business applications. *Specified:* the plan's estimator applied to three series of applications, for the months of each holiday and, separately, for the whole window, with no test declared; and a statement of whether the confidence interval for applications per additional registration contains the program's value of 0.23 and whether it contains one. *Outcome:* for the holiday months, both intervals contain 0.23 and exclude one, including when the error in the number of additional registrations is taken into account (Section E.9). For the whole window, the ratios are 0.46 [−0.46, 1.31] and 0.43 [0.08, 0.76]; both intervals contain 0.23, and that of the first holiday also contains one. Table A51.

C6, other acts in the registry. *Specified:* T_N for thirteen acts other than formation, with the prediction that acts whose fee was reduced to $1 in the first holiday increase in that holiday and not in the second; the reports of established companies falling due in the first holiday; and the later reporting of companies reinstated at $1. *Outcome:* Table A18. Of the seven acts predicted to increase in the first holiday, three increased (name reservations, amendments and trade names of reporting entities) and four did not differ from zero (reinstatements, withdrawals of foreign authority, dissolutions and trade names of individuals), including the two with the largest fee reductions, of $99 and $124. Changes of address and withdrawals of trade names, for which no direction was predicted, also increased. Periodic reports did not increase (−0.047, s.e. 0.046), and the on-time rate of established companies' reports due in the holiday was −1.4 percentage points from that of a year later (1.1), so a report fee of $1 instead of $10 did not increase reporting. Of the 274 companies reinstated for $1 instead of $100, 56% filed a report six to 24 months later, 10 percentage points fewer than in the reference years, with a confidence interval from −22 to +2.

C7, the plan's test with unaffected reference pairs. *Specified:* the primary test and the test of credit recomputed with the reference pairs that contain no cohort formed in a holiday, no cohort whose first reports fell due in one, and no cohort whose first reports fell due just after postcard reminders ended. *Outcome:* six pairs remain for the primary test, with a standard error of 1.15 in place of 0.98 and a permutation p-value of 0.143, again the smallest attainable; three pairs remain for credit, with $p = 0.070$ from the t distribution and a permutation p-value of 0.25. Table A44.

C8, the foreign-entity fee holiday of 2011. *Specified:* T_N for filings of foreign entities from September to November 2011, with no prediction. The addendum flags the estimate for two reasons: the regular fee was $125 before that holiday and $100 after it, so that the two reference years are at different fees, and one reference month, November 2012, is the month in which the first fee holiday was announced. *Outcome:* +0.018 (0.051) for a fee reduction from $125 to $1.

C9, the end of postcard reminders (declared exploratory). *Outcome:* the first-report rate fell when the Secretary of

State's office stopped mailing reminders, and for that reason reference runs whose first reports fell due just after that date are excluded throughout (Appendix C).

B.7. Provisions and Outcomes: Addendum 3

Tables A19 to A24 are generated from the result file and report every statistic that the addendum specified, as the addendum requires.

C0, definitions and inference. *Specified:* ordinary LLCs formed from January 2010 to December 2018 by month of formation; six outcomes, with a PPP loan as primary; the statistic T_r of equation (4) with no adjustment for established companies or for any other factor; standard errors from reference runs that contain no affected month, with the number of non-overlapping runs reported alongside every permutation p-value and the mean of the reference values reported alongside every estimate; ρ from joint draws of the two errors with the correlation that they exhibit across reference runs; and pooling by months, with the provision that each holiday is also reported alone and that "where the paper quotes one number it gives the second holiday's beside the pooled one, since report B found the first holiday's reference years less clean" (Table A6, row 15). The addendum also assessed the power of the test: a pooled standard error near 0.17 percentage points, compared with a deficit of about 0.77 percentage points if no marginal registration had a PPP loan. *Outcome:* the pooled standard error is 0.17 percentage points.

C1, the main test: PPP loans. *Specified:* four interpretations of the pooled confidence interval for ρ, with the marginal share measured in the holiday months. (1) Wholly below one: "the added companies were less often businesses with a payroll or an owner's income in 2020-21 than other companies formed in the same months." (2) Containing one with a lower end above one-half, or wholly above one: marginal registrations showed evidence of business activity about as often as inframarginal registrations, and the paper "may not describe the added registrations as having little behind them." (3) Containing one and extending below one-half: the measure does not discriminate. (4) Under any interpretation, if the lower end is above zero the paper states that some marginal registrations showed evidence of business activity. If the two holidays differ by more than twice the standard error of their difference, the pooled value is not used. *Outcome:* −0.74 (0.26) and −0.68 (0.22) percentage points, which do not differ; pooled −0.71 (0.17); ρ = 0.07 [−0.57, 0.60], and for the two holidays separately −0.18 [−1.61, 0.75] and 0.21 [−0.55, 0.81]. Under the addendum's rule, interpretation (1) applies, and the condition of (4) is not met. The paper does not adopt the wording of interpretation (1), which treats a company without a linked loan as a company without such a business; it states that marginal registrations less often had a PPP loan in the company's name (Table A6, row 13). Under other reference periods the estimate ranges from −0.41 to 0.81, and its interval includes one when the holiday months are compared with the flanking months (Section 6.2) [E]. Section 6.1; Table 3, Panel A.

C2, the other five outcomes. *Specified:* the same statistics, all reported, with no interpretation for individual outcomes; the paper "may not lead with one of them because it is the most favourable." *Outcome:* Table 3, Panel A; Table A19.

C3, the first month and the later months. *Specified:* for each outcome, T_r in the first month of each holiday and in its later months, and the difference between them. A pooled difference that is negative and exceeds twice its standard error in absolute value confirms the pattern that the second review found in registry outcomes; a pooled difference within one standard error of zero with a negative pooled estimate in both parts contradicts it; any other result is uninformative. *Outcome:* for a PPP loan the difference, later months less first month, is +0.26 percentage points (s.e. 0.32), with a negative estimate in both parts, so the pattern is contradicted. For any of the three records the difference is −0.18 (0.48). Section D.2; Table A21.

C4, by type of filer; C5, profit corporations. *Specified:* the same statistics for the three types of filer and for profit corporations, each with its own marginal share, and with no interpretation; results for new filers are reported wherever the total is reported. *Outcome:* Table 3, Panel B; Section 6.1; Section D.2; Table A22. The introduction and Sections 6.1 and 9 give the result for new filers beside the total, and the abstract does not (Table A6, row 15).

C6, an index of promise at founding. *Specified:* a penalized logit of a PPP loan with two or more jobs on characteristics of the name and the filing, fitted on half of the companies formed outside the held-out months; T_N for filings in the

top tenth, the middle and the bottom half of the index, and for filings weighted by the index. Two interpretations were specified: if the estimate for the top tenth is positive with a confidence interval that excludes zero, the marginal registrations also included companies of the most promising type; if the estimate for the bottom half exceeds that for the top tenth by more than twice the root sum of squares of their standard errors, the marginal registrations were disproportionately of low promise. *Outcome:* 14.4 log points (3.3) for the top tenth, with a confidence interval from 7.2 to 21.7, and 16.5 (1.9) for the bottom half. The first interpretation holds and the second does not. Section D.2; Table A23.

C7, other versions. *Specified:* for the primary outcome, the comparison with the year after only and with the year before only; standard errors from the sample of months that Addendum 2 used; links restricted to an identical full name and ZIP code; and ρ with the marginal share net of the flanking months. *Outcome:* Table A24. With the share net of the flanking months, $\rho = -0.03$ [−0.99, 0.57].

C8, analyses not conducted. The program's cohorts cannot be tested with these records, because PPP loans ended in May 2021, and for SBA loans the complete absence of such loans among the filings deterred by the reinstatement of the fee would amount to 1.2 standard errors at most, as the addendum computed from reference years before deciding not to run the test. A within-filer comparison has no power for outcomes that five companies in a hundred have, and it is conducted for first reports only (Section E.7) [E].

C9, assumptions underlying the estimates. The estimates assume that one ratio describes the whole window, which C3 tests; that a filing retimed from the flanking months behaves like any other inframarginal filing; and that linkage error does not differ between companies formed in a holiday and those formed a year before or after. The estimates are for an announced fee reduction of three or four months, and the same ratio is therefore not identified for a permanent fee. The follow-up of Section 6.2 bears on the second of these assumptions [E].

B.8. Provisions and Outcomes: Addendum 4

Tables A26 to A31 report every statistic that the addendum specified.

Part A and C0, definitions. *Specified:* the before and after periods of 42 days, with the five weeks between them omitted; effective working days; the statistics N, B, and R; estimates as the value in 2024 less the mean over reference years, with reference years chosen by rule (nine for registrations, four for each horizon of outcomes); ρ of equation (6) with joint draws at a pre-specified correlation; and a safeguard for the number of registrations, which is counted from the sequence of company numbers unless, in a period of 2024, 2025, or 2026, the companies seen in the files are fewer than 97% or more than 100.05% of that number. *Outcome:* for the fee increase of 2024 the companies seen are between 99.96% and 100.00% of the registrations counted from the number sequence, and the registration count from the number sequence is used.

C1, the main test. *Specified:* the prediction that registrations decline and that the share with non-dormant accounts increases, with the statement that, if the removed registrations were not businesses at all, the number of companies with such accounts does not change and the share increases in the proportion in which registrations decline; and four interpretations that are exhaustive and mutually exclusive. (1) The confidence interval for the change in registrations lies below zero and that for the change in the share lies above zero: "The rise removed registrations, and those it removed were less often businesses, by this measure, than those still made. The Colorado finding holds with the sign reversed"; if in addition the interval for ρ "is reported and lies wholly below one-half, the paper may say that most of the removed registrations were not businesses; if it reaches above one-half, the paper says only 'less often'." (2) The second interval contains zero: the paper "may not claim support from the United Kingdom." (3) The second interval lies below zero: "The Colorado finding fails out of sample, and the abstract says so." (4) The first interval contains zero or lies above it: the design did not detect a decline, and ρ is not interpreted. *Outcome:* registrations −19.2 log points (s.e. 6.7; $p = 0.021$); the share +19.2 (3.3; $p = 0.010$); the number of companies with such accounts +0.1% [−12, +14]; $\rho = 0.00$ [−1.62, 0.42]. The direct estimate of the change in that number, which the addendum reports beside it, is −0.8% [−14.2, +14.7] (Table A26, Panel A). The interval for ρ is that of the locked script, which

takes percentiles over the draws in which registrations fall by at least half a log point; the confidence set by test inversion runs from −2.5 to 0.42 (Table A6, row 20) [E]. Under the addendum's rule, interpretation (1) applies, and the condition of its additional clause is met. The paper does not make the statement that the clause permits. It states that deterred registrations would have had non-dormant accounts on file less often than the remaining registrations, and it reports the estimate with the six weeks before the announcement as the base period, for which the set lies below one and not wholly below one-half (Table A6, row 21). Measured against the four reference years of the outcome alone, the decline in registrations is 20.0 log points (s.e. 7.8; $p = 0.082$) [E]; the addendum specified the nine years, and on the four alone the interval for registrations would include zero. In 1.16% of the 400,000 draws of the locked script, registrations fell by less than half a log point; when that share exceeds 2.5%, the addendum provides that no interval for ρ is reported. Section 7.1; Table 4, Panel A.

C2, the other four outcomes. *Specified:* the same statistics for a registered charge, either indicator of business activity, a first confirmation statement and remaining active, with the expectation of a smaller effect on the last two. *Outcome:* Table 4, Panel A; Section 7.4; Table A26.

C3, checks. *Specified:* omission of each reference year in turn; the comparison with the period before the announcement, with a rule for when the before period is treated as inflated by anticipation; drift within each period, with a rule for when the decline may reflect a trend; the last days before and the first days after the fee increase; the comparison from the last days onward; companies struck off between July and October, with the provision: "If the two lead to different readings in C1, the paper adopts the one less favourable to the prediction"; and weekly series for a figure. *Outcome:* Section 7.3; Table 4, Panel B; Tables A27 and A28. No rule is triggered, and the interpretation is unchanged when the allowance for companies struck off is applied. The rule on anticipation compares registrations in the before period as a whole with those in the six weeks before the announcement (−2.6 log points, s.e. 4.9), and it is not triggered. Within the before period, however, registrations increased by 13.7 log points (s.e. 4.9) from its first half to its second, an increase that reflects in part a low first week (Section D.6). In the last 16 days before the fee increase, registrations were 19.3 log points (s.e. 6.8) above their level in the before period and the share with non-dormant accounts differed from its level in that period by −1.8 (4.0); Section D.6 takes its evidence of anticipatory filing from those days.

C4, types of registration at founding (descriptive). *Specified:* three types defined from the first file in which a company appears, with 2025 as the single reference year and no standard error; and the expectation that registrations of each type declined by more than other registrations. *Outcome:* the expectation holds for each type. Section 7.4; Table A30.

C5, the fee increase of February 1, 2026. *Specified:* the same contrast for registrations and for types of registration, with two cautions: the before period includes Christmas, so that a decline of less than about one-fifth cannot be distinguished from noise; and identity verification of directors became compulsory seventeen days before that period begins. The addendum also records that the registrar's web filing service was closed for a weekend within the after period. *Outcome:* the safeguard of Part A is triggered, because in one period the companies seen are 90.1% of the registrations counted from the number sequence. As the addendum provides, the number of companies seen is used, which gives −4.3 log points (s.e. 8.2), compared with −14.3 from the number sequence. Neither estimate is statistically distinguishable from zero. From November 20, 2025, two days after identity verification became compulsory for directors, to late December, the company numbers issued each day exceeded the number of companies that ever appeared in a monthly file by more than one-twentieth, and by 15% in December. Measured by companies seen in the files, registrations in November and December 2025 were approximately equal to their level a year earlier, after growth of 12 log points in the six months before, and from February to September 2026 they were 10 log points below their level a year earlier. The contributions of the fee and of identity verification to that decline cannot be separated [E]. Section 7.5; Table A31.

C6, two predictions. *Specified:* F1, that the share of companies with a first confirmation statement increased at February 1, 2026, to be evaluated with the file of October 2027 relative to the reference years 2018, 2021, 2022, 2023, and 2025; and F2, that the share with non-dormant accounts increased, to be evaluated with the file of October

2028 relative to 2017, 2021, 2022, 2023, and 2025. Both are to be computed with the hashed code, unchanged, and are to be evaluated whether or not the decline in registrations at that fee increase is statistically distinguishable from zero. By the addendum's calculation, an estimate below about 2.6 log points for F1 or 4.4 for F2 has a confidence interval that includes zero. *Outcome:* not yet available. Section 7.5 restates the predictions.

C7, analyses that the addendum excludes. The addendum specifies no test of the fee for a confirmation statement, because the approximate result had been seen; no survival outcome at the longer horizon; no measure based on the content of accounts; and no separate estimate for Scotland or Northern Ireland. The paper nevertheless reports the share of companies still on the register at the longer horizon as an exploratory outcome (Table 4, Panel B; Section 7.4) [E]. The addendum had excluded that outcome because, in some of its reference years, the fee for a confirmation statement changed between the dates on which the companies of the two periods were due to file one, which affects survival directly; the estimate is subject to that qualification (Table A6, row 23). The paper also reports estimates in which the increase in the fee for a confirmation statement is treated as an event (Section 7.3; Table A36, Panel D) [E]. The addendum specified no such test and provided that none would be reported as specified in advance (Table A6, row 22). The addendum also recorded no prediction about U.S. states that changed fees in 2026, because the facts of those changes had not been verified.

C8, assumptions underlying the estimates. The estimates assume that, in the absence of the fee increase, the relation of the after period to the before period in 2024 would have been the same as its average in the reference years. The addendum names five ways in which this assumption could fail and a check for four of them (C3). Of the first, anticipatory filing, it states that the companies formed early are ordinary companies, so that anticipation biases ρ toward one, against the prediction. For the remaining one, other events in the spring of 2024, the addendum has no check: tax changes took effect on April 6, within the before period, and a general election was called on May 22. If either event deterred businesses in May and June, the number of companies with accounts would be lower, which would work against the prediction. Section 7.3.

B.9. Consequential Departures

Table A6 lists the points at which the analysis or the text departs from the plan or an addendum in a way that bears on a number reported in the paper, on an interpretation, or on the claim that an analysis was specified before its results were known. It also lists the statements in the documents that are now known to be inexact and that bear on such a number or claim. The other departures do not affect a reported result: sources substituted for those that the plan named, statistics that a locked script did not store, and corrections of wording and of labels. The research log, which is archived with the code and the result files, records them and the corrections that followed the reviews. The plan and Addenda 3 and 4 provide that every departure be listed in the paper, and in this respect the paper departs from them (row 25). The scripts whose hashes the addenda record have not been changed since they were locked, and the build verifies their hashes.

Table A6. Consequential Departures from the Plan and the Addenda

	Provision, and the departure from it	Bearing on the results reported
The plan and Addendum 1: the 2022–23 program		
1.	*Fields as of the snapshot (the plan; Addenda 1 to 3).* The classifications by registered agent and by address presume the values at formation. The registry gives the values of October 2026, and about one-fifth of the ordinary LLCs formed in each of the three years around the program have a later filing that changes one of them. The history of filings records that a statement changing the agent was filed but not the agent that preceded it.	Every statement about types of filer, agents, and addresses is subject to this limitation. In Table A48, the share of additional filings with a formation service as registered agent falls from 0.37 (t = 2.4) to 0.22 (t = 1.3) among filings with no later change of agent (figures from the replication audit of the first draft) [E].

Table A6. (continued)

	Provision, and the departure from it	Bearing on the results reported
2.	*The interpretation of the plan's two tests (H2 and H3).* The plan specified bands for ρ and the rule that "a category is claimed only if the 95% interval lies inside it." Both of its intervals for first reports lie in the band of strong screening.	The paper does not adopt the interpretation. The plan treated the marginal share as known, and cohorts formed at \$50 in the two following years also show deficits, so that the part of the estimate attributable to the fee cannot be identified from these cohorts (Section 6.5; Section E.8). The paper draws no conclusion about ρ from the program.
3.	*Trade names (Addendum 1, N6).* A trade name registered within twelve months of formation was one of the two outcomes of N6 that had not been examined when the addendum was written.	The outcome cannot be computed, because trade-name filings in the history carry the trade name's own record number and not that of the registering entity, and the register of trade names lists only names in effect at the snapshot. No estimate is reported for it.
4.	*Pre-specified statistics computed after the locked scripts had run (Addenda 1 and 2).* Addendum 2 requires two further versions of every statistic, and Addendum 1 specifies a set of tabulations. The locked script of Addendum 2 did not compute every version, and the first report of the results gave a selection.	Of the 502 entries that Addendum 2 specified, 161 were computed after its other results were known, by code written later; of the 703 rows of Addendum 1, 123 were first reported after an internal review found them absent. Two of the added statistics qualify an interpretation: without the adjustment for established companies the pooled interval for first reports includes zero (C4 of Section B.6), and the second link of N8 gives a different result from the first (Section B.5). Both lists are in the replication files. Addenda 3 and 4 therefore require a generated table of every statistic.
	Addendum 2: registry outcomes of the two fee holidays	
5.	*Prior exposure to the adjustment series (sections A4 and B6 of the addendum).* The addendum's section on the held-out months states that no share was looked at for acts received from June to November 2014. Its section on calibration records that the on-time rate of established companies was printed for every month in which reports fell due other than November 2012 to March 2013. The second describes what occurred.	The amount that the adjustment adds to the primary estimate of the addendum, +1.3 and +1.3 percentage points in the two holidays, could have been read from the printed series before the adjusted outcome was specified as primary. Section 6.5 and Table A40 report the estimate with and without the adjustment, and the paper does not present the result as the addendum prescribes (C4 of Section B.6).
6.	*Two outcomes coded differently from their labels (C4).* The locked script counted a second-year report as on time only if the first report had been on time, and a third-year report as filed before delinquency only if the earlier reports had been on time.	Table A41 labels both as they were coded. As the addendum labeled it, the pooled estimate for the second-year report is −3.1 percentage points (0.7) with $\rho = 0.41$, compared with −2.3 (0.4) and 0.49 as coded; for the third-year report it is −3.2 (0.4) with $\rho = 0.42$, compared with −2.9 (0.4) and 0.37 [E].
7.	*The interval for ρ (C4).* The locked script held the inframarginal rate constant across the draws, set aside the draws with a marginal share of 0.01 or less, and drew the errors of the difference and of the share independently. The addendum states none of these three conventions.	Across reference runs the two errors are positively correlated (0.43 and 0.67 in runs of the two lengths). With a correlation of one-half, the pooled interval for the first report runs from −0.27 to 0.78 in place of −0.10 to 0.74 [E]. Table A9 compares the interval of the script with the set by test inversion. Addendum 3 states the first two conventions and draws the errors with the correlation that they exhibit across reference runs.
	Addendum 3: lending records of the two fee holidays	
8.	*A lapse in the calibration.* The first run of the calibration attached one outcome, an SBA loan within 24 months, to all companies before removing the held-out months. For the reference year 2013 it therefore compared companies formed in the weeks before May 20, 2013, which fall in the held-out months that follow the first holiday, with companies formed in the weeks after, and two contrasts were stored and seen (+0.16 and −0.11 percentage points), together with a mean rate over all months of formation and the mean and standard deviation, across years, of a contrast within May that included May 2013.	None of these is a rate for the held-out months, none concerns the holiday months, and the outcome is not one that the addendum uses. The block was corrected and rerun before the lock, the addendum records the lapse, and the log and result file of the first run are retained.

Table A6. (continued)

	Provision, and the departure from it	Bearing on the results reported
9.	*The reason given for excluding companies formed in 2019.* The addendum leaves those companies out of the sample "because a payroll loan required operation by February 2020 and a payroll in 2019, and the calibration shows their rate falling (4.7% against 5.9% for 2018), which would enter the reference runs as curvature." The requirement of a payroll in 2019 is incorrect: a borrower that was not in business in the first half of 2019 could base its loan on the payroll costs of January and February 2020, and self-employed borrowers without employees were eligible (United States Code, 2020; U.S. Small Business Administration and U.S. Department of the Treasury, 2020).	The sample is the one that the addendum specified, and its second reason stands. The estimates for the holidays do not use the months of 2019. With those months admitted, the runs of 2018 enter as reference runs; the pooled standard error for the PPP loan is then 0.24 percentage points with 10 degrees of freedom, compared with 0.17 with 7, and the interval for ρ runs from -0.92 to 0.79; for any of the three records it runs from -0.73 to 0.46 [E].
10.	*SBA loans approved on the day of formation.* The addendum defines the outcome as a loan "approved after its formation." The locked script counts every linked loan, and the linking rule admits entities formed on or before the day of approval, so that a loan approved on the day on which the formation filing was received is counted.	Five companies in the sample have an SBA loan only on that basis, and two of them have no other record. When only loans approved after the day of formation are counted, the pooled estimate for any of the three records is -1.723 percentage points, compared with -1.724 [E].
11.	*Bulk filings among profit corporations (C5).* The addendum's row for profit corporations includes all profit corporations, because the rule that identifies bulk filings had been applied to LLCs only.	Applied without change to profit corporations, the rule identifies 622 of the 7,543 formed in the holiday months, 619 of them in February 2013 and September 2014. The paper reports the pre-specified row and, in addition, the same statistics excluding those filings (Table 3, Panel B; Table A34, Panel C) [E].
12.	*The fee for financing statements (Addenda 2 and 3).* The first holiday reduced the fee for a UCC financing statement or amendment from \$8 to \$4 (Colorado Secretary of State, 2012a). A financing statement is one of the outcomes of Addendum 2 and a component of two of the six outcomes of Addendum 3. Neither addendum mentions the fee.	Table 1 and Table A7 list the fee. Section 6.1, Section D.2, and Table A37 report the outcomes with the statements of a company's first 90 days excluded and with only the statements of 2020 or later [E], together with the estimates for each holiday separately, which Addendum 3 specified [A3]. The second holiday did not change that fee.
13.	*The wording of the interpretation of the main test (C1).* For an interval for ρ that lies wholly below one, the addendum specified the reading that "the added companies were less often businesses with a payroll or an owner's income in 2020-21 than other companies formed in the same months." The condition is met.	The paper does not adopt that wording, which treats a company without a linked loan as a company without such a business. It states that marginal registrations less often had a PPP loan in the company's name, and it examines separately what the record captures (Sections 6.1 and 6.3). The departure narrows the claim.
14.	*The weight given to a secondary outcome (C2).* The addendum specifies interpretations for the primary outcome alone and provides that "the paper may summarize them [the other five outcomes] only together with the primary outcome and may not lead with one of them because it is the most favourable."	The paper reports the Colorado evidence by outcome and gives one of the five, any of the three records, more weight than the addendum did, because its estimate varies little with the reference period. That property was established after the results were known [E], and the outcome was not chosen in advance for it. To remain within the provision, the text states the result for the primary outcome first wherever it states the result for the broader record, and Table 3 reports all six outcomes.

Table A6. (continued)

	Provision, and the departure from it	Bearing on the results reported
15.	*Two reporting provisions (C0 and C4).* The addendum provides that "where the paper quotes one number it gives the second holiday's beside the pooled one" and that the paper "gives the first-company kind wherever it gives the total."	The abstract, the introduction, Sections 6.1 and 9, and Tables 5 and A5 give the second holiday's estimate beside the pooled one. The introduction, Sections 6.1 and 9, and Tables 3 and A5 give the result for new filers. The abstract, which is limited to 200 words, and Table 5 do not, and in this respect the paper departs from the second provision. Where the introduction and Sections 6.2, 8, and 9 quote estimates of ρ for the PPP loan and for any of the three records under alternative reference periods, they give those for the second holiday alone beside those for the two holidays together, apart from the estimate at the estimated weight on the flanking months, which is computed for the two holidays together only, and Tables A25 and A33 give each holiday under each period. The abstract quotes the estimates under alternative periods for the two holidays together only, and in this respect the paper departs from the first provision. The change in the number of companies with a PPP loan under the comparison with the flanking months (Sections 6.2 and 8) and the estimate for the financing statement under that comparison (Section 6.2) are also quoted for the two holidays together only, as are the two capture ratios for Colorado in the introduction and in Sections 6.3 and 9, which are the upper ends of the pooled intervals; the upper ends for the second holiday alone are 0.81 and 0.67. Estimates under alternative definitions of the outcome are quoted for the two holidays together, except that the introduction and Sections 6.3 and 9 also state, for the second holiday, the weight on loans in an agent's name at which the interval reaches one or the proportion of such loans to which that weight corresponds. For the second holiday alone, ρ for the PPP loan is 0.33 and 0.21 with a loan in the name of a company's agent counted (Section D.4), 0.28 with the links corrected by the review of record pairs, and between 0.17 and 0.49 under the eight other linking rules; for any of the three records with only the financing statements of 2020 or later it is 0.25 [E].
16.	*Address keys and the shares by type of filer (Addenda 2 and 3).* The classification of filers in the locked scripts uses address keys formed from a street number and a ZIP code, and 49% of LLCs share such a key with an LLC on another street.	With keys that add the street name, and with keys that use the full address line, new filers account for 44 and 52% of the holidays' additional registrations and repeat filers for 37 and 32%, compared with 40 and 41% under the original keys (Section 5; Table A38, Panel C) [E]. The estimates by type of filer that Addendum 3 specified are those of the original keys. The pooled estimates use an address key only in the rule that excludes bulk filings, and they are unchanged when that rule uses the finer keys (Table A38, Panel B).
17.	*The links between loans and companies.* The files that link loans to companies are hashed in Addendum 3, and the main estimates retain those links. The linking rule does not decode the HTML entities that the loan file contains in 787 borrowers' names, and it cannot link a loan made in a name other than the company's.	1,177 pairs of a company and a loan or a financing statement were read by two sets of four AI readers that were not shown the cohort of the company, and Table A38 reports the estimates with the entities decoded and with the links corrected by that review [E]. Loans made in the name of a company's registered agent are linked separately (Section D.4) [E]. The readers saw names and addresses from public records and recorded a judgment for each pair; the sheets that they read are not distributed. No person has read the pairs.
18.	*Simulated interval endpoints (Addenda 3 and 4).* The intervals for ρ and for the number of companies are based on 200,000 draws in Colorado and 400,000 in the United Kingdom, with a seed set in each configuration file.	The endpoints are subject to simulation error in the second decimal place. By numerical integration the pooled interval for the PPP loan runs from -0.573 to 0.598, whereas the draws gave -0.574 to 0.601, and the largest difference in any interval endpoint for the PPP loan or for any of the three records is 0.012 [E]. The paper reports the endpoints that the locked scripts stored, and no interpretation depends on the difference.
	Addendum 4: the U.K. fee increase	

Table A6. (continued)

	Provision, and the departure from it	Bearing on the results reported
19.	*The correlation of the simulated errors (C0).* The addendum states that the interval for ρ is obtained from joint draws of the errors of E_N and E_R, "each Student's t with its own degrees of freedom, with a correlation fixed in advance," which for the primary outcome is -0.68. The locked library assigns that correlation to a pair of normal variables and divides each by the square root of its own scaled chi-square variable. Errors drawn in this way have a correlation of -0.52, and the addendum does not state which correlation was intended.	Had the errors themselves been correlated at -0.68, the percentile interval for ρ would run from -1.36 to 0.34 and that for the number of companies with non-dormant accounts from -10% to $+11\%$ [E]. Section 7.2 and Table A10, Panel B, report how the confidence set depends on the correlation that is assumed.
20.	*The construction of the interval for ρ (C0 and C1).* The addendum provides that no interval for ρ is reported when registrations fall by less than half a log point in more than 2.5% of the draws. It does not state that the ends of a reported interval are percentiles over the remaining draws, which is what the locked library computes.	1.15% of the draws were set aside, and 87% of them imply a ratio above one-half. An outside reviewer identified this step. The paper reports confidence sets by test inversion for every U.K. ratio that the text cites, other than the 315 specifications of Section 7.3 and the comparison without reference years of Section 7.5, which has no standard error [E]. For the primary outcome the set runs from -2.5 to 0.42, compared with percentiles from -1.62 to 0.42 over the draws retained, so that setting draws aside changed the lower end and not the upper end. Percentiles over all draws run from -1.68 to 0.51 and are not the inversion of a test (Sections 4.5 and 7.2; Tables A9 and A10). The tables that reproduce the statistics of the addendum show the intervals of the locked script.
21.	*The statement that C1 permits.* If the interval for ρ "is reported and lies wholly below one-half, the paper may say that most of the removed registrations were not businesses." The interval of the locked script lies below one-half, and the rule under which the addendum treats the before period as inflated by anticipation was not triggered (C3).	The paper does not make the permitted statement. It states that deterred registrations would have had non-dormant accounts on file less often than the remaining registrations, for two reasons. The permitted statement equates a company without such accounts with a company that is not a business. And one-half is excluded by a direct test and, under the error model of the addendum, by test inversion at any within-year correlation of the two errors of -0.45 or stronger, and by test inversion it is not excluded at weaker correlations; a second test places that boundary at -0.50 or, with its p-value maximized, at -0.58 (Section 7.2). With the six weeks before the announcement as the base period, which the addendum specified as a check, the set excludes one and includes one-half (Section 7.3) [E]. The departure narrows the claim.
22.	*Reference years exposed to the higher statement fee (C0 and C7).* The addendum retains two reference years of the primary outcome in which the companies of the before and after periods owed a confirmation statement at different fees, 2022 at the second statement and 2023 at the first, on the ground that "a company that files accounts that are not dormant is unlikely to give up its registration over £21." Section C7 of the addendum states that three of the four reference years are so exposed, whereas C0 names two; the fee calendar shows two (Table A36, Panel A).	Estimates in which the statement fee is treated as an event, a test that the addendum did not specify, support the presumption for 2023 and leave it in doubt for 2022. Relative to the other reference years, the share of the companies incorporated in 2022 that were on the register at 27 to 31 months is 3.8 log points lower in the after period (s.e. 0.7; $p = 0.033$), and the share with non-dormant accounts is 5.4 lower (1.8; $p = 0.098$) [E]. The paper therefore reports the test without 2022, for which ρ is 0.01 (-2.6 to 0.41), and with the two years of equal fees alone, for which ρ is 0.10, with a set that extends to 1.33 on the standard error of those two years and to 0.45 on a standard error from 36 placebo dates (Section 7.3; Table 5, Panel B) [E].
23.	*An outcome that the addendum excluded (C7).* The addendum specifies no survival outcome at the longer horizon, because "three of its four reference years have periods on either side of a change in the statement fee, which acts on survival directly."	The paper reports the share of companies still on the register at 27 to 31 months as an exploratory outcome, subject to the qualification that the addendum gives (Table 4, Panel B; Section 7.4) [E].

Table A6. (continued)

	Provision, and the departure from it	Bearing on the results reported
24.	*The count of registrations at the second fee increase (Part A and C5).* The safeguard for the number of registrations counted from the number sequence was triggered for the fee increase of February 2026. The addendum provides that the paper then "uses the count of companies seen in a snapshot for the dates from 1 January 2024."	The provision is applied to the test of the fee increase whose periods contain the fault. Under the other reading, in which the fault of 2026 also determines the count used for the test of 2024, the estimates for 2024 are unchanged to the precision reported, and the lower end of the percentile interval for ρ is -1.65 instead of -1.62 (Table A28, Panel F). The description of the gap between the numbers issued and the companies seen in late 2025 is exploratory (C5 of Section B.8).
	The plan and Addenda 3 and 4: the list of departures	
25.	*Where departures are listed.* The plan provides that "Any departure from this document is listed in a 'Deviations from the preregistration' section of the paper with its reason." Addendum 3 provides that "Departures are listed with the others in the paper's appendix," and Addendum 4 that "Any departure from this document is listed with its reason."	This table lists the departures that bear on a reported result, an interpretation, or the claim that an analysis was specified in advance. The others are listed with their reasons in the research log, which is archived with the code and the result files and is not part of the paper. In this respect the paper departs from the three provisions.

B.10. Exploratory Analyses

The following analyses were specified neither in the plan nor in an addendum before they were run, and the text marks each of them [E] in the section that reports it.

In the main text. The framework of Section 3 is exploratory. It was written after the program's results were known, and the relation between the shares of companies with a record at the two fees (equation (2)) and the decomposition of the relative rate into business rates and capture (equation (3)) were stated after all results were known. Also exploratory are the estimate of the program's increase in registrations relative to the year before, the estimates for filings per weekday, and the shares of the holidays' additional filings by type of filer (Section 5; Table A14, Panel B).

For the lending records of the holiday cohorts, the exploratory analyses are the change in each record as a proportion of its inframarginal rate; the numbers of companies with each record; the estimates under every reference period other than the pre-specified one, with the single-month statistics of Figure 2 and the evidence on the source of the deficit in the flanking months; delinquency before the loans; the outcomes without the financing statements of a company's first 90 days and with only those of 2020 or later; profit corporations excluding bulk filings, and LLCs and corporations together; the loans made in the name of a company's registered agent; the checks of the record linkage, with the review of record pairs and the estimates corrected by it; the checks of the classification of filers; and the simulations of the inference (Sections 6.1 to 6.4; Sections D.3 to D.5). Addendum 3 had specified the statistic for the flanking months and for the months that follow each window, which include four of the eight months adjacent to the two windows (Table A20) [A3].

For the registry outcomes, the exploratory analyses are every version other than those that Addendum 2 specified (C4), the comparison of the first month with later months, and the within-filer comparison (Section 6.5; Section E.7; Table A40).

For the United Kingdom, the exploratory analyses are the confidence sets by test inversion, the second test whose null distribution is simulated, the direct test on the four common reference years, the dependence of the set on the correlation assumed, the tests at placebo dates, and the coverage simulations (Section 7.2; Section D.1); the change in the number of companies and ρ with the earlier base period; the calendar of maintenance fees, the estimates of the effect of the statement fee, and the tests without the reference years exposed to it; the specifications of Figure A1; the shares by week of incorporation and the step fitted to them; the measurement of outcomes at equal ages; the outcomes other than the five that Addendum 4 specified, under either base period, among them accounts ever filed and the classification of the deterred registrations by what the companies would have filed; registrations at shared addresses within 2024; the six-week periods after June 2024; the monthly series; and the gap between the company numbers issued and the companies seen in late 2025 (Sections 7.3 to 7.5; Section D.6; Table 4, Panel B; Figure 3).

For the program, the exploratory analyses are the estimates for later cohorts and by comparison group, the check

on reports received at $10, the decomposition of additional registrations by type of filer (Table A42), the filings of 2020 from the largest group's address, bulk filings by year, the eight agents' volumes and the tabulation by date of first observation among companies with no later statement, the share of the persistent increase due to the formation services first observed before July 2021, the comparison with Connecticut, shares of marked filings by cohort, and the linkage rule of N8 by period (Sections 5 and 6.5; Sections D.7 and E.8).

For business applications, the exploratory analyses are the ratios with the error of their denominators, the benchmark ratio of applications to filings and the comparison with it, the estimates for the year after the program without December 2023, the difference between the estimates for the year after the program and those for the program, the four-week comparison at the fee reinstatement, and for Illinois the counts from the Secretary of State's reports. The figures for fee relief in Section 5 are also exploratory.

In the appendices. The following displays are wholly exploratory: Tables A10, A12, A8, A15, A25, A35, and A42; Tables A36, A37, A38, and A39, apart from the rows and columns that reproduce pre-specified estimates, which their notes identify; the sets by test inversion in Table A9; every row of Table A11 except the first, and in its first row the numbers of companies with each record; every row except the first of Table A33, and in its first row the numbers of companies with each record; every row of Table A32 except the first row of Panel A; every row of Table A34 except the last row of Panel A and the first row of Panel C; and Figures A1, A2, A3, A4, A7, A8, A9, and A10. The following are exploratory in part, as their notes indicate: Tables A13, A14, A16, A40, A41, A43, A44, A45, A46, A47, A50, A51, A52, and A53, and Figures A6, A11, and A12. Also exploratory are the remark on the length of a fee reduction and the checks on two further channels in Appendix A, the decline in first reports when postcard reminders ended, the two checks of the adjustment for established companies (Appendix C; Sections A.5 and E.8), and the verification of the archive of financing statements (Section C.7).

Analyses run and not reported. Earlier versions of the paper reported several exploratory designs for the program that this version does not use: a comparison of half-years, discontinuities in the daily rate at the two fee changes, a regression across months of formation, regressions across groups of filers and ZIP codes, estimates of the change in the composition of business applications during the program, a comparison with the first annual reports of companies formed in Connecticut, and a direct comparison of the three episodes. A division of the program's deficit into a part that ended when the fee was reinstated and a part that persisted is reported by comparison group, together with the relative rates that it implies, on which no conclusion rests (Section E.8). The code and results of all these analyses remain in the replication files.

B.11. Production and Verification

The research question, the design, the code, the analysis, and the text were produced by an artificial-intelligence system, Claude, acting as a research agent under an instruction to identify an open empirical problem in entrepreneurship research, locate data, specify hypotheses and methods in advance, and write the paper. Assisting agents surveyed the literature, checked sources, transcribed the Illinois reports, and formatted tables; their unintended exposure to outcomes before the plan was locked is described in Section B.3.

The draft was revised after several rounds of review by AI readers that were instructed to act as referees or to check the text against the result files, and after two external reviews that the paper's owner supplied. In the first round, one reader reran every analysis from the raw files with independent code, and the appendices refer to that check as the replication audit of the first draft. In the third round, one reader recomputed the tests of Addenda 3 and 4 from the derived files with its own code and reproduced both; the estimates for the primary outcomes equal the stored values, and the ends of the simulated intervals agree within simulation error. A second reader recomputed parts of both tests, and the research log records how its figures differ. The requests of the second round, for an outcome recorded by a party other than the registry and for an out-of-sample test, led to Addenda 3 and 4. The first external review led to the analyses of Section E.6. The second led to the confidence sets by test inversion, to the statement of the results for records and by outcome, to Table 5, and to the present organization of the paper. The AI readers are instances of the system that wrote the paper, and their reviews are checks of quality and not independent verification; no review constitutes peer review by

a journal. A research log, archived with the code and the result files, records each round with its consequences and the corrections made to the code and to the text; the reports of the reviewers and the responses to them are in the same archive.

No person has verified the individual results. To make verification possible, every number in the text, tables, and figures is written by code from result files that the code also produces, with the exception of the constants quoted from the plan and the addenda, numbers quoted from cited sources, a few numbers that the appendices attribute to the replication audit, whose scripts are distributed with it, and the two values of ρ, 0.13 and 0.45, that Section D.6 attributes to an internal reviewer's report. Fractions and multiples stated in words, such as "about one-sixth," are hand-entered approximations of numbers that the code writes in the same passage or in the table cited. Bibliographic records of works that have a digital object identifier were taken from the registered metadata, and the remaining records were checked against a source that could be read. The description of each scholarly work cited was compared with the work, a working-paper version, or its abstract, and the research log states the limitations of that comparison. Facts about fees and dates were read in the sources cited, and the text states where a source could not be read. The data files that name borrowers, companies, and individuals are not distributed, but the scripts that retrieve and build them are. Before the results are relied upon, a person would need to rerun the code from the archived raw files, recompute the main estimates independently, and audit a stratified sample of the record links and of the sources cited.

C. Data

C.1. Colorado Registry Data

We use two files from the state's open-data portal (Colorado Secretary of State, 2026a): *Business Entities in Colorado*, which contains 3,118,181 entities, and *Business Entity Transaction History*, from which we extracted the 15,988,671 rows for documents received from January 1, 2010 to October 3, 2026. These rows were retrieved in 34 half-yearly pieces, and the row count of each piece was verified against the row count reported by the portal. Both files were downloaded on October 4, 2026 (Section 4.2), and their hashes are recorded with the code. Some rows of the transaction history are exact duplicates of an earlier row. These duplicates are relevant to the completeness check in the next subsection and to the counts of other registry filings in Table A18, which are counts of rows. They do not affect the counts of formation filings or the outcomes, in which each entity enters once.

A formation filing is a document described as "Form a Limited Liability Company (LLC)," "Form a Profit Corporation," "Form a Nonprofit Corporation" or "Apply for Foreign Entity Authority." Each entity enters once, at its first such document. A filing is dated by the day it was received, which is the day the fee was paid. The anniversary month, which determines when periodic reports are due, is taken from the effective date. The two dates differ for the few filings with a delayed effective date, and excluding these filings is one of the checks specified in the plan.

Periodic reports are documents described as "File Report." For each entity, we compute the number of months from its anniversary month to the month in which a report was received. A report can be filed as early as two months before the anniversary month. From the fifth month after the anniversary month, the entity is delinquent and cannot file a report without first curing the delinquency. We classify a report as *on time* if it is received by the end of the second month after the anniversary month and as *before delinquency* if it is received by the end of the fourth month. Reports of the second and third years are defined analogously around the later anniversaries. Voluntary dissolution is identified by a statement of dissolution or, for a foreign entity, by a withdrawal. Dissolution by a court is identified by a document described as "Court Order." Good standing is the status recorded in the entity file as of the snapshot. An entity is classified as marked unauthorized or fraudulent if a "Filing Officer Correction" received since January 2022 contains a comment to that effect.

C.2. Completeness of the Snapshot

A law in force since August 12, 2026 permits the Secretary of State's office to mark a record as unauthorized or fraudulent and to declare the entity delinquent, and to remove from its online system a filing whose payment was reversed (Colorado General Assembly, 2026). In a release of September 14, 2026, the office stated that it had marked

12,693 entities as fraudulent and had removed a further 10,214 from the registry (Colorado Secretary of State, 2026c). Because the snapshot was taken seven weeks after the law took effect, an entity may have lost its good standing for a reason other than a missing report, and some entities may be absent from the snapshot. We therefore compare counts from the snapshot with counts that were published before the law took effect. Two factors complicate the comparison. First, the history file contains exact duplicates of some rows, so that a count of rows overstates the number of filings. Second, the office's definition of a new entity, and the date by which it assigns a filing to a quarter, may differ from ours.

Fee holidays. Addendum 2 specified the check: new entities by quarter and, by month, the ten types of filing that the second holiday covered (item C1) [A2]. Its locked script counted rows. On that basis, the snapshot is within 1.1% of the published total in each of five quarters (the fourth of 2012, the first and third of 2013, and the third and fourth of 2014) and exceeds the published total in three of them. The second draft reported that agreement, which was obtained from a count that included duplicate rows. Of the 2,180,575 rows of the covered types and of applications for foreign entity authority, 22,227 are exact duplicates of an earlier row, with the same transaction number, entity, description, dates, and comment. Duplicates account for 2.7% of the rows received in 2010 and 1.2% of those received in 2019. When one filing is counted per entity, the snapshot is below the published count in each of the five quarters, by between 0.4% and 2.5%, and by 1.7% over the five together [E]. The estimates in the paper do not use row counts, because each entity enters once.

The deficit does not necessarily reflect the removal of records. The history also contains documents that convert an existing entity into another form. Such documents create an entity without one of the formation filings that we count, and the Secretary of State's office may or may not have counted them. When they are also counted, the differences from the published counts range from −1.1% to +1.2%, so the deficit may reflect what the office counted as a new entity and not the removal of records. We cannot determine which is the case. Dating is a further source of difference. The comparisons above are based on the date on which a document was received. The office's report for the third quarter of 2014 states that, from the fourth quarter of 2013, its figures are based on the effective date (Colorado Secretary of State, 2014d). Counted by effective date, the snapshot is 1.8% and 3.2% below the published counts for the third and fourth quarters of 2014, and the second of these differences is larger than any of the differences by date received. Growth from the same quarter a year earlier, which a difference of definition that is stable over time would not affect, agrees with the growth rates in the office's quarterly reports to within 1.8 percentage points in each comparison that we could make. For the third quarter of 2014, the office reported growth of 32.2% in new entities and of 34.9% in domestic LLCs (Colorado Secretary of State, 2014d), and the corresponding figures from the snapshot are 33.3% and 35.6%.

For the ten types of filing that the second holiday covered, a briefing by the legislature's budget staff dated November 19, 2014 reports 33,941 filings from July to October 2014. When one filing is counted per entity, the snapshot contains 33,451, which is 1.4% fewer. By month, the two counts differ by more. The snapshot contains 10.8% more filings than the staff's count for September and 12.0% fewer for October. Over the two months together, the staff's count exceeds that of the snapshot by 323 filings. An earlier draft attributed the monthly differences to dating. Counting by effective date does not remove them (10.4% more and 12.1% fewer), and we do not know the rule by which the staff assigned filings to months.

The 2022–23 fee-reduction program. The Secretary of State's office reported 48,058 new LLC filings in the first quarter of 2023 (Colorado Secretary of State, 2023). The snapshot contains 47,177 formation filings of domestic LLCs received in that quarter, 1.8% fewer, and for the first quarter of 2022 it is 2.7% below the figure implied by the same release. Most of this difference is attributable to dating. The office's quarterly figures are based on the effective date, and 7% of filings received in December take effect later, most of them on January 1. Counted by effective date, the snapshot contains 47,855 filings for the first quarter of 2023, 0.4% below the office's figure, and it is 0.7% below the corresponding figure for the year before. The office also reported "more than 155,000" LLCs from July 1, 2022 to May 19, 2023, and the snapshot contains 156,403. The companies of the largest bulk group, which are the companies most exposed to removal, are present in the snapshot and are recorded as dissolved by court order.

Later cohorts. Section E.8 relies on companies formed after the program, for which we have fewer published counts [E]. The quarterly reports of the Secretary of State's office count new filings of all entity types (for example

Colorado Secretary of State, 2024b). Relative to the counts that we transcribed from these reports, the registry's entity file, counted by date of formation, is 1.0%, 0.8%, 0.9%, and 0.9% lower in the four quarters from July 2022 to June 2023, and 1.3% and 1.2% lower in the third and fourth quarters of 2023. For calendar year 2023, a press account reports 160,611 new LLCs (Governing, 2024), and the snapshot contains 159,850 by effective date, 0.5% fewer. We compared no published count for 2024 or for 2025 after its first quarter. The figure for the first quarter of 2025, 1.1% lower, is implied by the count for the first quarter of 2026 and its growth relative to a year earlier, both of which were reported in April 2026, before the law took effect (Colorado Sun, 2026). For the first quarter of 2026 itself, the entity file is 1.0% lower. The deficits in the quarters after the program are about as large as those in the quarters of the program, or slightly larger. The published numbers in this paragraph, like those for the holidays, are our transcriptions from the office's reports and from press accounts.

These comparisons do not establish that records were removed, and they cannot establish that none were. Deficits of the same order appear in the quarters of 2012 to 2014 and in every later quarter that we could compare, and around the holidays a difference in the definition of a new entity may account for them. However, removals offset by differences of definition would not be detected, we do not know which entities the Secretary of State's office removed in 2026, and we have no published count for most of the two cohort years after the program.

C.3. Filing Fees since 2008

Table A7 reports the fee for each filing used in the paper and the source of each change. The fee for a periodic report was $10 from July 1, 2005 to June 30, 2024, except from December 1, 2012 to February 28, 2013, when it was $1, and it has been $25 since July 1, 2024 (Colorado Secretary of State, 2005b, 2012a, 2024a). One earlier episode is not covered by the table. When the Secretary of State's office introduced online filing of articles of organization in July 2004, it charged an introductory fee of 99 cents, which it ended on April 1, 2005 (Colorado Secretary of State, 2004, 2005a). We do not study that episode because the introductory fee coincided with a new method of filing, we have not established the cost of filing on paper in those months, and the history of documents that we use begins in 2010.

Table A7. Fees for Business Filings in Colorado, 2008 to 2026

Filing	Regular fee	At $1	Sources
Articles of organization of an LLC	$50	First holiday, second holiday, and the program	Colorado Department of State (2013); Colorado Joint Budget Committee Staff (2010); Colorado Secretary of State (2012a, 2013a, 2014a,c, 2022b, 2023)
Articles of incorporation, profit corporation	$50	First and second holiday	Colorado Department of State (2013); Colorado Secretary of State (2014a)
Articles of incorporation, nonprofit corporation	$50 to October 31, 2011; $20 from November 1, 2011 to November 30, 2012; $50 from March 1, 2013	First and second holiday	Colorado Secretary of State (2011b); Colorado Department of State (2013); Colorado Secretary of State (2014a)
Statement of foreign entity authority	$125 to August 31, 2011; $100 from December 1, 2011	September 1 to November 30, 2011; first holiday	Colorado Secretary of State (2011a); Colorado Department of State (2013)
Withdrawal of foreign entity authority	$125 to March 31, 2017; $10 since	First holiday	Colorado Department of State (2013); Colorado Secretary of State (2017)
Statement of trade name	$20	First holiday; the program, for business entities	Colorado Department of State (2013); Colorado Secretary of State (2022b)

Table A7. (continued)

Filing	Regular fee	At $1	Sources
Periodic report	$10 from July 1, 2005 to June 30, 2024; $25 from July 1, 2024	First holiday (December 1, 2012 to February 28, 2013)	Colorado Secretary of State (2005b, 2012a, 2024a)
Late fee on a periodic report	$40 to October 31, 2013; $50 since	Never	Colorado Department of State (2013); Colorado Department of State (2019)
Statement curing delinquency	$100 to July 31, 2012; $60 from August 1, 2012 to October 31, 2013; $100 since	Never	Colorado Secretary of State (2012b); Colorado Department of State (2013)
Reinstatement	$100	First holiday	Colorado Secretary of State (2012a); Colorado Department of State (2013)
Dissolution	$25 to September 30, 2016; $10 since	First holiday	Colorado Department of State (2013); Colorado Secretary of State (2016)
Amendment of articles; reservation of a name	$25	First holiday	Colorado Secretary of State (2012a)
Statement of change of address or agent; withdrawal of a trade name	$10	First holiday	Colorado Secretary of State (2012a)
Renewal of a trade name	$1 to October 31, 2011; $5 since	First holiday	Colorado Department of State (2013)
UCC financing statement or amendment	$8 when the first holiday was announced; other years were not examined	First holiday, at $4 ($1 and a $3 surcharge)	Colorado Secretary of State (2012a)

Notes. Fees are those for online filing. The first holiday ran from December 1, 2012 to February 28, 2013, the second from July 1 to October 31, 2014, and the program from July 1, 2022 to May 21, 2023. Amounts in force before 2010 are inferred from revenue per filing in a budget briefing (Colorado Joint Budget Committee Staff, 2010), and the date on which they first applied is not established. The list of filings covered by the second holiday is taken from the bulletin of October 30, 2014 issued by the Secretary of State's office, which names ten filings that form a domestic entity as returning to $50. That foreign entities, trade names, and reports were not covered is inferred from that list and from the wording of every official description (Colorado Secretary of State, 2014c; Colorado State Auditor, 2015). We consulted no dated fee schedule for 2015 to 2018 or for 2020 to 2023, and the fees shown for those years are based on the tables of December 2019 and July 2024 from the Secretary of State's office (Colorado Department of State, 2019; Colorado Secretary of State, 2026b) and on the absence of any announced change.

C.4. Report Reminders and the Adjustment for Established Companies

Three changes other than fees affected the periodic reports of all companies. The Secretary of State's office stopped mailing postcard reminders on January 1, 2012 and has sent reminders by e-mail since then. Reminders by text message began in August 2021, initially for newly formed LLCs. A new online process for filing reports, with reminders by e-mail or text message, was launched on August 3, 2022 (Colorado Secretary of State, 2021a, 2022a). The late fee and the fee to cure a delinquency also changed (Table A7).

The end of postcard reminders is visible in first reports. Among ordinary LLCs formed in the six months from August 2010 to January 2011, the monthly share that filed a first report on time averaged 45%. Among those formed in each of the months from February to May 2011, whose first reports fell due after the last postcards had been mailed, the share was between 23% and 39%, and among those formed in the following four months it averaged 49%. We observed this decline while calibrating the comparison for the holidays, and Addendum 2 declared it exploratory (item C9) [E]. Because of this decline, the sample for registry-based outcomes begins with companies formed in June 2011, so that no reference run contains a month whose first reports fell due while reminders were sent by postcard or shortly after they

stopped (see Reference Runs and Inference for the Fee Holidays, below). For the same reason, the plan's tests are also reported using only those reference pairs that are unaffected by such a cohort or by a holiday (Table A44; Figure A5). The decline is also relevant to the program, because reminders to new LLCs changed again in 2021 and 2022.

To remove the component that is common to all reports due in a given month, each new company's outcome is measured net of the rate among established companies whose reports fall due in the same month, where established companies are domestic LLCs in their third or later reporting year that filed the previous year's report on time. Companies formed since 2010 are drawn from the panel constructed for the plan, excluding bulk groups. Because that panel contains few companies that were three years old in 2013 and 2014, we add domestic LLCs formed from 2000 to 2009, for which the registry's formation date determines the anniversary month and reports are observed from 2010. Report months before April 2011 are not used, because the previous year's report is not fully observed for them. Section 6.5 shows that the estimate for the holidays depends on this adjustment and reports the estimate with the full adjustment, with the adjustment at the weight estimated from months unaffected by any holiday, and without the adjustment. The adjustment has a similar effect in the months that preceded the first holiday, when the fee was $50. For companies formed from June to November 2012, the statistic that Addendum 2 specified is −1.86 percentage points with the adjustment and −0.44 without it, because established companies filed on time 1.41 percentage points more often than in the adjacent years in the months when the first reports of that cohort fell due. None of the 60 six-month runs of unaffected months yields an adjusted value as low, and five yield one as large in absolute value. The run from June to November 2012 is itself affected in two ways: its last month is the month in which the first holiday was announced, and the same months of 2011, with which it is compared, include October and November, whose companies' first reports fell due during the holiday. For June to August 2012, to which neither applies, the statistic is −2.81 percentage points with the adjustment and −1.40 without it, and none of the 73 three-month runs of unaffected months yields an adjusted value as low [E]. Section E.8 reports the corresponding sensitivity of the estimates for the 2022–23 fee-reduction program.

C.5. Bulk Groups, Keys, and Filer Types

We construct three keys for each LLC formation filing: the street number of the principal address together with its five-digit ZIP code; the same elements of the registered agent's address; and the agent's name, which is the organization's name or, for a person, the first and last name together with the agent's ZIP code. Seven-day blocks are counted from July 1. A key and a block form a burst if the block contains at least 50 LLC formation filings with that key and at least 10 times the larger of one and the key's median count per block over the cohort year, the preceding year, and the following year, where blocks without filings count as zero; for the first and the last cohort years in the data, the median is taken over those of the three years that the data cover. A filing belongs to a bulk group if any of its keys has a burst in its cohort year, and every other filing is an ordinary filing. The rule uses counts only and was specified in the plan, together with the counts that it produced, after counts by address and by agent had been seen and with the knowledge of outcomes that Section B.3 describes. With thresholds of 25 and 100, the rule assigns 20,737 and 17,459 of the program's filings to bulk groups, compared with 19,789 at the threshold of 50. Keys that meet the rule are combined into one group when a filing has two of them (Table A47), and we refer to the largest group of the program as the largest bulk filer. Keys are never written to the result files, and the paper identifies filers by rank or by type.

The same keys define the filer types of Section 4.2. An ordinary filing is made *through a formation service* if its registered agent is an organization. If the agent is a person, the filing is that of a *repeat filer* when the agent's name key or the principal address key appears on an earlier ordinary LLC filing received in the preceding 365 days, and that of a *new filer* when neither key does. Addendum 2 defined a fourth type, which consists of filings whose principal address had twenty or more ordinary LLC filings in the preceding 365 days. Each filing is assigned to the first type that it fits in the following order: formation service, address with twenty or more filings, repeat filer, new filer. A filing that fits none of these types is classified as other. In the addenda, the formation-service type is called *organization agent*, the repeat-filer type *additional company*, and the new-filer type *first company in twelve months* (Table A4). The types used in the analysis of the end of the program are those of Addendum 1 and are defined differently (Table A45, Panel C).

The agent and the addresses are the values recorded in the registry in October 2026 and not those at formation.

According to the replication audit, about one-fifth of the LLCs formed in each of the three years around the program have a later filing that changes one of these fields (Table A6, row 1), so that a company that changed to a commercial agent after its formation is classified under that agent. Names that are new to the registry are defined as person-agent keys under which no entity was formed earlier.

For Table A48, Colorado ZIP codes are divided into thirds by adjusted gross income per return in 2021, weighted by the number of returns. Entity names are classified by keywords: property and holding (stems such as *propert*, *holding*, *real estate*, *invest*, *rental*, *capital*, *ventures*); building trades; transport; personal and business services (*consult*, *services*, *solutions*, *cleaning*, *marketing*, *design*, and similar stems); food and drink; retail; and whether the name contains a digit. The first name of a person who is the registered agent is classified as female or male if at least 90% of the births recorded under that name, in a public list of the thousand most common names of each sex in each year, were of that sex. The complete lists are provided in the code.

C.6. Loan Records and Record Linkage

Section 6 uses three lending records, each created by a party other than the company (U.S. Small Business Administration, 2024, 2026). Addendum 3 specified how these records are used [A3].

Sources and coverage. The first record is the loan-level file of the Paycheck Protection Program (PPP) in its release of September 30, 2024, which lists every loan approved from April 2020 to May 2021. A borrower was required to be in operation on February 15, 2020 and borrowed against its payroll or, if it had no employees, against the owner's income from the business. In this file, 192,267 loans list a Colorado borrower. The second record consists of the loan-level files of the 7(a) and 504 programs of the Small Business Administration (SBA) as of June 30, 2026, for loans approved from October 2009, which contain 26,927 loans to a Colorado borrower. We refer to a loan guaranteed under either program as an SBA loan. The third record consists of the Uniform Commercial Code (UCC) financing statements described in the next subsection, here those filed at any time up to the snapshot. These statements are filed by a lender and not by the company, but they are maintained by the Secretary of State's office, which also maintains the registry.

Linkage rule. The loan files contain no registry identifier, and loans are linked to entities by name. Names are standardized by converting them to upper case, writing out the ampersand, and removing punctuation. The full form of a name retains the words that designate the legal form, each in one spelling. The short form omits those words from the end of the name and the word *the* from its beginning. A borrower's name that includes a second name under which the borrower does business yields two names. The candidates for a loan are the entities of any type that were formed on or before the day on which the loan was approved and whose short name equals a short name of the borrower. A trade name that an entity had registered by that day counts as a name of the entity. Among the candidates, those matched on their own name take precedence over those matched on a trade name. We then retain the candidates whose full name equals the borrower's, if there are any; among those, the candidates whose ZIP code, of the principal or the mailing address, equals the borrower's, if there are any; and among those, the most recently formed entity. If two or more of these entities were formed on that same day, the loan is left unlinked. A loan is linked to at most one entity. The rule uses names, ZIP codes, and dates of formation and no other information; in particular, it uses neither the status nor any outcome of the entity. The rule was written and run before Addendum 3 was hashed, and the hashes of the two files of links are recorded in the addendum.

Linkage rates. The rule links 131,363 of the PPP loans, or 68.3%, to 97,983 entities and leaves 46 unlinked because of a tie. The share of loans linked is 87.5% among loans whose borrower is described as a limited liability company, 90.0% for corporations, and 90.7% for S corporations. It is 14.0% for sole proprietorships, most of which borrowed in a person's name, 10.5% for the self-employed, and 5.6% for independent contractors. Of the links, 87% match on the full name and 72% on the ZIP code, 84% have a single candidate, and 826 are made through a trade name. Of the SBA loans, the rule links 23,780, or 88.3%, to 18,944 entities: 88.9% of loans under the 7(a) program and 84.1% under the 504 program, and 73% of loans approved in fiscal year 2010 compared with 94% of those approved in 2026. Of these links, 93% match on the full name and 63% on the ZIP code, 84% have a single candidate, and 8 loans are left unlinked because of a tie. These counts include the 3,221 loans marked as canceled and the 176 marked as not funded, which are

excluded from the outcomes.

Outcomes and sample. Each outcome is an indicator variable defined at the company level. The primary outcome is that the company is linked to at least one PPP loan. The other outcomes are a PPP loan on which two or more jobs were reported; an SBA loan approved on or after the day on which the company was formed and by June 30, 2026; either federal loan; a UCC financing statement naming the company as debtor at any time to October 2026; and any of the three records. The sample consists of ordinary LLCs formed from January 2010 to December 2018, and from January 2011 in the analysis by filer type. Companies formed in 2019 are excluded, as Addendum 3 specified. The addendum gave two reasons. The first, that a PPP loan required a payroll in 2019, is incorrect: a borrower that was not in business in the first half of 2019 could base its loan on the payroll costs of January and February 2020, and a self-employed borrower without employees was eligible (United States Code, 2020; U.S. Small Business Administration and U.S. Department of the Treasury, 2020). The second is that the loan rate of the companies formed in 2019 is lower than that of 2018 (4.7 and 5.9%), which would enter the reference runs as curvature. The difference has a substantive source. A borrower had to be in operation on February 15, 2020, and a company formed in 2019 had existed for little more than a year at most on that date, so that the cohorts of 2019 are not comparable with the earlier cohorts for which they would serve as a reference. The exclusion therefore stands on the second reason, and it was specified before the outcome was tabulated. The estimates for the holidays do not use the months of 2019; their exclusion removes the runs of 2018, whose reference year is 2019, from the reference runs. With those runs admitted, the pooled standard error for the PPP loan is 0.24 percentage points with 10 degrees of freedom, compared with 0.17 with 7, and the confidence interval for ρ runs from −0.92 to 0.79 [E] (Table A6, row 9). When the addendum was hashed, no rate of any of these outcomes had been computed for companies formed from November 2012 to May 2013 or from June 2014 to March 2015. One lapse in the calibration, which concerned an outcome that the addendum does not use, is described in Table A6 (row 8).

Interpretation of a link. A PPP loan indicates a business with a payroll or an owner's income in 2020 or 2021, six to eight years after the holidays. It does not capture a business that existed and closed before 2020. A business whose owner borrowed under a personal name is outside the set of candidates of the rule, and Section D.4 links such loans to the companies for which the borrower is the registered agent [E]. An SBA loan indicates a business to which a bank was willing to lend, and 0.8% of the companies in the sample have one. A link made by name may be incorrect, and the rule may miss a true link. The estimates assume that these errors do not differ between companies formed during a holiday and those formed a year earlier or later. In a sample of 1,177 pairs of records read by AI readers that were not shown the cohort of the company, 98.0% of the links of the holiday cohorts and 97.4% of the links of the adjacent years were judged to join the same company, the links missed amount to 4.2 and 3.3 per hundred links made, and ρ corrected for both is 0.16, compared with 0.07 (Section 6.4; Table A38, Panel D) [E]; no person has read the pairs. In the loan file, some characters in the names of 787 borrowers are written as HTML entities, which the rule does not decode; decoding them adds 637 links and leaves ρ at 0.07 [E]. When links are restricted to those that match on both the full name and the ZIP code, the rate that the holiday cohorts would have had in the absence of marginal registrations is 3.2%, compared with 5.2% with all links, and ρ is 0.13, compared with 0.07 (Section D.2; Table A24).

The loan files identify borrowers by name and are not distributed. The scripts that retrieve and link them are distributed, and the files of links contain no names.

C.7. UCC Financing Statements and Secured Credit

From the UCC filings maintained by the Secretary of State's office, we retain original financing statements, excluding amendments and other types of lien, together with their organizational debtors. A debtor is linked to a registered entity if the record contains the entity's identification number, or if its name, after removing punctuation and standardizing designators such as "LLC" and "Inc.," equals that of a registered entity formed on or before the date of the statement. When several entities share the name, the most recently formed entity is selected. *Secured credit* equals one if a linked statement was filed within 730 days of the day on which the formation filing was received. The outcome of Addendum 3 uses the same statements at any time up to the snapshot. The fee for filing a statement was $8 and was reduced to $4 during the first holiday; Section D.2 examines the consequences for the outcome.

The replication audit of the first draft examined this linkage. More than nine-tenths of the links underlying the outcome are made by name, using the entity's name as recorded in October 2026, and 4.3% of name-matched records have more than one candidate. When links are restricted to unique names, the pre-specified estimate is −0.98 percentage points (s.e. 0.33), compared with −1.02 (0.34). When links are made by identification number alone, the base rate falls to 0.24% and the estimate is −0.07 percentage points (0.19). The measure therefore depends on lenders recording the borrower's exact registered name, which they have reason to do because a UCC financing statement is effective only under that name.

Retention of lapsed statements. The outcome of Addendum 3 counts a statement filed at any time, and it is therefore complete only if the file retains statements that have lapsed. A financing statement lapses at the end of its period of effectiveness unless a continuation is filed, and the file records the date of lapse; for the statements filed in 2012, the median interval between the filing date and the lapse date is 5.0 years. The rules of the filing office commit it to retain a lapsed record "for at least two years after lapse" (Colorado Secretary of State, 2013b, Rule 4.14), which does not establish that older records remain. Three features of the file establish that they do for the cohorts of the sample [E]. First, of the 198,723 initial financing statements filed from 2012 to 2015, 83% have passed their lapse date without a continuation and are in the file, and the proportion is similar for each year of filing from 2006 to 2019 (Table A8). Second, the description of the dataset on the open-data portal on which the Secretary of State's office publishes the file gives March 9, 2014 as the date of its creation and 1,077,737 as the number of records at its initial publication. The file downloaded in October 2026 contains 1,076,141 filings dated on or before that day, or 99.85% of that number, and the corresponding proportion for the file of debtors is 99.87%. The description also carries a later publication date, November 22, 2016, and the filings dated on or before that day are 129% of the recorded number. The recorded number therefore refers to the file as it stood in March 2014, and almost no record of that date has been removed since. Third, the lapse dates in the file begin in May 2010: 5,190 statements lapsed in June 2010, 866 in May 2010, and 528 on all earlier dates together. Statements that lapsed before then are absent, and the file accordingly holds an average of 7,057 statements for each year of filing from 2000 to 2004, nearly all of them continued, compared with 47,632 for each year from 2006 to 2011. The earliest companies of the sample were formed in January 2010, and a statement that names one of them could not lapse before January 2015. For every cohort of the sample, the file therefore contains the statements that have lapsed as well as those in effect. The proportion of 83% is a share of the statements now in the file and not a rate of retention. Its similarity across years of filing shows that lapsed statements are not removed as they age, and the second feature shows that almost none has been removed since March 2014.

Table A8. Financing Statements in the File, by Year of Filing

Year of filing	Initial financing statements in the file	Continued, percent	Lapsed without continuation and in the file, percent
2000	4,564	92.2	0.1
2001	6,914	95.5	0.1
2002	7,434	97.1	2.3
2003	7,991	99.3	0.1
2004	8,382	99.5	0.0
2005	36,918	22.9	77.0
2006	54,480	16.5	83.4
2007	57,464	17.4	82.4
2008	53,011	17.6	82.4
2009	39,945	15.0	84.8
2010	39,249	14.7	85.1
2011	41,645	14.9	84.9
2012	43,429	17.7	82.2
2013	49,709	16.8	82.9
2014	52,244	16.0	83.7
2015	53,341	15.7	84.0
2016	55,865	15.8	83.8
2017	58,996	16.2	83.4
2018	54,871	18.2	81.2
2019	59,004	20.4	79.1
2020	85,948	35.4	63.9
2021	67,818	29.0	51.1

Notes. Initial financing statements in the file of UCC filings downloaded on October 4, 2026, by year of filing. A statement is continued if the file contains a continuation of it, and it has lapsed without continuation if its lapse date precedes the day of the download and the file contains no continuation. The two percentages do not sum to 100 because statements without a recorded lapse date, and those not yet lapsed, are in neither category. Statements that lapsed before May 2010 are absent from the file, so that the rows for 2000 to 2004 and part of the row for 2005 contain only statements that were continued or that have no early lapse date. Many statements filed in 2020 and 2021 had not reached their lapse date. All entries are exploratory [E].

C.8. Reference Runs and Inference for the Fee Holidays

For a fee holiday of k months, the statistic of Section 4.4 is computed for every run of k consecutive months for which neither the run nor the same months one year earlier and one year later contain a month affected by a holiday. The affected months are each holiday's announcement month, its months at \$1, the month after it ended, and the later months (the next two months for the first holiday and the next four for the second), and, for registry-based outcomes, the months of formation from October 2011 to April 2012, whose first reports fell due during the first holiday. Counts of filings use months from January 2010 to December 2019, or from January 2011 in the analysis by filer type, because a filing is classified using the twelve months that precede it. Registry-based outcomes use months of formation from June 2011 to December 2018, because the first reports of earlier cohorts fell due while reminders were sent by postcard or shortly after they stopped, and those of later cohorts fell due during the pandemic. The loan outcomes of Addendum 3 use months of formation from January 2010 to December 2018, with November 2012 to May 2013 and June 2014 to March 2015 as the affected months, and no adjustment is made for established companies. The standard error is the standard deviation of the statistic over these runs, which overlap. Confidence intervals use Student's t distribution with degrees of freedom equal to the number of non-overlapping runs minus one, and a permutation p-value is the rank of the estimate among all runs. An estimate pooled over the two holidays weights them by their number of months, treats them as independent, and uses the smaller of the two degrees of freedom.

C.9. U.K. Register Data

Each month, Companies House publishes one file that lists every company on the register on the date of the file (Companies House, 2026). Earlier files remain available, and on October 5, 2026 we downloaded the 45 files that we found, dated from October 1, 2019 to October 1, 2026. The files are monthly from October 2023 and sparse before then: October 2019; May 2020; January, February, and May 2021; September and October 2022; and August 2023. Each file contains live companies only. Section 4.1 describes the fees. The fee that applies is the one in force on the day on which an application is delivered and not on the day of incorporation. Addendum 4 specified what follows [A4]. When the addendum was written, the monthly totals of registrations were known, and the companies incorporated from January 2024 were held in a sealed file that no script had read (Section B.3).

Population. We use every company in the three numbered sequences of the register: England and Wales, whose company numbers have eight digits, and Scotland and Northern Ireland, whose numbers have a prefix. These companies account for 97.6% of the companies incorporated from July 2019 to December 2023 that appear in a file. The remainder are partnerships, overseas entities, charitable organizations, and societies, which are numbered in other series.

Registrations. Within a sequence, numbers are assigned in order of registration, so the number of registrations on a given day equals the increase in the highest number reached by the end of that day. A company that was dissolved before it appeared in any file is therefore still counted. A count of the companies seen in the files would omit such companies. From August 2019 to December 2023, the companies seen are between 93.3% and 100.0% of the registrations so counted in a month, and never more. Registrations before July 2019 are counted only from the file of October 1, 2019, and later registrations from the panel of all files (see *Outcomes and horizons* below); the two counts are identical on every day for which both are available. A company whose date of incorporation is more than three days from the median date of the 201 companies numbered around it is excluded from the calculation, because one misdated company would distort the running maximum. Nine such companies are among those incorporated before 2024, and zero among those incorporated since.

The safeguard and its outcome. With monthly files, nearly every registration is seen (99.95% or more in each month from August to December 2023). The addendum used this property as the basis for a check on the registration counts in the years for which the data had not been read. If, in the before or the after period of 2024, 2025, or 2026, the companies seen were fewer than 97% of the registrations counted from the sequence, or more than 100.05%, the sequence would be treated as having a gap or a fault in that period, and the number of companies seen in a file would be used for the dates from January 1, 2024. Both counts are computed and stored in every case. For the fee increase of 2024, the companies seen are between 99.96% and 100.00% of the registrations counted, which is within the bounds, and the sequence-based count is used. For the fee increase of 2026, the lowest value, that of the before period of 2026, is 90.1%, which is outside the bounds, so the number of companies seen is used for that fee increase (Sections 5 and 7.5; Table A31). We apply the safeguard to each fee increase separately. Under the other reading of the provision, in which a fault found in a period of 2026 also determines the count used for the test of 2024, the estimates for 2024 are unchanged to the precision reported, apart from the lower end of the confidence interval for ρ (Table A6, row 24; Table A28, Panel F).

Outcomes and horizons. Outcomes are measured in the file of October 1 of a later year (October 4 in 2023), and a company that is no longer on the register at that date has none of the outcomes. A cohort year runs from December 1 to November 30. The first horizon is the October of the cohort year plus one, and the second horizon is the October of the cohort year plus two. For a company incorporated from March to June, these horizons correspond to ages of 15 to 19 months and of 27 to 31 months. The primary outcome is that the company has non-dormant accounts on file at the second horizon. First accounts are due 21 months after incorporation, and a company that never files is struck off the register. The other outcomes are at least one charge registered, at the second horizon; either of these two outcomes; a confirmation statement on file at the first horizon; and being on the register as active at the first horizon. Non-dormant accounts are weaker evidence of business activity than a PPP loan, because a company with trivial dealings may file the simplified accounts available to the smallest companies. The panel contains companies incorporated from July 1, 2019. Registrations per day from 2015, and the outcomes of cohort year 2017 at the second horizon and of cohort year 2018 at the first horizon, are taken from the file of October 1, 2019 alone.

Types of registration at founding. Three types are defined using the first file in which a company appears. The first type is a company whose registered office is an address at which 100 or more companies in the panel were incorporated in the preceding 365 days. Such companies account for 17.6% of those incorporated from August to December 2023, and 22.5% of them had filed non-dormant accounts by October 2025, compared with 35.5% of other companies. The second type is a company at whose address ten or more companies were incorporated on the same day (12.4%; 20.6% compared with 35.0%). The third type is a company whose first industry code is retail sale by mail order or internet, or other retail sale not in stores (11.0%; 14.8% compared with 35.5%). The address at first appearance is comparable only between years with monthly files. Consequently, 2025 is the single reference year for the types, the statistics for the types have no standard error, and we present them as descriptive statistics (Table A30).

Effective working days. Registrations are expressed per effective working day. A weekday counts as one day, a weekday that is a bank holiday in England and Wales counts as 0.3 of a day for all three sequences, and a weekend day counts as zero. Registrations on weekends remain in the totals. On the 38 bank holidays from July 2019 to 2023, registrations averaged 0.29 of those on an ordinary working day.

The sealed file. The panel was written to two files. Companies incorporated before January 1, 2024 are in an open file, which every calibration read. Companies incorporated from that date are in a second file of 2,283,396 rows, covering all number series, whose hash is recorded in the addendum's configuration file and whose contents were read by no script between the writing of the file and the hashing of the addendum. The library that reads the panel does not open the second file unless the addendum and its configuration file are present and match their recorded hashes. From January 1, 2024, 2,261,088 registrations are counted in the three sequences, and 2,247,954 of those companies appear in a file. The files identify companies by name and are not distributed. The scripts that download and assemble them are distributed.

C.10. Other Registries

Connecticut's *Business Master* and *Business Filing History* files list every domestic LLC registered since 2016 and every annual report filed. Connecticut changed neither its $120 fee to form an LLC nor its $80 fee for an annual report between 2021 and 2025. Monthly counts of new LLCs are taken from the same source and, for New York, from that state's register of active corporations and LLCs. A comparison of the first annual reports of companies formed in the two states was run and is not reported (Section B.10).

Connecticut's registry also records the agent of every business. For Panel B of Table A50, we match the eight Colorado formation services with a persistent increase in filings to agents in Connecticut that are themselves businesses, using the first two words of the name after punctuation and words designating the legal form are removed. The eight services correspond to six such names, of which five belong to agents in Connecticut through which at least 20 new LLCs were formed in one of the years 2018–19 to 2024–25. In both states, the panel counts the new LLCs formed through every agent with one of those five names.

Illinois publishes no file of entities. Its Secretary of State issues quarterly statistics on business filings, and we transcribed the counts of new domestic LLCs and corporations from the 32 reports for July 2013 to June 2021. Each report gives figures for its own quarter and for the same quarter a year earlier. We use the figure that each report gives for its own quarter and retain the later report's figure as a check. Annual totals before 2015 are those reported by the Illinois Secretary of State's office.

C.11. Census Bureau Series and the State-Level Estimator

From the Census Bureau's Business Formation Statistics (U.S. Census Bureau, 2026a), we use the monthly, seasonally adjusted state series for business applications, high-propensity business applications, business applications with planned wages, and business applications from corporations; the series of business formations within four and eight quarters, which count employer businesses formed within four and eight quarters of the application; and the weekly series of business applications, not seasonally adjusted, for a check around May 21, 2023. We use the release of September 11, 2026, which incorporates the Bureau's revision of the high-propensity and corporation series. Since February 2026, these series have excluded applications associated with internet sales over their entire history. Actual counts of business

formations within four quarters end with the applications of December 2022.

The synthetic difference-in-differences estimator follows Arkhangelsky et al. (2021). Unit weights are chosen, with an intercept and a ridge penalty proportional to the standard deviation of first differences of the comparison states' outcomes, to match the treated state's path over the pre-period. Time weights are chosen to match the comparison states' means in the post-period, and the estimate is the weighted double difference. The standard difference-in-differences estimator uses equal weights, and the synthetic control estimator uses unit weights without an intercept. For inference, each comparison state in turn is treated as if it were the treated state, with the remaining comparison states as its donor pool. The standard error is the root mean square of these placebo estimates, and confidence intervals use Student's t distribution with degrees of freedom equal to the number of placebo estimates. This is the placebo procedure of Arkhangelsky et al. (2021, Algorithm 4), with the root mean square in place of the centered standard deviation and Student's t in place of the normal quantile. Both modifications widen the confidence interval. The procedure assumes that the series for Colorado is as variable as those of the comparison states. For Colorado's program, the comparison states are the 46 others, including the District of Columbia and excluding California, Montana, Delaware, and Wyoming, and the pre-period is the 90 months from January 2015. The largest weights for business applications are on Utah, Oregon, Kansas, Massachusetts, New York, and Washington, and the largest of them is 6.7%.

The number of additional applications is $\sum_t B_t(1 - e^{-\hat{\tau}})$ over the months of an episode, where B_t is the number of applications in the state in month t. Dividing this number by the number of additional registrations of the entity types whose fee was reduced yields applications per additional registration. The confidence interval specified in the plan and reported in Table A51 reflects the error in the numerator only, and Section E.9 reports confidence intervals that also reflect the error in the number of additional registrations [E].

D. Supplementary Analyses

This appendix reports in full the analyses that the main text summarizes: the construction of the confidence sets (Section D.1); the other lending records, the types of filer, and the corporations of the Colorado holidays (Section D.2); the reference period for the holidays (Section D.3); the loans made in the name of a company's registered agent (Section D.4); the record linkage, the classification of filers, and the inference (Section D.5); the sensitivity of the U.K. test (Section D.6); and the 2022–23 program (Section D.7). Passages marked [P] or [A1] to [A4] report what the plan or an addendum specified. Everything else was computed after the results of the pre-specified tests were known and is exploratory [E].

D.1. Confidence Sets for the Relative Rate

Two constructions. The relative rate is a ratio of two estimates, and its denominator, the share of registrations that respond to the fee, has sampling error. The plans specified an interval formed from joint draws of the errors of the numerator and the denominator, and the scripts locked with them take the 2.5th and 97.5th percentiles of the ratio over the draws. The ratio is undefined at a denominator of zero and changes sign with the denominator, and the locked scripts set aside the draws in which the denominator is near zero or has the opposite sign before they take the percentiles: in Colorado, the draws with a marginal share of 0.01 or less, and in the United Kingdom, the draws in which registrations decline by less than half a log point. Addendum 3 states this step, which the scripts of Addendum 2 had applied without stating it. Addendum 4 says of such a draw that it "has no removed registrations to speak of" and provides that an interval is reported only if at most 2.5% of the draws are of that kind; it does not state that the percentiles are taken over the remaining draws (Table A6, row 20). We refer to the result as the percentile interval over the draws retained. An interval that conditions on a subset of the draws need not cover at its nominal rate.

The second construction follows Fieller (1954) and uses every draw: a value ρ_0 belongs to the set unless a two-sided test at the 5% level rejects the hypothesis that the numerator equals ρ_0 times the denominator. In Colorado, the hypothesis $\rho = \rho_0$ in equation (5) is the linear hypothesis $d - xs = 0$ with $x = (\rho_0 - 1)\mu_0$, where d is the statistic for the share of companies with the record, s is the marginal share, and the inframarginal rate μ_0 is treated as fixed, as it is in the locked scripts. The scripts for the lending records draw the errors of d and s with standard errors σ_d and σ_s,

correlation c, and one common scale with the degrees of freedom of the reference runs. Under that model the statistic

$$T(x) = \frac{d - xs}{\sqrt{\sigma_d^2 - 2xc\,\sigma_d\sigma_s + x^2\sigma_s^2}} \tag{10}$$

has Student's t distribution. The set of values x for which $|T(x)|$ does not exceed the critical value is the solution of a quadratic inequality, and it is an interval whenever the marginal share differs from zero at the level of the test. The scripts for the registry outcomes draw the two errors independently, each with its own scale, and for those estimates the set is computed from the draws: a value x is retained if the share of draws in which $d - xs$ is positive lies between 2.5% and 97.5%. In the United Kingdom, the hypothesis $\rho = \rho_0$ in equation (6) is $L(\rho_0) = (e^{-E_B} - 1) - \rho_0(e^{-E_N} - 1) = 0$. With the joint draws of the locked script, in which two normal errors with a fixed correlation are each divided by a scale with its own degrees of freedom, a value ρ_0 is retained if the share of draws with $L(\rho_0) > 0$ lies between 2.5% and 97.5%. The set is bounded when fewer than 2.5% of the draws show an increase in registrations. A rule of this form draws no distribution under the hypothesis; it reads the probability from draws centered on the estimates. At $\rho_0 = 1$ in the United Kingdom, where the hypothesis is that the share did not change, the rule is the t test of that change. At other values its coverage must be computed, and the paragraphs on the United Kingdom below report simulations of it and a second test whose null distribution is simulated.

Colorado. Table A9 reports both constructions for 22 Colorado ratios: the six outcomes of the pre-specified comparison, the PPP loan and the broadest record in each holiday, the same two outcomes under the alternative reference periods of Table A11, and two measures of registration maintenance. In each case at most 0.13% of the draws were set aside, the set by test inversion is an interval, and it agrees with the percentile interval to within 0.03 at each end. The text therefore reports the Colorado intervals as pre-specified. Other estimates for the pooled holidays on the same sample use the marginal share of one of these rows, so that nearly the same proportion of their draws is set aside.

The result files store the inputs of the draws for 851 Colorado intervals, which cover every outcome, holiday, type of filer, and version that the scripts of the addenda compute; where a file does not store the degrees of freedom of its draws, they are recovered as the value at which recomputed percentiles are closest to the stored interval. For 843 of them the set by test inversion is an interval, and among these the upper ends of the two constructions agree to within 0.05 in 98.6% and the lower ends in 84.0%. The constructions differ where the marginal share is imprecisely estimated, because the draws set aside are then more numerous and the set by test inversion extends further than the percentile interval, usually at its lower end. Four groups of the estimates printed in this paper are affected, and their tables show the percentile intervals of the scripts.

First, for the first holiday alone, the marginal share is small in the estimates net of the flanking months (Table A19, Panel C; Table A24), in those for the later months of the holiday (Table A21, Panel C), and in those for the whole window (Table A25, Panel C). In these 18 estimates the upper ends of the two constructions agree to within 0.04, and the lower end of the set lies below that of the interval. For the PPP loan net of the flanking months, the interval runs from −4.47 to 0.73 and the set from −9.46 to 0.73. Second, for filings through formation services (Table A22), both ends differ. For the PPP loan with the two holidays pooled, the interval runs from −0.28 to 1.71 and the set from −0.28 to 1.82. For either federal loan in the first holiday alone, the interval ends at 0.83 and the set at 1.05, so that the interval excludes one and the set does not; the text draws no conclusion from the estimates for formation services, which it describes as the least conclusive case. Third, for new filers in the first holiday alone (the same table), the upper ends agree, and the lower end for the PPP loan is −4.93 in the interval and −5.68 in the set. Fourth, for profit corporations outside bulk groups with the two holidays pooled (Section 6.1; Table A34, Panel C; Table A38, Panel B), the interval for the PPP loan runs from −0.64 to 0.80 and the set from −0.83 to 0.80. For those corporations in the first holiday alone, 5.5 to 7.9% of the draws are set aside and the set is unbounded in each of the eight cases computed; the paper reports no interval for them. Where the text draws a conclusion from a Colorado interval, the conclusion is that the interval excludes one, and in each such case the upper ends of the two constructions agree.

Table A9. Percentile Intervals over the Draws Retained and Confidence Sets by Test Inversion

A. Colorado

	ρ	Percentile interval, draws retained	Draws set aside, percent	Set by test inversion
Colorado, pre-specified comparison [A3]				
PPP loan	0.07	[−0.57, 0.60]	0.002	[−0.57, 0.60]
PPP loan reporting two or more jobs	−0.08	[−0.89, 0.52]	0.002	[−0.89, 0.53]
SBA loan	0.00	[−1.28, 1.00]	0.002	[−1.29, 1.00]
Either federal loan	0.02	[−0.56, 0.47]	0.002	[−0.56, 0.47]
UCC financing statement	−0.02	[−0.83, 0.58]	0.002	[−0.83, 0.58]
Any of the three records	−0.02	[−0.65, 0.41]	0.002	[−0.65, 0.41]
PPP loan, first holiday	−0.18	[−1.61, 0.75]	0.040	[−1.62, 0.74]
PPP loan, second holiday	0.21	[−0.55, 0.81]	0.005	[−0.55, 0.81]
Any of the three records, first holiday	−0.44	[−2.03, 0.33]	0.040	[−2.05, 0.33]
Any of the three records, second holiday	0.20	[−0.53, 0.67]	0.005	[−0.52, 0.67]
Colorado, other reference periods [E]: PPP loan; any of the three records				
All unaffected months, linear trend: PPP loan	0.19	[−0.27, 0.68]	0.006	[−0.27, 0.68]
any of the three records	0.31	[−0.24, 0.91]	0.006	[−0.24, 0.92]
All unaffected months, quadratic trend: PPP loan	0.07	[−0.34, 0.42]	0.002	[−0.34, 0.42]
any of the three records	0.12	[−0.30, 0.43]	0.002	[−0.31, 0.43]
Whole window: PPP loan	−0.41	[−1.22, 0.32]	0.088	[−1.23, 0.32]
any of the three records	−0.23	[−1.17, 0.46]	0.088	[−1.19, 0.45]
Holiday less flanking months, adjacent years: PPP loan	0.81	[−0.19, 1.78]	0.096	[−0.20, 1.79]
any of the three records	0.26	[−0.38, 0.78]	0.096	[−0.38, 0.78]
Holiday less flanking months, every other unaffected year: PPP loan	0.67	[0.24, 1.17]	0.008	[0.24, 1.17]
any of the three records	0.34	[−0.04, 0.67]	0.008	[−0.04, 0.67]
Colorado, registration maintenance [A2]				
First periodic report on time	0.44	[−0.10, 0.74]	0.128	[−0.10, 0.74]
In good standing, October 2026	0.35	[−0.19, 0.61]	0.128	[−0.20, 0.61]

Table A9. (continued)

B. United Kingdom

	ρ	Percentile interval, draws retained	Draws set aside, percent	Set by test inversion
United Kingdom, non-dormant accounts				
Pre-specified comparison [A4]	0.00	[−1.62, 0.42]	1.148	[−2.5, 0.42]
Net of companies struck off, July to October [A4]	−0.06	[−1.80, 0.37]	1.152	[−2.8, 0.37]
Base period: the six weeks before the announcement	0.28	[−0.39, 0.95]	0.330	[−0.4, 0.95]
Without the reference year 2022	0.01	[−1.71, 0.41]	1.115	[−2.6, 0.41]
Earlier base period and without 2022	0.34	[−0.39, 0.76]	0.462	[−0.5, 0.77]
After period beginning 16 days before the fee increase	−0.09		3.265	≤0.51 or ≥19.72
Reference years with equal maintenance fees, their own s.e.	0.10	[−1.95, 1.22]	1.150	[−2.8, 1.33]
The same two years, s.e. from 36 placebo dates	0.10	[−1.51, 0.45]	1.154	[−2.5, 0.45]
Accounts ever filed, s.e. from 36 placebo dates	−0.16	[−2.39, 0.28]	1.143	[−3.7, 0.28]
Accounts ever filed, s.e. of its two reference years	−0.16	[−2.66, 2.47]	1.145	[−3.6, 3.18]
One scale for both errors, eight degrees of freedom	0.00		1.156	[−2.6, 0.34]
One scale for both errors, three degrees of freedom	0.00		3.394	≤0.41 or ≥6.27
United Kingdom, other outcomes				
A charge registered by a lender [A4]	−0.35	[−3.51, 0.25]	1.144	[−5.3, 0.25]
Non-dormant accounts or a charge [A4]	−0.01	[−1.65, 0.42]	1.148	[−2.6, 0.42]
First confirmation statement filed [A4]	0.22	[−1.51, 0.59]	1.144	[−2.5, 0.59]
On the register and active, 15 to 19 months [A4]	0.22	[−1.52, 0.62]	1.146	[−2.5, 0.61]
Still on the register, 27 to 31 months	0.09	[−1.64, 0.47]	1.157	[−2.6, 0.46]
Accounts of any type on file	0.10	[−1.59, 0.47]	1.153	[−2.6, 0.47]
Dormant accounts on file	0.43	[−1.26, 0.86]	1.159	[−2.2, 0.86]
Micro-entity accounts on file	0.12	[−1.35, 1.03]	1.148	[−2.0, 1.08]
Total exemption full accounts on file	−0.25	[−2.23, 0.62]	1.159	[−3.2, 0.64]
Still on the register, earlier base period	0.30	[−0.47, 0.83]	0.331	[−0.5, 0.83]
Dormant accounts on file, earlier base period	0.28	[−1.01, 0.77]	0.322	[−1.1, 0.77]

Notes. Each row reports an estimate of ρ with two 95% sets. *Percentile interval, draws retained*: the 2.5th and 97.5th percentiles of the ratio over the joint draws that the scripts retain, which are those with a marginal share above 0.01 (Colorado) or with a decline in registrations of at least half a log point (United Kingdom). *Draws set aside*: the percentage of draws not retained. *Set by test inversion*: the values that a two-sided test at the 5% level does not reject, formed from every draw. For the Colorado lending records the set follows from equation (10); for registration maintenance and for the United Kingdom it is computed from the draws on a grid of 0.001. The U.K. sets use 4 million draws, and their lower ends, which carry simulation error, are given to one decimal. The Colorado rows correspond to Tables 3, A11, and A40, with the two holidays pooled unless a row states otherwise, and the U.K. rows to Tables 4 and 5. *One scale for both errors*: the two errors are divided by a single scale with the stated degrees of freedom and not by separate scales with eight and three. No interval was reported under this model. With three degrees of freedom more than 2.5% of the draws are set aside, so that the reporting rule of Addendum 4 yields no interval, and the set by test inversion consists of two rays. *After period beginning 16 days before the fee increase*: the after period is extended to include the filings advanced into the last 16 days at the old fee (Section D.6); more than 2.5% of the draws are set aside, so that no interval was reported, and the set again consists of two rays. The estimates and percentile intervals in the rows marked [A2], [A3], or [A4] are those that the addenda specified. Every set by test inversion and every other row is exploratory [E].

United Kingdom. In the pre-specified U.K. comparison, 1.15% of the draws are set aside, and 87% of them imply a ratio above one-half, so that setting them aside could have lowered the upper end of the interval. Table A10 reports the calculations that Section 7.2 summarizes. Panel A gives three sets formed from the same joint draws: the percentiles over the draws retained, the percentiles over all draws, and the set by test inversion. The set by test inversion and the percentile interval over the draws retained have the same upper end, so that setting draws aside did not lower it, and the set extends further at the lower end. The percentile interval over all draws differs from both because of the 1.0% of

draws in which registrations rise. In 96% of those draws the ratio exceeds one, so that a percentile of the ratio counts them among the largest values of ρ. The statistic $L(\rho_0)$ changes sign with the denominator: in every draw it records whether the number of companies with the outcome is higher or lower than ρ_0 implies, and it counts these draws with the smallest values. The percentile interval over all draws is therefore not the inversion of a test, and its upper end of 0.51 reflects those draws. Panel A also gives the three sets computed from the estimates and standard errors rounded to three decimals and the correlation rounded to two, which shows that they can be reproduced from the printed values. Panel B varies the correlation between the change in registrations and the change in the share within a year, which Addendum 4 fixed at −0.72.

A second test. As a check on the rule, Panel A reports a second test whose null distribution is simulated. Let $h(n;\rho_0) = -\log\{1+\rho_0(e^{-n}-1)\}$ denote the change in the log number of companies with the outcome that ρ_0 implies when the log number of registrations changes by n, and let $D(\rho_0) = E_B - h(E_N;\rho_0)$, which is negative where $L(\rho_0)$ is positive. If $\rho = \rho_0$ and the true change in registrations is m, then $D = e_N + e_R - \{h(m+e_N;\rho_0) - h(m;\rho_0)\}$, where e_N and e_R are the errors of the estimated changes in registrations and in the share, so that the distribution of D under the hypothesis can be drawn from the error model of Addendum 4. The p-value is twice the smaller of the two tail probabilities of the observed value. With m set at the estimated change, the test retains every value of ρ_0 from zero to 0.44 and rejects one-half ($p = 0.034$). With the p-value maximized over a 99.9% confidence interval for m and increased by 0.001, which makes it valid under that error model whatever the true change in registrations (Berger and Boos, 1994), the test retains every value from zero to 0.46 and rejects one-half ($p = 0.037$); with intervals of 99% and 99.99% the p-values at one-half are 0.045 and 0.038. These four p-values are computed from one billion draws, with a simulation standard error of at most 0.00001, because with the 4 million draws of Panel A three of them lie within one simulation standard error of a rounding boundary in the third decimal. The set by test inversion uses the same simulated distribution, with m at its estimate, and reads the opposite tail: it retains ρ_0 when the share of that distribution below $-D$ lies between 2.5% and 97.5%, and the second test when the share above D does. The two rules coincide where the distribution is symmetric about zero, as it is at ρ_0 of zero and of one. We applied the second test to values of ρ_0 from zero to one, the range in which h is defined at every change in registrations. Panel B reports both forms of the second test at each assumed correlation. Its upper end lies 0.01 to 0.04 above that of the set by test inversion. It rejects one-half at a within-year correlation of −0.50 or stronger with m at its estimate, and of −0.58 or stronger with the p-value maximized, against −0.45 for the set by test inversion. At the correlation pooled over the placebo dates, the second test thus rejects one-half in its first form, with an upper end of 0.49, and not in its second, with an upper end of 0.51.

Panel C reports the direct test. For a hypothesized ratio λ_0 of the log change in the number of companies with the outcome to the log change in registrations, the contrast $B - \lambda_0 N$ is computed for 2024 and for each of the four reference years in which both changes are observed. The value for 2024 less the mean of the four years, divided by their standard deviation multiplied by $\sqrt{1+1/4}$, is a t statistic with three degrees of freedom. The test uses the observed spread of the contrast and therefore requires no assumption about the correlation of the two changes. Because $e^{\lambda a} - 1 \le \lambda(e^a - 1)$ for $a > 0$ and $0 \le \lambda \le 1$, the relative rate is no greater than the ratio of the two log changes when registrations decline and that ratio lies between zero and one, so that the rejection of every λ_0 from 0.35 to one implies the rejection of every ρ in that range. Across the four reference years at the date of the test, the spread of the contrast is smallest at intermediate values of λ_0 (the second column of the panel), which makes the t statistic large there. The last two columns therefore replace that spread with the spread pooled over 36 placebo dates. The ratio so formed has no exact reference distribution. It is 3.54 at one-half; every value of λ_0 from 0.35 to one lies more than 1.96 standard errors from the estimate, and every value from 0.47 to one lies beyond the critical value for three degrees of freedom.

Panel D omits each of the nine reference years in turn. The upper end of the set by test inversion is at most 0.52 except when 2017 or 2023 is omitted, when it is 0.81 or 0.89. Each of those two omissions leaves three reference years for the share, and the direct test on the three remaining years still rejects every ratio from 0.42 or 0.40 to one.

Panels E and F apply the tests at the placebo dates, taking each of the four reference years in turn against the other three, and report the simulations of coverage. At a placebo date both true changes are zero, so that every value of ρ_0 is true. Tests of five values from zero to one reject in 1.4 to 6.9% of the 144 cases by test inversion, in 0.7 to 6.9% by the

second test, and in 3.5 to 6.9% by the direct test. No placebo estimate yields a set wholly below one-half. The placebo dates are a weak check of that claim, because registrations did not decline at them and only five of the estimates yield a bounded set.

In each of 46 simulated designs, 40,000 sets of years are drawn with the standard deviations observed across the reference years at the date of the test (6.3 log points for the change in registrations and 3.0 for the change in the share), a true decline in registrations, a true value of ρ, a true correlation, and normal or heavy-tailed errors. Each construction is then applied as it is applied to the data, with the correlation assumed to be -0.72; the second test is applied, here and at the placebo dates, with m at its estimate. A simulated proportion near 95% has a standard error of 0.11 percentage points, and one near 2.5% a standard error of 0.08. These standard errors reflect the number of sets of years only. Every set and every design is evaluated with the same 20,000 draws of the errors, which adds an error that is common to the cells and is not included: a tail share of 2.5% of that many draws has a standard error of 0.11 percentage points, and the common error of a rate of coverage is of that order. With the assumed correlation and normal errors, the set by test inversion contains the true value in 95.1 to 98.8% of samples, the second test in 95.0 to 98.8%, and the percentile interval over all draws in 95.9 to 99.4%. The percentile interval over the draws retained contains it in 95.2 to 98.7% of the samples in which it is reported at the estimated decline, and in 92.2% at a true decline of 10 log points with ρ equal to one, where an interval is reported in 24% of samples. Across all designs, the lowest rate at which a set contains the true value is 89.5% for the set by test inversion and 89.5% for the second test, both when the true correlation is zero; 93.2% for the percentiles over all draws; 90.3% for the percentiles over the draws retained, among the samples in which that interval is reported; and 93.4% for the direct test, whose highest rate is 95.2%. When the true value is one-half or more (Panel F), the set by test inversion or the set of the direct test lies wholly below one-half in at most 1.2% of samples with the assumed correlation, and in 2.4% in the least favorable design, in which the true correlation is zero and the errors have heavy tails. When the true value of ρ is zero, with the estimated decline in registrations and the assumed correlation, the set by test inversion lies wholly below one-half in 49% of samples, and the direct test rejects every admissible value of one-half or more in 48%, where a value is admissible if the rate that it implies among deterred registrations does not exceed one. A set below one-half was therefore about as likely as not even if the deterred registrations never had the outcome.

Table A10. United Kingdom: The Confidence Set for the Relative Rate under Alternative Constructions

A. Three sets from the same joint draws, and a second test

	Stored estimates		Estimates as printed, rounded	
	Lower end	Upper end	Lower end	Upper end
Percentiles over the draws with a decline of at least half a log point [A4]	−1.62	0.42	−1.64	0.42
Percentiles over all draws	−1.68	0.51	−1.69	0.51
Set by test inversion, every draw used	−2.5	0.42	−2.6	0.42
Second test, null distribution simulated: values from zero to one	0	0.44		
its p-value maximized over the change in registrations	0	0.46		

B. By the correlation assumed between the two errors

Correlation of registrations and share within a year	Source	Percentile interval, draws retained	Set by test inversion	Share of draws above one-half, percent	Second test: upper end m at its estimate	Second test: upper end p-value maximized
0.00	independent errors	[−2.44, 0.59]	[−3.8, 0.58]	4.4	0.60	0.62
−0.20		[−2.22, 0.55]	[−3.4, 0.55]	3.5	0.56	0.59
−0.40		[−2.01, 0.51]	[−3.1, 0.51]	2.7	0.52	0.55
−0.55	pooled over 36 placebo dates	[−1.82, 0.47]	[−2.8, 0.47]	2.1	0.49	0.51
−0.60		[−1.77, 0.46]	[−2.7, 0.46]	2.0	0.47	0.49
−0.72	Addendum 4	[−1.63, 0.42]	[−2.5, 0.42]	1.6	0.44	0.46
−0.85		[−1.46, 0.37]	[−2.3, 0.38]	1.3	0.39	0.41
−0.98	the four reference years at the date of the test	[−1.31, 0.32]	[−2.1, 0.32]	1.1	0.34	0.36

C. Direct test on the four common reference years

Hypothesized ratio of the change in the number to the change in registrations	Spread of the four reference years at the date: s.d., log points	t (3 d.f.)	p	Spread pooled over 36 placebo dates: s.d., log points	Ratio
0.00	4.1	−0.18	0.871	4.1	−0.18
0.25	2.4	1.58	0.211	3.1	1.22
0.33	1.8	2.89	0.063	2.8	1.88
0.50	0.8	10.08	0.002	2.3	3.54
0.75	1.3	9.66	0.002	2.1	6.15
1.00	3.0	5.79	0.010	2.5	6.95

D. Omitting one reference year

Reference year left out	Years for registrations, for the share	ρ	Percentile interval, draws retained	Set by test inversion	Direct test on the remaining common years: values of the ratio not rejected
None	9, 4	0.00	[−1.62, 0.42]	[−2.5, 0.42]	≤0.35 or ≥2.89
2015	8, 4	0.00	[−1.94, 0.43]	[−4.7, 0.43]	
2016	8, 4	0.04	[−1.23, 0.44]	[−1.6, 0.44]	
2017	8, 3	−0.02	[−1.91, 0.78]	[−4.1, 0.81]	≤0.42 or ≥1.10
2018	8, 4	−0.01	[−1.99, 0.43]	[−5.1, 0.43]	
2019	8, 4	0.02	[−1.63, 0.44]	[−2.8, 0.44]	
2021	8, 3	−0.01	[−1.50, 0.52]	[−2.0, 0.52]	≤0.31 or ≥3.75
2022	8, 3	0.01	[−1.73, 0.41]	[−2.7, 0.41]	≤0.40 or ≥2.24
2023	8, 3	−0.02	[−1.93, 0.84]	[−4.4, 0.89]	≤0.40 or ≥1.05
2025	8, 4	−0.06	[−1.68, 0.39]	[−2.5, 0.39]	

Table A10. (continued)

E. Placebo dates and simulated coverage

	Value of ρ tested or simulated				
	0	0.25	0.50	0.75	1
Placebo dates: percent of 144 estimates at 36 dates without a fee change at which the value is rejected at the 5% level					
Set by test inversion	1.4	1.4	1.4	4.9	6.9
Second test, null distribution simulated	1.4	0.7	1.4	5.6	6.9
Direct test	4.2	3.5	4.2	4.9	6.9
Simulated years: percent of samples in which the 95% set by test inversion contains the true value					
True correlation −0.72, as assumed; normal errors	97.6	98.5	98.7	96.4	95.1
True correlation −0.72; errors with heavy tails	95.9	97.1	97.5	95.5	93.6
True correlation −0.40; normal errors	94.5	95.3	95.6	94.4	95.2
True correlation −0.40; errors with heavy tails	93.2	94.0	94.1	93.0	93.8
True correlation zero; normal errors	90.2	90.3	90.7	91.5	95.0
True correlation zero; errors with heavy tails	89.5	89.6	90.1	90.4	94.1
True correlation −0.95; normal errors	99.1	99.9	100.0	97.8	95.2
True correlation −0.95; errors with heavy tails	97.9	99.3	99.8	97.2	93.9
True decline of 10 log points; correlation and errors as assumed	97.5		98.8		95.1
True decline of 30 log points; correlation and errors as assumed	97.3		98.7		95.2
The same for the percentile interval over the draws retained, among the samples in which it is reported					
True correlation −0.72, as assumed; normal errors	97.9	98.7	98.4	95.8	95.2
True decline of 10 log points; correlation and errors as assumed	93.7		97.3		92.2
True decline of 30 log points; correlation and errors as assumed	97.6		98.7		95.3
The same for the percentile interval over all draws					
True correlation −0.72, as assumed; normal errors	98.7	99.2	99.1	97.7	97.2
True decline of 10 log points; correlation and errors as assumed	98.5		99.4		98.7
True decline of 30 log points; correlation and errors as assumed	97.9		98.8		95.9
The same for the second test, null distribution simulated					
True correlation −0.72, as assumed; normal errors	97.6	98.5	98.8	96.3	95.0
True decline of 10 log points; correlation and errors as assumed	97.4		98.7		95.2
True decline of 30 log points; correlation and errors as assumed	97.3		98.8		95.2
The same for the direct test on the four reference years					
True correlation −0.72, as assumed; normal errors	95.1	95.0	95.1	95.0	94.9
True correlation −0.72; errors with heavy tails	93.6	93.4	93.5	93.7	93.4
True correlation −0.40; normal errors	94.9	94.8	95.1	95.2	95.1
True correlation −0.40; errors with heavy tails	93.6	93.7	93.5	93.7	93.7
True correlation zero; normal errors	95.1	94.8	95.1	95.0	94.8
True correlation zero; errors with heavy tails	93.4	93.5	93.8	93.5	94.0
True correlation −0.95; normal errors	94.8	95.0	94.9	95.0	95.0
True correlation −0.95; errors with heavy tails	93.6	93.7	93.7	93.6	93.7
True decline of 10 log points; correlation and errors as assumed	94.9		95.0		95.0
True decline of 30 log points; correlation and errors as assumed	94.8		95.0		95.0

Table A10. (continued)

F. Simulated years in which a set lies wholly below one-half

	True value of ρ				
	0	0.25	0.50	0.75	1
Percent of samples in which the set by test inversion lies wholly below one-half					
True correlation −0.72, as assumed; normal errors	48.9	12.8	0.5	0.0	0.0
True correlation −0.72; errors with heavy tails	59.4	20.2	0.9	0.1	0.0
True correlation zero; normal errors	38.7	12.4	1.7	0.1	0.0
True correlation zero; errors with heavy tails	50.9	18.6	2.4	0.2	0.0
True decline of 10 log points; correlation and errors as assumed	4.4		0.1		0.0
True decline of 30 log points; correlation and errors as assumed	93.8		0.7		0.0
The same for the direct test: every admissible value of one-half or more rejected					
True correlation −0.72, as assumed; normal errors	47.8	12.6	0.7	0.0	0.0
True correlation −0.72; errors with heavy tails	60.1	21.5	1.2	0.1	0.0
True correlation zero; normal errors	22.7	6.6	1.1	0.1	0.0
True correlation zero; errors with heavy tails	32.7	10.1	1.5	0.2	0.0
True decline of 10 log points; correlation and errors as assumed	6.5		0.3		0.0
True decline of 30 log points; correlation and errors as assumed	91.8		0.9		0.0

Notes. The pre-specified comparison for non-dormant accounts (Table 4). None of the entries was specified in advance [E] except the first row of Panel A under *Stored estimates*, which is the interval of the locked script [A4]. Panel A, *Stored estimates*: sets formed from the stored estimates, standard errors, and degrees of freedom, with 4 million joint draws. *Estimates as printed, rounded*: the same sets from the estimates and standard errors rounded to three decimals and the correlation rounded to two. *Second test*: the test of Section D.1 whose null distribution is simulated, applied to values from zero to one; every value from zero to the upper end is retained. Panel B: the correlation is that between the change in registrations and the change in the share within a year, from which the script derives the correlation of the two errors; the fifth column is the share of all draws in which $L(0.5) > 0$, and the last two columns give the upper end of the values that the second test retains, with m at its estimate and with the p-value maximized. Each row uses 2 million draws, and its entries differ from those of Panel A by simulation error; in the row of Addendum 4 the last two columns repeat Panel A. Panel C: for each hypothesized ratio λ_0, the standard deviation of $B - \lambda_0 N$ across the four reference years (2017 and 2021 to 2023) at the date of the test, the t statistic and its two-sided p-value, and the same standard deviation pooled over the placebo dates with the ratio of the estimate to its standard error under that spread. Panel D: the number of reference years for registrations and for the share; the direct test is reported when the omitted year is one of the four common years. Panels E and F: the placebo estimates take each of the four reference years in turn as the year of a fee change at 36 placebo dates a week apart, and a value is rejected when it lies outside the set. The second test is applied with m at its estimate. The direct test is the test of the ratio λ_0 of the two log changes at the value shown. In the simulations, each row is a true correlation and a distribution of the errors, either normal or Student's t with four degrees of freedom, and each column a true value of ρ; the decline in registrations is the estimated one except in the rows that state another, which were simulated at three values of ρ. Each cell is a percentage of 40,000 simulated sets of years. Every construction assumes a correlation of −0.72.

D.2. Colorado: Other Lending Records, Types of Filer, and Corporations

This section reports the estimates for the Colorado holidays that Section 6.1 summarizes. The intervals for ρ are the percentile intervals of the locked scripts, and Section D.1 compares them with sets by test inversion.

The primary outcome. The statistics of the first and second holidays for the PPP loan (Table 3, Panel A) are more extreme than every one of their 30 and 27 reference runs, of which 11 and 8 do not overlap. Relative to the year after alone, the pooled estimate is −0.62 percentage points (s.e. 0.21), and relative to the year before alone it is −0.79. With links restricted to an identical full name and ZIP code, it is −0.41 (0.15) on a rate about three-fifths as high, with $\rho = 0.13$ (Table A24) [A3]. The pooled decline of 0.71 percentage points from an inframarginal rate of 5.2% is what equation (2) implies if the holidays added 14.7% of the cohort and almost none of the marginal registrations had a loan. An estimated 0.4 per hundred marginal registrations had a PPP loan, with a confidence interval from 0 to 3.1, compared with 5.2 among inframarginal registrations (Table A19, Panel B). Of the approximately 6,800 additional registrations during the holidays, about 350 would have had a loan if they had received loans at the inframarginal rate; the estimate is

30, and the upper end of the confidence interval is 210.

Other lending records. The share is also lower among the holiday cohorts for a PPP loan on which two or more jobs were reported, for an SBA loan, and for a UCC financing statement filed at any time up to October 2026 (Table 3, Panel A). As a proportion of its inframarginal rate, the decline is 14% for the PPP loan, 16% for a PPP loan with two or more jobs, 15% for the SBA loan, 15% for the UCC financing statement, and 15% for any of the three records, and the marginal share is 14.7% (Table A34, Panel A) [E]. Equation (2) implies this pattern if marginal registrations rarely have a record of any type, because each share then declines by the marginal share whatever its level. The agreement is in part mechanical, since the PPP loan with two or more jobs is a subset of the PPP loan and any of the three records is their union. The PPP loan and the UCC financing statement, however, are separate records. The first is a loan to a borrower that reported a payroll or an owner's income in 2020 or 2021, and the second records borrowing against collateral at any time up to October 2026; 5.3% of LLCs have the first and 7.7% the second. The SBA loan, which fewer than one company in a hundred has, is too infrequent to be informative by itself, as its confidence interval for ρ runs from -1.28 to 1.00. For any of the three records, the broadest record available for Colorado, the share declines by 1.72 percentage points (s.e. 0.26) from an inframarginal rate of 11.5%. The relative rate is $\rho = -0.02$, with a confidence interval from -0.65 to 0.41; the rate among marginal registrations is 0.0 per hundred, with a confidence interval that extends to 4.7. For the second holiday alone, ρ is 0.20 (-0.53 to 0.67). For every cohort of the sample, the file of financing statements contains the statements that have lapsed as well as those in effect (Section C.7; Table A8).

The fee for financing statements. The first holiday reduced every online filing fee, including the fee for a UCC financing statement, which was reduced from \$8 to \$4; the second holiday applied to formation filings alone (Table 1). The lender files the statement, and a lower fee would, if it had any effect, raise the share of a cohort with a financing statement. Such an effect would be opposite in sign to the decline that we observe. Four results indicate that the reduced fee did not affect the outcome. First, the number of financing statements filed during the first holiday differed from the number in the same months of the adjacent years by -1.3 log points (s.e. 6.2), and 0.70% of the LLCs formed in the first holiday were named in a statement filed in those three months, compared with 0.78% and 0.66% of those formed a year earlier and a year later [E]. Second, when statements filed in a company's first 90 days are excluded, the share with a financing statement declines by 1.13 percentage points (0.26), and ρ for any of the three records is -0.03 (-0.63 to 0.38) [E]. Third, when only the statements filed in 2020 or later are counted, which every cohort's lenders filed at the same fee, ρ for any of the three records is 0.08 (-0.47 to 0.47) [E]. Fourth, in the second holiday alone, the share with any of the three records declines by 1.55 percentage points (0.36), and ρ is 0.20 (-0.53 to 0.67) [A3] (Table A37).

New filers. The framework does not determine whether new founders at the margin of the fee have businesses (Section 3). Among new filers, who had formed no company in the previous year, the change in the loan share is as large as among repeat filers: -0.72 percentage points (0.17) compared with -0.79 (0.28), on a smaller marginal share; for any of the three records, the changes are -1.45 (0.31) and -1.97 (0.34) (Table 3, Panel B) [A3]. An estimated 0.0 per hundred marginal registrations of this type had a PPP loan, and the upper end of the confidence interval is 3.2. Under the pre-specified estimate, almost none of the marginal registrations of new filers have a PPP loan, a result that depends on the reference period (Section 6.2). Filings through formation services are the least conclusive case, with a confidence interval that includes the inframarginal rate; for these filings the set by test inversion is wider than the percentile interval at both ends (Section D.1). The deficit among new filers does not depend on how finely addresses are matched, although the share of the additional registrations attributed to them does (Section D.5).

Corporations. Profit corporations, whose fee was reduced by the same \$49, are twice as likely as LLCs to have a PPP loan. The row that the addendum specified includes all profit corporations and gives a decline of 1.90 percentage points (0.39), or 3.2 loans per hundred marginal corporations compared with 11.9 among inframarginal corporations [A3]. That row, however, includes two episodes of bulk filing, because the rule that identifies bulk groups had been applied to LLCs only. Applied unchanged to corporations, the rule identifies 622 of the 7,543 formed in the holiday months, 619 of them in February 2013 and September 2014 and none with a PPP loan (Table A34, Panel C) [E]. Excluding these filings, corporation formations increased by 16.2 log points (4.8), as LLC formations did, and the decline in their loan share is 1.01 percentage points (0.26), or 5.1 loans per hundred marginal corporations compared

with 11.9, a relative rate of 0.43 with a confidence interval from −0.64 to 0.80; the set by test inversion runs from −0.83 to 0.80. With the threshold of the rule halved, the decline is 0.89 percentage points (0.39) and ρ is 0.44 (−0.92 to 1.02). For the entity type with the highest loan rate, the interval thus excludes one when the rule is applied as it was to LLCs and reaches one when its threshold is halved, and it does not exclude zero in either case.

First month and later months, and an index of founding characteristics. Addendum 3 specified two further tabulations [A3]. First, the decline in the loan share is no smaller among companies formed in the first month of each holiday than among those formed later (Table A21). Second, an index of founding characteristics, fitted to what the registry records at founding and of the type used to adjust registration counts for quality (Guzman and Stern, 2020), increased by as much as the unadjusted number of registrations when summed over the filings of the holiday months (Table A23). The characteristics that the registry records at founding therefore do not distinguish marginal registrations from inframarginal registrations, although the index is a weak predictor of the loan outcome, and richer characteristics might distinguish them.

D.3. Colorado: The Reference Period

Section 6.2 states the estimates under six reference periods and summarizes the evidence on the source of the deficit in the flanking months. This section reports that evidence in full. Row 1 of Table A11 is the pre-specified comparison [A3]. Addendum 3 also specified the statistic for the flanking months and for the months that follow the month after each holiday (Table A20) [A3]. Every other analysis of this section was conducted after the results of the pre-specified test were known [E].

Figure 2 shows the two records month by month. For the broader record, the share in each of the seven holiday months is between 1.1 and 2.7 percentage points below the share in its reference months, and in the four flanking months the difference from the reference months is between −0.9 and +0.1 percentage points. Panel B of Table A11 reports the estimate when the statistic for the flanking months is subtracted with a weight between zero (the pre-specified comparison) and one (the comparison with the flanking months).

Table A11. Colorado: Estimates under Alternative Reference Periods

A. Six reference periods

	Reference period	Registrations, log points	Share with a PPP loan, pct. points	Companies with a PPP loan, log points	Marginal relative to inframarginal, ρ: PPP loan	Marginal relative to inframarginal, ρ: Any of the three records
1	Same months of the adjacent years [A3]	+16.0 (1.1)	−0.71 (0.17)	+1.4 (3.7)	0.07 [−0.57, 0.60]	−0.02 [−0.65, 0.41]
2	All unaffected months, linear trend	+15.7 (2.1)	−0.60 (0.17)	+3.9 (3.0)	0.19 [−0.27, 0.68]	0.31 [−0.24, 0.91]
3	All unaffected months, quadratic trend	+17.0 (1.9)	−0.76 (0.13)	+1.6 (2.6)	0.07 [−0.34, 0.42]	0.12 [−0.30, 0.43]
4	Whole window, same months of the adjacent years	+9.6 (0.8)	−0.68 (0.11)	−4.1 (2.2)	−0.41 [−1.22, 0.32]	−0.23 [−1.17, 0.46]
	Holiday months less the flanking months, relative to the same contrast in:					
5	the adjacent years	+19.0 (1.9)	−0.15 (0.24)	+15.4 (4.7)	0.81 [−0.19, 1.78]	0.26 [−0.38, 0.78]
6	every other unaffected year	+21.4 (2.3)	−0.30 (0.17)	+15.2 (3.2)	0.67 [0.24, 1.17]	0.34 [−0.04, 0.67]

B. Marginal relative to inframarginal, ρ, by the weight given to the flanking months [E]

	Flanking months: Statistic (s.e.)	Flanking months: Pairs as extreme	Slope in unaffected years	Weight on the flanking months: 0 (row 1) [A3]	Weight on the flanking months: The slope	Weight on the flanking months: 0.50	Weight on the flanking months: 1 (row 5)	Interval below one up to
PPP loan	−0.56 (0.17)	0 of 239	0.06 (0.40)	0.07 [−0.57, 0.60]	0.13 [−0.83, 1.15]	0.46 [−0.33, 1.19]	0.81 [−0.19, 1.78]	0.30
Either federal loan	−0.39 (0.17)	4 of 239	−0.03 (0.38)	0.02 [−0.56, 0.47]	0.04 [−0.72, 0.75]	0.29 [−0.39, 0.92]	0.53 [−0.35, 1.32]	0.60
Financing statement	−0.09 (0.29)	181 of 239	0.37 (0.40)	−0.02 [−0.83, 0.58]	0.03 [−0.88, 0.85]	0.09 [−0.69, 0.79]	0.18 [−0.72, 0.95]	every weight
Any of the three records	−0.31 (0.25)	52 of 239	0.32 (0.40)	−0.02 [−0.65, 0.41]	0.05 [−0.58, 0.61]	0.13 [−0.39, 0.59]	0.26 [−0.38, 0.78]	every weight

Notes. Ordinary LLCs formed in the two holidays, pooled with weights of three and four months (five and six months in row 4, the lengths of the two windows); standard errors in parentheses and 95% confidence intervals in brackets. Panel A: the first three columns of estimates report the statistic for the log number of registrations, for the share of companies with a PPP loan, and for the log number of companies with a PPP loan. *Marginal relative to inframarginal, ρ*: equation (5), with percentile intervals from joint draws, for the PPP loan and for any of the three records; Table A9 compares the intervals of rows 2 to 6 with sets by test inversion. Row 1 is the pre-specified comparison (its first entry is the pooled estimate of Table A14, Panel A, with a different standard error because the reference runs here end in 2018; the interval for ρ in row 1 is that of the pre-specified test, which uses the standard error of that table). Rows 2 and 3 take as the reference period all unaffected months of 2010 to 2018, fitted with twelve month-of-year effects and a linear or quadratic trend; their confidence intervals use the 7 degrees of freedom of the pre-specified test, and with the 21 given by the rule for non-overlapping blocks, those in the *PPP loan* column are [−0.22, 0.62] and [−0.28, 0.38]. Row 4 treats the month of the announcement, the holiday months, and the month after as one window. Rows 5 and 6 take the holiday months less the two flanking months, when the fee was $50, against the same contrast in the two adjacent years (3 degrees of freedom) and in every other year of 2010 to 2018 in which none of those months is affected (6 and 7 years; 5 degrees of freedom); with months of 2019 admitted to the reference period, ρ in row 6 is 0.63 [0.09, 1.09]. All rows except the first, and the number of companies with a PPP loan in the first row, were computed after the results of the pre-specified test were known [E]. Panel B, *Flanking months*: the statistic for the share of the companies formed in the flanking months that have the record, in percentage points, and the number of pairs of unaffected blocks, one with the structure of each holiday, in which the same statistic for the two outer months is at least as far from zero. *Slope in unaffected years*: across blocks of unaffected months with the structure of the two holidays (two outer months with three or four months between them), the slope through the origin of the statistic for the inner months on the statistic for the two outer months, over the blocks of both lengths together. The blocks overlap, and the standard error rests on the seven blocks that do not. *Weight on the flanking months*: ρ when the statistic for the flanking months, multiplied by the weight shown, is subtracted from the statistic for the holiday months, both for the share with the record and for the log number of registrations. A weight of zero is the comparison of row 1, and its entries are the pre-specified estimates and intervals of that row; a weight of one is the comparison of row 5. At every other weight the standard errors are standard deviations over the unaffected blocks, with 3 degrees of freedom, and the errors of the share and of registrations are drawn independently; computed in that way, the intervals at a weight of zero are [−0.63, 0.73] for the PPP loan and [−0.58, 0.48] for any of the three records. *The slope*: the weight on the share is the slope in unaffected years, and the weight on registrations is the corresponding slope for registrations, 0.15 (0.26). The interval includes the sampling error of the two slopes; with the slopes taken as known, it is [−0.57, 0.78] for the PPP loan and [−0.47, 0.52] for any of the three records. *Interval below one up to*: the largest weight, on a grid of 0.05, at which the 95% confidence interval for ρ lies below one; for the PPP loan the upper end of the interval at that weight is within simulation error of one. Tables A33, A37, and A39 report each holiday and each record.

Three explanations of the deficit in the flanking months. The deficit in PPP loans among companies formed in the flanking months is 3.3 standard errors from zero (3 degrees of freedom, $p = 0.047$), and it has three possible sources, which have different implications for ρ. First, it may be a chance difference between four months and their reference months, in which case the pre-specified comparison is appropriate. Chance is an unlikely source, because in none of the 239 pairs of unaffected blocks with the structure of the two holidays is the statistic for the outer months as far from zero (Table A11, Panel B). Second, the deficit may reflect retiming. Owners of operating businesses who delayed a filing

until a holiday began, or advanced one before it ended, are absent from the flanking months and present in the holiday months. The holiday cohorts then include businesses that the fee did not create, the pre-specified estimate of ρ is too high, and the appropriate comparison is over the whole window (row 4). Third, the deficit may reflect a disturbance unrelated to the fee that lowered the loan rate of companies formed over a period that includes both the flanking and the holiday months. The holiday months should then be compared with the flanking months (rows 5 and 6). Only the third explanation implies that the pre-specified estimate understates ρ. The three explanations therefore bracket the estimate for the PPP loan. Retiming implies the smallest value, −0.41 (row 4), and a common disturbance implies the largest, 0.67 to 0.81 (rows 5 and 6); the pre-specified estimate lies between them. If retiming and a common disturbance were both present, the comparison with the flanking months would overstate ρ, because it counts a retimed business twice, once as absent from the flanking months and once as present in the holiday months.

Retiming of filings. Retiming occurred around the first holiday: relative to the reference period, filings per weekday differed by −5.8 log points between the announcement and the start and by −9.6 in the two weeks after the end. Around the second holiday there was little retiming (+1.4 and +2.3 log points in the same two periods), although filings in the following two weeks were lower (−6.9). The Poisson standard errors of these five numbers are about two to three log points (Table A34, Panels D and E). Retiming is unlikely to account for the whole deficit. In the flanking months, pooled with weights of three and four months, registrations were 3.0 log points lower than in the reference period (s.e. 1.7) and the number of companies with a PPP loan was 14.0 log points lower (3.1). If retimed filings accounted for both differences, they would need to have had PPP loans at a rate of 24 per hundred, compared with 5.4 for all ordinary LLCs formed from 2011 to 2018, at most 5.7 for any type of filer, and 11.9 for profit corporations (Table A34, Panel F). This calculation does not exclude retiming as a partial explanation, and to the extent that retiming occurred, the pre-specified estimate of ρ is too high. Over the whole window, within which retiming has no effect, registrations increased by 9.6 log points (s.e. 0.8), and the number of companies with a PPP loan and the number with any of the three records differed from the reference period by −4.1 (2.2) and −2.3 (2.3) log points (row 4; Table A25, Panel A). The difference for the PPP loan comes from the first holiday, at −8.3 log points (3.8), compared with −0.7 (2.6) for the second. No account of a fee reduction predicts fewer companies with a PPP loan over a window that contains the reduction, so that the deficit over the first window, if it is not due to chance, indicates a disturbance of the type that the third explanation describes.

A disturbance common to the flanking and holiday months. Some evidence is consistent with the third explanation. Loan rates were lower than in the reference period before either announcement, by 0.69 percentage points (binomial s.e. 0.49) for companies formed on November 1 to 18, 2012, and by 0.74 (0.33) for those formed in May 2014, a month before the second holiday was announced. The first difference is within two standard errors of zero and the second is slightly more than two standard errors from it, and in the first eight days of June 2014, the last days before that announcement, the rate was not low (+0.46, s.e. 0.73). Five findings weigh against a common disturbance. First, the deficit in the flanking months is confined to the PPP loans of LLCs. Among the companies formed in those months, the share with a UCC financing statement differs from the reference period by −0.09 percentage points (0.29), the share with an SBA loan by +0.14 (0.06), and the share with a financing statement filed in 2020 or later, a lending record of the same years as the loan program, by +0.12 (0.14). The share that had become delinquent before the loans began differs by +0.2 percentage points (1.0), compared with +3.2 (1.3) in the holiday months, and profit corporations outside bulk groups formed in the flanking months show no deficit in PPP loans (+0.46 percentage points, s.e. 0.59). A disturbance to the composition of the cohorts formed around the holidays would be expected to appear in these records as well. Second, in the holiday months the share with each of the five outcomes compared in Section D.2 declines by nearly the same proportion of its inframarginal rate, 14 to 16%, and the marginal share is 14.7%. Marginal registrations that rarely have a record of any type produce this pattern (Section 6.1). Under the comparison with the flanking months, in contrast, the same marginal registrations would be 0.81 times as likely as inframarginal registrations to have a PPP loan and 0.18 times as likely to be named in a financing statement, and the agreement among the records in the holiday months would be a coincidence. Third, part of the deficit in the flanking months is attributable to the two adjacent years and not to the months. With all unaffected months of 2010 to 2018 as the reference period, the difference for the flanking months is −0.28 percentage points (0.21) with a linear trend and −0.44 (0.18) with a quadratic one, and the

difference for the holiday months remains −0.60 (0.17) and −0.76 (0.13) (Table A33, Panel A, and its note).

Fourth, disturbances that extend over a block of months are not a general feature of this record. Across 38 unaffected months, the standard deviation of the single-month statistic for the PPP loan is 0.36 percentage points, compared with the 0.35 that sampling error alone implies. The statistic is correlated between adjacent months (0.41, from 34 pairs) and not between months that are two to five months apart (−0.21 to 0.19), and only the first and the last month of a holiday adjoin a flanking month. Across unaffected blocks of months with the structure of the two holidays, the slope through the origin of the statistic for the inner months on the statistic for the two outer months is 0.06 (s.e. 0.40) for the PPP loan and 0.32 (0.40) for any of the three records, with standard errors that rest on the seven blocks that do not overlap. The autocorrelations of single months imply slopes of 0.09 and 0.04 for the PPP loan in blocks of three and four months (Table A39, Panel D). The corresponding correlations across blocks are +0.04 and −0.12 (Table A34, Panel G). The comparison with the flanking months presumes a slope of one. For the PPP loan, the estimate is far below that value but imprecise, so that a slope of one is rejected at the 10% level and not at the 5% level ($p = 0.058$; the 95% confidence interval extends to 1.04).

Fifth, the deficit ends with the month after each holiday and is absent from most of the months adjacent to the two windows. Addendum 3 specified the statistic for the months that follow the month after each holiday (the later months). For the PPP loan it is +0.34 percentage points (s.e. 0.31) after the first holiday and +0.08 (0.22) after the second (Table A20) [A3]. After the other results were known, we tabulated the statistic for the two months that precede and the two that follow each window, that is, each holiday with its flanking months (Figure 2; Table A39, Panel G) [E]. For the two months of each holiday that lie one month beyond the flanking months, namely the month before the month of the announcement and the second month after the end, the statistic for the PPP loan is −0.04 percentage points (binomial s.e. 0.18). For the two months one further out it is +0.26 (0.18), compared with −0.56 for the flanking months. Of these eight months, four of which are among the later months, one has a loan rate below that of its reference months, whereas all four flanking months and all seven holiday months do. The exception is May 2014, the month cited above. One of its two reference months, May 2013, has the highest statistic of the eight, but each of these months is a reference month of the other, so that the two statistics are not independent. The flanking months differ from the months next to them by −0.52 percentage points. The p-value of this difference is 0.055 with the binomial standard error (0.27) and 0.078 with the standard deviation of the same difference across unaffected placements of the four months (0.15, with 2 degrees of freedom), and the difference is −0.88 for the first holiday and −0.24 for the second. Further from the holidays, the statistic for the PPP loan is −0.59 (0.22) for June to August 2012 and −0.07 (0.20) for February to April 2015; the other months at that distance have a reference month within the window of the other holiday. A disturbance unrelated to the fee would thus have had to end with each window, and before the windows it appears in one of the four months that precede the months of the announcements. This weighs against the third explanation, although it does not identify the source of the deficit. The low rates of mid-2012 show, moreover, that the loan rate can remain below that of the reference months for several months in which the formation fee does not change, and the evidence is weaker for the second holiday than for the first.

These findings indicate that the cohorts formed in the flanking months do not differ from their reference cohorts in the other records and that the deficit does not extend to the months adjacent to the two windows, May 2014 excepted. They do not identify the source of the deficit, and each of the three explanations meets an objection. Chance is an unlikely source of a deficit of this size. Retiming is a change in the composition of the flanking cohorts, which the other records do not show, and it would require a loan rate of 24 per hundred among the retimed filings. A disturbance unrelated to the fee would have had to affect the PPP loans of LLCs alone and to end with both windows. The findings do not exclude a disturbance of that type. Among registrations that had not become delinquent, the loan rate is lower by 0.82 percentage points (0.28) in the flanking months and by 0.76 (0.24) in the holiday months, and such a disturbance would produce deficits of similar size in the two sets of months. A disturbance specific to participation in the loan program need not appear in secured borrowing, in SBA lending, or among corporations. The data therefore do not determine the comparison for the PPP loan, and Section 6.2 states the conclusion for that outcome under each comparison.

The weight given to the flanking months. The pre-specified comparison and the comparison with the flanking

months are the limiting cases of a family of comparisons. We subtract the statistic for the flanking months, multiplied by a weight, from the statistic for the holiday months, both for the share with the record and for the number of registrations. A weight of zero gives the pre-specified comparison and a weight of one the comparison with the flanking months. If a disturbance to the flanking months carries over to the holiday months in the proportion observed between the outer and the inner months of unaffected blocks, the appropriate weight is the slope reported above, which is also the weight that minimizes the variance of the estimate. Panel B of Table A11 reports ρ by weight. With the weight equal to that slope, ρ for the PPP loan is 0.13. Its confidence interval runs from −0.57 to 0.78 when the slope is taken as known and from −0.83 to 1.15 when its sampling error is included, and 96% of the joint draws lie below one. At weights of one-half and one, ρ is 0.46 and 0.81, and the confidence interval includes one at every weight above 0.30. The slope in unaffected years is small, but its confidence interval extends to 1.04, so that the data do not determine the weight for the PPP loan. For any of the three records, ρ is between −0.02 and 0.26 and the upper end of the confidence interval is at most 0.78 at every weight; with the weight equal to the slope and the slope's sampling error included, the interval runs from −0.58 to 0.61. For a UCC financing statement alone, ρ is between −0.02 and 0.18.

Estimates for each holiday and for corporations. The second holiday reduced formation fees alone, and over its whole window the number of companies with a PPP loan did not differ from the reference period (−0.7 log points, s.e. 2.6). For that holiday, the pre-specified comparison gives $\rho = 0.21$ (−0.55 to 0.81) for the PPP loan and 0.20 (−0.53 to 0.67) for any of the three records [A3], the whole window gives −0.05 (−0.73 to 0.67) and 0.11 (−0.78 to 0.78), and the comparison with the flanking months gives 0.73 (−0.59 to 1.99) and 0.36 (−0.47 to 1.02). For the first holiday, the pre-specified comparison gives −0.18 (−1.61 to 0.75) for the PPP loan and −0.44 (−2.03 to 0.33) for any of the three records [A3], the whole window gives −1.24 (−3.72 to 0.17) and −1.01 (−3.72 to 0.24), and the comparison with the flanking months gives 0.95 (−0.30 to 2.19) and 0.11 (−0.78 to 0.72) (Table A25, Panel C, and Table A37, Panel B). The estimates for each holiday taken separately are imprecise. Under the pre-specified comparison and over the whole window, the confidence interval nevertheless lies below one for both records in each holiday. Under the comparison with the flanking months relative to the adjacent years (Table A11, row 5), which is the comparison just reported, it lies below one only for the broader record in the first holiday. Relative to every other unaffected year (row 6), it lies below one for the broader record in each holiday, and for the PPP loan ρ is 0.83 (−0.26 to 1.96) in the first holiday and 0.58 (0.26 to 0.99) in the second (Table A33, Panel B). For the broader record, the estimate is at most 0.36 in either holiday under any of these comparisons. For ordinary LLCs and profit corporations taken together, ρ for the PPP loan is 0.17 (−0.38 to 0.57) under the pre-specified comparison and 0.68 (−0.10 to 1.40) under the comparison with the flanking months; for any of the three records it is 0.07 (−0.55 to 0.46) and 0.26 (−0.30 to 0.70) (Table A38, Panel B). For the first holiday alone the marginal share over the whole window is imprecisely estimated, and the lower ends of the sets by test inversion lie below those of the intervals reported here (Section D.1).

Numbers of companies under the two comparisons. At a value of 0.81, the estimate under the comparison with the flanking months relative to the adjacent years, about 290 of the approximately 6,800 marginal registrations would have had a PPP loan, compared with 30 at the pre-specified estimate and about 350 at an equal rate. The number of companies with a PPP loan in the holiday months accordingly differed from its reference period by +1% under the pre-specified comparison (confidence interval −7 to +11) and by +16 to +17% under the comparison with the flanking months, while registrations increased by 17% and by 21 to 24%. For any of the three records, the confidence interval for ρ lies below one in every row of Panel A and at every weight in Panel B. When the financing statements of a company's first 90 days, which could have been filed at the reduced fee, are excluded, it also lies below one under both the pre-specified comparison and the comparison with the flanking months. The statistic for the flanking months separates the two outcomes. For the PPP loan, it is outside the range of the same statistic in unaffected blocks; for any of the three records, it is within that range (52 of the 239 pairs are as far from zero). We applied this criterion after the results were known. The number of companies with any of the three records in the holiday months differed from its reference period by −0.3% under the pre-specified comparison (−7 to +7) and by +6 to +8% under the comparison with the flanking months. This record was a secondary outcome, for which Addendum 3 specified no interpretation and which it permits the paper to summarize only together with the primary outcome (Table A6, row 14). The 24-month

component of the UCC financing statement had been tabulated under Addendum 2 before Addendum 3 was written, and the comparison of the two outcomes across reference periods was made after the results were known [E].

D.4. PPP Loans in the Name of a Company's Registered Agent

The rule of Addendum 3 links a PPP loan to a company by the company's name. A business whose owner borrowed under a personal name is outside the set of candidates of that rule, and neither the alternative linking rules nor the review of record pairs (Section D.5) can find it. Of the 192,267 PPP loans with a Colorado borrower, 60,904 are not linked to an entity by the rule, and 47,364 of these name the borrower in the form of a person's name. Of the 47,096 such loans that the rule described below can use, which have a first and a last name of at least two letters, a ZIP code, and an approval date, 41,934 describe the borrower as a sole proprietorship, a self-employed individual, or an independent contractor, and 3,496 as an LLC or a single-member LLC. This section links these loans to companies through the registered agent [E].

The rule. The registry names each company's registered agent, who for a small company is often its owner. The rule was written in the project's working notes on October 7, 2026, before any count by cohort was formed. Unlike the plan and the addenda, the notes were not recorded in the version history before the script was run, so that this order rests on our own account. A loan enters if its borrower's name, in upper case and with punctuation removed, has two to four words, no digit, and no word that marks a legal form or a trade name. Its key is the first and the last word of the name, with a trailing suffix dropped and a name written with the surname first reversed. For every entity whose registered agent is a person, the key is the first word of the agent's first name and the last word of the agent's last name. A loan is linked to an entity if the two keys are equal, the borrower's ZIP code equals the agent's ZIP code or the entity's principal or mailing ZIP code, and the entity was formed on or before the day on which the loan was approved. The rule fixed four outcomes: that a company's agent holds such a loan; that the company has a loan in either name, that is, a loan linked by the rule of Addendum 3 or a loan of its agent; and the same two outcomes with only the loans that match a single entity. It also fixed the two comparisons, the pre-specified one and the comparison with the flanking months, and the reading of the results. If the owners of marginal registrations had borrowed in their own names in place of the company's, the share of companies whose agent holds a loan would rise in the holiday months, and ρ for a loan in either name would lie well above ρ for the loan in the company's name. If marginal registrations had no loan under either name, that share would fall in the same proportion as the share with a loan in the company's name. Links made by chance would move ρ for the agent's loans toward one. Four tabulations were added after the first results had been seen: the share of loans linked by the type of business that the borrower stated, the estimates by that type, the comparison by delinquency, and the weighted estimates. A fifth, the weighted estimates at larger weights, was added after a reader of the revision observed that the weighted estimates test a hypothesis.

The links. The rule links 13,036 loans to 18,720 entities, and 8,834 of the linked loans match one entity only. When each loan is given the ZIP code of another loan drawn at random, an average of 434 loans are linked over five draws, or 3.3% of the number linked, so that few links arise from the coincidence of a common name. The rule finds a loan only if the borrower is the registered agent of the company and one of the ZIP codes agrees. Where the borrower states that the business is an LLC, a registered company should exist, and 48.1% of those loans are linked, compared with 26.6% of the loans whose borrower states a sole proprietorship, self-employment, or independent contracting. We take the first share as a lower bound on the proportion of true links that the rule finds, because some of those borrowers have no company in the Colorado registry, and one version of the estimates weights the agent's loans by its inverse, 2.08.

Results. Of the ordinary LLCs in the sample, 1.21% have an agent with a loan in the agent's name, 5.26% have a loan in the company's name, and 0.14% have both (Table A12, Panel A). The share of companies whose agent holds a loan differs from its reference level by −0.02 percentage points in the holiday months (s.e. 0.07), the number of such companies is higher by 13.8 log points (5.4), and ρ is 0.85, with an interval from −0.09 to 2.02 that includes both zero and one. This outcome counts a person's loan for every company of which that person is the agent, so that a person who forms a further company during a holiday carries the loan to the new registration. With only the loans that match a single entity, 0.51% of companies have an agent with a loan, the share is lower in the holiday months by 0.09 percentage

points (0.03), and ρ is −0.26 (−1.16 to 0.68). That outcome excludes by construction every company of a person who is the agent of more than one entity, and with it the further companies that such persons formed during the holidays. The first outcome therefore attributes too many of the agents' loans to marginal registrations, provided that a marginal registration whose agent holds a loan is no more likely than another such registration to be the business for which the agent borrowed, and the second attributes too few. On that condition the two bracket the attribution of an agent's loan to a company.

With a loan in either name counted, ρ is 0.18 (−0.34 to 0.63) under the first outcome and 0.04 (−0.56 to 0.54) under the second, compared with 0.07 (−0.57 to 0.60) for the loan in the company's name alone. Under the comparison with the flanking months, the three estimates are 0.75, 0.73, and 0.81, each with an interval that includes one. When the agent's loans are weighted by 2.08 to allow for links that the rule did not find, the estimate under the pre-specified comparison for a loan in either name is 0.26 (−0.21 to 0.70), and with weights of three, four, five, and six it is 0.32 (−0.16 to 0.79), 0.36 (−0.14 to 0.88), 0.40 (−0.14 to 0.97), and 0.43 (−0.13 to 1.05). When only the agent's loans whose borrower states an LLC are counted, which are the loans most likely to belong to the company, the share of companies with a loan in either name is 5.42% and ρ is 0.08 (−0.53 to 0.56). Among the LLCs whose agent is a person, to which alone the link can apply, ρ is 0.01 (−0.67 to 0.58) for the loan in the company's name and 0.14 (−0.39 to 0.61) for a loan in either name.

By the reading fixed in advance, the results do not show that owners of marginal registrations borrowed in their own names in place of the company's: the share of companies whose agent holds a loan did not rise in the holiday months, and ρ for a loan in either name is not well above ρ for the loan in the company's name. The weighted estimates, which were added afterward, test that hypothesis more sharply. Suppose that marginal registrations had borrowed as often as other registrations, in the agent's name where not in the company's. If the rule finds the same proportion of the agents' loans in every cohort, the share of companies with a loan in either name is estimated without bias when a company with an agent's loan and no loan in its own name is counted with a weight equal to the inverse of that proportion, and ρ for the weighted outcome would then be one. The interval excludes one at the weight of 2.08, which corresponds to the 48.1% of loans linked where the borrower states an LLC, and at every weight up to five; it reaches one at a weight of six. Under the pre-specified comparison, borrowing in the agent's name in place of the company's therefore does not account for the deficit unless the rule finds fewer than about one such loan in five. The weighted outcome uses the first measure of the agent's loans, which favors the hypothesis. For the second holiday alone, ρ for a loan in either name is 0.33 (−0.26 to 0.83) under the first outcome and 0.21 (−0.48 to 0.76) under the second, compared with 0.21 (−0.55 to 0.81) for the loan in the company's name. The weighted estimate for that holiday is 0.42 (−0.11 to 0.93) at the weight of 2.08, and its interval reaches one at a weight of three, so that for the second holiday considered alone the conclusion requires that the rule find more than about one such loan in three. Under the comparison with the flanking months the intervals include one, as they do for the loan in the company's name.

The results do not determine whether the agents of marginal registrations borrowed as individuals as often as other agents. For the first outcome ρ is 0.85, with an interval from −0.09 to 2.02 that includes one; the second outcome omits the agents of more than one company and measures a different quantity. Counting the agent's loans moves the pre-specified estimate for the PPP loan from 0.07 to 0.04 or 0.18, according to the attribution, and to between 0.26 and 0.40 at weights from 2.08 to five, and every one of these intervals excludes one.

What an agent's loan indicates. A loan in an agent's name is weak evidence that the company operates a business. Among the 543,934 ordinary LLCs whose agent is a person, 0.85% of the companies that had become delinquent before February 15, 2020, the date on which a borrower had to be in operation, have an agent with such a loan, compared with 1.79% of the other companies. A loan in the company's name is held by 0.37% and 7.95% (Panel B). A company that was delinquent by that date is unlikely to be the business behind a loan of 2020 or 2021, so that an agent's loan often belongs to a business of the person that is not the company. In the holiday months, in the same months of the adjacent years, and in the flanking months, the rates of an agent's loan are nearly the same, at 0.80, 0.80, and 0.85% among delinquent companies and 1.65, 1.67, and 1.68% among the others.

Limits. The link measures one category of loans outside the set of candidates of Addendum 3. It does not find a

loan in the name of an owner who is not the registered agent or a loan under a trade name unrelated to the company's name, and 13,540 of the unlinked loans are not in the form of a person's name. The links were formed by rule, and no reader, human or AI, has examined a sample of them.

Table A12. Colorado: PPP Loans in the Name of a Company's Registered Agent

A. Shares of companies and estimates for the two fee holidays, pooled

	Share of companies, percent	Holiday months, pre-specified comparison: Change in the share, pct. points	Change in the number, log points	ρ	Flanking months: Statistic, pct. points	ρ, holiday less flanking months
All ordinary LLCs						
PPP loan linked by the company's name [A3]	5.26	−0.71 (0.17)	+1.4 (3.7)	0.07 [−0.57, 0.60]	−0.56 (0.17)	0.81 [−0.19, 1.78]
Loan in the name of the company's agent	1.21	−0.02 (0.07)	+13.8 (5.4)	0.85 [−0.09, 2.02]	+0.05 (0.08)	0.63 [−1.45, 2.62]
borrower states an LLC	0.21	−0.02 (0.03)	+8.2 (12.8)	0.41 [−2.09, 2.53]	−0.01 (0.04)	0.68 [−4.53, 5.85]
borrower states a personal business	0.99	−0.01 (0.07)	+15.4 (6.6)	0.95 [−0.17, 2.39]	+0.05 (0.08)	0.66 [−1.71, 2.96]
loan matched to one company only	0.51	−0.09 (0.03)	−3.9 (4.8)	−0.26 [−1.16, 0.68]	−0.01 (0.04)	−0.02 [−2.45, 2.23]
Either a linked loan or a loan in the agent's name	6.33	−0.74 (0.17)	+3.2 (3.0)	0.18 [−0.34, 0.63]	−0.50 (0.19)	0.75 [−0.17, 1.63]
counting only agent's loans that state an LLC	5.42	−0.72 (0.16)	+1.5 (3.4)	0.08 [−0.53, 0.56]	−0.57 (0.16)	0.81 [−0.17, 1.75]
counting only agent's loans matched to one company	5.70	−0.79 (0.17)	+0.9 (3.4)	0.04 [−0.56, 0.54]	−0.56 (0.19)	0.73 [−0.26, 1.69]
agent's loans weighted by 2.08 for links not found				0.26 [−0.21, 0.70]		
agent's loans weighted threefold				0.32 [−0.16, 0.79]		
agent's loans weighted fourfold				0.36 [−0.14, 0.88]		
agent's loans weighted fivefold				0.40 [−0.14, 0.97]		
agent's loans weighted sixfold				0.43 [−0.13, 1.05]		
Ordinary LLCs whose agent is a person						
PPP loan linked by the company's name	5.19	−0.76 (0.18)	+0.3 (4.2)	0.01 [−0.67, 0.58]	−0.42 (0.18)	0.59 [−0.43, 1.56]
Either a linked loan or a loan in the agent's name	6.46	−0.81 (0.19)	+2.6 (3.3)	0.14 [−0.39, 0.61]	−0.34 (0.21)	0.56 [−0.36, 1.41]
Ordinary LLCs whose agent is an organization						
PPP loan linked by the company's name	5.62	−0.42 (0.32)	+6.7 (4.7)	0.43 [−0.38, 1.67]	−1.29 (0.42)	2.50 [−0.19, 6.33]

B. By delinquency before the loans began: ordinary LLCs whose agent is a person

	Companies	Delinquent before Feb. 15, 2020, percent	PPP loan linked by name, percent: Delinquent	Not delinquent	Loan in the agent's name, percent: Delinquent	Not delinquent
Holiday months	38,562	43.3	0.29	7.51	0.80	1.65
Same months of the adjacent years	65,678	40.0	0.34	8.38	0.80	1.67
Flanking months	17,147	40.3	0.20	7.85	0.85	1.68
All months, 2010 to 2018	543,934	36.4	0.37	7.95	0.85	1.79
borrower states an LLC					0.11	0.34
borrower states a personal business					0.73	1.43

Notes. Ordinary LLCs formed from 2010 to 2018. A loan in the name of the company's agent is a PPP loan with a Colorado borrower that the rule of Addendum 3 leaves unlinked, whose borrower is named in the form of a person's name, and whose key and ZIP code match those of the company's registered agent. Panel A, *Share of companies*: the percentage of the companies in the sample that have the record. *Change in the share* and *Change in the number*: the statistic of equation (4) for the share of the companies formed in the holiday months that have the record and for the log number of such companies, with standard deviations over reference runs in parentheses. ρ: equation (5), with 95% percentile intervals from joint draws. *Flanking months*: the statistic for the share among the companies formed in the flanking months, and ρ when the holiday months are compared with the flanking months relative to the adjacent years (row 5 of Table A11). *Borrower states an LLC* and *a personal business*: the type of business stated on the loan, an LLC or a single-member LLC in the first case and a sole proprietorship, self-employment, or independent contracting in the second. *Loan matched to one company only*: the key and ZIP code of the loan match a single entity in the registry. *Weighted*: a company with an agent's loan and no loan in its own name is counted with the weight shown; 2.08 is the inverse of the share of loans linked where the borrower states an LLC. Panel B: a company is delinquent if the registry records a date of delinquency before February 15, 2020. The first row of Panel A reproduces the pre-specified estimate [A3], and its second interval is that of Table A11, row 5. All other entries are exploratory [E].

D.5. Colorado: Record Linkage, Filer Classification, and Inference

The estimates depend on the linkage of loan records to the registry, on the classification of filers, and on a standard error computed from a small number of reference periods. This section reports checks of each. Apart from one linking rule

that Addendum 3 specified, none was specified in advance [E], and Section E.6 gives the tables.

Record linkage. A PPP loan is linked to a company by name, and the ZIP code and the date of formation decide among companies that share the name (Appendix C). The estimate would be biased if links were made less often or less accurately for the holiday cohorts. The checks reported in this paragraph and the next indicate that any such difference is small. Among the links of companies formed in the holiday months, in the same months of the adjacent years, in the flanking months, and in the other months of 2010 to 2018, the borrower's full name is identical to the company's in 85.9, 85.3, 84.5, and 85.2% of cases; the company is the only candidate in 84.7, 85.8, 85.1, and 85.5%; and the link is decided by the date of formation in 2.6, 2.6, 2.8, and 2.9% (Table A38, Panel A). Under eight other linking rules, which include requiring a single candidate, disregarding the date of formation, admitting every candidate, and the rule of an identical full name and ZIP code that Addendum 3 specified [A3], the estimate of ρ for the PPP loan under the pre-specified comparison is between 0.06 and 0.30, with every confidence interval below one, and the estimate under the comparison with the flanking months is between 0.66 and 1.07 (Table A38, Panel B). The loan files used for the main estimates are restricted to borrowers with a Colorado address. When the loans of every other state are searched for the names and addresses of Colorado companies, 0.35% of the companies formed in the holiday months and 0.41% of those formed in the same months of the adjacent years have a loan elsewhere, and with those loans included ρ is 0.09 (−0.50 to 0.55). This search requires agreement with an address that the company reports to the registry, and it cannot find a business in another state whose registered addresses are all in Colorado. The share of companies that report a principal or mailing address in another state is 7.6% in the holiday months and 7.9% in the same months of the adjacent years. A UCC financing statement is linked by the registry's identification number or by the debtor's name and, where several companies had the name, to the most recently formed company (Appendix C). Of the companies named in a statement, 4.2, 4.7, 3.8, and 4.2% in the four cohorts are linked only by the rule that prefers the most recently formed company. Under three other rules (links only by the number or by a name that a single company had; links to the company formed first; links to every company that had the name), ρ for any of the three records is between −0.03 and −0.01 under the pre-specified comparison and between 0.27 and 0.28 under the comparison with the flanking months, with every confidence interval below one.

Review of record pairs. The characteristics of the links that the rule made do not show how many of them are wrong or how many true links the rule missed. To measure both, we drew 1,177 pairs of a company and a loan or a financing statement, and each pair was read by one reader from each of two sets of four AI readers. The readers saw the names and addresses in the two records, the legal form and the age that a borrower stated, and an industry code; they did not see the date on which the company was formed. The sample consists of 400 links to PPP loans that the rule made; 517 pairs of a company without a linked loan and an unlinked loan whose borrower's name is close to the company's; 160 financing statements linked by name; and 100 pairs with a known answer, drawn from financing statements that carry the registry's identification number, in half of which the debtor is the company although the names differ and in half of which it is another company with a similar name. Of the pairs with a known answer, the readers judged 91% of those in which the debtor is the company to be the same company and 89% of those in which the debtor is another company to be different companies, and the two sets of readers agreed on 98% of all pairs. Of the links that the rule made, the readers judged 98.0, 97.4, 97.1, and 96.3% to join the same company in the four cohorts, and the difference between the holiday months and the same months of the adjacent years is +0.6 percentage points (s.e. 1.2). The links that the rule missed amount to 4.2, 3.3, 3.9, and 3.6 per hundred links made. When the share of each cohort with a PPP loan is corrected for the links judged wrong and for the links judged missed, the estimate of ρ under the pre-specified comparison is 0.16 (−0.47 to 0.68), compared with 0.07, and the estimate under the comparison with the flanking months is 0.83, compared with 0.81. Of the financing statements linked by name, the readers judged 95 to 99% to name the same company in the four cohorts and none to name another company (Table A38, Panel D). The review also showed that 787 borrowers' names in the loan file contain an HTML entity in place of a character such as an ampersand, which the linking rule did not decode. Decoding these names adds 637 links, of which 5 are to companies formed in the holiday months and 12 to companies formed in the same months of the adjacent years, and leaves ρ at 0.07 (−0.58 to 0.60). The main estimates retain the links of Addendum 3. The review measures the precision of the links and their

recall within the set of candidates, that is, among the loans whose borrower's name is close to the company's. It cannot find a link outside that set, and Section D.4 examines one such category, the loans made in the name of a company's registered agent. The pairs were read by AI readers only, and an audit by a person remains to be done.

Filer classification. The pooled estimates use no classification of filers other than the exclusion of bulk filings, and they do not depend on that exclusion: for all domestic LLCs, ρ is 0.07 (−0.56 to 0.54) for the PPP loan and −0.02 (−0.52 to 0.40) for any of the three records. The classification by type of filer has two weaknesses. First, the registry reports each company's agent and addresses as of October 2026 and not as of formation. Periodic reports filed through 2021 state whether they change the agent or an address. By that record, 17.5% of the companies formed in the holiday months had changed their agent and 31.0% their principal address, compared with 19.2 and 33.0% of those formed in the same months of the adjacent years, so that the classification is not less accurate for the holiday cohorts. Second, the address keys consist of a street number and a ZIP code, and 49% of LLCs share such a key with an LLC on another street. With keys that add the street name and with keys that use the full address line, new filers account for 44 and 52% of the additional registrations, compared with 40% under the original keys, and repeat filers for 37 and 32%, compared with 41%. The change in the loan share among new filers is −0.75 (0.19) and −0.72 (0.16) percentage points under the two finer keys, compared with −0.72 (0.17) under the original keys (Table A38, Panel C). The division of the marginal registrations between new and repeat filers is therefore known only within these ranges, whereas the finding that the additional registrations of new filers show the same deficit as those of repeat filers does not depend on the key.

Inference. The standard error of each statistic is its standard deviation over reference runs of unaffected months. These runs overlap, and runs twelve months apart share reference months, so that they are not independent draws. We examine the procedure in three ways (Table A39). First, we apply it to simulated data in which no holiday has an effect, using the actual numbers of companies, binomial sampling, and persistent disturbances to cohorts that make the statistic up to twice as variable as it is under sampling alone. Across ten such designs, a test of nominal size 5% rejects in 3.8 to 5.6% of 20,000 replications for the pooled statistic and in 3.9 to 7.4% for a single holiday, and the standard error is between 0.98 and 1.02 times the standard deviation of the estimate. Across the designs, the dependence among the runs leaves at least 6.0 and 4.7 degrees of freedom for the two holidays, compared with the 10 and 7 that the test uses, and the pooled statistic requires a critical value of at most 2.44, compared with the 2.36 used. At the largest critical values that the simulations require, the confidence interval for ρ runs from −0.60 to 0.61 for the PPP loan and from −0.67 to 0.43 for any of the three records, and for the second holiday alone from −0.59 to 0.83 and from −0.57 to 0.69 (Panel F). In simulations in which the holidays add registrations with a true ρ of zero, one-half, or one, the 95% confidence interval contains the true value in 94 to 98% of replications, and when the true ρ is one the interval lies wholly below one in at most 1.3%. Second, we compute the complete pooled contrast for every pair of a three-month and a four-month reference run whose months and reference months are disjoint. Of 567 such pairs, which share months with one another, none gives a statistic as far from zero as the estimate for the PPP loan, for either federal loan, for a financing statement, or for any of the three records. Third, we compare the standard error with sampling error. Binomial sampling alone implies a standard error of 0.13 percentage points for the PPP loan, compared with the 0.17 that we use, and across the four records the standard error that we use is between 1.08 and 1.79 times the one that sampling implies. The precision of the estimates is therefore limited by the two episodes and their reference periods, which the degrees of freedom reflect, and not by the number of company records. The simulations concern the pre-specified statistic under stationary disturbances. They do not cover the alternative reference periods of Table A11 or the U.K. test, for which Section D.1 reports placebo dates and simulations of coverage.

D.6. United Kingdom: Further Analyses of the Test

This section reports the analyses that Sections 7.3 and 7.4 summarize. The confidence sets for ρ are sets by test inversion (Section D.1), and Table A9 gives the percentile intervals over the draws retained beside them. Passages marked [A4] report what the addendum specified, and the other analyses are exploratory [E].

The direct estimate and the four common reference years. The pre-specified estimate of the change in the number of companies with non-dormant accounts combines a change in registrations measured against nine reference years with a

change in the share measured against four. Addendum 4 also specified a direct estimate, in which both changes are measured against the four years in which the outcome is observed. It is −0.8 log points (s.e. 4.6), or −0.8%, with a confidence interval from −14.2 to +14.7% (Table A26, Panel A) [A4]. The two estimates of the change in that number agree. Measured against those four years alone, registrations declined by 20.0 log points (s.e. 7.8, $p = 0.082$) and the share increased by 19.2 (3.3, $p = 0.010$) (Table A36, Panel B) [E]. The decline in registrations is thus less precisely estimated from four reference years than from nine, and on the four years alone its confidence interval includes zero, so that the first interpretation of Addendum 4, which requires a decline in registrations, would not be reached with those years alone. That registrations declined is established by the nine-year comparison that the addendum specified (19.2 log points, s.e. 6.7, $p = 0.021$), by the placebo dates reported below, and by the persistence of the decline (Section 7.5). The hypothesis that the number of such companies declined in proportion to registrations is the hypothesis that the share did not change, and it is rejected in both comparisons. The direct test of Section 7.2 is computed from the same four years.

Anticipatory filing. Filings were advanced ahead of the fee increase. In the last 16 days before it, which the pre-specified test excludes from both periods, registrations were 19.3 log points (s.e. 6.8) above their level in the base period, relative to the reference years, and the share with non-dormant accounts was nearly the same as in the base period (−1.8 log points, s.e. 4.0) [A4]. The number of companies with such accounts formed in those days was accordingly higher, by 18.4 log points when estimated directly (s.e. 2.2) (Table A28). The additional companies formed in those days thus had such accounts about as often as the companies of the base period, as Addendum 4 had assumed of companies formed early (Section B.8). Anticipatory filing does not, however, bias the pre-specified estimate of ρ downward. A registration that would have been filed in any case and that was advanced, whether into the base period or out of the after period, is counted among the deterred registrations although it is not one, which raises the estimate. A registration that the higher fee would have deterred and that was advanced into the base period is a deterred registration, and if it resembles the other deterred registrations it introduces no bias (Section 3). When the after period is extended to begin 16 days before the increase, so that it includes the filings advanced into those days, registrations decline by 11.3 log points (s.e. 5.1) and the share increases by 12.5 (2.8) [A4]; the number of companies with such accounts then changes by +1% (−9 to +12), and ρ is −0.09 [E]. In 3.3% of the joint draws for this comparison, registrations decline by less than half a log point, so that the rule of Addendum 4 reports no interval for ρ. The set by test inversion consists of the values up to 0.51 and the values above 19.7, and it excludes one (Table A9).

The base period. It is less clear that filings were also advanced into the base period of the pre-specified test, which falls after the announcement of February 19. Relative to the reference years, registrations increased by 13.7 log points (s.e. 4.9) between its first and second halves, and the share was nearly unchanged (−0.9 log points, s.e. 2.5) [A4]. That increase reflects in part the first week of the period, the week of March 4, in which registrations per working day were lower than in any other week from January to April 2024 (Table A35, Panel D) [E]. Registrations in the base period as a whole were not higher than in the six weeks before the announcement, the earlier base period (−2.6 log points, s.e. 4.9), so that the addendum's criterion for treating the base period as inflated by anticipation was not met (Section B.8). The share with such accounts in the base period was nevertheless 4.0 log points (s.e. 1.5) below the share in the weeks before the announcement, relative to the reference years (Table A28, Panel A). We cannot determine the source of this difference, which is not statistically distinguishable from zero at the 5% level ($p = 0.083$). The earlier base period precedes March 4, 2024, when the first measures of the 2023 Act took effect, so that a comparison with it also includes any effect of those measures, whereas both periods of the pre-specified test fall under them (Section 4.1). The data do not determine which base period is correct, and the paper reports the estimates under both (Section 7.3; Table 5).

Alternative windows and reference years. We recompute the test for 315 combinations of the length of the two periods (four to eight weeks), the gaps that exclude the bunching of registrations before the fee increase and the temporary decline after it, and the reference years (Figure A1). The decline in registrations ranges from 8 to 29 log points, with a median of 18, and the increase in the share ranges from 16 to 22 log points. The change in the number of companies with non-dormant accounts ranges from −9 to +12 log points, with a median of +0.4. Its confidence interval includes zero in every specification, and in no specification is the estimate as large as half the decline in registrations. The largest value of ρ is 0.28, and the confidence interval for the increase in the share lies above zero in 303 of the 315

specifications. Among these specifications, all of which take a base period after the announcement, the estimate thus varies little with the window or the reference years. Under the addendum's rule, a confidence interval for ρ is reported for 135 of the specifications, those in which at most 2.5% of the joint draws show a decline in registrations of less than half a log point. It lies below one in 133 of them, and in the other two, which omit the reference year 2023 and so have two degrees of freedom for the share, it extends to at most 1.08. These are percentile intervals over the draws retained. The result file does not store the inputs of their draws, and sets by test inversion were not computed for the specifications; in the comparisons of Table A9, the upper ends of the two constructions agree to within 0.01 where the share has three degrees of freedom and to within 0.05 where it has two. The specifications are computed from overlapping data and are not independent of one another. They are not a test of equivalence, and the bounds on a possible effect on the number of companies are the confidence intervals of Section 7.1. The specifications differ in the precision with which the decline in registrations itself is estimated: its confidence interval lies below zero in 157 of the 315 specifications. No specification uses gaps narrower than the pre-specified ones, because with narrower gaps the before period includes the bunching of registrations that preceded the fee increase; with such gaps, an internal reviewer obtained values of ρ between 0.13 and 0.45 (Section B.11).

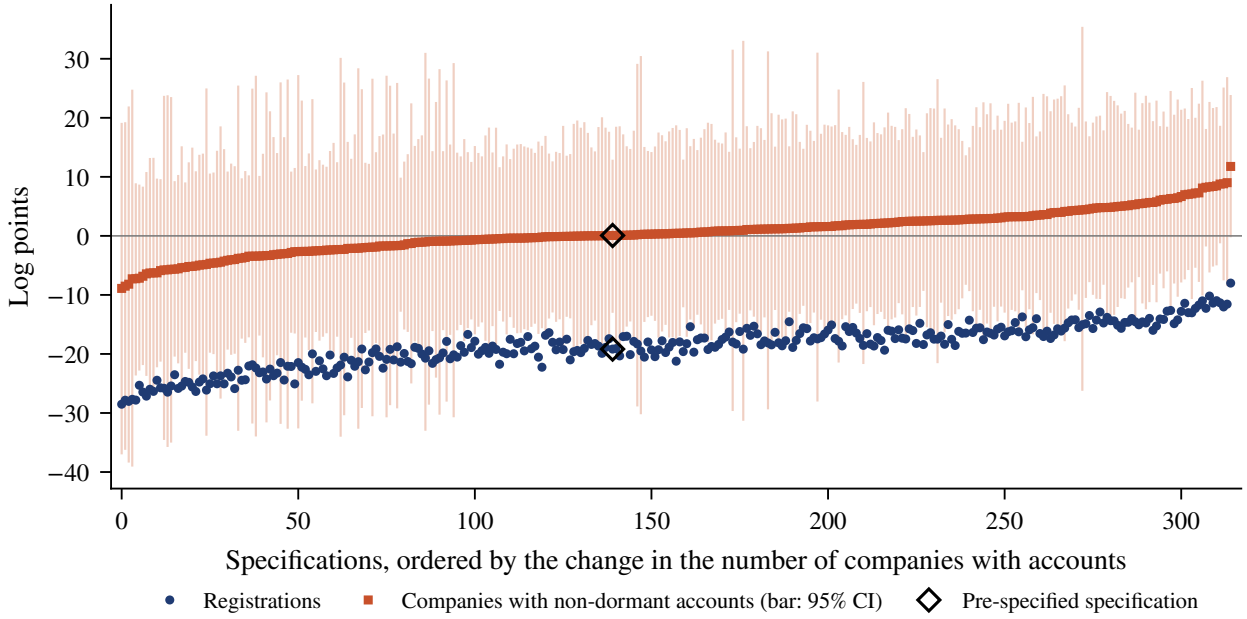


Figure A1. United Kingdom: The Test under Alternative Windows and Reference Years

Notes. Each of the 315 specifications is a choice of the length of the before and after periods (28 to 56 days), of the days excluded before and after May 1, 2024, and of the reference years. For each specification, the figure shows the estimated change in registrations per working day and the estimated change in the number of companies with non-dormant accounts, in log points, with the 95% confidence interval of the latter. The diamond marks the specification of Addendum 4. Not pre-specified [E].

Placebo dates. With four reference years, a t ratio can be large because its denominator happens to be small. We therefore compute the statistic at placebo dates on which no fee changed. At 36 such dates a week apart from late January to the end of September, taking each reference year in turn against the other three, the 144 placebo estimates for the share have a standard deviation of 2.9 log points, and the largest in absolute value is 7.3, compared with 19.2 at the fee increase. The placebo t ratios reject at the 5% level in 7% of cases, close to the nominal rate, but 4% of them exceed the ratio of 5.8 at the fee increase. The estimate is unusual in its magnitude and not in its t ratio. For registrations, with nine reference years and 33 placebo dates from March to October, the largest in absolute value of the 289 placebo estimates is 16.2 log points, compared with 19.2, and two exceed 15 (Table A35, Panel C). The year 2025 is omitted at the eight dates for which its after period would extend beyond October 20, 2025, so that the period ends before the

second fee increase was announced on October 30 (Companies House, 2025b).

Timing of the change. By week of incorporation, the share of companies with non-dormant accounts is between 32 and 38% in every week from January to April 2024, is 41.0% in the first full week at £50, and is between 41 and 43% in each of the nine weeks that follow (Table A35, Panel D). A line and a step fitted to the weekly shares of the nine weeks that end on April 16 and the nine that begin on May 1 give a step of 19.3 log points (s.e. 2.1) in 2024, compared with −5.6, +0.5, −1.5, and −2.3 at the same date in the four reference years (Table A36, Panel E) [E]. The estimate for 2024 uses no reference year, so that the fees that the reference cohorts owed do not affect it. The share does not change in the week of March 4, when the registrar's new powers came into force, or in the week of April 6, when changes to national insurance and to the dividend allowance took effect, and it had increased before the general election was called on May 22. The timing of the step matches the fee increase and not these other events. We cannot determine from this series whether the other events of that spring affected the number of businesses.

Age at measurement. Outcomes are measured in October 2026, when companies of the before period are eleven weeks older than those of the after period. When outcomes are instead measured at equal ages from the monthly files, with 2022 and 2023 as reference years, the increase in the share is 22.3 log points, compared with 21.3 when both periods are measured on one day with the same two reference years (Table A35, Panel E). Adjusting instead for companies struck off between July and October raises the pre-specified estimate of the increase in the share to 20.4 log points [A4], and ρ is then −0.06 (−2.8 to 0.37).

Maintenance fees in the reference years. The fee for the annual confirmation statement was increased from £13 to £34 on the day of the fee increase. The companies of the two periods of 2024 owed their statements at the same fees, £34 for the first statement and £50 for the second statement, so that the comparison within 2024 is unaffected. Two of the four reference years are affected. The first statement of the cohort of 2023 and the second statement of the cohort of 2022 fell due at £13 for companies of the before period and at £34 for those of the after period (Table A36, Panel A). Addendum 4 noted this exposure and retained both years. A higher statement fee in the after period of a reference year would lower the reference change in the share and would therefore raise the estimate for 2024, in the direction of the prediction. We examine this possibility in three ways [E]. First, we treat the increase in the statement fee as an event. For the cohort of 2023, relative to 2017 and 2021, the two reference years in which both periods owed equal fees, the share on the register at 27 to 31 months is 0.7 log points lower in the after period (s.e. 0.8) and the share with non-dormant accounts is 2.2 lower (1.6), neither of which is statistically distinguishable from zero; the share that filed a first statement, measured against the four reference years of that outcome, differs by −0.5 log points (1.9). For the cohort of 2022, relative to the other three reference years, the share on the register is 3.8 log points lower (s.e. 0.7, $p = 0.033$) and the share with non-dormant accounts is 5.4 lower (1.8, $p = 0.098$) (Panel D). The higher statement fee may thus have lowered survival in the cohort of 2022, and its effect on the share with non-dormant accounts is not statistically distinguishable from zero in either cohort. Second, we repeat the test without the reference years concerned. Without 2022, the one year in which an effect on survival is detected, the increase in the share is 17.9 log points (s.e. 1.8, $p = 0.010$), the number of companies with such accounts changes by −0.1% (−12.2 to +13.5), and ρ is 0.01 (−2.6 to 0.41). With 2017 and 2021 as the only reference years, the increase in the share is 17.2 log points. Two reference years leave one degree of freedom, and with their own standard error (1.6) the p-value is 0.061 and the confidence set for ρ runs from −2.8 to 1.33, which includes one. Computed from the same contrast at 36 placebo dates in the two years, of which three have periods that do not overlap, the standard error is 2.3 log points with 3 degrees of freedom ($p = 0.005$). With that standard error, the number of companies with such accounts changes by −2.0% (−14.2 to +12.0) and ρ is 0.10 (−2.5 to 0.45) (Panel B). Omitting both years thus changes the estimate from 19.2 to 17.2 log points, a difference that is within sampling error. The point estimate thus does not depend on the two exposed years, and the inference does: with their own standard error, the two unexposed years do not bound ρ below one, and with the standard error from other dates of the same years they bound it at 0.45. Third, by week of incorporation the share with non-dormant accounts shows no step at May 1 in the cohorts of 2022 and 2023 (−1.5 and −2.3 log points, s.e. 3.6 and 3.6), compared with +19.3 (2.1) in 2024 (Panel E). With the earlier base period and without 2022, the increase in the share is 13.4 log points (s.e. 2.3, $p = 0.029$), the number of companies with such accounts changes by −7% (−18 to +5), and ρ is 0.34 (−0.5 to

0.77) (Panel B).

Accounts ever filed. The primary outcome requires that a company still be on the register when it is observed, so that the outcome combines the filing of accounts with survival on the register (Section 7.4). From the monthly register files we reconstruct whether a company ever had non-dormant accounts on file by the October of its second year, whether or not it was later removed; the files permit this for the cohorts of 2022, 2023, and 2024 [E]. Of the companies registered in the before period of 2024, 39.2% ever had such accounts on file, compared with 44.9% of those registered in the after period. Relative to 2022 and 2023, the share increased by 22.5 log points, compared with 21.3 for the primary outcome with the same two reference years and 18.4 for the share still on the register. The standard errors that the two reference years give are 3.1, 3.4, and 2.9, with one degree of freedom. Computed from the same contrast at 36 placebo dates, the standard error for the share that ever had such accounts is 2.2, with 3 degrees of freedom. With outcomes measured at equal ages, the increase in the share that ever had such accounts is 22.6 log points. When the decline in registrations is measured against nine reference years, the number of companies that ever filed such accounts changed by +3.4% (−9.6 to +18.2), and ρ is −0.16 (−3.7 to 0.28), with a set from −3.6 to 3.18 when the standard error of the two reference years themselves is used; when the decline is measured against the same two years, that number changed by −2.5% (Table A36, Panel C). The estimate therefore does not depend on whether a company remained on the register. In both reference years of this comparison the statement fee differed between the two periods, so that the comparison addresses survival and the preceding paragraph addresses the fees.

Survival and types of accounts. Of the registrations of the before period, 47.3% were still on the register in October 2026, compared with 57.0% of those of the after period. At that age, nearly every company still on the register has accounts of any type on file (46.4 and 55.2% of registrations), so that the primary outcome in effect indicates a company that is still on the register and is not dormant. The point estimates indicate that the deterred registrations were mostly of companies that would have left the register within thirty months without having filed anything: at 27 to 31 months, a deterred registration would have been on the register 0.09 times as often as a remaining registration (−2.6 to 0.46), with or without accounts (Table 4, Panel B), and 0.30 times as often (−0.5 to 0.83) with the earlier base period [E]. Addendum 4 had excluded a survival outcome at this horizon, on the ground that in some of its reference years the fee for a confirmation statement changed between the dates on which the companies of the two periods owed one, which affects survival directly; the estimate is subject to that qualification (Table A6, row 23), and the preceding paragraphs measure the effect of the statement fee on the exposed reference years. The increase in the share does not arise from one type of non-dormant account alone. The share with micro-entity accounts, the simplest type of non-dormant accounts, increased by 17 log points, and the share with fuller accounts increased by 25. Dormant accounts are the only indicator of a company that is maintained without trading. Their share increased by 10.5 log points (s.e. 2.4), less than the share with other accounts, and their number changed by −8% (−24 to +10), so that ρ is 0.43 (−2.2 to 0.86). With the earlier base period, ρ for dormant accounts is 0.28 (−1.1 to 0.77), equal to the estimate for the primary outcome under that comparison [E]. Under the pre-specified base period, the point estimates suggest that companies with dormant accounts were deterred more nearly in proportion to their number than companies with non-dormant accounts; with the earlier base period the two estimates are equal, and under neither base period do the confidence sets distinguish the two types of company. At 15 to 19 months, a deterred registration would have filed a first confirmation statement 0.22 times as often as a remaining registration [A4], with a set from −2.5 to 0.59. Because the two horizons differ and no pair of these estimates differs reliably, we do not infer an ordering of these acts from them.

Classification of the deterred registrations. The register files allow every company of the two periods of 2022, 2023, and 2024 to be assigned to one of nine classes according to what it had filed and whether it remained on the register in the October of its second year (Table A36, Panel F) [E]. For each class, we compare the number of registrations per working day in the after period of 2024 with the number expected from the before period and from the change in that class between the same periods of 2022 and 2023. Measured against these two years, the number of registrations that the fee deterred is 879 per working day, 23% of those expected. This proportion exceeds the 17% of the test because registrations declined less between the same periods of 2022 and 2023 than on average over the nine reference years of the test. Of the deterred registrations, 76% were of companies that would have left the register with nothing on file (74

and 79% when each reference year is used alone), compared with 41% of all expected registrations, and a further 12% were of companies that would have left it having filed only a confirmation statement or dormant accounts. In all, 88% would have left the register within thirty months without having filed non-dormant accounts, compared with 51% of all expected registrations, and 4.5% would at some time have filed such accounts, compared with 35.5%. With the earlier base period, 74% of the deterred registrations were of companies that would have left the register with nothing on file, 83% were of companies that would have left it without having filed non-dormant accounts, and 11.3% would at some time have filed such accounts, compared with 37.2% of all expected registrations. Deterred registrations would thus have filed non-dormant accounts at 0.13 or 0.30 times the expected rate under the two base periods, which is similar to the estimates of ρ under the two base periods (Section 7.3). The classification is descriptive. It has no standard error, and both of its reference years are years in which the statement fee differed between the two periods.

Types of registration at founding. We describe the composition of the deterred registrations from the first monthly file in which each company appears (Table A30). Relative to the same weeks of 2025, the only year with comparable files, registrations at addresses where a hundred or more companies had been incorporated within a year declined by 33 log points, registrations on days when ten or more companies were incorporated at one address declined by 50, and registrations under the industry codes for retail sale by mail order or internet declined by 43, compared with 28 for all registrations under the same comparison. With one reference year, these estimates are descriptive. Each type declined by more than registrations as a whole. Within 2024, the share of registrations at such addresses was 15.2% before the fee increase and 15.3% after it, and the share in same-day batches changed from 11.5 to 9.9%. The two types together account for 19% of the decline in registrations per working day between the two periods, so most of the deterred registrations were ordinary single registrations (Table A35, Panel G) [E].

A comparison that uses every month. A coarser comparison that uses every month yields a result closer to that of the comparison with the earlier base period than to the pre-specified result (Figure 3) [E]. Registrations per working day had grown by 10.1 log points a year in 2023. From April to November 2024, a period that includes the month of anticipatory filing, they were 10.2 log points below the same months a year earlier, a change of 20.3. The number of companies with non-dormant accounts had been growing by 5.4 log points a year and grew by 0.9, a change of 4.5, which by equation (6) corresponds to a ρ of 0.20. This comparison has no reference years. The same double difference for the number of companies with accounts, taken from the weeks before the announcement to the months from late May to November, was +1 and +4 log points in 2022 and 2023, when no fee changed, and −4 in 2024 (Table A35, Panel F).

D.7. The 2022–23 Fee-Reduction Program

The plan for the 2022–23 fee-reduction program is the only one of the five documents written before the project had produced any result other than counts of filings, outcomes for companies formed before July 2019, and five facts that had become known unintentionally (Section B.3). The plan compared ordinary LLCs formed from July 2022 to March 2023 with those formed on the same dates a year earlier. Net of the same change among corporations and foreign entities, the share that filed a first report on time declined by 7.0 percentage points (s.e. 1.0), which the plan interprets as a marginal registration filing at 0.22 times the inframarginal rate [P]. We do not adopt that interpretation, because cohorts formed at $50 in the two years after the program also filed less often than the 2021–22 cohort, by 5.8 and 6.4 percentage points net of established companies, compared with 8.6 for the program cohort on the same basis. The deficit ends with the program only relative to the plan's own comparison group, which includes profit corporations, whose own rate fell sharply after the program. A deficit among companies formed after the regular fee was reinstated does not reflect selection on the reduced fee, because those companies paid the regular fee (Section E.8 reports the estimates for each comparison group and at the reinstatement of the fee). The program's cohorts were also formed after the PPP loans had been made, so the test of Section 6 cannot be applied to them. We use the program to study outcomes that cannot be observed in the short fee holidays: the filings attracted by a $1 fee maintained for almost eleven months, the changes that persisted after the program ended, and its fiscal cost.

Bulk filings. The program year had 47 thousand more registrations than the preceding year. Bulk filings account for 42% of this increase, eight formation services whose combined volume more than doubled account for 15%, and the

two types of filer that made up most of the response to the holidays, repeat filers and new filers, account for 19% and 17% (Table A42) [E]. Bulk filing did not increase immediately (Figure A2). In its first three months the program had 375 bulk filings, no more than the second holiday had in four. One filer then formed 15,527 companies on 95 days between November 9, 2022 and May 19, 2023, with as many as 599 in one day. These companies account for one-tenth of the LLCs registered in Colorado during the program and for 9% of the appropriation. Eight of these companies filed a first report on time, and 99.5% have been dissolved by court order. Two facts complicate the interpretation of these filings as a response to a fee that had fallen below the value of an entity. First, nearly all of the filings had been made by May 13, 2023, six days before the Secretary of State's office announced that the regular fee would be reinstated, so the end of this wave of filings cannot be attributed to the fee. Second, 1,625 companies had been formed from the same address on 42 days of 2020, at $50 each [E]. No agent name links the two waves and we cannot establish that a single filer made both, but bulk filing from that address was worth $50 per entity before the fee was reduced. Bulk filing has since resumed at the regular fee. The rule that identifies bulk groups classifies 61 and 87 filings as bulk in the two years after the program, 6,390 in 2025–26, and 2,421 from July to early October 2026 [E].

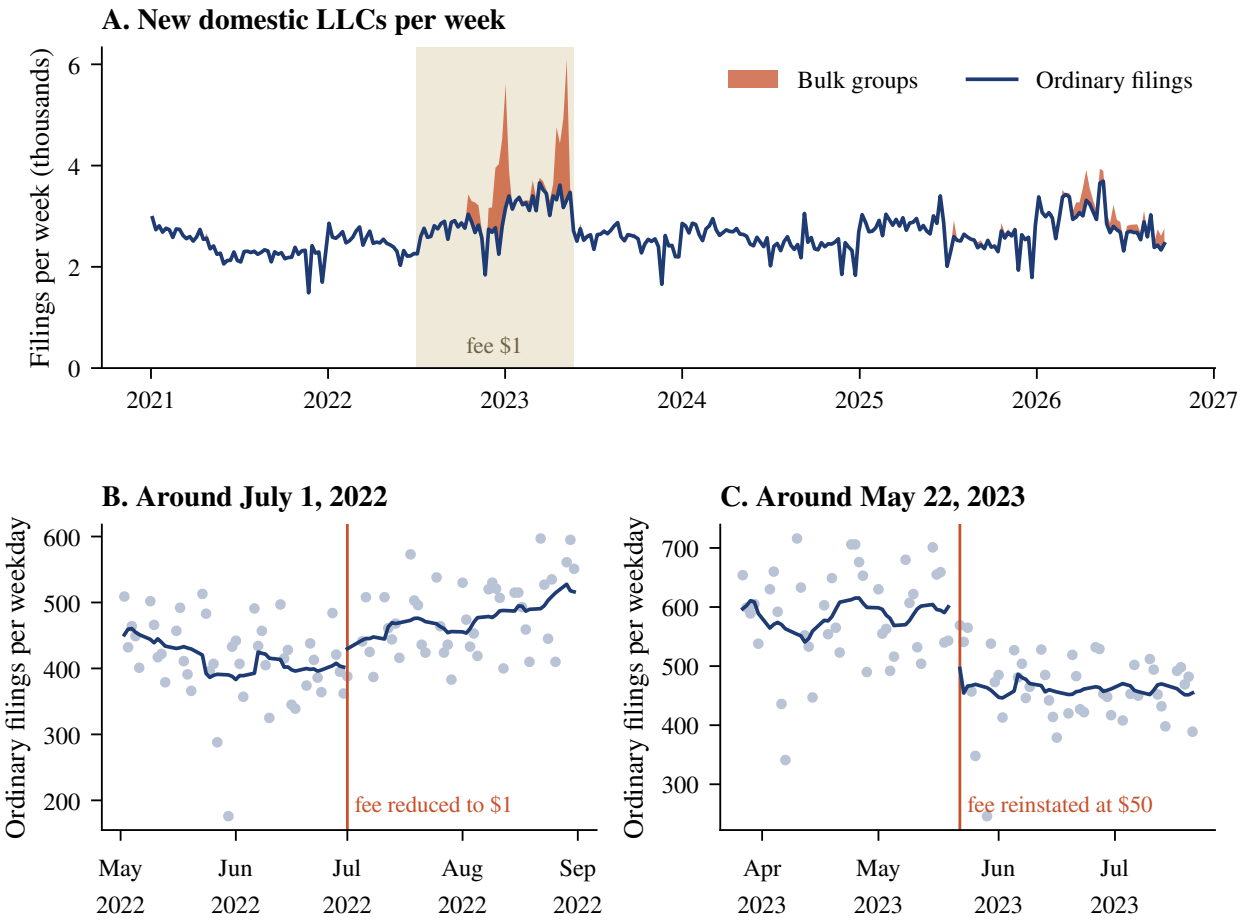


Figure A2. Filings to Form a Domestic LLC in Colorado, January 2021 to September 2026

Notes. Panel A: filings received per week, from January 2021 to the last complete week before the snapshot of October 3, 2026; shading marks the weeks of the program. Panels B and C: ordinary filings per weekday (dots; public holidays omitted) and a ten-day centered moving average computed separately on each side of the date (line). Panel B shows no level shift at the date and Panel C shows a sharp one. On the day the reduction took effect, the Secretary of State's office announced for the first time which fees were reduced to $1, and filings increased over the following two months. The reinstatement of the $50 fee was announced on the preceding Friday and applied to every filer from its first day, and filings in the three days after the announcement were 1.01 times their usual number, so the level shift does not reflect anticipatory filing.

Formation services. Eight formation services made 5,997 filings in 2021–22, 13,090 in the program year, 13,156 in the year after, and 12,265 in the year after that. These services are defined by the persistence of their higher volume, and their increase accounts for 64% of the increase in ordinary filings that remained one year after the fee was reinstated [E]. A sunk cost of setting up in a state would explain a level shift that outlasts the fee reduction, but only for services new to the state, and a pre-specified tabulation of which filers account for the level shift shows the opposite pattern: 62% of it is accounted for by formation services first observed more than two years before the year of the filing [A1], and 58% by the services first observed before July 2021, taken as a fixed group [E]. Because the registry reports each company's agent as of October 2026, a service that later became the agent of an older company would be dated too early. When each service is dated only from companies whose agent information has not been changed by a later statement, the first share is 52%; it is 29% when companies covered by a statement that an agent filed for many companies on one day are also excluded. Services first observed in the program year account for 3 and 4% of the level shift in the two cases (Section E.8) [E]. Services established in Colorado thus gained volume that, under this explanation, they could not have gained, and they retained it after the fee was reinstated. Relative to a state with no fee change, the level shift is diminishing: for those of the eight services that can be found in Connecticut's registry, where no fee changed, new LLCs formed through them in Colorado outgrew those formed through them in Connecticut by 74 log points in the program year and by 39 log points two years after it (Figure A8) [E].

Filings later marked as unauthorized or fraudulent. The share of ordinary LLCs that the state has marked as unauthorized or fraudulent is 0.04% for those formed in 2021–22, 0.30% in the program year, and 0.76% and 0.97% in the two years after [E]. Marks follow complaints, and most were entered in 2026, so they measure enforcement as well as fraud. Addendum 1 specified a rule for linking marked and bulk filings made after the program to the program's filers, and over the whole period since the fee was reinstated the rule is not satisfied [A1]. Applied separately by period of formation, a division that the addendum did not specify, the rule is satisfied for the thirteen months after the fee was reinstated but not for the larger waves since then (Section E.8), so that the first wave after the program came in part from the same filers, whereas nothing recorded in the registry connects the later waves to the program's filers [E].

Business applications. Applications for Employer Identification Numbers (EINs), which the Census Bureau publishes by state and month as *business applications* (U.S. Census Bureau, 2026a), are an outcome intermediate between a registration and a payroll, and the literature uses this series as its timeliest measure of business formation. We compare Colorado with a weighted combination of other states by synthetic difference-in-differences (Arkhangelsky et al., 2021). The estimate for business applications is positive in each episode and smaller than the increase in registrations; in the first holiday it is not statistically distinguishable from zero (5.0 log points, s.e. 4.5). In the second holiday, business applications increased by 9.8 log points (s.e. 3.3), which corresponds to 0.35 applications per additional registration [A2]; with the uncertainty in the number of additional registrations taken into account, the 95% confidence interval extends from 0.12 to 0.65 [E]. In the program the corresponding figure is 0.23 per additional registration of any filer type [P]. As a benchmark, the business applications that Colorado would have had in the program's months in the absence of the program are 0.76 per filing that formed an entity of any type in the same months one year earlier, a ratio that overstates the applications associated with one filing, because sole proprietors apply without forming an entity. The program's additional registrations were accompanied by business applications at an estimated 0.3 of that ratio (confidence interval 0.1 to 0.7, which allows for the error in the number of additional registrations) [E]. The increase in applications persisted after the fee was reinstated, however, so it cannot be attributed to the registrations made at $1 alone (Section E.9). A business application is also weaker evidence of business activity than a loan or a payroll, because an EIN is free, is issued online within minutes, and is sold by formation services together with the company. High-propensity business applications, which the Bureau classifies as likely to become employers, increased together with other applications in the holidays; that classification is assigned from the information that the applicant enters on the form, and it includes applications from corporations other than those associated with internet sales, which, since the release for January 2026, the Bureau has excluded from the whole history of the series (U.S. Census Bureau, 2026b). Business applications with planned wages show no increase that is statistically distinguishable from zero in any episode, and neither do business formations within four quarters (employer businesses formed within a year of the application),

which were estimated for the program and not for the holidays; none of these estimates excludes an increase of several percent (Section E.9).

Fiscal cost. The legislature appropriated $8.4 million. Fee relief of $49 on the 156,797 LLCs formed at $1 totals $7.7 million, of which 70% went to a number of filings equal to the number made at $50 in the preceding year and 13% went to bulk groups. The $6.7 million of relief on ordinary filings amounts to $246 per additional ordinary registration, or $362 net of a usual year's growth. For the holidays, the relief per additional registration, net of the filings retimed from the surrounding months, is $365.

Relative to the holidays, the program adds evidence on the filers that appeared when a fee near zero was maintained for almost eleven months and that had not appeared within three or four months: a bulk filer, a small number of formation services, and some of the filers whose filings the state later marked as unauthorized or fraudulent. For the bulk filer, the dates do not establish that the fee was the cause. None of these can be identified as a founder at the margin of starting a business, although the filings of formation services may include such founders. We cannot determine from the holidays whether a short holiday does not attract such filers or merely ends before they arrive.

E. Additional Tables and Figures

Each note states the status of what it reports, denoted by the letters of Table A5: [P] for what the plan specified, [A1] to [A4] for what an addendum specified before the new tabulations that it governs and after the results that preceded it, and [E] for what neither specified. Appendix B lists each provision with its outcome. Standard errors are in parentheses throughout. Unless a note states otherwise, they are computed from the same statistic in reference years and not from sampling variation. A log point is one-hundredth of a unit of the natural logarithm. A column headed "log points" is in that unit. A column or panel headed "log" and any entry that a note defines as a difference of logarithms (as in Table A14) are in units of the natural logarithm, as printed in the tables specified in the earlier documents (0.160 is 16.0 log points). In some of those tables, rates are printed as proportions and differences between rates in percentage points; each heading or note indicates which applies. The plan and the addenda use some terms that differ from those of the paper and of these tables; Table A4 lists the correspondence.

E.1. Earlier Evidence on Barriers to Entry

Table A13 summarizes studies of barriers to entry, most of which Section 2 discusses, together with two studies of fees outside company law. For each study, the table reports what changed and by how much, the response of entry, and what is known about the marginal entrants.

Table A13. Earlier Evidence on Barriers to Entry: Size of the Change, Response of Entry, and Marginal Entrants

Study and setting	Barrier and size of the change	Change in entry	Marginal entrants
Faster or cheaper registration			
Branstetter et al. (2014), Portugal	One-stop shop: cost from almost €2,000 to about €360; delay from 54–78 days to between an hour and a day	New firms up 17%	Small firms in low-technology sectors, with less educated founders; two-year survival of all firms formed after the reform about 4 percentage points lower
Bruhn (2011), Mexico	One-stop shop: 30 days to 1.4, eight procedures to three; no change in monetary cost reported	Registered businesses up 5% in eligible industries	Former wage earners; owners of informal businesses no more likely to register
Kaplan et al. (2011), Mexico	The same reform	Monthly number of new firms up about 5% in eligible industries, for about 15 months	Not observed directly. The effect was temporary, and the authors conjecture that it mostly registered informal firms that already existed
Mullainathan and Schnabl (2010), Lima	Municipal license: average cost from $212 to $124; median time from 40 days to 16	Newly licensed firms from 1,758 to 8,517 in a year	About three-quarters had been operating without a license
Barwick et al. (2025), Guangdong	Pilot of 2012 to 2014 that reduced the cost of registering a firm	Entry up 25% and exit up 8.7% in manufacturing	Entrants 1.1% more productive than those before the reform

Table A13. (continued)

Study and setting	Barrier and size of the change	Change in entry	Marginal entrants
Capital, credit and insurance			
Bacher et al. (2026), Norway	Minimum equity from NOK 100,000 to NOK 30,000 (about $17,000 to $5,000)	Incorporations nearly doubled	Smaller firms; survival, profitability and productivity no worse
Cui and Wei (2022), China	Capital to be paid in at formation, the greater of 20% of registered capital and CNY 30,000, no longer required from March 2014	Entry of limited liability companies up 33–36%	Total assets 31.6% smaller; revenues similar; more firms owned by one person
Braun et al. (2013), five European countries	Reforms of the private company in Spain, France, Hungary, Germany and Poland; minimum capital reduced or abolished in some	More incorporations, most where minimum capital was reduced or abolished	No evidence
Massenz (2025), Netherlands	Minimum capital of a private company abolished	More incorporation among the self-employed near a kink in the income tax	No significant change in total business activity in the short term; many keep an unincorporated firm alongside the company
Hombert et al. (2020), France	The unemployed keep rights to benefits while starting a firm	New firms up by about a quarter (10% in the regressions)	Start smaller; growth, productivity and survival similar
Kerr and Nanda (2009), United States	Deregulation of interstate branch banking	Start-up births up 6%	Most closures were of the new ventures; start-ups that closed within three years up 22% relative to new establishments of existing firms, and lasting entry up 6%
Andersen and Nielsen (2012), Denmark	An unexpected inheritance (no reform)	Not summarized here	Founders who started after a windfall had lower survival and profits than comparable founders
Field experiments with informal firms			
de Mel et al. (2013), Sri Lanka	Refund of the cost of registering, or payments of $88 to $350 for registering	Refund: 1 of 104 registered, no more than without it. Payments: 17–22% and 48% registered	Higher mean profits, driven by a few firms that grew fast
Benhassine et al. (2018), Benin	A free legal status obtainable in a day, with or without personal help	With help, registration up 10 to 16 percentage points from 2%; information alone, no effect	No gain in sales or profits
Asuming et al. (2025), Ghana	Waiver of the registration fee, information, or logistical support for registering, among almost 3,000 informal firms	Support: registration up 7.2 percentage points. Fee waiver and information: no effect	Not summarized here
Cross-country comparisons			
Klapper et al. (2006)	Cost of entry from 1% to 86% of income per capita	Less entry in industries where entry is naturally high	Costly entry is associated with larger entrants
Klapper and Love (2016)	Reforms of cost, time, procedures and capital in 91 countries	Registrations increase after reductions of half or more in cost or time; for procedures, a reduction of a fifth is sufficient	No evidence
Fees outside company law			
de Rassenfosse and Jaffe (2018), United States	Patent fees raised in 1982, from $239 to $800 up to grant	Applications from 116,052 in 1982 to 96,847 in 1983	About a tenth of patents in the lowest tenth by quality were removed, among holders of more than twenty patents

Table A13. (continued)

Study and setting	Barrier and size of the change	Change in entry	Marginal entrants
Internet Corporation for Assigned Names and Numbers (2009), internet domain names	A charge on deleting newly registered names for a refund: none, then $0.20, then $6.75 or more	Such deletions down 99.7%, from 17.7 million to 58 thousand a month	The free option had been used to test names in bulk
This paper			
Colorado and the United Kingdom	Colorado: LLC formation fee reduced from $50 to $1 for three, four and almost eleven months. United Kingdom: incorporation fee increased from £12 to £50	Registrations increased by 13% and 20% during the two fee holidays and decreased by 17% after the U.K. fee increase	Relative rate at which the registrations that responded to the fee have each record. Colorado, PPP loan (the primary outcome): 0.07 as pre-specified (95% CI −0.57 to 0.60), and −0.41 to 0.81 under other reference periods, with intervals that include one when the holiday months are compared with the flanking months. Any of three lending records: −0.02 (−0.65 to 0.41), and −0.23 to 0.34 under other reference periods, with every interval below one. United Kingdom, non-dormant accounts: 0.00 (95% confidence set −2.5 to 0.42), and 0.28 (−0.4 to 0.95) relative to the weeks before the announcement (Sections 6 and 7)

Notes. Magnitudes are those reported by the authors of each study. For Branstetter et al. (2014), the increase in entry, the costs, the delays and the difference in survival are from the working-paper version. For Kaplan et al. (2011), the published abstract reports 5% and 15 months, whereas the working paper reports 4% and ten months, and the conjecture in the last column is from the working paper. For Mullainathan and Schnabl (2010), the averages changed differently from the medians: average time fell from 110 days to 15. For Cui and Wei (2022), the entries are from the version of December 2023. For Kerr and Nanda (2009), the comparison is with new establishments of existing firms. For Andersen and Nielsen (2012) and Braun et al. (2013), we rely on the abstracts, and for Bacher et al. (2026), Barwick et al. (2025), Benhassine et al. (2018) and Hombert et al. (2020), on a working-paper version or the authors' manuscript. The studies in the second group changed capital at risk, credit or the cost of failure and not a cost of registration. Klapper et al. (2006) compare countries and study no reform; the range of costs is from the working-paper version. For de Rassenfosse and Jaffe (2018), the number of applications is from the working paper, which records bunching of filings before the increase in 1982; for the share in the last column, the published abstract reports about 10% and the working paper 14%, with no effect among small and medium-sized holders. The entries in the last row are this paper's estimates: [A2] for registrations in Colorado, [A3] for the pre-specified estimates for the lending records, [A4] for the pre-specified U.K. estimates, and [E] for the estimates under other reference periods, the confidence set by test inversion, and the U.K. estimate relative to the weeks before the announcement.

E.2. Registrations

This subsection presents the tables and figures that underlie Section 5: filings during the two fee holidays by entity type and by filer type, monthly series for the three episodes, the cohort years around the 2022–23 fee-reduction program, the timing of filings around each holiday, and the other types of filing whose fees were reduced in the first holiday.

Table A14. Formation Filings during the Two Fee Holidays

A. By entity type

	First holiday			Second holiday			
Filings to form	Fee reduction	Holiday months	Whole window	Fee reduction	Holiday months	Whole window	Pooled, holiday months
Ordinary domestic LLCs	$49	+0.124 (0.026)	+0.063 (0.024)	$49	+0.186 (0.025)	+0.124 (0.023)	+0.160 (0.018)
Profit corporations	$49	+0.252 (0.053)	+0.136 (0.047)	$49	+0.242 (0.050)	+0.162 (0.044)	+0.246 (0.037)
Nonprofit corporations	$19	−0.019 (0.044)	−0.058 (0.038)	$49	+0.203 (0.040)	+0.147 (0.036)	
Foreign entities	$99	+0.055 (0.051)	−0.009 (0.046)	none	+0.008 (0.048)	−0.005 (0.043)	
Additional ordinary LLC filings, share		0.117 [0.07, 0.16]	0.094 [0.01, 0.17]		0.170 [0.12, 0.22]	0.161 [0.09, 0.23]	

B. Ordinary LLC filings by filer type

Ordinary LLC filings in the holiday months	First holiday	Second holiday	Pooled	Percent of the additional filings
Formation service	+0.114 (0.048)	+0.172 (0.047)	+0.147 (0.034)	14
Repeat filer	+0.206 (0.026)	+0.255 (0.024)	+0.234 (0.018)	41
New filer	+0.085 (0.023)	+0.145 (0.021)	+0.119 (0.016)	40
Address with 20 or more in the previous year	+0.321 (0.092)	+0.514 (0.078)	+0.432 (0.060)	5
People who act as their own agent, by name				
Names new to the registry (number of names)	+0.071 (0.029)	+0.165 (0.028)	+0.125 (0.020)†	
Filings by names seen before	+0.210 (0.030)	+0.201 (0.028)	+0.205 (0.020)	
Share of new names with two or more filings (percentage points)	+1.51 (0.13)	+1.65 (0.10)	+1.59 (0.08)	

Notes. Each entry is T_N of equation (4): log filings in the window minus the mean of log filings in the same months one year earlier and one year later, with the standard deviation of the same statistic over unaffected reference runs in parentheses. *Whole window*: the month of the announcement, the holiday months and the month after. *Fee reduction*: the decrease in the fee to form that entity type. *Additional ordinary LLC filings, share*: additional filings as a share of filings in the holiday months, counted in the holiday months (under *Holiday months*) and net of the flanking months (under *Whole window*), with 95% confidence intervals. For nonprofit corporations in the first holiday, the regular fee differed between the two reference years ($20 and $50). For foreign entities in the first holiday, the year-before reference period follows their own fee holiday of 2011; relative to the year after only, the estimate is +0.026 (0.065). Panel B: the filer types are those of Section 4.2, which Addendum 2 labeled *organization agent* (a filing through a formation service), *person agent: additional company* (a filing by a repeat filer) and *person agent: first company in twelve months* (a filing by a new filer); Appendix C gives the order in which a filing is assigned. The last column allocates the pooled additional filings among the four types. The last three rows refer to filings whose registered agent is a person: the number of names new to the registry in the month, defined as names under which no entity had been formed earlier (Appendix C); filings by names seen before; and the share of new names with two or more filings in their first month, in percentage points. † The two holidays differ by more than twice the standard error of their difference. Specified in Addendum 2 (items C1 and C3) [A2]; the last column of Panel B was not pre-specified [E].

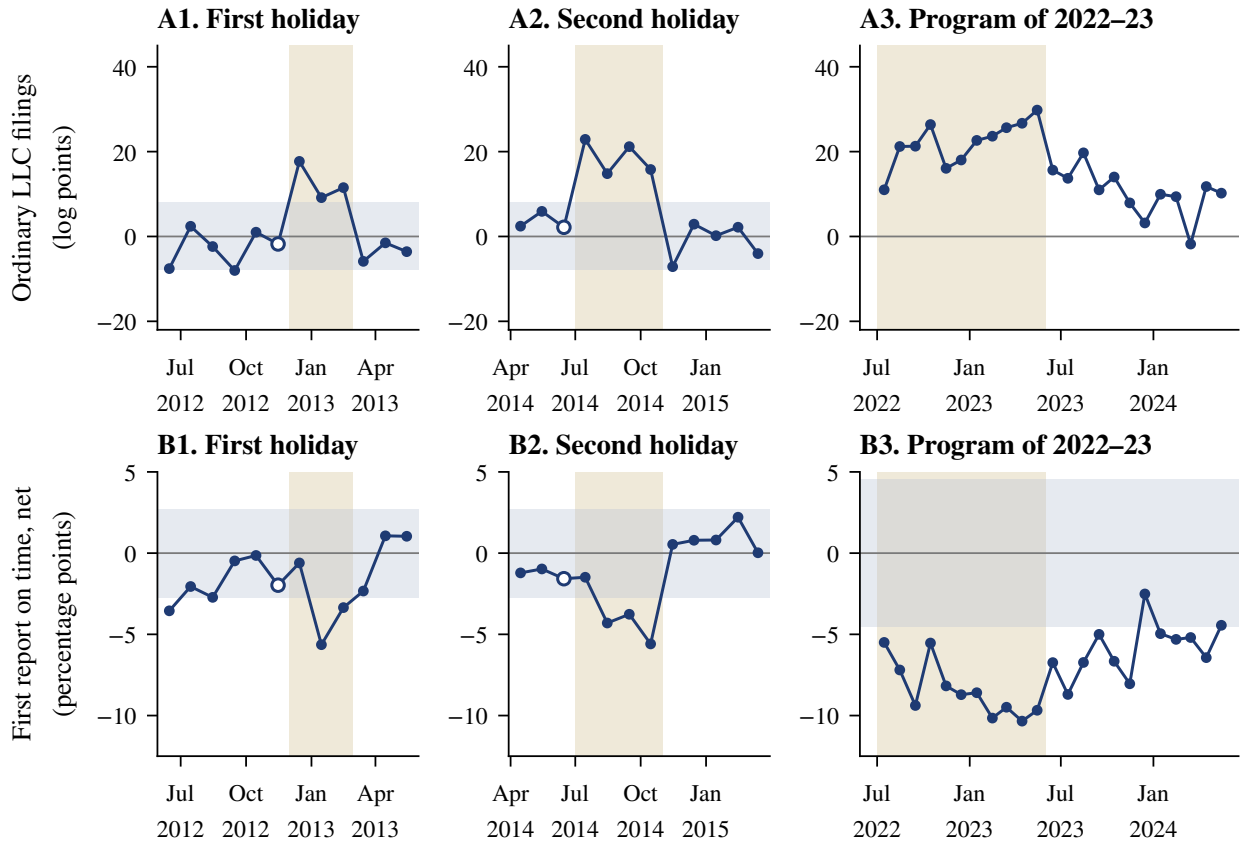


Figure A3. Three Episodes with a $1 Fee: Filings and First Reports by Month of Formation

Notes. Ordinary filings to form a domestic LLC (top) and the share of the companies formed in each month that filed a first report on time, net of established companies (bottom). For the holidays, each month is compared with the mean of the same month one year earlier and one year later; the open circle marks the month of the announcement. For the program, each month is compared with the same calendar month of July 2021 to June 2022. Shading marks the months in which the fee was $1. The band is two standard deviations of the same statistic for a single unaffected month. The bottom panels subtract the rate of established companies; Section 6.5 reports the extent to which the estimates for the holidays depend on that adjustment, and Section E.8 explains why the months after the program, when the fee was $50, bear on the interpretation of Panel B3. The monthly series were not pre-specified [E].

Table A15. Formation Filings, Filers and Outcomes by Cohort Year, 2019–20 to 2024–25

	2019–20	2020–21	2021–22	2022–23	2023–24	2024–25
Fee to form an LLC	$50	$50	$50	$1	$50	$50
A. Formation filings received from July 1 to May 21						
Domestic LLCs	83,403	110,052	109,705	156,797	120,886	120,567
in bulk groups	0	524	0	19,789	61	87
ordinary filings	83,403	109,528	109,705	137,008	120,825	120,480
Profit corporations	9,715	13,842	12,029	10,116	16,963	21,794
Nonprofit corporations	3,198	3,327	3,394	3,433	3,838	3,927
Foreign entities	10,144	10,827	12,215	10,564	9,941	10,056
B. Ordinary LLC filings: filer characteristics (shares)						
Formation service as registered agent	0.190	0.213	0.232	0.259	0.273	0.263
Principal address outside Colorado	0.054	0.058	0.056	0.059	0.056	0.053
Another new LLC at the same address in the year	0.411	0.448	0.460	0.516	0.483	0.487
New filer	0.486	0.459	0.432	0.403	0.401	0.414
Repeat filer	0.287	0.286	0.292	0.298	0.285	0.283
C. Ordinary LLCs: subsequent outcomes (shares)						
First report on time	0.476	0.457	0.471	0.406	0.419	0.403
First report before delinquency	0.598	0.573	0.567	0.486	0.507	0.486
Second-year report before delinquency	0.477	0.448	0.457	0.371	0.383	
In good standing, October 2026	0.336	0.354	0.395	0.352	0.435	0.497
Secured credit within 24 months	0.035	0.034	0.034	0.022	0.024	
D. First report on time, entity types whose fee did not change						
Profit corporations	0.453	0.394	0.382	0.390	0.319	0.252
Nonprofit corporations	0.503	0.502	0.516	0.513	0.473	0.448
Foreign entities	0.632	0.661	0.668	0.679	0.672	0.661

Notes. A cohort year begins on July 1. Panels A and B count filings received from July 1 to May 21, the dates of the program. In Panel B, *new filer* and *repeat filer* are the filer types defined in Section 4.2; the two shares do not sum to one because filings through formation services and filings from addresses with twenty or more filings are classified separately. Panels C and D: reports refer to companies formed from July 1 to March 31, and good standing and secured credit to those formed through May 21. A blank cell indicates an outcome that is not yet observable. Bulk groups are identified by the rule of Section 4.2. The 524 bulk filings of 2020–21 belong to two linked groups, and 351 of them carry the address key of the program's largest bulk group (Section 5). Descriptive and not pre-specified [E].

Table A16. Log Change in Formation Filings by Entity Type around the Program

	2021–22 to 2022–23	Earlier years: mean (s.d.)	2021–22 to 2023–24	2021–22 to 2024–25
Domestic LLCs, all filings (fee reduced)	+0.357	+0.074 (0.084)	+0.097	+0.094
ordinary filings	+0.222	+0.074 (0.082)	+0.097	+0.094
Profit corporations	−0.173	+0.026 (0.134)	+0.344	+0.594
Nonprofit corporations	+0.011	+0.027 (0.043)	+0.123	+0.146
Foreign entities	−0.145	+0.080 (0.073)	−0.206	−0.194

Notes. Filings received from July 1 to May 21 of each cohort year. *Earlier years* are the eleven changes between adjacent cohort years ending 2011–12 to 2021–22, which include the years of the two holidays. Among formation fees, the program reduced only the fee for LLCs. The first column is item H7 of the plan, which the plan itself described as exploratory and for which it specified no test [P]. The comparisons with 2023–24 and 2024–25 were added later [E].

Table A17. Timing of Filings around the Two Fee Holidays and Companies Formed in Each Window

A. Filings per business day in windows defined by the announced dates (log)

Log filings per business day	First holiday		Second holiday	
	Net of other years	Permutation p	Net of other years	Permutation p
Announcement to start, relative to the four weeks before (delay predicts a decline)	+0.047 (0.023)	0.125	−0.072 (0.048)	0.111
First two weeks at $1, relative to the middle of the holiday	+0.053 (0.042)	0.250	+0.027 (0.045)	0.444
Last two weeks at $1, relative to the middle of the holiday	+0.076 (0.038)	0.125	+0.015 (0.044)	0.778
Last week at $1, relative to the first week at $50	+0.292 (0.079)	0.125	+0.182 (0.050)	0.111
First two weeks after, relative to weeks five to eight after	−0.052 (0.039)	0.375	−0.054 (0.024)	0.111

B. Companies formed in each window (shares)

Ordinary LLCs formed in	Filings	Report on time	Before delinquency	Good standing, Oct. 2026	Formation service as agent	Repeat filer
First holiday (announced November 19, 2012; December 1, 2012 to February 28, 2013)						
Four weeks before the announcement	4,388	0.478	0.607	0.240	0.150	0.280
Announcement to start	1,726	0.499	0.617	0.236	0.159	0.263
First two weeks at $1	2,712	0.507	0.602	0.251	0.157	0.288
Middle of the holiday	12,388	0.472	0.574	0.216	0.146	0.283
Last two weeks at $1	3,248	0.461	0.544	0.210	0.151	0.276
First two weeks after	2,654	0.447	0.555	0.214	0.146	0.261
Weeks five to eight after	5,079	0.371	0.539	0.225	0.156	0.269
Second holiday (announced June 9, 2014; July 1 to October 31, 2014)						
Four weeks before the announcement	5,576	0.459	0.576	0.223	0.158	0.270
Announcement to start	4,250	0.464	0.580	0.223	0.172	0.280
First two weeks at $1	2,780	0.444	0.545	0.207	0.153	0.294
Middle of the holiday	21,364	0.428	0.534	0.215	0.158	0.296
Last two weeks at $1	3,160	0.416	0.541	0.217	0.179	0.281
First two weeks after	2,640	0.472	0.590	0.257	0.169	0.282
Weeks five to eight after	4,964	0.496	0.615	0.280	0.160	0.292

Notes. Ordinary LLC filings. Panel A: the log ratio of filings per business day in the first window to that in the second, minus the mean of the same log ratio at the same calendar dates in the years from 2010 to 2019 whose span contains no month of either holiday, which are seven years for the first holiday and eight for the second. The standard error is the standard deviation across those years times $\sqrt{1 + 1/n}$. The permutation p-value is the rank of the estimate among the values for those years, so that 0.125 and 0.111 are the smallest attainable values. The predictions were a decline in the first row if filers who are able to delay a filing do so, an increase in the second, third and fourth rows, and a decline in the fifth. The middle of a holiday runs from its fifteenth day to the day before its last two weeks begin. Panel B: raw shares, not net of other years or of established companies. *Formation service as agent* is a filing whose registered agent is an organization, and *repeat filer* is a filing whose agent is a person with another filing under the same name or address in the preceding 365 days. First reports of companies formed from December 2012 to February 2013 fell due at the regular fees. Specified in Addendum 2 (item C2) [A2]; Panel B was to be reported without a prediction.

Table A18. Other Registry Filings during the Two Fee Holidays

	Regular fee	Fee in the first holiday	Filings in the first holiday	First holiday (log)	Second holiday, no fee reduction (log)
Name reservations	$25	$1	2,059	+0.639 (0.107)	+0.071 (0.087)
Amendments of articles	$25	$1	2,562	+0.194 (0.079)	−0.003 (0.079)
Dissolutions	$25	$1	5,805	+0.124 (0.121)	+0.005 (0.114)
Reinstatements	$100	$1	276	+0.130 (0.104)	−0.146 (0.097)
Withdrawals of foreign authority	$125	$1	745	+0.088 (0.116)	−0.043 (0.110)
Trade names of individuals	$20	$1	7,341	+0.057 (0.101)	−0.023 (0.085)
Trade names of reporting entities	$20	$1	4,471	+0.152 (0.040)	+0.077 (0.038)
Trade-name withdrawals	$10	$1	574	+0.233 (0.094)	+0.036 (0.083)
Trade-name renewals	$5	$1	11,027	−0.013 (0.033)	−0.008 (0.027)
Changes of principal address	$10	$1	3,386	+0.182 (0.054)	−0.037 (0.049)
Changes of registered agent	$10	$1	9,751	+1.028 (0.711)	−0.613 (0.635)
Periodic reports	$10	$1	105,508	−0.047 (0.046)	+0.002 (0.041)
Statements curing delinquency	$100	$60	7,188	+0.271 (0.044)	−0.037 (0.035)

Notes. Each estimate is the log of the number of filings received in the holiday months minus the mean of the logs for the same months one year earlier and one year later. The standard error is the standard deviation of the same statistic over runs of months of the same length that are unaffected by a holiday. The first holiday (December 2012 to February 2013) reduced every fee shown except the fee for a statement curing delinquency; the second (July to October 2014) reduced none of them. The fee to cure a delinquency was $60 from August 2012 to October 2013 and $100 in the same months of the adjacent years, so its estimate for the first holiday measures that difference and not the effect of the holiday. Changes of registered agent are dominated by a few agents updating many records at once, and their estimates are uninformative. Trade names of reporting entities and changes of address were expected to increase with the number of new companies. Filings are counted as rows of the history file as the locked script read them, and the file contains exact duplicates of some rows (Appendix C); an estimate is affected only to the extent that the share of duplicates in the holiday months differs from the share in the same months of the adjacent years. Specified in Addendum 2 (item C6) [A2].

E.3. Addendum 3: All Pre-specified Statistics

Tables A19 to A24 report every statistic specified in Part C of Addendum 3 for Colorado companies in the lending records, which the addendum calls external records: Paycheck Protection Program (PPP) loans, loans guaranteed by the Small Business Administration (SBA loans), and Uniform Commercial Code (UCC) financing statements. A single script generates the tables from the result file of the locked analysis and stops if any stored value is neither reported nor listed as omitted, with the reason for the omission, so that no statistic can be removed by hand. These analyses were specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known. Table A25 reports a follow-up analysis that was run after the results of these analyses were known and is exploratory. The intervals for ρ in these tables are those of the locked script, which takes percentiles over the joint draws with a marginal share above 0.01. Section D.1 compares them with confidence sets by test inversion. Apart from the estimates for filings through formation services, the two agree closely for the pooled estimates and for the second holiday. They differ at the lower end for the first holiday alone, where the marginal share is small, and at both ends for filings through formation services.

Table A19. Colorado, External Records: Holiday Months, the Ratio ρ, and Rates per Hundred

A. The statistic T for the holiday months, in percentage points (sections C1 and C2)

		T, pct. points (s.e.)	95% CI	d.f.	Reference runs	Permutation p	Mean of the reference values
PPP loan (primary)	First holiday	−0.74 (0.26)	[−1.32, −0.16]	10	30	0.032	+0.04
	Second holiday	−0.68 (0.22)	[−1.20, −0.17]	7	27	0.036	+0.05
	Pooled	−0.71 (0.17)	[−1.10, −0.31]	7			
PPP loan, two or more jobs	First holiday	−0.63 (0.18)	[−1.02, −0.24]	10	30	0.032	+0.03
	Second holiday	−0.43 (0.15)	[−0.79, −0.08]	7	27	0.036	+0.04
	Pooled	−0.52 (0.11)	[−0.79, −0.25]	7			
SBA loan	First holiday	−0.26 (0.08)	[−0.44, −0.07]	10	30	0.032	−0.01
	Second holiday	−0.04 (0.07)	[−0.21, +0.14]	7	27	0.750	−0.02
	Pooled	−0.13 (0.06)	[−0.26, 0.00]	7			
Either federal loan	First holiday	−0.95 (0.23)	[−1.47, −0.44]	10	30	0.032	+0.02
	Second holiday	−0.72 (0.19)	[−1.18, −0.27]	7	27	0.036	+0.04
	Pooled	−0.82 (0.15)	[−1.17, −0.47]	7			
UCC financing statement	First holiday	−1.31 (0.42)	[−2.25, −0.36]	10	30	0.032	−0.05
	Second holiday	−1.20 (0.39)	[−2.13, −0.28]	7	27	0.036	−0.04
	Pooled	−1.25 (0.29)	[−1.93, −0.57]	7			
Any of the three records	First holiday	−1.95 (0.39)	[−2.82, −1.09]	10	30	0.032	−0.06
	Second holiday	−1.55 (0.36)	[−2.40, −0.71]	7	27	0.036	−0.04
	Pooled	−1.72 (0.26)	[−2.35, −1.10]	7			

Table A19. (continued)

B. The ratio ρ and rates per hundred, with the marginal share computed for the holiday months (sections C1 and C2)

		Marginal share (s.e.)	Correlation in the draws	ρ [95% CI]	Per 100 marginal registrations [95% CI]	Per 100 infra-marginal	Draws discarded (percent)
PPP loan (primary)	First holiday	0.117 (0.023)	−0.03	−0.18 [−1.61, 0.75]	0.0 [0.0, 4.0]	5.4	0.04
	Second holiday	0.170 (0.021)	0.03	0.21 [−0.55, 0.81]	1.0 [0.0, 4.1]	5.1	0.01
	Pooled	0.147 (0.015)	0.01	0.07 [−0.57, 0.60]	0.4 [0.0, 3.1]	5.2	0.00
PPP loan, two or more jobs	First holiday	0.117 (0.023)	0.22	−0.55 [−2.53, 0.48]	0.0 [0.0, 1.7]	3.5	0.04
	Second holiday	0.170 (0.021)	0.26	0.18 [−0.77, 0.86]	0.6 [0.0, 2.7]	3.1	0.01
	Pooled	0.147 (0.015)	0.24	−0.08 [−0.89, 0.52]	0.0 [0.0, 1.7]	3.3	0.00
SBA loan	First holiday	0.117 (0.023)	0.29	−1.35 [−4.74, 0.46]	0.0 [0.0, 0.4]	0.9	0.04
	Second holiday	0.170 (0.021)	0.31	0.74 [−0.67, 1.88]	0.6 [0.0, 1.6]	0.9	0.00
	Pooled	0.147 (0.015)	0.30	0.00 [−1.28, 1.00]	0.0 [0.0, 0.9]	0.9	0.00
Either federal loan	First holiday	0.117 (0.023)	0.04	−0.37 [−1.82, 0.42]	0.0 [0.0, 2.5]	5.9	0.04
	Second holiday	0.170 (0.021)	0.11	0.23 [−0.43, 0.73]	1.3 [0.0, 4.1]	5.5	0.01
	Pooled	0.147 (0.015)	0.08	0.02 [−0.56, 0.47]	0.1 [0.0, 2.7]	5.7	0.00
UCC financing statement	First holiday	0.117 (0.023)	0.28	−0.33 [−2.22, 0.68]	0.0 [0.0, 5.7]	8.4	0.05
	Second holiday	0.170 (0.021)	0.31	0.14 [−0.83, 0.82]	1.2 [0.0, 6.7]	8.2	0.01
	Pooled	0.147 (0.015)	0.30	−0.02 [−0.83, 0.58]	0.0 [0.0, 4.8]	8.3	0.00
Any of the three records	First holiday	0.117 (0.023)	0.27	−0.44 [−2.03, 0.33]	0.0 [0.0, 3.8]	11.6	0.04
	Second holiday	0.170 (0.021)	0.34	0.20 [−0.53, 0.67]	2.2 [0.0, 7.6]	11.3	0.00
	Pooled	0.147 (0.015)	0.31	−0.02 [−0.65, 0.41]	0.0 [0.0, 4.7]	11.5	0.00

C. The same with the marginal share net of the flanking months (section C7, version iv)

		Marginal share, net (s.e.)	ρ [95% CI]	Per 100 marginal registrations [95% CI]	Draws discarded (percent)
PPP loan (primary)	First holiday	0.094 (0.035)	−0.47 [−4.47, 0.73]	0.0 [0.0, 3.9]	2.11
	Second holiday	0.161 (0.028)	0.16 [−0.94, 0.85]	0.8 [0.0, 4.3]	0.15
	Pooled	0.132 (0.022)	−0.03 [−0.99, 0.57]	0.0 [0.0, 3.0]	0.04
PPP loan, two or more jobs	First holiday	0.094 (0.035)	−0.93 [−6.82, 0.48]	0.0 [0.0, 1.7]	2.12
	Second holiday	0.161 (0.028)	0.14 [−1.26, 0.92]	0.4 [0.0, 2.9]	0.14
	Pooled	0.132 (0.022)	−0.20 [−1.46, 0.50]	0.0 [0.0, 1.6]	0.05
SBA loan	First holiday	0.094 (0.035)	−1.93 [−11.45, 0.46]	0.0 [0.0, 0.4]	2.15
	Second holiday	0.161 (0.028)	0.73 [−1.12, 2.02]	0.6 [0.0, 1.7]	0.13
	Pooled	0.132 (0.022)	−0.11 [−1.93, 0.99]	0.0 [0.0, 0.9]	0.05
Either federal loan	First holiday	0.094 (0.035)	−0.71 [−5.25, 0.41]	0.0 [0.0, 2.4]	2.13
	Second holiday	0.161 (0.028)	0.19 [−0.81, 0.76]	1.1 [0.0, 4.2]	0.14
	Pooled	0.132 (0.022)	−0.08 [−1.03, 0.44]	0.0 [0.0, 2.5]	0.03
UCC financing statement	First holiday	0.094 (0.035)	−0.65 [−6.15, 0.68]	0.0 [0.0, 5.7]	2.16
	Second holiday	0.161 (0.028)	0.10 [−1.36, 0.87]	0.8 [0.0, 7.2]	0.13
	Pooled	0.132 (0.022)	−0.13 [−1.38, 0.56]	0.0 [0.0, 4.6]	0.04
Any of the three records	First holiday	0.094 (0.035)	−0.79 [−5.97, 0.34]	0.0 [0.0, 3.9]	2.11
	Second holiday	0.161 (0.028)	0.15 [−0.98, 0.71]	1.7 [0.0, 8.1]	0.14
	Pooled	0.132 (0.022)	−0.13 [−1.17, 0.40]	0.0 [0.0, 4.5]	0.04

Table A19. (continued)

D. Comparison of the two holidays (the rule at the end of section C1)

	First holiday less second, pct. points (s.e.)	Ratio to its standard error	More than twice its standard error
PPP loan (primary)	−0.06 (0.34)	−0.17	no
PPP loan, two or more jobs	−0.20 (0.23)	−0.86	no
SBA loan	−0.22 (0.11)	−1.95	no
Either federal loan	−0.23 (0.30)	−0.77	no
UCC financing statement	−0.10 (0.58)	−0.18	no
Any of the three records	−0.40 (0.53)	−0.76	no

Notes. Ordinary domestic LLCs by month of formation, which is the month in which the formation filing was received. The holiday months are December 2012 to February 2013 (first holiday) and July to October 2014 (second holiday). An outcome is an indicator that the company is linked to the record named: a Paycheck Protection Program loan (PPP loan); a PPP loan on which two or more jobs were reported; a loan guaranteed by the Small Business Administration (SBA loan), approved on or after the day the company was formed and by June 30, 2026 (Table A6, row 10); either of these federal loans; a UCC financing statement that names the company as debtor, at any time to October 2026; or any of the three records. T is the share of a window's companies with the outcome less the mean of the shares for the same calendar months one year earlier and one year later, in percentage points; nothing else is subtracted. The standard error (in parentheses) is the standard deviation of T over the reference runs, which are all runs of the same length among companies formed from January 2010 to December 2018 for which neither the run nor its two comparison runs contain a month from November 2012 to May 2013 or from June 2014 to March 2015. The confidence interval uses Student's t distribution with the degrees of freedom shown (d.f.), which equal the number of non-overlapping reference runs less one. *Reference runs*: the number of reference runs. The addendum requires the number of non-overlapping runs to be reported alongside every permutation p-value; the result file stores the degrees of freedom, and the number of non-overlapping runs equals the degrees of freedom plus one. The permutation p-value is the share of reference values at least as large in absolute value as the estimate, the estimate counted among them. The pooled row is the month-weighted mean of the two holidays (weights 3/7 and 4/7), treated as independent, with the smaller of the two degrees of freedom; the result file holds no permutation p-value and no mean of reference values for it. Panel B: the marginal share, which is the share of the holiday months' filings that are additional, is $s = 1 - \exp(-T_N)$, where T_N is the same statistic for the log number of filings (Table A21, Panel A). With μ_0 denoting the rate in the window less T, which is the rate that the same months would have had in the absence of marginal registrations, $\rho = 1 + T/(s\mu_0)$ is the rate of the outcome among marginal registrations relative to μ_0. In the pooled row, T, s, and μ_0 are the month-weighted means of the two holidays and ρ is computed from them. Of a hundred marginal registrations, $100 \max(\rho, 0)\mu_0$ have the outcome, compared with $100\mu_0$ of a hundred inframarginal registrations. Confidence intervals are computed from 200,000 joint draws of the errors of T and of s from a bivariate t distribution with the correlation shown, which is the correlation of the two statistics across reference runs; μ_0 is held fixed, and draws with a share of 0.01 or less are discarded (last column), so that each interval consists of percentiles over the draws retained (Section D.1). Panel C repeats Panel B with the marginal share net of the flanking months, defined as the additional filings of the whole window from the announcement month to the month after the holiday as a share of the filings of the holiday months. Panel D: for no outcome do the two holidays differ by more than twice the standard error of their difference, so the pooled value is used for the interpretation. Of the interpretations that section C1 specified for the primary outcome, the first applies: the pooled confidence interval for ρ in Panel B lies wholly below one. Its lower bound is not above zero, so the fourth interpretation, that some of the marginal registrations were businesses, does not apply. No interpretation was specified for the other five outcomes individually. Specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known.

Table A20. Colorado, External Records: Months around Each Holiday

	First holiday					Second holiday				
	T, pct. points (s.e.)	d.f.	Runs	Perm. p	Ref. mean	T, pct. points (s.e.)	d.f.	Runs	Perm. p	Ref. mean
PPP loan (primary)										
Announcement month	−0.74 (0.36)	37	38	0.077	+0.03	−0.41 (0.36)	37	38	0.231	+0.03
Month after	−0.68 (0.36)	37	38	0.128	+0.03	−0.50 (0.36)	37	38	0.154	+0.03
Later months	+0.34 (0.31)	17	34	0.257	+0.03	+0.08 (0.22)	7	27	0.786	+0.05
PPP loan, two or more jobs										
Announcement month	−0.18 (0.28)	37	38	0.462	+0.03	−0.64 (0.28)	37	38	0.077	+0.03
Month after	−0.55 (0.28)	37	38	0.103	+0.03	+0.05 (0.28)	37	38	0.821	+0.03
Later months	+0.34 (0.22)	17	34	0.143	+0.03	+0.02 (0.15)	7	27	0.964	+0.04
SBA loan										
Announcement month	+0.18 (0.13)	37	38	0.179	−0.01	+0.05 (0.13)	37	38	0.641	−0.01
Month after	−0.01 (0.13)	37	38	0.923	−0.01	+0.34 (0.13)	37	38	0.051	−0.01
Later months	−0.11 (0.09)	17	34	0.257	−0.01	−0.03 (0.07)	7	27	0.750	−0.02
Either federal loan										
Announcement month	−0.64 (0.34)	37	38	0.077	+0.02	−0.16 (0.34)	37	38	0.641	+0.02
Month after	−0.63 (0.34)	37	38	0.077	+0.02	−0.24 (0.34)	37	38	0.410	+0.02
Later months	+0.30 (0.28)	17	34	0.286	+0.02	−0.01 (0.19)	7	27	1.000	+0.04
UCC financing statement										
Announcement month	+0.59 (0.53)	37	38	0.205	−0.06	−0.21 (0.53)	37	38	0.718	−0.06
Month after	−0.57 (0.53)	37	38	0.231	−0.06	0.00 (0.53)	37	38	0.974	−0.06
Later months	+0.01 (0.46)	17	34	0.943	−0.05	+0.18 (0.39)	7	27	0.750	−0.04
Any of the three records										
Announcement month	+0.10 (0.54)	37	38	0.744	−0.06	−0.17 (0.54)	37	38	0.667	−0.06
Month after	−0.87 (0.54)	37	38	0.128	−0.06	−0.23 (0.54)	37	38	0.590	−0.06
Later months	+0.33 (0.43)	17	34	0.400	−0.06	+0.04 (0.36)	7	27	0.964	−0.04

Notes. The statistic T of Table A19 for the months around each holiday (section C1): the announcement month (November 2012; June 2014), the month after the holiday (March 2013; November 2014) and the later months (April and May 2013; December 2014 to March 2015). T is in percentage points with its standard error in parentheses. *d.f.*: degrees of freedom, the number of non-overlapping reference runs less one. *Runs*: the number of reference runs. *Perm. p*: the permutation p-value. *Ref. mean*: the mean of the reference values, in percentage points. The result file holds these months for each holiday and does not pool them. Specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known.

Table A21. Colorado, External Records: First Month and Later Months of Each Holiday

A. Ordinary filings and the marginal share, by window

Filings received in		T_N, log (s.e.)	d.f.	Reference runs	Permutation p	Mean of the reference values	Marginal share (s.e.)
Holiday months							
	First holiday	+0.124 (0.026)	14	42	0.023	+0.002	0.117 (0.023)
	Second holiday	+0.186 (0.025)	10	39	0.025	+0.003	0.170 (0.021)
	Pooled						0.147 (0.015)
Announcement month to month after (share net of the flanking months)							
	First holiday	+0.063 (0.024)	8	36	0.027	+0.005	0.094 (0.035)
	Second holiday	+0.124 (0.023)	5	33	0.029	+0.006	0.161 (0.028)
	Pooled						0.132 (0.022)
First month of the holiday							
	First holiday	+0.177 (0.040)	49	50	0.020	−0.002	0.162 (0.033)
	Second holiday	+0.229 (0.040)	49	50	0.020	−0.002	0.205 (0.031)
	Pooled						0.183 (0.023)
Later months of the holiday							
	First holiday	+0.103 (0.029)	23	46	0.021	+0.001	0.097 (0.026)
	Second holiday	+0.172 (0.026)	14	42	0.023	+0.002	0.158 (0.022)
	Pooled						0.134 (0.017)

B. The statistic T for the first month and for the later months, in percentage points

		First month					Later months				
		T, pct. points (s.e.)	d.f.	Runs	Perm. p	Ref. mean	T, pct. points (s.e.)	d.f.	Runs	Perm. p	Ref. mean
PPP loan (primary)	First holiday	−0.78 (0.36)	37	38	0.051	+0.03	−0.72 (0.31)	17	34	0.086	+0.03
	Second holiday	−1.01 (0.36)	37	38	0.026	+0.03	−0.58 (0.26)	10	30	0.097	+0.04
	Pooled	−0.90 (0.26)	37				−0.64 (0.20)	10			
PPP loan, two or more jobs	First holiday	−0.61 (0.28)	37	38	0.103	+0.03	−0.64 (0.22)	17	34	0.057	+0.03
	Second holiday	−0.55 (0.28)	37	38	0.103	+0.03	−0.39 (0.18)	10	30	0.065	+0.03
	Pooled	−0.58 (0.20)	37				−0.49 (0.14)	10			
SBA loan	First holiday	−0.20 (0.13)	37	38	0.179	−0.01	−0.28 (0.09)	17	34	0.029	−0.01
	Second holiday	−0.03 (0.13)	37	38	0.897	−0.01	−0.04 (0.08)	10	30	0.645	−0.01
	Pooled	−0.11 (0.09)	37				−0.14 (0.06)	10			
Either federal loan	First holiday	−0.90 (0.34)	37	38	0.051	+0.02	−0.97 (0.28)	17	34	0.029	+0.02
	Second holiday	−1.00 (0.34)	37	38	0.026	+0.02	−0.63 (0.23)	10	30	0.032	+0.02
	Pooled	−0.95 (0.24)	37				−0.77 (0.18)	10			
UCC financing statement	First holiday	−0.78 (0.53)	37	38	0.154	−0.06	−1.55 (0.46)	17	34	0.029	−0.05
	Second holiday	−1.43 (0.53)	37	38	0.051	−0.06	−1.13 (0.42)	10	30	0.032	−0.05
	Pooled	−1.10 (0.37)	37				−1.30 (0.31)	10			
Any of the three records	First holiday	−1.57 (0.54)	37	38	0.026	−0.06	−2.12 (0.43)	17	34	0.029	−0.06
	Second holiday	−1.61 (0.54)	37	38	0.026	−0.06	−1.54 (0.39)	10	30	0.032	−0.06
	Pooled	−1.59 (0.38)	37				−1.77 (0.29)	10			

Table A21. (continued)

C. The ratio ρ for each part, and the difference between the parts

		First month		Later months			
		ρ [95% CI]	Corr.	ρ [95% CI]	Corr.	Later months less first month, pct. points (s.e.)	The pattern, by the interpretation of C3
PPP loan (primary)	First holiday	0.08 [−1.09, 0.95]	−0.07	−0.36 [−2.67, 0.88]	−0.05	+0.06 (0.48)	
	Second holiday	0.06 [−0.79, 0.74]	−0.07	0.27 [−0.59, 1.00]	−0.03	+0.43 (0.44)	
	Pooled	0.07 [−0.58, 0.61]	−0.07	0.08 [−0.69, 0.72]	−0.04	+0.26 (0.32)	contradicted
PPP loan, two or more jobs	First holiday	−0.09 [−1.68, 0.94]	0.10	−0.88 [−4.40, 0.56]	0.21	−0.03 (0.36)	
	Second holiday	0.14 [−1.01, 1.03]	0.10	0.20 [−0.90, 1.00]	0.22	+0.16 (0.33)	
	Pooled	0.04 [−0.81, 0.72]	0.10	−0.13 [−1.15, 0.61]	0.22	+0.09 (0.24)	
SBA loan	First holiday	−0.60 [−3.89, 1.46]	0.28	−1.83 [−7.49, 0.28]	0.34	−0.08 (0.16)	
	Second holiday	0.85 [−0.71, 2.13]	0.28	0.67 [−1.04, 2.03]	0.29	−0.01 (0.15)	
	Pooled	0.28 [−1.13, 1.42]	0.28	−0.13 [−1.68, 1.01]	0.31	−0.02 (0.11)	
Either federal loan	First holiday	0.01 [−1.12, 0.77]	−0.02	−0.63 [−3.19, 0.43]	0.03	−0.06 (0.44)	
	Second holiday	0.15 [−0.61, 0.74]	−0.02	0.27 [−0.50, 0.87]	0.04	+0.37 (0.41)	
	Pooled	0.09 [−0.50, 0.56]	−0.02	0.00 [−0.72, 0.54]	0.04	+0.19 (0.30)	
UCC financing statement	First holiday	0.45 [−0.49, 1.21]	−0.06	−0.92 [−4.19, 0.38]	0.14	−0.77 (0.70)	
	Second holiday	0.15 [−0.64, 0.79]	−0.06	0.13 [−0.91, 0.87]	0.28	+0.30 (0.68)	
	Pooled	0.29 [−0.27, 0.78]	−0.06	−0.17 [−1.13, 0.51]	0.23	−0.19 (0.49)	
Any of the three records	First holiday	0.18 [−0.62, 0.75]	−0.16	−0.89 [−3.67, 0.11]	0.10	−0.55 (0.69)	
	Second holiday	0.30 [−0.26, 0.77]	−0.16	0.14 [−0.63, 0.66]	0.27	+0.07 (0.67)	
	Pooled	0.25 [−0.18, 0.61]	−0.16	−0.16 [−0.90, 0.33]	0.20	−0.18 (0.48)	

Notes. Section C3. The first month of a holiday is December 2012 or July 2014; its later months are January and February 2013 or August to October 2014. Panel A: T_N is the statistic of Table A19 computed for the log number of ordinary filings received in the window, in log units, over the sample months that Addendum 2 used for filing counts (January 2010 to December 2019). The share of a window's filings that are additional (the marginal share) is $1 - \exp(-T_N)$, with a standard error computed by the delta method. In the second block, T_N is for the whole window from the announcement month to the month after the holiday, and the share is the marginal share net of the flanking months that is used in Panel C of Table A19. The pooled share is the month-weighted mean of the two holidays; the result file holds no pooled T_N. Panel B: columns as in Table A20. Panel C: ρ for each part of a holiday, computed with the marginal share of that part from Panel A and with the correlation of the two errors next to it (*Corr.*), and the difference in T, later months less first month, with a standard error that treats the two parts as independent. The second review had found, for registry outcomes, a deficit among companies formed in the later months and none among those formed in the first month. Section C3 specified three interpretations of the pooled difference for the primary outcome. If the difference is more negative than twice its standard error, the pattern is confirmed on an outcome that had not been examined when the pattern was found. If it is within one standard error of zero, with a negative pooled T in both parts, the pattern is contradicted. Any other result is uninformative. The pooled difference for the PPP loan is positive and smaller than its standard error, and the pooled T is negative in both parts, so the pattern is contradicted for this outcome. Estimates for single months are imprecise. Specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known.

Table A22. Colorado, External Records: Estimates by Type of Filer and for Profit Corporations

		T, pct. points (s.e.)	d.f.	Runs	Perm. p	Ref. mean	Marginal share (s.e.)	ρ [95% CI]	Per 100 marginal [95% CI]	Per 100 infra-marginal
Formation service										
PPP loan	First holiday	−0.64 (0.46)	7	22	0.304	+0.06	0.107 (0.043)	−0.05 [−1.65, 2.10]	0.0 [0.0, 11.9]	5.7
	Second holiday	−0.26 (0.38)	5	20	0.524	+0.10	0.158 (0.040)	0.69 [−0.13, 2.65]	3.6 [0.0, 13.8]	5.2
	Pooled	−0.42 (0.29)	5				0.136 (0.029)	0.43 [−0.28, 1.71]	2.3 [0.0, 9.2]	5.4
Either federal loan	First holiday	−1.06 (0.45)	7	22	0.043	+0.06	0.107 (0.043)	−0.55 [−3.43, 0.83]	0.0 [0.0, 5.3]	6.4
	Second holiday	−0.45 (0.36)	5	20	0.238	+0.09	0.158 (0.040)	0.50 [−0.26, 1.87]	2.8 [0.0, 10.6]	5.7
	Pooled	−0.71 (0.28)	5				0.136 (0.029)	0.13 [−0.65, 1.00]	0.8 [0.0, 6.0]	6.0
Any of the three records	First holiday	−2.72 (0.99)	7	22	0.043	−0.30	0.107 (0.043)	−0.63 [−6.54, 0.83]	0.0 [0.0, 12.8]	15.5
	Second holiday	−1.69 (0.83)	5	20	0.095	−0.35	0.158 (0.040)	0.31 [−1.89, 1.14]	4.9 [0.0, 17.8]	15.6
	Pooled	−2.13 (0.64)	5				0.136 (0.029)	−0.01 [−1.88, 0.81]	0.0 [0.0, 12.6]	15.5
Repeat filer										
PPP loan	First holiday	−0.93 (0.42)	7	22	0.043	−0.11	0.186 (0.021)	0.07 [−0.73, 1.08]	0.4 [0.0, 5.8]	5.4
	Second holiday	−0.68 (0.37)	5	20	0.095	−0.12	0.225 (0.019)	0.39 [−0.35, 1.29]	1.9 [0.0, 6.3]	4.9
	Pooled	−0.79 (0.28)	5				0.208 (0.014)	0.26 [−0.31, 0.92]	1.3 [0.0, 4.7]	5.1
Either federal loan	First holiday	−1.10 (0.43)	7	22	0.043	−0.10	0.186 (0.021)	0.00 [−0.79, 0.92]	0.0 [0.0, 5.4]	5.9
	Second holiday	−0.67 (0.39)	5	20	0.143	−0.12	0.225 (0.019)	0.45 [−0.27, 1.30]	2.4 [0.0, 7.0]	5.4
	Pooled	−0.85 (0.29)	5				0.208 (0.014)	0.27 [−0.28, 0.89]	1.5 [0.0, 5.0]	5.6
Any of the three records	First holiday	−2.40 (0.51)	7	22	0.043	−0.16	0.186 (0.021)	−0.12 [−0.65, 0.39]	0.0 [0.0, 4.4]	11.5
	Second holiday	−1.65 (0.45)	5	20	0.048	−0.18	0.225 (0.019)	0.33 [−0.12, 0.79]	3.6 [0.0, 8.6]	11.0
	Pooled	−1.97 (0.34)	5				0.208 (0.014)	0.15 [−0.19, 0.50]	1.7 [0.0, 5.6]	11.2
New filer										
PPP loan	First holiday	−0.68 (0.29)	7	22	0.087	0.00	0.081 (0.022)	−0.56 [−4.93, 1.02]	0.0 [0.0, 5.5]	5.4
	Second holiday	−0.75 (0.20)	5	20	0.048	+0.03	0.135 (0.018)	−0.08 [−1.57, 0.75]	0.0 [0.0, 3.9]	5.2
	Pooled	−0.72 (0.17)	5				0.112 (0.014)	−0.22 [−1.64, 0.60]	0.0 [0.0, 3.2]	5.2
Either federal loan	First holiday	−0.89 (0.28)	7	22	0.043	−0.01	0.081 (0.022)	−0.84 [−5.32, 0.66]	0.0 [0.0, 3.9]	5.9
	Second holiday	−0.75 (0.21)	5	20	0.048	+0.02	0.135 (0.018)	0.01 [−1.39, 0.78]	0.1 [0.0, 4.4]	5.6
	Pooled	−0.81 (0.17)	5				0.112 (0.014)	−0.26 [−1.61, 0.52]	0.0 [0.0, 3.0]	5.8
Any of the three records	First holiday	−1.49 (0.46)	7	22	0.043	−0.05	0.081 (0.022)	−0.74 [−3.93, 0.58]	0.0 [0.0, 6.1]	10.6
	Second holiday	−1.41 (0.41)	5	20	0.048	0.00	0.135 (0.018)	−0.03 [−1.06, 0.74]	0.0 [0.0, 7.5]	10.2
	Pooled	−1.45 (0.31)	5				0.112 (0.014)	−0.25 [−1.23, 0.45]	0.0 [0.0, 4.7]	10.4
Profit corporations										
PPP loan	First holiday	−2.03 (0.59)	10	30	0.032	+0.06	0.222 (0.042)	0.25 [−0.48, 0.73]	3.0 [0.0, 8.9]	12.1
	Second holiday	−1.81 (0.51)	7	27	0.036	+0.03	0.215 (0.039)	0.28 [−0.43, 0.76]	3.3 [0.0, 8.9]	11.7
	Pooled	−1.90 (0.39)	7				0.218 (0.029)	0.27 [−0.20, 0.62]	3.2 [0.0, 7.4]	11.9
Either federal loan	First holiday	−2.09 (0.59)	10	30	0.032	+0.04	0.222 (0.042)	0.27 [−0.37, 0.72]	3.5 [0.0, 9.3]	12.9
	Second holiday	−1.95 (0.53)	7	27	0.036	+0.03	0.215 (0.039)	0.28 [−0.40, 0.74]	3.6 [0.0, 9.4]	12.7
	Pooled	−2.01 (0.40)	7				0.218 (0.029)	0.28 [−0.16, 0.61]	3.6 [0.0, 7.8]	12.8
Any of the three records	First holiday	−3.63 (0.75)	10	30	0.032	+0.05	0.222 (0.042)	0.18 [−0.34, 0.53]	3.6 [0.0, 10.5]	20.0
	Second holiday	−2.79 (0.67)	7	27	0.036	+0.05	0.215 (0.039)	0.33 [−0.15, 0.68]	6.5 [0.0, 13.3]	19.5
	Pooled	−3.15 (0.50)	7				0.218 (0.029)	0.27 [−0.06, 0.52]	5.3 [0.0, 10.3]	19.7

Notes. The statistics of Table A19 for the holiday months, for the three outcomes that the addendum names, by type of filer among ordinary domestic LLCs (section C4) and for profit corporations (section C5). The types are those of Addendum 2. *Formation service*: the registered agent is an organization. *Repeat filer*: the agent is a person, and an earlier ordinary filing of the previous 365 days carries the same agent or the same principal address. *New filer*: the agent is a person and no earlier filing does. A filing whose agent is not an organization and whose principal address had twenty or more ordinary filings in the previous 365 days belongs to none of the three types. For the estimates by type, the sample of months starts in January 2011. Each type, and the profit corporations, has its own marginal share, computed from its own filings statistic. T is in percentage points with its standard error in parentheses; *d.f.*, *Runs*, *Perm. p* and *Ref. mean* are as in Table A20; ρ and the rates per hundred are as in Panel B of Table A19. No interpretation was specified for these statistics. Specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known.

Table A23. Colorado: Index of Promise at Founding

A. The index among companies not used to fit it

Companies with an odd entity number	Top tenth	Middle	Bottom half	All
Number of companies				273,289
With a PPP loan, two or more jobs (percent)	6.52	3.67	2.18	3.21
Share of the companies in each group (percent)	10.0		50.0	

B. Statistics for the holiday months

		Estimate (s.e.)	95% CI	d.f.	Reference runs	Permutation p	Mean of the reference values
Filings in the top tenth (log)	First holiday	+0.067 (0.048)	[−0.035, +0.170]	14	42	0.116	−0.009
	Second holiday	+0.202 (0.044)	[+0.103, +0.301]	10	39	0.025	−0.010
	Pooled	+0.144 (0.033)	[+0.072, +0.217]	10			
Filings in the middle (log)	First holiday	+0.132 (0.026)	[+0.076, +0.187]	14	42	0.023	0.000
	Second holiday	+0.177 (0.025)	[+0.122, +0.231]	10	39	0.025	0.000
	Pooled	+0.158 (0.018)	[+0.118, +0.197]	10			
Filings in the bottom half (log)	First holiday	+0.131 (0.028)	[+0.071, +0.190]	14	42	0.023	+0.007
	Second holiday	+0.191 (0.026)	[+0.132, +0.250]	10	39	0.025	+0.008
	Pooled	+0.165 (0.019)	[+0.122, +0.208]	10			
Filings weighted by the index (log)	First holiday	+0.116 (0.028)	[+0.056, +0.177]	14	42	0.023	0.000
	Second holiday	+0.187 (0.027)	[+0.127, +0.247]	10	39	0.025	0.000
	Pooled	+0.157 (0.020)	[+0.113, +0.200]	10			
Mean index among the filings (percentage points)	First holiday	−0.026 (0.019)	[−0.066, +0.015]	14	42	0.233	−0.009
	Second holiday	+0.001 (0.015)	[−0.033, +0.036]	10	39	0.975	−0.009
	Pooled	−0.010 (0.012)	[−0.037, +0.016]	10			
relative to its level without the holiday	First holiday	−0.008 (0.006)					
	Second holiday	0.000 (0.005)					

C. The two interpretations of section C6, evaluated at the pooled values

Pooled values	Log	Condition of the interpretation
T_N for the top tenth, with its 95% CI	+0.144 [+0.072, +0.217]	Positive, with a confidence interval that excludes zero: holds
T_N for the bottom half less T_N for the top tenth	+0.021	
Twice the root sum of squares of their standard errors	0.076	The difference exceeds this: does not hold

Notes. Section C6. The index is the fitted probability of a PPP loan with two or more jobs from a logistic regression on characteristics known from the filing: the number of words in the name, the log of its length, whether it contains a digit, whether it contains a common given name, an indicator for each of the 150 words most frequent in names, a delayed effective date, receipt on a weekend, a principal address in another state or abroad or missing, and the income tercile of the principal ZIP code, 164 characteristics in all, counting every indicator. Year and month indicators are held at their mean, and a ridge penalty of 1 applies. The regression is fitted on the 272,597 ordinary LLCs with an even entity number formed from January 2010 to December 2018, outside the months from November 2012 to May 2013 and from June 2014 to March 2015. The top tenth and the bottom half are defined by the 90th and 50th percentiles of the index in that sample. Panel A: the companies formed in the same months with an odd entity number, which were not used in fitting. The index is a weak predictor. Panel B: the filings statistic T_N of Table A21 for the filings of each group and for the sum of the index over a window's filings; and the rate statistic for the mean index among a window's filings, in percentage points, and the same as a proportion of its level without the holiday, which the result file does not pool. Inference is as in Table A19. Panel C: the first condition holds, because the pooled T_N for the top tenth is positive with a confidence interval that excludes zero, so the marginal registrations include companies in the most promising category as well. The second condition does not hold: the pooled T_N for the bottom half does not exceed that for the top tenth by more than twice the root sum of squares of their standard errors, so the marginal registrations are not shown to be skewed toward low promise. Specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known.

Table A24. Colorado, External Records: Alternative Versions for the Primary Outcome

	T, pct. points (s.e.)	d.f.	Runs	Perm. p	Ref. mean	Marginal share (s.e.)	ρ [95% CI]	Per 100 marginal [95% CI]	Per 100 infra-marginal
The main version, repeated: both adjacent years, companies formed January 2010 to December 2018, all links									
First holiday	−0.74 (0.26)	10	30	0.032	+0.04	0.117 (0.023)	−0.18 [−1.61, 0.75]	0.0 [0.0, 4.0]	5.4
Second holiday	−0.68 (0.22)	7	27	0.036	+0.05	0.170 (0.021)	0.21 [−0.55, 0.81]	1.0 [0.0, 4.1]	5.1
Pooled	−0.71 (0.17)	7				0.147 (0.015)	0.07 [−0.57, 0.60]	0.4 [0.0, 3.1]	5.2
(i) Relative to the year after only									
First holiday	−0.78 (0.31)	18	54	0.018	−0.23				
Second holiday	−0.50 (0.27)	13	51	0.038	−0.22				
Pooled	−0.62 (0.21)	13							
(i) Relative to the year before only									
First holiday	−0.70 (0.31)	18	54	0.018	+0.23				
Second holiday	−0.87 (0.27)	13	51	0.019	+0.22				
Pooled	−0.79 (0.21)	13							
(ii) Standard errors from the sample and the affected months that Addendum 2 used for outcomes									
First holiday	−0.74 (0.25)†	7	21	0.045	−0.02				
Second holiday	−0.68 (0.19)	5	19	0.050	+0.01				
Pooled	−0.71 (0.15)	5							
(iii) Links by full name and ZIP code only									
First holiday	−0.38 (0.23)	10	30	0.129	+0.04	0.117 (0.023)	0.05 [−1.71, 1.32]	0.2 [0.0, 4.6]	3.5
Second holiday	−0.44 (0.20)	7	27	0.036	+0.06	0.170 (0.021)	0.17 [−0.95, 1.08]	0.5 [0.0, 3.3]	3.1
Pooled	−0.41 (0.15)	7				0.147 (0.015)	0.13 [−0.76, 0.89]	0.4 [0.0, 2.9]	3.2
(iv) ρ with the share net of the flanking months									
First holiday						0.094 (0.035)	−0.47 [−4.47, 0.73]	0.0 [0.0, 3.9]	5.4
Second holiday						0.161 (0.028)	0.16 [−0.94, 0.85]	0.8 [0.0, 4.3]	5.1
Pooled						0.132 (0.022)	−0.03 [−0.99, 0.57]	0.0 [0.0, 3.0]	5.2

Notes. Section C7 specified four alternative versions for the primary outcome, the PPP loan. The first block repeats the main version for comparison. Columns are as in Table A22. (i) The comparison with one adjacent year only. A rate that drifts over the years differs from the year after, or from the year before, even in the absence of a holiday, so the locked script subtracts the mean of the reference values from the estimate; in these two blocks the column *Ref. mean* gives the mean that was subtracted. (ii) Reference runs among companies formed from June 2011 to December 2018, with the months whose first reports fell due in the first holiday also treated as affected. The estimates are those of the main version; the reference runs change, and with them the standard errors, the degrees of freedom, the permutation p-values and the mean of the reference values. The dagger marks an estimate whose own comparison months include such months, namely the months one year before the first holiday. (iii) Links restricted to loans whose full name equals the name of the entity and whose ZIP code is that of the entity. (iv) ρ with the share net of the flanking months, as in Panel C of Table A19. Specified in advance of the tabulations, in an addendum written after all other results and the second-round reports were known.

Table A25. Colorado, External Records: Numbers of Companies, the Whole Window, and the Announcement Dates

A. Number of companies with each outcome, and ordinary filings: log difference from the same months of the adjacent years (log units)

	Number of companies with the outcome				Filings less companies with the outcome	
	Holiday months	Whole window	Flanking months	Later months	Holiday months	Whole window
PPP loan						
First holiday	−0.023 (0.056)	−0.083 (0.038)	−0.185 (0.066)	+0.044 (0.066)	+0.148 (0.054)	+0.145 (0.038)
Second holiday	+0.041 (0.048)	−0.007 (0.026)	−0.106 (0.066)	+0.016 (0.048)	+0.145 (0.046)	+0.131 (0.030)
Pooled	+0.014 (0.037)	−0.041 (0.022)	−0.146 (0.047)	+0.025 (0.039)	+0.146 (0.035)	+0.137 (0.024)
PPP loan, two or more jobs						
First holiday	−0.076 (0.064)	−0.108 (0.044)	−0.162 (0.079)	+0.082 (0.079)	+0.200 (0.059)	+0.171 (0.039)
Second holiday	+0.036 (0.056)	−0.014 (0.026)	−0.114 (0.079)	+0.007 (0.056)	+0.150 (0.050)	+0.138 (0.024)
Pooled	−0.012 (0.042)	−0.057 (0.024)	−0.138 (0.056)	+0.032 (0.046)	+0.171 (0.038)	+0.153 (0.022)
SBA loan						
First holiday	−0.197 (0.114)	−0.103 (0.086)	+0.054 (0.123)	−0.164 (0.123)	+0.321 (0.108)	+0.166 (0.080)
Second holiday	+0.150 (0.100)	+0.159 (0.074)	+0.176 (0.123)	−0.033 (0.100)	+0.036 (0.094)	−0.034 (0.067)
Pooled	+0.001 (0.075)	+0.040 (0.056)	+0.115 (0.087)	−0.077 (0.078)	+0.158 (0.071)	+0.056 (0.052)
Either federal loan						
First holiday	−0.049 (0.047)	−0.090 (0.032)	−0.160 (0.057)	+0.032 (0.057)	+0.173 (0.044)	+0.153 (0.030)
Second holiday	+0.047 (0.041)	+0.013 (0.022)	−0.057 (0.057)	0.000 (0.041)	+0.139 (0.036)	+0.111 (0.024)
Pooled	+0.006 (0.031)	−0.034 (0.019)	−0.108 (0.040)	+0.011 (0.033)	+0.154 (0.028)	+0.130 (0.019)
UCC financing statement						
First holiday	−0.044 (0.063)	−0.045 (0.053)	−0.047 (0.068)	−0.024 (0.068)	+0.168 (0.057)	+0.108 (0.048)
Second holiday	+0.029 (0.059)	+0.008 (0.049)	−0.035 (0.068)	+0.024 (0.059)	+0.158 (0.052)	+0.116 (0.045)
Pooled	−0.003 (0.043)	−0.016 (0.036)	−0.041 (0.048)	+0.008 (0.045)	+0.162 (0.038)	+0.113 (0.033)
Any of the three records						
First holiday	−0.059 (0.043)	−0.067 (0.035)	−0.081 (0.048)	+0.005 (0.048)	+0.183 (0.036)	+0.130 (0.029)
Second holiday	+0.039 (0.041)	+0.014 (0.030)	−0.039 (0.048)	+0.004 (0.041)	+0.147 (0.033)	+0.110 (0.026)
Pooled	−0.003 (0.030)	−0.023 (0.023)	−0.060 (0.034)	+0.004 (0.031)	+0.163 (0.024)	+0.119 (0.019)
All ordinary filings						
First holiday	+0.124 (0.016)	+0.063 (0.013)	−0.041 (0.022)	−0.025 (0.022)		
Second holiday	+0.186 (0.015)	+0.124 (0.011)	−0.022 (0.022)	+0.001 (0.015)		
Pooled	+0.160 (0.011)	+0.096 (0.008)	−0.032 (0.016)	−0.008 (0.012)		

B. Ordinary filings: the whole window, and the holiday months relative to the flanking months

	Whole window		Holiday months relative to the flanking months		
Ordinary filings	Log (s.e.)	Marginal share (s.e.)	Holiday months less flanking months, log (s.e.)	Flanking months, log (s.e.)	Marginal share (s.e.)
First holiday	+0.063 (0.013)	0.061 (0.012)	+0.165 (0.028)	−0.041 (0.024)	0.153 (0.024)
Second holiday	+0.124 (0.011)	0.117 (0.010)	+0.208 (0.026)	−0.022 (0.024)	0.188 (0.021)
Pooled	+0.096 (0.008)	0.091 (0.008)	+0.190 (0.019)	−0.030 (0.017)	0.173 (0.016)

Table A25. (continued)

C. Rates: the whole window, and the holiday months relative to the flanking months (percentage points)

	Whole window			Holiday months relative to the flanking months			
	T, pct. points (s.e.)	d.f.	ρ [95% CI]	Holiday less flanking months, pct. points (s.e.)	d.f.	Flanking months, pct. points (s.e.)	ρ [95% CI]
PPP loan							
First holiday	−0.72 (0.18)	6	−1.24 [−3.72, 0.17]	−0.04 (0.33)	6	−0.70 (0.23)	0.95 [−0.30, 2.19]
Second holiday	−0.64 (0.15)	3	−0.05 [−0.73, 0.67]	−0.23 (0.33)	3	−0.45 (0.24)	0.73 [−0.59, 1.99]
Pooled	−0.68 (0.11)	3	−0.41 [−1.22, 0.32]	−0.15 (0.24)	3	−0.56 (0.17)	0.81 [−0.19, 1.78]
PPP loan, two or more jobs							
First holiday	−0.54 (0.12)	6	−1.59 [−4.98, 0.00]	−0.24 (0.28)	6	−0.39 (0.18)	0.48 [−1.18, 1.96]
Second holiday	−0.42 (0.07)	3	−0.11 [−0.87, 0.50]	−0.11 (0.28)	3	−0.32 (0.18)	0.78 [−1.09, 2.57]
Pooled	−0.48 (0.07)	3	−0.56 [−1.57, 0.18]	−0.17 (0.20)	3	−0.35 (0.13)	0.66 [−0.68, 1.95]
SBA loan							
First holiday	−0.13 (0.06)	6	−1.52 [−7.07, 1.29]	−0.33 (0.10)	6	+0.07 (0.08)	−1.15 [−3.54, 0.53]
Second holiday	+0.02 (0.05)	3	1.24 [−0.67, 2.65]	−0.23 (0.11)	3	+0.19 (0.08)	−0.15 [−2.12, 1.55]
Pooled	−0.05 (0.04)	3	0.40 [−1.63, 1.89]	−0.27 (0.07)	3	+0.14 (0.06)	−0.52 [−2.12, 0.80]
Either federal loan							
First holiday	−0.83 (0.16)	6	−1.34 [−3.81, −0.12]	−0.32 (0.32)	6	−0.63 (0.21)	0.61 [−0.51, 1.59]
Second holiday	−0.60 (0.13)	3	0.10 [−0.53, 0.69]	−0.52 (0.33)	3	−0.21 (0.24)	0.49 [−0.68, 1.52]
Pooled	−0.70 (0.10)	3	−0.34 [−1.09, 0.27]	−0.43 (0.23)	3	−0.39 (0.17)	0.53 [−0.35, 1.32]
UCC financing statement							
First holiday	−0.85 (0.36)	6	−0.69 [−3.86, 1.07]	−1.26 (0.46)	6	−0.05 (0.38)	0.02 [−1.20, 0.90]
Second holiday	−0.92 (0.34)	3	0.06 [−1.30, 1.16]	−1.09 (0.49)	3	−0.11 (0.41)	0.29 [−0.93, 1.32]
Pooled	−0.89 (0.25)	3	−0.17 [−1.51, 0.88]	−1.16 (0.34)	3	−0.09 (0.29)	0.18 [−0.72, 0.95]
Any of the three records							
First holiday	−1.40 (0.32)	6	−1.01 [−3.72, 0.24]	−1.51 (0.42)	6	−0.44 (0.34)	0.11 [−0.78, 0.72]
Second holiday	−1.21 (0.28)	3	0.11 [−0.78, 0.78]	−1.35 (0.44)	3	−0.20 (0.36)	0.36 [−0.47, 1.02]
Pooled	−1.29 (0.21)	3	−0.23 [−1.17, 0.46]	−1.42 (0.31)	3	−0.31 (0.25)	0.26 [−0.38, 0.78]

Table A25. (continued)

D. Sub-periods defined by the announcement dates: the first holiday

	Filings		With a PPP loan			With any of the three records		
Filings received	Number	Log (s.e.)	Number	Log (s.e.)	Rate, pct. points	Number	Log (s.e.)	Rate, pct. points
November 1 to 18 (before the announcement)	2,631	−0.102 (0.023)	117	−0.240 (0.109)	−0.69	304	−0.052 (0.070)	+0.51
November 19 to 30 (announced, fee still $50)	1,726	+0.126 (0.030)	75	−0.046 (0.141)	−0.82	191	+0.071 (0.090)	−0.64
December 1 to 15	2,765	+0.176 (0.024)	129	+0.170 (0.111)	−0.05	290	+0.099 (0.073)	−0.85
December 16 to 31	2,746	+0.178 (0.024)	115	−0.124 (0.112)	−1.51	278	−0.025 (0.073)	−2.30
January	6,732	+0.091 (0.015)	316	−0.068 (0.068)	−0.82	662	−0.063 (0.047)	−1.65
February 1 to 19	3,737	+0.086 (0.020)	181	−0.054 (0.090)	−0.73	336	−0.197 (0.065)	−2.94
February 20 to 28 (end announced)	2,368	+0.233 (0.026)	106	+0.144 (0.122)	−0.42	209	+0.015 (0.085)	−2.18
March 1 to 15	2,880	−0.049 (0.023)	132	−0.144 (0.104)	−0.48	284	−0.145 (0.071)	−1.00
March 16 to 31	2,761	−0.069 (0.023)	129	−0.240 (0.104)	−0.88	297	−0.134 (0.070)	−0.73
April	5,512	−0.015 (0.016)	282	+0.030 (0.074)	+0.18	606	−0.001 (0.050)	+0.13

E. Sub-periods defined by the announcement dates: the second holiday

	Filings		With a PPP loan			With any of the three records		
Filings received	Number	Log (s.e.)	Number	Log (s.e.)	Rate, pct. points	Number	Log (s.e.)	Rate, pct. points
June 1 to 8 (before the announcement)	1,441	−0.047 (0.032)	82	+0.047 (0.137)	+0.46	186	+0.056 (0.091)	+1.25
June 9 to 30 (announced, fee still $50)	4,250	+0.046 (0.019)	210	−0.088 (0.083)	−0.71	487	−0.010 (0.056)	−0.66
July 1 to 15	3,092	+0.185 (0.023)	142	+0.046 (0.104)	−0.70	323	+0.104 (0.069)	−0.92
July 16 to 31	3,917	+0.266 (0.021)	156	−0.008 (0.098)	−1.26	357	+0.055 (0.065)	−2.17
August and September	13,446	+0.180 (0.011)	598	+0.086 (0.051)	−0.44	1,324	+0.045 (0.034)	−1.42
October 1 to 15 (end announced)	3,222	+0.130 (0.022)	139	−0.026 (0.104)	−0.74	316	−0.004 (0.069)	−1.41
October 16 to 31 (end announced)	3,627	+0.183 (0.021)	162	−0.010 (0.096)	−0.95	354	−0.012 (0.065)	−2.11
November 1 to 15	2,703	−0.030 (0.023)	136	−0.102 (0.103)	−0.41	327	+0.038 (0.068)	+0.76
November 16 to 30	2,063	−0.121 (0.026)	108	−0.213 (0.114)	−0.54	230	−0.238 (0.078)	−1.40
December	5,552	+0.029 (0.017)	349	+0.093 (0.067)	+0.39	678	+0.015 (0.047)	−0.17

Notes. These analyses were run after the results under Addendum 3 were known, and none was specified in advance. The sample, outcomes, links and affected months are those of the locked analysis. The *whole window* runs from the announcement month to the month after the holiday; the *flanking months* are those two months; the *later months* follow them. Panel A: the log of the number of companies with the outcome in the window less the mean of the logs for the same months one year earlier and one year later, shown alongside the same statistic for all ordinary filings, with the standard deviation over reference runs in parentheses. The last two columns give the statistic for filings less the statistic for companies with the outcome, with a standard error from the same difference in the reference runs. Panel B: the filings statistic for the whole window with the share of its filings that are additional (its marginal share); and the filings statistic for the holiday months less that for the two flanking months, with reference blocks of the same shape, shown alongside the statistic for the flanking months and the marginal share implied by the local comparison. Panel C: the rate statistic T for the whole window, in which filings retimed from the flanking months net out, with ρ from the whole-window share; and T for the holiday months less T for the flanking months, with ρ from the local share, the two errors drawn independently. The local comparison is biased toward zero if the filings retimed from the flanking months into the holiday were more likely to have the record. The pooled entries for the flanking months differ between Panels A and B because Panel A weights the two holidays equally, each having two flanking months, whereas Panel B weights them by their numbers of holiday months. The pooled entries for the whole window weight the holidays by the lengths of their windows, five and six months. Panels D and E: *Number* is the number of filings received in the sub-period and, of these, the number of companies with the outcome. *Log* is the log of the number less the mean of the logs for the same days one year earlier and one year later, in log units, with a Poisson standard error in parentheses. *Rate* is the share of the sub-period's companies with the outcome less the mean of the shares for the same days of the adjacent years, in percentage points. The log statistics of Panels D and E are not adjusted for the number of weekdays in a sub-period, which differs between years; Table A34, Panels D and E, reports registrations per weekday. A negative ρ indicates that the rate declined by more than it would have declined if no marginal registration had the outcome. Exploratory.

E.4. Addendum 4: All Pre-specified Statistics

Tables A26 to A31 report every statistic specified in Part C of Addendum 4 for the U.K. fee increases; they are generated by the same script from the result file of the locked analysis. Where the addendum specified an interpretation or a decision rule, the script evaluates it from the stored values and the table reports the result, so that readers can verify it. These analyses were specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known. Table A32 reports analyses that were run after the results of these pre-specified analyses were known and is exploratory.

Table A26. United Kingdom: Fee Increase of May 1, 2024, All Outcomes

A. Estimates: after relative to before in 2024, less the mean of the same contrast in the reference years (sections C1 and C2)

	Estimate (s.e.)	95% CI	d.f.	p	Value in 2024	Mean of the reference years	Reference years
Registrations per working day, E_N (log points)	−19.2 (6.7)	[−34.5, −3.8]	8	0.021	−32.6	−13.4	9
share of registrations deterred, $1 - \exp(E_N)$ (percent)	17.4						
Non-dormant accounts (primary)							
E_R: rate among registrations (log points)	+19.2 (3.3)	[+8.7, +29.8]	3	0.010	+16.7	−2.5	4
P: rate among registrations (percentage points)	+7.5 (1.3)	[+3.4, +11.6]	3	0.010	+6.5	−1.0	4
B: number of companies, estimated directly (log points)	−0.8 (4.6)	[−15.3, +13.7]	3	0.871	−15.9	−15.1	4
A charge registered							
E_R: rate among registrations (log points)	+26.9 (2.4)	[+19.4, +34.4]	3	0.001	+24.7	−2.1	4
P: rate among registrations (percentage points)	+1.2 (0.1)	[+0.9, +1.5]	3	0.001	+1.1	−0.1	4
B: number of companies, estimated directly (log points)	+6.8 (8.3)	[−19.6, +33.3]	3	0.471	−7.9	−14.7	4
Either of the two							
E_R: rate among registrations (log points)	+19.3 (3.3)	[+8.9, +29.8]	3	0.010	+17.0	−2.3	4
P: rate among registrations (percentage points)	+7.6 (1.3)	[+3.5, +11.8]	3	0.010	+6.7	−0.9	4
B: number of companies, estimated directly (log points)	−0.7 (4.6)	[−15.4, +14.0]	3	0.888	−15.6	−14.9	4
Confirmation statement on file							
E_R: rate among registrations (log points)	+14.7 (1.9)	[+8.6, +20.7]	3	0.004	+10.4	−4.3	4
P: rate among registrations (percentage points)	+9.4 (1.1)	[+5.9, +12.9]	3	0.003	+6.6	−2.8	4
B: number of companies, estimated directly (log points)	−7.4 (7.8)	[−32.3, +17.5]	3	0.414	−22.2	−14.8	4
On the register and active							
E_R: rate among registrations (log points)	+14.5 (2.0)	[+8.1, +21.0]	3	0.006	+10.9	−3.7	4
P: rate among registrations (percentage points)	+9.2 (1.2)	[+5.4, +13.1]	3	0.005	+6.8	−2.4	4
B: number of companies, estimated directly (log points)	−7.5 (7.9)	[−32.6, +17.5]	3	0.408	−21.7	−14.2	4

Table A26. (continued)

B. The number of companies with the outcome, the ratio ρ, and the interpretation under section C1

	$E_B = E_N + E_R$ [95% CI]	ρ [95% CI]	Correlation assigned to the normal draws	Draws with a decline below half a log point (percent)	Interpretation under C1	Interval for ρ wholly below one-half
Non-dormant accounts (primary)	+0.1 [−12.9, +13.0]	0.00 [−1.62, 0.42]	−0.68	1.2	1	yes
A charge registered	+7.7 [−9.3, +24.8]	−0.35 [−3.51, 0.25]	−0.02	1.1	1	yes
Either of the two	+0.2 [−12.9, +13.2]	−0.01 [−1.65, 0.42]	−0.67	1.1	1	yes
Confirmation statement on file	−4.5 [−20.0, +11.1]	0.22 [−1.51, 0.59]	−0.21	1.2	1	no
On the register and active	−4.6 [−20.6, +11.3]	0.22 [−1.52, 0.62]	−0.15	1.2	1	no

Notes. The fee to incorporate a company increased from £12 to £50 on May 1, 2024. *Before*: companies incorporated in the 42 days from March 4 to April 14, 2024. *After*: in the 42 days from May 20 to June 30, 2024. The 35 days between these periods, which contain the bunching of registrations before the fee increase and the temporary decline after it, are excluded. Registrations are counted from the sequence of company numbers, so that a company dissolved before it appeared in any snapshot of the register is still counted. An effective working day is a weekday; a weekday bank holiday counts as 0.3 of a day and a weekend day as zero. N is the log of registrations per effective working day, after less before. B is the same for the number of companies with the outcome, $R = B - N$ is the log of the rate of the outcome among registrations, after less before, and P is the same rate in percentage points. Logs are multiplied by 100. An estimate is the value in 2024 less the mean over the reference years at the same dates of the year: 2015 to 2019, 2021, 2022, 2023 and 2025 for registrations; 2017, 2021, 2022 and 2023 for the three outcomes measured at the second October after the year of incorporation (non-dormant accounts on file, a charge registered, either of the two; the companies of 2024 are 27 to 31 months old when the register is read on October 1, 2026); 2018, 2021, 2022 and 2025 for the two measured at the first October (a confirmation statement on file, on the register with the status active; 15 to 19 months). A company that is no longer on the register when the register is read has none of the outcomes. The standard error (in parentheses) is the standard deviation over the reference years times $\sqrt{1 + 1/n}$, where n is the number of reference years, and confidence intervals and p-values use Student's t distribution with $n - 1$ degrees of freedom. E_N and E_R are the estimates of N and of R. Panel B: $E_B = E_N + E_R$ is the change in the number of companies with the outcome, and $\rho = (\exp(-E_B) - 1)/(\exp(-E_N) - 1)$ is the rate of the outcome among deterred registrations relative to its rate among registrations that were not deterred. Their confidence intervals are computed from 400,000 joint draws of the errors of E_N and E_R, each Student's t with its own degrees of freedom. The locked script draws two normal variables with the correlation shown, which follows from correlations specified in advance, and divides each by its own chi-square draw, so that the errors as drawn are less strongly correlated (−0.52 for the primary outcome; Table A6, row 19). A draw in which registrations decline by less than half a log point contains essentially no deterred registrations; a confidence interval for ρ is reported only if such draws are at most 2.5% of all draws, and its ends are percentiles over the remaining draws (Table A6, row 20). The script that generates the table evaluates the four interpretations specified in section C1 of Addendum 4 from the confidence intervals for E_N and E_R. For the primary outcome the first interpretation applies. The addendum states it as follows: "The interval for E_N lies below zero and the interval for E_R lies above zero. The rise removed registrations, and those it removed were less often businesses, by this measure, than those still made. The Colorado finding holds with the sign reversed. If in addition the interval for ρ is reported and lies wholly below one-half, the paper may say that most of the removed registrations were not businesses; if it reaches above one-half, the paper says only 'less often'." The interval for ρ is reported and lies wholly below one-half. The confidence set by test inversion, which uses every draw, runs from −2.5 to 0.42, and the paper does not make the statement that the addendum permits (Table A6, row 21; Section D.1) [E]. Section C2 applies the same interpretations to the other four outcomes, with no adjustment for the number of outcomes: the first interpretation applies to each, and the confidence interval for ρ lies wholly below one-half for a charge registered and for either of the two, and reaches above one-half for the two outcomes measured at the first October. Specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known.

Table A27. United Kingdom: Statistics by Reference Year and Leave-One-Out Estimates

A. After relative to before in each year (log points)

		Rate among registrations, R				
After relative to before in	Registrations per working day, N	Non-dormant accounts	A charge registered	Either of the two	Confirmation statement on file	On the register and active
2015	−14.1					
2016	−21.5					
2017	−14.3	−1.4	−0.7	−1.2		
2018	−12.8				−2.3	−1.7
2019	−18.1					
2021	−20.9	+0.5	−4.1	+0.6	−3.7	−3.1
2022	−4.2	−6.5	−3.8	−6.3	−6.3	−6.0
2023	−10.8	−2.6	0.0	−2.4		
2025	−4.2				−4.8	−3.8
Mean of the reference years	−13.4	−2.5	−2.1	−2.3	−4.3	−3.7
Standard deviation over the reference years	6.3	3.0	2.1	2.9	1.7	1.8
Number of reference years	9	4	4	4	4	4
2024, the year of the fee increase	−32.6	+16.7	+24.7	+17.0	+10.4	+10.9
2024 less the mean: the estimate (s.e.)	−19.2 (6.7)	+19.2 (3.3)	+26.9 (2.4)	+19.3 (3.3)	+14.7 (1.9)	+14.5 (2.0)

B. Leave-one-out estimates by reference year (section C3; log points)

	Registrations per working day, E_N		Rate of non-dormant accounts, E_R		
Reference year omitted	Estimate (s.e.)	95% CI	Estimate (s.e.)	95% CI	Interpretation under C1
None: the main estimates	−19.2 (6.7)	[−34.5, −3.8]	+19.2 (3.3)	[+8.7, +29.8]	1
2015	−19.3 (7.2)	[−36.2, −2.3]			1
2016	−20.2 (6.3)	[−35.1, −5.3]			1
2017	−19.3 (7.2)	[−36.2, −2.4]	+19.6 (4.1)	[+2.1, +37.1]	1
2018	−19.1 (7.2)	[−36.0, −2.2]			1
2019	−19.8 (6.9)	[−36.0, −3.5]			1
2021	−20.1 (6.4)	[−35.3, −4.9]	+20.2 (3.1)	[+6.9, +33.6]	1
2022	−18.0 (6.0)	[−32.2, −3.8]	+17.9 (1.8)	[+10.1, +25.7]	1
2023	−18.8 (7.1)	[−35.6, −2.1]	+19.2 (4.2)	[+1.1, +37.3]	1
2025	−18.0 (6.0)	[−32.2, −3.8]			1

Notes. Statistics, periods and units are as in Table A26. Panel A: the value of each statistic at the dates of the fee increase in every reference year and in 2024. A blank cell indicates that the year is not a reference year for the statistic. The year 2020 is excluded because of the lockdown and 2026 because of the second fee increase. An outcome has a reference year only where a snapshot of the register exists for the October at which it is measured, and 2023 is not a reference year for the two outcomes measured at the first October because the fee for a confirmation statement increased between the dates on which the companies of its two periods first owed one. The standard error of an estimate is the standard deviation in the table times $\sqrt{1 + 1/n}$. Panel B: both estimates with each reference year omitted in turn, with seven degrees of freedom for registrations and two for the rate. A blank cell indicates that the year is not a reference year for the rate, whose estimate is then the main one. The last column evaluates the interpretations of section C1 for the two confidence intervals of the row: the first interpretation applies with each year omitted. Specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known.

Table A28. United Kingdom: Pre-specified Checks and Evaluation of Their Decision Rules

A. Contrasts between periods: registrations, and the rate of non-dormant accounts (log points)

	Registrations per working day, N			Rate of non-dormant accounts, R		
	Estimate (s.e.)	p	95% CI	Estimate (s.e.)	p	95% CI
After relative to before: the main estimates	−19.2 (6.7)	0.021	[−34.5, −3.8]	+19.2 (3.3)	0.010	[+8.7, +29.8]
After relative to the six weeks before the announcement	−21.8 (5.8)	0.006	[−35.2, −8.3]	+15.3 (4.6)	0.045	[+0.6, +29.9]
Before relative to the six weeks before the announcement	−2.6 (4.9)	0.614	[−14.0, +8.8]	−4.0 (1.5)	0.083	[−8.9, +1.0]
Last 16 days before the increase relative to before	+19.3 (6.8)	0.022	[+3.6, +35.1]	−1.8 (4.0)	0.676	[−14.6, +10.9]
First 19 days after the increase relative to before	−31.3 (5.9)	<0.001	[−44.9, −17.8]	+17.1 (1.9)	0.003	[+10.9, +23.3]
From the last 16 days on (77 days) relative to before	−11.3 (5.1)	0.056	[−23.1, +0.4]	+12.5 (2.8)	0.021	[+3.5, +21.4]
Within the before period: second 21 days relative to first	+13.7 (4.9)	0.024	[+2.4, +25.0]	−0.9 (2.5)	0.728	[−8.8, +6.9]
Within the after period: second 21 days relative to first	+6.9 (3.3)	0.070	[−0.7, +14.5]	−1.7 (0.6)	0.069	[−3.7, +0.3]
Rule on anticipation: the estimate of the third row	−2.6					
compared with twice its standard error	9.9					not triggered
Rule on drift: the two drifts of the last two rows, summed	+20.6					
compared with half of E_N	−9.6					not triggered

B. The same contrasts: the rate in percentage points, and the number of companies with such accounts (log points)

	The rate in percentage points, P			Number of companies with such accounts, B		
	Estimate (s.e.)	p	95% CI	Estimate (s.e.)	p	95% CI
After relative to before: the main estimates	+7.5 (1.3)	0.010	[+3.4, +11.6]	−0.8 (4.6)	0.871	[−15.3, +13.7]
After relative to the six weeks before the announcement	+5.8 (1.9)	0.054	[−0.2, +11.9]	−4.1 (4.0)	0.373	[−16.7, +8.4]
Before relative to the six weeks before the announcement	−1.7 (0.8)	0.125	[−4.2, +0.8]	−3.3 (2.6)	0.291	[−11.6, +5.0]
Last 16 days before the increase relative to before	−0.4 (1.5)	0.800	[−5.2, +4.4]	+18.4 (2.2)	0.003	[+11.5, +25.4]
First 19 days after the increase relative to before	+6.6 (0.8)	0.004	[+4.1, +9.2]	−17.7 (5.4)	0.046	[−34.8, −0.7]
From the last 16 days on (77 days) relative to before	+4.8 (1.1)	0.022	[+1.3, +8.2]	0.0 (2.5)	0.996	[−7.9, +7.9]
Within the before period: second 21 days relative to first	−0.3 (1.0)	0.800	[−3.3, +2.8]	+10.9 (3.6)	0.057	[−0.6, +22.5]
Within the after period: second 21 days relative to first	−0.7 (0.3)	0.125	[−1.8, +0.4]	+5.4 (4.3)	0.299	[−8.3, +19.1]

C. Companies struck off between July and October, among those with non-dormant accounts on file in July

	Companies of the before period			Companies of the after period			
	With such accounts on July 1	Removed by October	Share removed (percent)	With such accounts on July 1	Removed by October	Share removed (percent)	Before less after (pct. points)
2022, read in July and October of 2024	42,714	1,806	4.23	35,375	820	2.32	+1.91
2023, read in July and October of 2025	43,012	2,426	5.64	36,759	862	2.35	+3.30
2024, read in July and October of 2026	43,003	1,907	4.43	36,503	1,097	3.01	+1.43
2024 less the mean of 2022 and 2023: δ							−1.17

D. The primary outcome net of the difference in Panel C

Non-dormant accounts	E_R (s.e.)	p	95% CI	$E_B = E_N + E_R$ [95% CI]	ρ [95% CI]	Interpretation under C1	Interval for ρ wholly below one-half	Rule of C3
As estimated	+19.2 (3.3)	0.010	[+8.7, +29.8]	+0.1 [−12.9, +13.0]	0.00 [−1.62, 0.42]	1	yes	
Less $\delta/100$	+20.4 (3.3)	0.009	[+9.8, +31.0]	+1.2 [−11.7, +14.2]	−0.06 [−1.80, 0.37]	1	yes	not triggered

Table A28. (continued)

E. Safeguard on the registration count

	Registrations by the number sequence	Effective working days	Registrations per working day	Companies seen in a snapshot	Seen per registration (percent)
Before, Mar. 4 to Apr. 14, 2024	116,808	28.6	4,084	116,764	99.96
After, May 20 to June 30, 2024	86,369	29.3	2,948	86,333	99.96
Before, Mar. 4 to Apr. 14, 2025	104,731	30.0	3,491	104,723	99.99
After, May 20 to June 30, 2025	98,057	29.3	3,347	98,056	100.00
Lowest and highest of the four					99.96 and 100.00
Bounds specified in the addendum					97.00 to 100.05
The safeguard					not triggered
All days from January 1, 2024	2,261,088			2,247,954	

F. The main statistics under either registration count (log points; P in percentage points)

	By the number sequence (the registration count used)		By companies seen in a snapshot	
	Estimate (s.e.)	95% CI	Estimate (s.e.)	95% CI
Registrations per working day, E_N (log points)	−19.2 (6.7)	[−34.5, −3.8]	−19.2 (6.7)	[−34.5, −3.8]
Rate of non-dormant accounts, E_R (log points)	+19.2 (3.3)	[+8.7, +29.8]	+19.2 (3.3)	[+8.7, +29.8]
The same rate in percentage points, P	+7.5 (1.3)	[+3.4, +11.6]	+7.5 (1.3)	[+3.4, +11.6]
Number of companies with such accounts, B (log points)	−0.8 (4.6)	[−15.3, +13.7]	−0.8 (4.6)	[−15.3, +13.7]
$E_B = E_N + E_R$ (log points)	+0.1	[−12.9, +13.0]	+0.1	[−12.9, +13.1]
ρ	0.00	[−1.62, 0.42]	0.00	[−1.65, 0.42]

Notes. Section C3, for registrations and for the primary outcome. Statistics and units are as in Table A26. Panels A and B: each row is a contrast between two periods of 2024 less the same contrast in the reference years, with 8 degrees of freedom for registrations and 3 for the outcome. The six weeks before the announcement are the 42 days that end 73 days before the fee increase, which are January 8 to February 18 in 2024 and 2016 and January 7 to February 17 in the other reference years; the fee increase was announced on February 19, 2024. The last 16 days before the increase are April 15 to 30, the first 19 days after the increase are May 1 to 19, and the 77 days run from April 15 to June 30. The last four rows of Panel A evaluate the two rules that the addendum attached to these contrasts, with the result in the last column. Anticipation: if the estimate for the before period relative to the six weeks before the announcement exceeds twice its standard error, the weeks before the fee increase were inflated by anticipation and the estimate from before the announcement is the more cautious one. The estimate for the before period relative to those weeks is negative, so the rule is not triggered. Drift: if the two drifts within the periods sum to more than half of E_N in its direction, the decline may be a trend and not a level shift. The two drifts sum to an increase and E_N is a decline, so the rule is not triggered. Panel C: an outcome is measured on a snapshot, so a company that filed accounts and was struck off before October is not counted. Of the companies of each period with non-dormant accounts on file on July 1 of their second year, the panel reports the share no longer on the register at the October snapshot; δ is the difference, before less after, in 2024 less its mean in 2022 and 2023, the years with a July snapshot. Panel D: $E_R - \delta/100$ with the standard error of E_R, and E_B and ρ computed with it. The rule is that, if the two estimates, with and without this adjustment, lead to different interpretations under section C1, the paper adopts the one less favorable to the prediction. Both lead to the first interpretation, with a confidence interval for ρ wholly below one-half, so the rule is not triggered. Panel E: the safeguard of section A3 of the addendum. If, in the before or after period of a year from 2024 on, the companies seen in a monthly snapshot are fewer than 97% of the registrations counted from the number sequence, or more than 100.05%, the sequence is taken to have a gap or a fault there, and the number of companies seen is used as the registration count for the dates from January 1, 2024. For this fee increase the periods are those of 2024 and 2025, and the safeguard is not triggered; Table A31 evaluates it for the second fee increase. No company incorporated from January 1, 2024, was excluded from the calculation of the sequence, and on no day did the highest number of a sequence fail to rise. Panel F: the addendum requires both versions to be computed and stored in every case. Across all contrasts and statistics for this fee increase, no estimate, standard error or bound of a t interval differs between the two registration counts by more than 0.1 (log points) or 0.05 (percentage points), no bound of a confidence interval for E_B by more than 0.2, and no value of ρ or bound of its confidence interval by more than 0.05; with companies seen as the registration count, the first interpretation also applies. The weekly series that section C3 stores for a figure are not tabulated, because the addendum takes no statistic from them. Specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known.

Table A29. United Kingdom: Levels by Period in 2023, 2024 and 2025

A. Registrations in the periods around May 1 of each year

	Dates	Registrations by the number sequence	Effective working days	Registrations per working day	Companies seen in a snapshot	Seen per registration (percent)
2023						
Six weeks before the announcement	Jan. 7 to Feb. 17	101,489	30.0	3,383	100,171	98.70
Before	Mar. 4 to Apr. 14	104,695	28.6	3,661	104,053	99.39
Last 16 days before the increase	Apr. 15 to Apr. 30	34,462	10.0	3,446	34,375	99.75
First 19 days after the increase	May 1 to May 19	49,741	13.6	3,657	49,725	99.97
After	May 20 to June 30	96,258	29.3	3,285	96,235	99.98
2024, the year of the fee increase						
Six weeks before the announcement	Jan. 8 to Feb. 18	115,072	30.0	3,836	115,029	99.96
Before	Mar. 4 to Apr. 14	116,808	28.6	4,084	116,764	99.96
Last 16 days before the increase	Apr. 15 to Apr. 30	55,260	12.0	4,605	55,234	99.95
First 19 days after the increase	May 1 to May 19	34,011	12.3	2,765	33,999	99.96
After	May 20 to June 30	86,369	29.3	2,948	86,333	99.96
2025						
Six weeks before the announcement	Jan. 7 to Feb. 17	97,430	30.0	3,248	97,427	100.00
Before	Mar. 4 to Apr. 14	104,731	30.0	3,491	104,723	99.99
Last 16 days before the increase	Apr. 15 to Apr. 30	37,680	10.6	3,555	37,677	99.99
First 19 days after the increase	May 1 to May 19	40,191	12.3	3,268	40,189	100.00
After	May 20 to June 30	98,057	29.3	3,347	98,056	100.00

B. Share of a period's registrations with each outcome, by year of incorporation (percent)

	Non-dormant accounts		A charge registered		Either of the two		Confirmation statement on file			On the register and active		
Companies incorporated in	2023	2024	2023	2024	2023	2024	2023	2024	2025	2023	2024	2025
Six weeks before the announcement	37.7	35.5	3.6	4.1	38.0	35.9	63.9	59.7	64.9	63.0	58.8	64.3
Before	39.2	35.7	3.6	3.9	39.6	36.1	65.2	59.9	66.1	64.8	59.3	66.0
Last 16 days before the increase	37.2	33.5	3.4	3.6	37.7	33.9	63.7	58.4	63.7	63.5	57.7	63.5
First 19 days after the increase	37.9	41.5	3.8	4.9	38.5	42.1	63.3	66.4	64.7	64.0	65.7	64.5
After	38.2	42.2	3.6	5.0	38.7	42.8	62.2	66.5	63.0	63.2	66.2	63.5

Notes. The levels underlying the estimates of Tables A26 to A28, for the year before the fee increase, the year of the increase and the year after. Panel A: registrations are counted from the number sequence; effective working days are as in Table A26. *Companies seen in a snapshot*: the companies of the period that appear in at least one snapshot of the register. Panel B: the share of a period's registrations with the outcome when the register was read, at the second October after the year of incorporation for the first three outcomes and at the first October for the last two. The periods are those of Panel A. A column is shown for a year only where the snapshot exists; for the companies of 2025 the second October is that of 2027. The year 2023 is not a reference year for the two outcomes measured at the first October (see the note to Table A27). Specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known.

Table A30. United Kingdom: Types of Registration at Founding

A. After relative to before, 2024 less 2025 (section C4)

	Registrations per working day (log points)			Share of all registrations (percentage points)			
	2024	2025	2024 less 2025, K	2024	2025	2024 less 2025, S	Declined by more than all registrations
Address of 100 or more incorporations in a year	−32.4	+1.0	−33.4	+0.03	+0.78	−0.75	yes
Ten or more incorporations at the address that day	−47.2	+2.7	−49.9	−1.56	+0.72	−2.29	yes
Retail by mail order or internet, or not in stores	−37.3	+6.0	−43.3	−0.37	+0.78	−1.16	yes
All registrations, for comparison	−32.6	−4.2	−28.4				

B. Registrations of each type in the two periods

	2024		2025	
	Before	After	Before	After
All registrations	116,808	86,369	104,731	98,057
Effective working days	28.6	29.3	30.0	29.3
Address of 100 or more incorporations in a year	17,802	13,187	15,402	15,187
share of all registrations (percent)	15.2	15.3	14.7	15.5
Ten or more incorporations at the address that day	13,445	8,591	10,604	10,639
share of all registrations (percent)	11.5	9.9	10.1	10.8
Retail by mail order or internet, or not in stores	9,612	6,784	7,606	7,890
share of all registrations (percent)	8.2	7.9	7.3	8.0

Notes. Three types of registration are defined from the information recorded in the first snapshot in which a company appears. First: the registered office is an address at which 100 or more companies were incorporated in the preceding 365 days. Second: ten or more companies were incorporated at the address on the same day. Third: the first industry code is retail sale by mail order or internet, or other retail sale not in stores (codes 47910 and 47990). Panel A: for each type, the log of its registrations per effective working day (multiplied by 100) and its share of all registrations (in percentage points), after relative to before, in 2024 and in 2025, and the difference, which is the statistic K or S specified in section C4. Periods are as in Table A26. The address in the first snapshot is comparable only between years with monthly snapshots, so 2025 is the single reference year, there is no standard error, and the addendum presents these statistics as descriptive. Its expectation was that registrations of each of the three types declined by more than other registrations. The last row reports all registrations for comparison with the types: the values of N for 2024 and for 2025 from Table A27 and their difference, which is not a statistic of the addendum. Relative to 2025, each type declined by more than all registrations, and the share of each type fell; within 2024 the first type declined by about as much as all registrations (Table A35, Panel G). Panel B: the numbers underlying Panel A. Specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known.

Table A31. United Kingdom: Fee Increase of February 1, 2026

A. Registrations per working day under either registration count (section C5; log points)

	By the number sequence			By companies seen (the registration count to be used)		
Registrations per working day	Estimate (s.e.)	p	95% CI	Estimate (s.e.)	p	95% CI
After relative to before: E_N	−14.3 (8.2)	0.125	[−33.6, +5.1]	−4.3 (8.2)	0.618	[−23.6, +15.1]
Last 16 days before the increase relative to before	+6.0 (4.3)	0.203	[−4.1, +16.2]	+15.9 (4.3)	0.008	[+5.8, +26.1]
First 19 days after the increase relative to before	−21.5 (6.3)	0.011	[−36.4, −6.6]	−11.8 (6.3)	0.104	[−26.7, +3.1]
From the last 16 days on (77 days) relative to before	−11.8 (6.4)	0.107	[−26.8, +3.3]	−1.9 (6.4)	0.777	[−16.9, +13.2]
Within the before period: second 21 days relative to first	−10.9 (9.0)	0.267	[−32.1, +10.4]	+9.0 (9.0)	0.353	[−12.3, +30.2]
Within the after period: second 21 days relative to first	+1.5 (3.6)	0.697	[−7.0, +10.0]	+1.6 (3.6)	0.673	[−6.9, +10.1]

Table A31. (continued)

B. After relative to before in each year (log points)

After relative to before, N	Reference years 2016	2017	2018	2019	2021	2022	2023	2025	Mean	Standard deviation	2026
By the number sequence	+45.1	+25.9	+23.4	+33.7	+20.5	+24.5	+28.0	+29.6	+28.8	7.7	+14.6
By companies seen in a snapshot	+45.1	+25.9	+23.4	+33.7	+20.5	+24.5	+28.0	+29.6	+28.8	7.7	+24.6

C. Levels in the periods of 2025 and 2026, and the safeguard on the registration count

	Registrations by the number sequence	Effective working days	Registrations per working day	Companies seen in a snapshot	Seen per registration (percent)
Before, Dec. 5, 2024 to Jan. 15, 2025	72,522	27.9	2,599	72,518	99.99
Last 16 days before the increase, Jan. 16 to 31, 2025	37,526	12.0	3,127	37,525	100.00
First 19 days after the increase, Feb. 1 to 19, 2025	44,885	13.0	3,453	44,884	100.00
After, Feb. 20 to Apr. 2, 2025	104,825	30.0	3,494	104,818	99.99
Before, Dec. 5, 2025 to Jan. 15, 2026	79,140	27.9	2,837	71,328	90.13
Last 16 days before the increase, Jan. 16 to 31, 2026	40,974	11.0	3,725	40,767	99.49
First 19 days after the increase, Feb. 1 to 19, 2026	41,886	14.0	2,992	41,590	99.29
After, Feb. 20 to Apr. 2, 2026	98,464	30.0	3,282	98,063	99.59
Lowest and highest of the before and after periods					90.13 and 99.99
Bounds specified in the addendum					97.00 to 100.05
The safeguard					triggered

D. Types of registration at founding: after relative to before, 2026 less 2025

	Registrations per working day (log points)			Share of registrations by the number sequence (pct. points)			Share of companies seen (pct. points)		
	2026	2025	2026 less 2025, K	2026	2025	2026 less 2025, S	2026	2025	2026 less 2025, S
Address of 100 or more incorporations in a year	+5.8	+22.0	−16.2	−1.40	−1.16	−0.24	−3.14	−1.16	−1.99
Ten or more incorporations at the address that day	+5.8	+20.6	−14.8	−0.99	−0.94	−0.04	−2.23	−0.94	−1.28
Retail by mail order or internet, or not in stores	−3.5	+10.9	−14.5	−1.27	−1.49	+0.21	−2.09	−1.49	−0.60
All registrations by the number sequence	+14.6	+29.6	−15.0						
All companies seen in a snapshot	+24.6	+29.6	−5.0						

Notes. The fee increased from £50 to £100 on February 1, 2026. *Before*: December 5, 2025 to January 15, 2026. *After*: February 20 to April 2, 2026. Statistics and units are as in Tables A26, A28 and A30. The reference years for registrations are 2016 to 2019, 2021, 2022, 2023 and 2025, so that every contrast of Panel A has seven degrees of freedom; 2025 is the single reference year for the types. No outcome is yet available for these companies. Panel C evaluates the safeguard of section A3 (see the note to Table A28) for the before and after periods of 2025 and 2026. In the before period of 2026 the companies seen in a snapshot are fewer than 97% of the registrations counted from the number sequence, so the safeguard is triggered: the sequence is taken to have a gap or a fault there, and the number of companies seen is the registration count to be used for the dates from January 1, 2024. Panels A, B and D report both counts. The number of registrations of a type does not depend on which count of all registrations is used, so K is the same under both counts, whereas S is not. The last two rows of Panel D report all registrations for comparison with the types, as in Table A30. The rules of section C3 were specified for the fee increase of 2024 and are not evaluated here. Section C5 specified two cautions in advance. First, the before period contains Christmas, and N at this date had a standard deviation of 8 log points over the seven reference years whose data were not sealed when the addendum was written (all but 2025), so that a decline of less than about 20% cannot be distinguished from noise. Second, identity verification of directors became compulsory on November 18, 2025, seventeen days before the before period starts; temporarily lower registrations after that date would depress the before period and mask a decline. The web filing service of the registrar was also closed from the afternoon of March 13 to the morning of March 16, 2026, within the after period. The two predictions of section C6 for these companies concern the snapshots of October 2027 and October 2028 and cannot yet be evaluated. Specified in advance of the tabulations, in an addendum written after all Colorado results and the monthly totals were known.

Table A32. United Kingdom: Later Six-Week Blocks and Months of Incorporation

A. Each later six-week block relative to the before period, less the same contrast in the reference years (log points)

	Registrations		Measured at the second October				Measured at the first October			
			Non-dormant accounts		A charge registered		Confirmation statement on file		On the register and active	
Six weeks ending	*N* (s.e.)	Ref. years	*R* (s.e.)	*B* (s.e.)	*R* (s.e.)	Ref. years	*R* (s.e.)	*B* (s.e.)	*R* (s.e.)	Ref. years
June 30, 2024	−19.2 (6.7)	9	+19.2 (3.3)	−0.8 (4.6)	+26.9 (2.4)	4	+14.7 (1.9)	−7.4 (7.8)	+14.5 (2.0)	4
Aug. 11, 2024	−18.5 (7.1)	9	+20.3 (3.4)	+1.3 (5.2)	+32.4 (4.8)	4	+15.2 (1.5)	−4.5 (9.6)	+12.7 (7.2)	4
Sept. 22, 2024	−16.6 (7.5)	9	+21.2 (4.4)	+3.2 (3.2)	+43.3 (6.4)	4	+17.9 (5.5)	−1.7 (12.5)	+13.5 (4.4)	4
Nov. 3, 2024	−16.3 (7.0)	8	+21.0 (6.5)	+0.5 (3.8)	+46.5 (18.5)	4	+26.7 (13.6)	+8.7 (15.8)	+14.1 (5.7)	3
Dec. 15, 2024	−18.7 (7.0)	8					+11.5 (3.4)	−8.1 (0.2)	+12.5 (4.5)	2
Jan. 26, 2025	−16.6 (9.8)	8					+12.8 (3.3)	−6.2 (9.6)	+13.3 (2.6)	2
Mar. 9, 2025	−17.3 (7.2)	6					+12.6 (3.1)	−6.2 (11.9)	+13.1 (2.3)	2
Apr. 20, 2025	−19.2 (10.8)	6					+14.2 (4.1)	−6.0 (11.2)	+14.6 (3.2)	2

B. By month of incorporation, January 2023 to September 2026

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
Registrations per effective working day, by the number sequence												
2023	3,232	3,533	3,591	3,565	3,640	3,178	3,418	3,245	3,580	3,500	3,394	3,206
2024	3,807	3,821	3,924	4,579	2,765	3,033	2,776	2,956	3,091	3,059	2,887	2,441
2025	3,079	3,337	3,606	3,474	3,348	3,299	3,184	3,314	3,431	3,274	3,055	2,746
2026	3,357	3,015	3,357	3,265	3,178	2,884	2,958	2,859	3,092			
The same: change from the same month a year earlier (log points)												
2023	+3.6	+8.1	+7.4	+5.9	+11.1	−0.6	+14.9	+7.5	+15.9	+8.8	+10.7	+28.4
2024	+16.4	+7.9	+8.9	+25.0	−27.5	−4.7	−20.8	−9.3	−14.7	−13.5	−16.2	−27.3
2025	−21.2	−13.5	−8.5	−27.6	+19.2	+8.4	+13.7	+11.4	+10.4	+6.8	+5.7	+11.8
2026	+8.6	−10.1	−7.2	−6.2	−5.2	−13.4	−7.4	−14.8	−10.4			
Share of the month's registrations with non-dormant accounts at the second October (percent)												
2023	37.0	38.1	40.1	37.3	37.6	38.6	37.2	35.7	36.5	33.4	31.8	32.5
2024	34.1	36.6	35.9	34.2	41.7	42.3	42.0	41.6	42.7	40.1	37.9	
Companies with such accounts per working day: change from a year earlier (log points)												
2023	−1.1	+4.0	+5.0	+2.7	+7.3	+0.8	+12.8	+2.2	+11.5	+3.1	+2.7	+14.3
2024	+8.1	+4.0	−2.1	+16.5	−17.3	+4.3	−8.8	+6.1	+0.8	+4.6	+1.2	
Share of the month's registrations with a confirmation statement on file at the first October (percent)												
2023	63.6	64.4	66.0	63.7	62.6	62.5	58.7	51.6	33.1	7.3	6.0	54.8
2024	57.7	61.6	60.5	58.7	66.4	66.5	63.9	58.1	39.9	8.6	6.8	63.6
2025	64.3	65.2	65.9	65.0	64.2	62.7	60.5	54.9	37.1	7.2	5.2	

Notes. These analyses were run after the results under Addendum 4 were known, and none was specified in advance. Panel A: the contrast of Table A26 with the after period replaced in turn by each later block of 42 days, identified by its last day, relative to the same before period, less the same contrast in the reference years. The first row is the after period of the pre-specified test. A reference year is used only if the block in that year ends before a later event affects it (the lockdown of 2020, the announcements of February 2024 and October 2025), so the number of reference years (*Ref. years*) declines from block to block; with two reference years, a standard error has one degree of freedom. A blank cell indicates that the October at which the outcome is measured has not yet occurred. Panel B: registrations are counted from the number sequence, which the safeguard of Table A31 found to have a gap or a fault between December 5, 2025 and January 15, 2026; the entries for those two months, and the changes that use them, should be interpreted with this limitation in mind. An outcome is measured for the companies of December to November together, so the share with a confirmation statement is measured at ages from 10 to 22 months; it is low by construction for companies incorporated from August to November, whose first statement was not yet due or had only just become due. Exploratory.

E.5. Further Tables: Reference Periods, the Holiday Cohorts, and the U.K. Test

The tables and the figure in this subsection report analyses conducted after the results under Addenda 3 and 4 were known. They support Sections D.2, D.3, and D.6. None was specified in advance [E], except for the rows that the table notes identify as pre-specified. The intervals for ρ are percentile intervals over the draws retained, and Section D.1 compares them with confidence sets by test inversion.

Table A33. Colorado: Estimates under Alternative Reference Periods, by Holiday and Outcome

A. Change in the share of companies with each record (percentage points)

	First holiday	Second holiday	Pooled
PPP loan			
1. Same months of the adjacent years [A3]	−0.74 (0.26)	−0.68 (0.22)	−0.71 (0.17)
2. All unaffected months, linear trend	−0.64 (0.25)	−0.57 (0.22)	−0.60 (0.17)
3. All unaffected months, quadratic trend	−0.77 (0.20)	−0.75 (0.16)	−0.76 (0.13)
5. Less the flanking months; adjacent years	−0.04 (0.33)	−0.23 (0.33)	−0.15 (0.24)
6. Less the flanking months; all other years	−0.14 (0.33)	−0.43 (0.17)	−0.30 (0.17)
Any of the three records			
1. Same months of the adjacent years [A3]	−1.95 (0.39)	−1.55 (0.36)	−1.72 (0.26)
2. All unaffected months, linear trend	−1.41 (0.59)	−0.84 (0.57)	−1.08 (0.41)
3. All unaffected months, quadratic trend	−1.80 (0.31)	−1.36 (0.28)	−1.55 (0.21)
5. Less the flanking months; adjacent years	−1.51 (0.42)	−1.35 (0.44)	−1.42 (0.31)
6. Less the flanking months; all other years	−1.19 (0.38)	−1.57 (0.38)	−1.41 (0.27)

B. Rate among marginal registrations relative to inframarginal registrations, ρ

	First holiday	Second holiday	Pooled
PPP loan			
1. Same months of the adjacent years [A3]	−0.18 [−1.61, 0.75]	0.21 [−0.55, 0.81]	0.07 [−0.57, 0.60]
2. All unaffected months, linear trend	−0.09 [−0.99, 0.73]	0.32 [−0.15, 0.86]	0.19 [−0.27, 0.68]
3. All unaffected months, quadratic trend	−0.18 [−1.05, 0.41]	0.20 [−0.20, 0.54]	0.07 [−0.34, 0.42]
5. Less the flanking months; adjacent years	0.95 [−0.30, 2.19]	0.73 [−0.59, 1.99]	0.81 [−0.19, 1.78]
6. Less the flanking months; all other years	0.83 [−0.26, 1.96]	0.58 [0.26, 0.99]	0.67 [0.24, 1.17]
Any of the three records			
1. Same months of the adjacent years [A3]	−0.44 [−2.03, 0.33]	0.20 [−0.53, 0.67]	−0.02 [−0.65, 0.41]
2. All unaffected months, linear trend	−0.14 [−1.20, 0.79]	0.54 [−0.04, 1.21]	0.31 [−0.24, 0.91]
3. All unaffected months, quadratic trend	−0.30 [−1.33, 0.25]	0.33 [−0.06, 0.63]	0.12 [−0.30, 0.43]
5. Less the flanking months; adjacent years	0.11 [−0.78, 0.72]	0.36 [−0.47, 1.02]	0.26 [−0.38, 0.78]
6. Less the flanking months; all other years	0.35 [−0.48, 0.90]	0.34 [−0.04, 0.69]	0.34 [−0.04, 0.67]

Table A33. (continued)

C. Number of registrations and of companies with each record (log points)

	First holiday	Second holiday	Pooled
Registrations			
1. Same months of the adjacent years [A3]	+12.4 (1.6)	+18.6 (1.5)	+16.0 (1.1)
2. All unaffected months, linear trend	+11.8 (3.0)	+18.7 (2.9)	+15.7 (2.1)
3. All unaffected months, quadratic trend	+12.9 (2.7)	+20.1 (2.6)	+17.0 (1.9)
5. Less the flanking months; adjacent years	+16.5 (2.8)	+20.8 (2.6)	+19.0 (1.9)
6. Less the flanking months; all other years	+18.4 (2.6)	+23.7 (3.5)	+21.4 (2.3)
Companies with a PPP loan			
1. Same months of the adjacent years [A3]	−2.3 (5.6)	+4.1 (4.8)	+1.4 (3.7)
2. All unaffected months, linear trend	0.0 (4.5)	+6.7 (4.1)	+3.9 (3.0)
3. All unaffected months, quadratic trend	−1.7 (4.0)	+4.1 (3.5)	+1.6 (2.6)
5. Less the flanking months; adjacent years	+16.2 (6.3)	+14.8 (6.7)	+15.4 (4.7)
6. Less the flanking months; all other years	+16.6 (6.2)	+14.1 (3.2)	+15.2 (3.2)
Companies with any of the three records			
1. Same months of the adjacent years [A3]	−5.9 (4.3)	+3.9 (4.1)	−0.3 (3.0)
2. All unaffected months, linear trend	−1.6 (4.8)	+10.3 (4.7)	+5.2 (3.4)
3. All unaffected months, quadratic trend	−4.1 (3.9)	+7.0 (3.7)	+2.2 (2.7)
5. Less the flanking months; adjacent years	+2.2 (3.4)	+7.8 (3.6)	+5.4 (2.5)
6. Less the flanking months; all other years	+7.0 (4.9)	+8.9 (3.8)	+8.1 (3.0)

Notes. Ordinary LLCs formed in the two holidays. Rows are numbered as in Table A11, whose pooled entries this table reproduces and extends to each holiday and to the outcome defined by any of the three records; row 4 of that table, the whole window, is reported by holiday in Table A25. Standard errors are in parentheses and 95% confidence intervals in brackets. Row 1 is the pre-specified comparison [A3], except for the numbers of companies with each record in Panel C, which the addendum did not specify [E]. All other rows were computed after its results were known [E]. Rows 2 and 3 use all unaffected months of 2010 to 2018 as the reference period, in a regression with month-of-year effects and a linear or quadratic trend. In the same regressions, the difference for the PPP loan in the flanking months, pooled, is −0.28 (0.21) and −0.44 (0.18) percentage points. For these rows, the confidence intervals by holiday in Panel B use the degrees of freedom given by the rule for non-overlapping blocks (29 and 21), and the pooled confidence interval uses the 7 degrees of freedom of the pre-specified test, as in Table A11. Rows 5 and 6: the statistic for the holiday months less the statistic for the two flanking months (the month of the announcement and the month after the holiday), compared with the same difference in the two adjacent years (row 5) or in every other year of 2010 to 2018 in which none of those months is affected (row 6, with 6 and 7 reference years). In Panel C the reference runs of row 1 end in 2018.

Table A34. Colorado: Further Analyses of the Holiday Cohorts

A. Each record relative to its baseline rate

	Holiday months			Flanking months		
	Baseline, per 100	Change, pct. points	Percent of baseline	Baseline, per 100	Change, pct. points	Percent of baseline
PPP loan, 2020–21	5.2	−0.71 (0.17)	−14	5.4	−0.56 (0.17)	−10
reporting two or more jobs	3.3	−0.52 (0.11)	−16	3.5	−0.35 (0.13)	−10
SBA loan, to June 2026	0.9	−0.13 (0.06)	−15	0.8	0.14 (0.06)	17
UCC financing statement, to Oct. 2026	8.3	−1.25 (0.29)	−15	8.5	−0.09 (0.29)	−1
Any of the three records	11.5	−1.72 (0.26)	−15	11.6	−0.31 (0.25)	−3
UCC financing statement within 24 months	4.1	−0.70 (0.17)	−17	4.2	−0.10 (0.21)	−2

B. Delinquency before the PPP loans (percentage points)

	Holiday months			Flanking months,
	First holiday	Second holiday	Pooled	pooled
Share delinquent before February 15, 2020	3.16 (2.03)	3.17 (1.63)	3.17 (1.27)	0.17 (0.98)
PPP loan, all registrations	−0.74 (0.26)	−0.68 (0.22)	−0.71 (0.17)	−0.56 (0.17)
PPP loan, registrations not delinquent	−0.76 (0.37)	−0.76 (0.31)	−0.76 (0.24)	−0.82 (0.28)

C. Profit corporations, with and without bulk filings

	Formed in the holiday months	Registrations, log points	PPP loan, pct. points	Marginal relative to inframarginal, ρ	Flanking months: PPP loan, pct. points
All profit corporations [A3]	7,543	+24.6 (3.7)	−1.90 (0.39)	0.27 [−0.20, 0.62]	+0.46 (0.71)
Excluding bulk filings, threshold 50	6,921	+16.2 (4.8)	−1.01 (0.26)	0.43 [−0.64, 0.80]	+0.46 (0.59)
Excluding bulk filings, threshold 25	6,801	+14.4 (4.1)	−0.89 (0.39)	0.44 [−0.92, 1.02]	+0.50 (0.62)

Table A34. (continued)

D. Sub-periods defined by the announcement dates: the first holiday

	Registrations	Registrations per weekday, log points	Share with a PPP loan, %	PPP loan, pct. points
1 to 18 Nov (before the announcement)	2,631	−1.8 (2.3)	4.45	−0.69 (0.49)
19 to 30 Nov (announced, fee still $50)	1,726	−5.8 (3.0)	4.35	−0.82 (0.63)
1 to 15 Dec	2,765	+22.4 (2.4)	4.67	−0.05 (0.51)
16 to 31 Dec	2,746	+22.5 (2.4)	4.19	−1.51 (0.51)
January	6,732	+6.7 (1.5)	4.69	−0.82 (0.33)
1 to 19 Feb	3,737	+12.6 (2.0)	4.84	−0.73 (0.45)
20 to 28 Feb (end announced)	2,368	+15.6 (2.6)	4.48	−0.42 (0.55)
1 to 15 Mar	2,880	−9.6 (2.3)	4.58	−0.48 (0.48)
16 to 31 Mar	2,761	+2.7 (2.3)	4.67	−0.88 (0.50)
April	5,512	−3.8 (1.6)	5.12	+0.18 (0.36)

E. Sub-periods defined by the announcement dates: the second holiday

	Registrations	Registrations per weekday, log points	Share with a PPP loan, %	PPP loan, pct. points
May (before the announcement)	6,158	+5.8 (1.6)	4.38	−0.74 (0.33)
1 to 8 Jun (before the announcement)	1,441	+4.4 (3.2)	5.69	+0.46 (0.73)
9 to 30 Jun (announced, fee still $50)	4,250	+1.4 (1.9)	4.94	−0.71 (0.42)
1 to 15 Jul	3,092	+18.5 (2.3)	4.59	−0.70 (0.49)
16 to 31 Jul	3,917	+26.6 (2.1)	3.98	−1.26 (0.43)
August and September	13,446	+18.0 (1.1)	4.45	−0.44 (0.23)
1 to 15 Oct (end announced)	3,222	+13.0 (2.2)	4.31	−0.74 (0.46)
16 to 31 Oct (end announced)	3,627	+13.9 (2.1)	4.47	−0.95 (0.45)
1 to 15 Nov	2,703	+2.3 (2.3)	5.03	−0.41 (0.52)
16 to 30 Nov	2,063	−6.9 (2.6)	5.24	−0.54 (0.60)
December	5,552	+0.6 (1.7)	6.29	+0.39 (0.40)

Table A34. (continued)

F. Retiming of filings: magnitudes that the flanking months would imply

	Flanking months, log points		Implied share retimed, %		PPP loan rate required
	Registrations	Companies with a loan	Registrations	Companies with a loan	of retimed filings, %
First holiday	−4.1 (2.4)	−18.5 (4.5)	4.0	16.9	22
Second holiday	−2.2 (2.4)	−10.6 (4.2)	2.2	10.1	26
Pooled	−3.0 (1.7)	−14.0 (3.1)	3.0	13.1	24

G. Outer and intervening months in unaffected years

	Blocks	Not overlapping	Correlation	Slope	Holiday less flanking, pct. points
PPP loan					
Blocks with three months between	24	7	+0.04	+0.05	−0.04 (0.33)
Blocks with four months between	21	4	−0.12	−0.10	−0.23 (0.33)
UCC financing statement					
Blocks with three months between	24	7	+0.40	+0.48	−1.26 (0.46)
Blocks with four months between	21	4	+0.29	+0.29	−1.09 (0.49)
Any of the three records					
Blocks with three months between	24	7	+0.37	+0.44	−1.51 (0.42)
Blocks with four months between	21	4	+0.23	+0.22	−1.35 (0.44)

Notes. Computed after all results under Addendum 3 were known [E], except where a row is marked otherwise. Unless stated otherwise, standard errors in parentheses are standard deviations of the same statistic over unaffected reference runs, and pooled entries weight the two holidays by their numbers of months. The *flanking months* are the month in which each holiday was announced and the month after it ended. Panel A: the baseline is the mean rate in the same months of the two adjacent years; the change is the statistic T of equation (4). The last row is the outcome specified in Addendum 2 [A2], here with the reference runs of Addendum 3; with its own reference runs, the pooled standard error is 0.15 (Table 3). Panel B: a registration is delinquent if the registry records a date of delinquency before February 15, 2020. The statistic is T for the share delinquent and for the share with a PPP loan, among all registrations and among those not delinquent. Panel C: the rule that identifies bulk filings among LLCs (Section C.5), applied to profit corporations at thresholds of 50 and 25; ρ uses the marginal share of the same row, and for the two rows that exclude bulk filings the sets by test inversion run from −0.83 to 0.80 and from −1.09 to 1.02. The first row is the pre-specified row of Table A22 [A3]. At the threshold of 50, the rule assigns 622 of the 7,543 corporations formed in the holiday months to bulk groups, 619 of them formed in February 2013 and September 2014, and none of the 622 has a PPP loan. Panels D and E: registrations received in each sub-period; the log of registrations per weekday less its mean for the same dates one year earlier and one year later, with a Poisson standard error; the share of the sub-period's companies with a PPP loan; and that share less its mean for the same dates in the two adjacent years, with a binomial standard error. Panel F: T for the two flanking months, in its form for log numbers, computed for registrations and for the number of companies with a PPP loan. The standard errors are standard deviations over unaffected blocks with the structure of each holiday, and the pooled row weights the holidays by three and four months; Table A25, Panel A, reports the same two estimates with standard errors from reference runs of two adjoining months and pools them with equal weights. The implied shares are one minus the exponential of each statistic, and the last column is the PPP loan rate that retimed filings would need in order to account for both, which is the rate of the adjacent years multiplied by the ratio of the two shares. Among all ordinary LLCs formed from 2011 to 2018 the rate is 5.4%; it is at most 5.7% for any type of filer and 11.9% for profit corporations. Panel G: blocks of months of which none is affected in its own year or in the adjacent years, each with two outer months and three or four months between them, as in the two holidays. *Correlation* and *slope* relate the statistic for the intervening months to that for the outer months across blocks; the slope is that of a regression with an intercept, and Panel D of Table A39 reports the slope through the origin. The last column is the statistic for the holiday months less that for the flanking months.

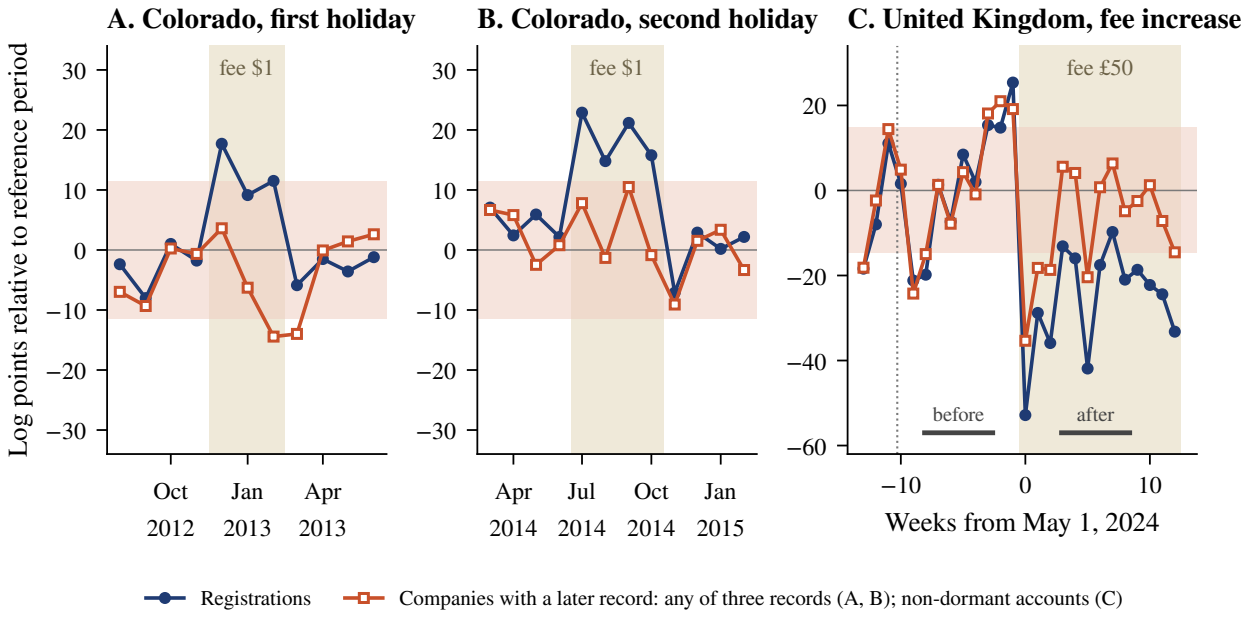


Figure A4. Registrations and the Number of Companies with Any of the Three Records around Three Fee Changes

Notes. Identical to Figure 1, except that Panels A and B omit the number of companies that later had a PPP loan, and the band is two standard errors of the single-month statistic for the number with any of the three records (a PPP loan, an SBA loan or a UCC financing statement). Panel C is unchanged. Not specified in advance [E].

Table A35. United Kingdom: Sensitivity Analyses for the Test of the Fee Increase of May 1, 2024

A. The pre-specified before period

	Registrations, log points	Share with accounts, log points	Number with accounts, log points (direct)	Number, percent [95% CI]
Before period relative to the six weeks before the announcement	−2.6 (4.9)	−4.0 (1.5)	−3.3 (2.6)	−6 [−15, 3]
Second half of the before period relative to its first half	+13.7 (4.9)	−0.9 (2.5)	+10.9 (3.6)	+14 [3, 25]

B. Confidence intervals under alternative assumptions about the two errors

	Correlation of the errors as drawn	Number, percent [95% CI]	ρ [95% CI]	Draws with a decline below half a log point, %
As stored by the locked script (400,000 draws)	−0.52	[−12, +14]	[−1.62, 0.42]	1.2
The same construction, 24 million draws	−0.52	[−12, +14]	[−1.63, 0.42]	
Errors at the stated correlation, 8 million draws	−0.68	[−10, +11]	[−1.36, 0.34]	
Independent errors	0.00	[−17, +21]	[−2.48, 0.59]	1.1
Bivariate t with a common scale	−0.68	[−15, +17]	not reported[a]	3.4

C. Placebo dates

	Cutoff dates	Placebo estimates	S.d., log points	Largest, log points	Rejecting at 5%, %	$\|t\|$ above that at the increase, %	At the increase, log points
Share with non-dormant accounts	36	144	2.9	7.3	7	4	+19.2
Registrations per working day	33	289	5.0	16.2	6	4	−19.2

Table A35. (continued)

D. By week of incorporation, January to July 2024

Week starting	Registrations per working day	Share with accounts, % 2024	2023	Week starting	Registrations per working day	Share with accounts, % 2024	2023
01/01	3,748	32.1	35.5	04/08	4,518	36.0	36.1
01/08	3,573	33.9	37.3	04/15	4,097	35.5	37.6
01/15	3,999	34.5	37.8	04/22	4,882	32.6	37.7
01/22	4,078	34.9	37.7	04/29	3,332	36.1	38.0
01/29	3,762	34.5	36.6	05/06	2,983	41.0	36.6
02/05	3,616	37.2	38.4	05/13	2,976	41.0	39.0
02/12	3,986	38.1	38.5	05/20	3,128	42.2	38.1
02/19	4,019	35.9	37.6	05/27	3,091	41.2	37.3
02/26	3,795	36.2	39.3	06/03	2,555	43.1	37.9
03/04	3,142	36.3	39.6	06/10	3,147	42.6	38.5
03/11	4,278	36.3	40.0	06/17	3,182	42.3	38.4
03/18	4,072	35.8	40.5	06/24	2,604	41.5	39.0
03/25	3,972	35.2	40.1	07/01	2,785	42.6	38.1
04/01	4,576	34.2	38.2	07/08	3,017	42.4	37.1

E. Outcomes measured at equal ages

	Share with non-dormant accounts, %			Log change in the share, log points	
	After, on October 1	Before, on October 1	Before, on July 16	On one day	At equal ages
2022	38.1	40.7	41.8	−6.5	−9.4
2023	38.2	39.2	41.0	−2.6	−7.0
2024	42.2	35.7	36.6	+16.7	+14.1
2024 less the mean of 2022 and 2023				+21.3 (3.4)	+22.3 (2.1)

Table A35. (continued)

F. Change from the same dates one year earlier (log points)

	2022		2023		2024		2024 less 2023		
	N	*B*	*N*	*B*	*N*	*B*	*N*	*B*	ρ
December 1 to February 18	−4.7	−4.4	+5.0	+0.8	+17.3	+8.4	+12.2	+7.6	
January 22 to March 3	−4.4	−5.5	+5.8	+1.7	+12.5	+7.3	+6.7	+5.6	
February 19 to March 3	−17.0	−22.3	+4.2	+2.7	+18.0	+11.1	+13.8	+8.3	
March 4 to April 14	−1.4	−2.0	+7.5	+3.9	+10.9	+1.5	+3.4	−2.4	
April 15 to April 30	+9.4	+1.5	+3.1	+3.5	+29.0	+18.4	+25.8	+14.8	
May 1 to May 19	−7.0	−10.5	+14.4	+9.7	−28.0	−19.0	−42.3	−28.7	0.63
May 20 to June 30	+15.3	+7.6	+0.9	+1.2	−10.8	−1.0	−11.8	−2.2	0.18
July 1 to August 31	+11.1	+4.0	+11.1	+7.5	−15.1	−1.4	−26.2	−8.9	0.31
September 1 to November 30	+6.0	−2.3	+11.8	+5.9	−14.7	+2.4	−26.5	−3.5	0.12
May 20 to November 30	+9.5	+1.8	+9.3	+5.3	−14.0	+0.5	−23.3	−4.9	0.19
March 4 to November 30	+6.5	+0.4	+9.0	+5.2	−7.3	+0.6	−16.3	−4.6	0.27
April 15 to November 30	+8.1	+0.7	+9.3	+5.7	−11.2	+0.4	−20.5	−5.3	0.24
January 8 to February 18	+0.3	+1.3	+6.5	+1.7	+10.6	+4.7	+4.2	+3.0	

G. Registrations at addresses shared by many companies, before and after the fee increase

	Share of registrations, %		Per working day		Log change,	Share of the decline
	Before	After	Before	After	log points	in registrations, %
Address with 100 or more incorporations in a year	15.2	15.3	622	450	−32	15
Ten or more incorporations at the address that day	11.5	9.9	470	293	−47	16
Either of the two	16.3	15.5	667	457	−38	19
Industry code for retail by mail order or internet	8.2	7.9	336	232	−37	9
All registrations			4,084	2,948	−33	

Notes. Computed after all results under Addendum 4 were known [E]. The outcome is non-dormant accounts on file, measured in the October that falls two years after the cohort year. Panel A: the statistics of Table A26 for two other pairs of periods, namely the pre-specified before period (March 4 to April 14, 2024) relative to the six weeks before the announcement (January 8 to February 18), and the second half of the before period relative to its first half. Reference years are those of Table A26. Panel B: 95% confidence intervals for the change in the number of companies with such accounts and for ρ under alternative joint distributions of the errors of the two statistics. The locked script obtains two t variates from a correlated normal pair, so the errors as drawn are less strongly correlated than the correlation that the addendum states (Table A6, row 19). The second row repeats that construction with more draws, and the third sets the correlation of the normal pair so that the errors have the stated correlation. [a] Under the addendum's rule no confidence interval for ρ is reported when more than 2.5% of draws show a decline in registrations of less than half a log point. The intervals for ρ in this panel are percentiles over the draws retained (Table A6, row 20), and Table A10 reports the confidence sets by test inversion under the same assumptions about the errors; Table A9 reports the set under a common scale. Panel C: the pre-specified statistic computed at placebo dates one week apart, at which no fee changed, for each reference year relative to the other reference years: 36 dates from January 28 to September 30 with four reference years for the share, and 33 dates from March 5 to October 15 with nine reference years for registrations (2025 is omitted where its after period would extend beyond October 20, 2025, ten days before the second fee increase was announced). The columns give the standard deviation and the largest absolute value of the placebo estimates, the percentage of placebo t ratios that reject at the 5% level, and the percentage that exceed in absolute value the t ratio at the fee increase. Panel D: weeks begin on Monday; the columns for 2023 give the week 364 days earlier, measured one year earlier. Panel E: the after period is measured on October 1 and the before period 77 days earlier, by linear interpolation between monthly snapshots, so that both are measured at the same age. The reference years are 2022 and 2023, the only years for which the monthly snapshots permit the calculation, which leaves one degree of freedom. Panel F: N is the log change in registrations per working day and B the log change in the number of companies with such accounts, each from the same dates one year earlier; ρ is equation (6) applied to the differences between 2024 and 2023 and is shown for the periods that end after the fee increase. The comparison has no reference years and hence no standard errors. The double difference cited in Section 7.5 is the row for May 20 to November 30 less the row for January 8 to February 18. Panel G: the before and after periods of the pre-specified test within 2024. The types of registration are those of Table A30, which compares them with 2025; the last column is each type's share of the decline in registrations per working day.

E.6. Further Tables: Maintenance Fees, Financing Statements, Record Linkage, and Inference

The tables in this subsection report analyses of the U.K. reference years and maintenance fees, of the fee for financing statements in Colorado, of the record linkage, and of the inference. They support Sections D.2 to D.6. All of the analyses were run after the results under Addenda 3 and 4 were known, and none was specified in advance [E]; the rows marked [A3] or [A4] reproduce pre-specified estimates for comparison. Where a row repeats an estimate that an earlier script stored and that the text reports, the table prints the stored confidence interval, because an interval computed from a new set of draws can differ from it in the second decimal place (Table A6, row 18). The intervals for ρ are percentile intervals over the draws retained, and Section D.1 compares them with confidence sets by test inversion.

Table A36. United Kingdom: Maintenance Fees, Reference Years, and Accounts Ever Filed

A. Fees owed by the companies of each period, by year of incorporation

	Incorporation		First statement		Second statement		Fees equal in
Year of incorporation	Before	After	Before	After	Before	After	the two periods
2017	£12	£12	£13	£13	£13	£13	Yes
2021	£12	£12	£13	£13	£13	£13	Yes
2022	£12	£12	£13	£13	£13	£34	No: second statement
2023	£12	£12	£13	£34	£34	£34	No: first statement
2024 (fee increase)	£12	£50	£34	£34	£50	£50	No: incorporation

B. The test under other sets of reference years and with the earlier base period (primary outcome)

	Reference years for registrations	Reference years for the share	Registrations, log points	Share, log points	Number of companies, percent [95% CI]	ρ [95% CI]
Base period of the test (March 4 to April 14)						
Pre-specified [A4]	2015–19, 2021–23, 2025	2017, 2021, 2022, 2023	−19.2 (6.7)	+19.2 (3.3)	+0.1 [−12.1, +13.9]	0.00 [−1.62, 0.42]
The four years that have both [the number: A4]	2017, 2021, 2022, 2023	2017, 2021, 2022, 2023	−20.0 (7.8)	+19.2 (3.3)	−0.8 [−14.2, +14.7]	0.04
Without 2022	2015–19, 2021, 2023, 2025	2017, 2021, 2023	−18.0 (6.0)	+17.9 (1.8)	−0.1 [−12.2, +13.5]	0.01 [−1.71, 0.41]
Without 2022, same years for both	2017, 2021, 2023	2017, 2021, 2023	−17.3 (5.9)	+17.9 (1.8)	+0.6 [−15.7, +20.1]	−0.03
Years with equal fees	2015–19, 2021–23, 2025	2017, 2021	−19.2 (6.7)	+17.2 (1.6)	−2.0 [−20.1, +20.7]	0.10 [−1.95, 1.22]
Years with equal fees, s.e. from other dates	2015–19, 2021–23, 2025	2017, 2021	−19.2 (6.7)	+17.2 (2.3)	−2.0 [−14.2, +12.0]	0.10 [−1.51, 0.45]
Years with equal fees, same years for both	2017, 2021	2017, 2021	−15.0 (5.7)	+17.2 (1.6)	+2.2 [−39.1, +71.6]	−0.13
Years with unequal fees	2015–19, 2021–23, 2025	2022, 2023	−19.2 (6.7)	+21.3 (3.4)	+2.2 [−30.6, +48.9]	−0.10 [−2.55, 2.97]
Years with unequal fees, same years for both	2022, 2023	2022, 2023	−25.1 (5.7)	+21.3 (3.4)	−3.7 [−28.2, +29.1]	0.13
Base period before the announcement (January 8 to February 18)						
Pre-specified reference years [A4; the number and ρ: E]	2015–19, 2021–23, 2025	2017, 2021, 2022, 2023	−21.8 (5.8)	+15.3 (4.6)	−6.3 [−17.7, +6.8]	0.28 [−0.39, 0.94]
Without 2022	2015–19, 2021, 2023, 2025	2017, 2021, 2023	−21.0 (5.8)	+13.4 (2.3)	−7.4 [−18.4, +5.1]	0.34 [−0.39, 0.76]

Notes. Companies incorporated in the before period (March 4 to April 14) and the after period (May 20 to June 30) of each year, as Addendum 4 defines the periods for 2024; standard errors in parentheses and 95% confidence intervals in brackets. None of the entries of the six panels was specified in advance [E] except those marked [A4]. Panel A: the fee for incorporating online and the fee for the confirmation statement in force on the last day for delivering the first and the second statement (a statement is made up to the day before each anniversary of incorporation and must be delivered within fourteen days). The four years above the last row are the reference years of the primary outcome. For 2017 and 2021 the outcome was measured before the statement fee changed. Panel B: the test of Addendum 4 for the primary outcome with other sets of reference years and, in the last two rows, with the six weeks before the announcement as the base period (the note to Table A28 gives its dates in each year). *Registrations* and *share* are the after period less the base period in 2024, less the mean of the same difference in the reference years, in log points; each standard error is computed as in Section 4.4, as the standard deviation over the n reference years multiplied by $\sqrt{1 + 1/n}$, with $n - 1$ degrees of freedom. Where the two sets of years differ, the number of companies and ρ are obtained as in Addendum 4, with intervals from joint draws. The intervals for ρ in Panels B and C are the percentile intervals over the draws retained, those in which registrations decline by at least half a log point. Table A9 reports the sets by test inversion for the rows that the text cites; their lower ends lie further below zero, and their upper ends differ from those shown here by at most 0.05 except where the share has one degree of freedom. Where the same years serve for both, the change in the number of companies is the same contrast computed for the log number, with its own standard error, and ρ is the implied point estimate; in the second row, that change is the direct estimate that Addendum 4 specified. In the first row for the earlier base period, the estimates for registrations and for the share are those of a check that Addendum 4 specified, and the number of companies and ρ are not pre-specified. *Years with equal fees* are 2017 and 2021, and *years with unequal fees* are 2022 and 2023 (Panel A). *S.e. from other dates*: the contrast for the share is computed in each of the two reference years at 36 placebo dates, every seven days from January 28 to September 30. The root mean square of the difference between the two years, divided by $\sqrt{2}$, estimates the standard deviation of one year's contrast, and the standard error is that estimate multiplied by $\sqrt{1.5}$, with as many degrees of freedom as there are placebo dates whose periods do not overlap (three).

Table A36. (continued)

C. Outcomes that do not require survival on the register (reference years 2022 and 2023)

Outcome at the October of the second year	Share in 2024, percent		Change in the share, log points		With the s.e. from other dates		Number, same
	Before	After	Estimate (s.e.)	S.e., other dates	Number, percent [95% CI]	ρ [95% CI]	two years, percent
On the register with non-dormant accounts (the primary outcome)	35.7	42.2	+21.3 (3.4)	2.4	+2.2 [−10.6, +16.7]	−0.10 [−2.17, 0.32]	−3.7
Non-dormant accounts ever on file	39.2	44.9	+22.5 (3.1)	2.2	+3.4 [−9.6, +18.2]	−0.16 [−2.39, 0.28]	−2.5
On the register with accounts of any type	46.4	55.2	+18.3 (2.8)	2.4	−0.9 [−13.3, +13.3]	0.04 [−1.72, 0.42]	−6.6
Accounts of any type ever on file	52.7	59.6	+18.7 (2.3)	2.3	−0.5 [−12.9, +13.8]	0.02 [−1.78, 0.40]	−6.2
On the register with a registered charge	3.9	5.0	+26.6 (3.3)	15.4	+7.7 [−30.1, +65.6]	−0.34 [−2.31, 3.32]	+1.5
A charge ever registered	4.0	5.1	+26.3 (2.9)	15.3	+7.4 [−30.1, +64.9]	−0.32 [−2.28, 3.35]	+1.2
Non-dormant accounts or a charge, ever	39.6	45.5	+22.6 (3.1)	2.2	+3.5 [−9.5, +18.3]	−0.16 [−2.41, 0.28]	−2.5
On the register	47.3	57.0	+18.4 (2.9)	2.6	−0.8 [−13.2, +13.3]	0.04 [−1.66, 0.43]	−6.5
Base period before the announcement							
On the register with non-dormant accounts			+18.3 (4.6)		−3.4	0.14	
Non-dormant accounts ever on file			+17.8 (4.0)		−3.9	0.16	
Both periods measured at equal ages							
Non-dormant accounts ever on file			+22.6 (3.0)				

D. The increase in the statement fee as an event (after period less before period, relative to reference years)

	Reference years	Difference, log points	p
Incorporated in 2023: first statement at £13 (before) or £34 (after)			
A confirmation statement on file at 15 to 19 months	2018, 2021, 2022, 2025	−0.5 (1.9)	0.80
On the register and active at 15 to 19 months	2018, 2021, 2022, 2025	+1.2 (2.0)	0.59
On the register at 27 to 31 months	2017, 2021	−0.7 (0.8)	0.56
Non-dormant accounts on file at 27 to 31 months	2017, 2021	−2.2 (1.6)	0.41
Incorporated in 2022: second statement at £13 (before) or £34 (after)			
A second-year statement on file at 27 to 31 months	2021, 2023	−5.3 (2.6)	0.29
On the register at 27 to 31 months	2017, 2021, 2023	−3.8 (0.7)	0.03
Non-dormant accounts on file at 27 to 31 months	2017, 2021, 2023	−5.4 (1.8)	0.10
On the register at 27 to 31 months	2017, 2021	−4.0 (0.8)	0.13
Non-dormant accounts on file at 27 to 31 months	2017, 2021	−6.1 (1.6)	0.17

Notes. Panel C: outcomes reconstructed from the monthly register files, which exist from October 2023 and therefore permit the reconstruction for the cohorts of 2022, 2023, and 2024; the two reference years are 2022 and 2023. A company is counted as having an outcome *ever on file* if its accounts of the stated type, or its charge, appeared in any register file up to the October of its second year, whether or not the company was later removed from the register. *Estimate (s.e.)*: as in Panel B, with one degree of freedom. *S.e., other dates*: computed as in Panel B from the years 2022 and 2023. *Number, percent* and ρ: the change in the share, with the standard error from other dates, combined with the change in registrations measured against nine reference years. *Number, same two years*: the change in the share combined with the change in registrations measured against 2022 and 2023. The two rows for the base period before the announcement give point estimates, and in the last row each cohort is measured at the same age in months. Panel D: for the cohort of 2023, whose first statement fell due at £13 in the before period and at £34 in the after period, and for the cohort of 2022, whose second statement did, the after period less the before period in the share with each record, less the mean of the same difference in the reference years shown, in log points; standard errors are computed as in Panel B, and p-values use Student's t with degrees of freedom equal to the number of reference years less one.

Table A36. (continued)

E. Step at May 1 in each share, by week of incorporation

Step at May 1 in the share, log points	2017 (no change)	2021 (no change)	2022 (second statement)	2023 (first statement)	2024 (incorporation)
On the register at 27 to 31 months	−3.4 (1.4)	+1.3 (1.9)	−0.2 (2.8)	+0.8 (2.7)	+15.9 (1.7)
Non-dormant accounts on file (primary)	−5.6 (1.9)	+0.5 (1.9)	−1.5 (3.6)	−2.3 (3.6)	+19.3 (2.1)
Non-dormant accounts ever on file			−4.1 (3.2)	−6.3 (3.3)	+19.7 (2.6)
A second-year statement on file		−0.5 (1.7)	−3.8 (3.2)	−3.4 (2.7)	+18.1 (2.0)
A confirmation statement on file at 15 to 19 months		−1.7 (1.4)	−0.7 (2.4)	−1.7 (1.9)	+13.9 (1.6)

F. State of the companies of each period in the October of their second year, and classification of the deterred registrations

	Share of the period's registrations, percent						After period of 2024: share of the registrations, percent			
	2022		2023		2024		Base: before period		Base: before announcement	
State of the company in the October of its second year	Before	After	Before	After	Before	After	Expected	Deterred	Expected	Deterred
On the register, non-dormant accounts on file	40.7	38.1	39.2	38.2	35.7	42.2	33.7	5.5	34.9	10.6
On the register, such accounts on file earlier only	0.2	0.2	0.2	0.2	0.1	0.1	0.1	0.1	0.1	0.3
Left the register after filing such accounts	4.1	2.1	4.3	2.4	3.4	2.6	1.7	−1.1	2.1	0.4
On the register, dormant accounts only	12.2	13.5	11.7	13.2	10.6	13.0	11.7	7.5	11.0	4.2
On the register, a statement and no accounts	1.0	1.5	0.9	1.5	0.7	1.6	1.1	−0.4	1.3	0.5
On the register, nothing on file	0.1	0.2	0.2	0.2	0.2	0.2	0.2	0.3	0.3	0.9
Left the register; a statement or dormant accounts only	10.0	9.6	11.2	9.6	11.6	9.8	10.2	11.6	9.6	8.8
Left the register with nothing on file	30.9	34.9	31.7	34.8	37.6	30.5	41.1	76.4	40.6	74.3
In no register file	0.9	0.1	0.6	0.0	0.0	0.0	0.1	0.1	0.1	0.1
Non-dormant accounts ever on file (first three rows)	45.0	40.4	43.8	40.7	39.2	44.9	35.5	4.5	37.2	11.3
Left the register without such accounts (last three rows)	41.7	44.5	43.4	44.4	49.3	40.4	51.4	88.1	50.2	83.1
Registrations per working day	3,396	3,255	3,661	3,285	4,084	2,948	3,827	879	3,822	874

Notes. Panel E: nine seven-day blocks of dates of incorporation ending on April 16 and nine beginning on May 1 of each year, with the fourteen days between them omitted; the entry is the step at the boundary in a regression of the log share on a linear trend in the date and an indicator for the later blocks, weighted by registrations, with the conventional least-squares standard error. The label under each year states which fee differed across the boundary. Panel F: every registration of a period is assigned to one of nine classes by the state of the company in the October of its second year, and the first six columns give each class as a percentage of the period's registrations. The last four columns concern the after period of 2024. *Expected*: the registrations per working day of each class in the base period of 2024, multiplied by the ratio of the after period to the base period in a reference year and averaged over the reference years 2022 and 2023, as a percentage of the expected registrations of all classes. *Deterred*: the expected less the observed registrations of the class, as a percentage of that difference over all classes. For these four columns, in the comparison with each reference year, the companies of 2024 are classified from the register files for the same months of a company's life for which files exist for that year's cohort, because fewer files exist for the earlier cohorts. The base period is the before period of the test or the six weeks before the announcement. The last row gives registrations per working day and, in the last four columns, the expected registrations and the difference between the expected and the observed registrations.

Table A37. Colorado: The Fee for Financing Statements, and Each Holiday Separately

A. Financing statements and amendments filed in the holiday months, and their timing

	Same months, year before	Holiday months	Same months, year after	Difference, log points
Financing statements filed, first holiday (fee $8 to $4)	9,717	10,427	11,481	−1.3 (6.2)
Financing statements filed, second holiday (fee unchanged)	17,745	17,112	18,180	−4.8 (5.8)
Amendments filed, first holiday (fee $8 to $4)	10,223	9,432	9,065	−2.0 (5.8)
Amendments filed, second holiday (fee unchanged)	12,235	11,899	12,854	−5.3 (5.3)
Ordinary LLCs formed from December to February, percent				
Named in a statement filed in the same three months	0.78	0.70	0.66	
Named in a statement within 90 days of formation	1.49	1.27	1.41	
Named in a statement at any time	8.22	7.11	8.62	

B. Lending records by the date of the financing statement, and by holiday

	Rate, percent	Same months of the adjacent years		Comparison with the flanking months	
		Difference	ρ	Difference	ρ
Both holidays pooled, by the date of the financing statement					
Financing statement at any time [A3]	7.06	−1.25 (0.29)	−0.02 [−0.83, 0.58]	−1.16 (0.34)	0.18 [−0.72, 0.95]
after the first 90 days only	6.28	−1.13 (0.26)	−0.03 [−0.87, 0.57]	−1.16 (0.26)	0.10 [−0.70, 0.74]
filed in 2020 or later only	3.35	−0.42 (0.17)	0.24 [−0.72, 0.98]	−0.54 (0.18)	0.19 [−0.80, 1.04]
Any of the three records [A3]	9.75	−1.72 (0.26)	−0.02 [−0.65, 0.41]	−1.42 (0.31)	0.26 [−0.38, 0.78]
statements after the first 90 days only	9.06	−1.62 (0.22)	−0.03 [−0.63, 0.38]	−1.39 (0.25)	0.23 [−0.36, 0.69]
statements of 2020 or later only	6.70	−1.05 (0.16)	0.08 [−0.47, 0.47]	−0.91 (0.25)	0.31 [−0.43, 0.92]
Second holiday alone (formation fee only)					
PPP loan [A3]	4.38	−0.68 (0.22)	0.21 [−0.55, 0.81]	−0.23 (0.33)	0.73 [−0.59, 1.99]
Either federal loan [A3]	4.82	−0.72 (0.19)	0.23 [−0.43, 0.73]	−0.52 (0.33)	0.49 [−0.68, 1.52]
Financing statement at any time [A3]	7.03	−1.20 (0.39)	0.14 [−0.83, 0.82]	−1.09 (0.49)	0.29 [−0.93, 1.32]
Any of the three records [A3]	9.79	−1.55 (0.36)	0.20 [−0.53, 0.67]	−1.35 (0.44)	0.36 [−0.47, 1.02]
First holiday alone (formation and financing-statement fees)					
PPP loan [A3]	4.62	−0.74 (0.26)	−0.18 [−1.61, 0.75]	−0.04 (0.33)	0.95 [−0.30, 2.19]
Either federal loan [A3]	4.99	−0.95 (0.23)	−0.37 [−1.82, 0.42]	−0.32 (0.32)	0.61 [−0.51, 1.59]
Financing statement at any time [A3]	7.11	−1.31 (0.42)	−0.33 [−2.22, 0.68]	−1.26 (0.46)	0.02 [−1.20, 0.90]
Any of the three records [A3]	9.67	−1.95 (0.39)	−0.44 [−2.03, 0.33]	−1.51 (0.42)	0.11 [−0.78, 0.72]

Notes. None of the entries was specified in advance [E] except in the rows marked [A3], in which the columns for the same months of the adjacent years reproduce estimates of Addendum 3; the columns for the comparison with the flanking months are not pre-specified. Panel A: UCC financing statements and amendments filed in Colorado for all debtors, in the months of each holiday and in the same months one year earlier and one year later. *Difference*: the statistic of equation (4) for the log number, with the standard deviation over unaffected reference runs in parentheses. The first holiday reduced the fee for both filings from $8 to $4; the second did not change it. The lower rows give, for the ordinary LLCs formed in the three months of the first holiday and in the same months of the adjacent years, the percentage named as debtor in a statement filed within those three months, within 90 days of formation, and at any time to October 2026. Panel B: *rate* is the percentage of the companies formed in the holiday months that have the record. *Same months of the adjacent years*: the pre-specified comparison. *Comparison with the flanking months*: the statistic for the holiday months less the statistic for the two flanking months (row 5 of Table A11). *Difference* is in percentage points, with standard errors in parentheses; ρ is equation (5), with 95% confidence intervals from joint draws in brackets. In the first block, both holidays are pooled with weights of three and four months, and the financing statement is counted at any time, only if filed after the company's first 90 days, or only if filed in 2020 or later; any of the three records is formed with each version. The second and third blocks report each holiday separately for four of the six outcomes of Addendum 3.

Table A38. Colorado: Record Linkage, Samples, and Address Keys

A. Characteristics of the links to PPP loans and to financing statements, by cohort

	Holiday months	Same months of adjacent years	Flanking months	Other months of 2010–18
Ordinary LLCs	45,652	78,005	20,455	504,560
Linked to a PPP loan, percent	4.48	5.20	4.84	5.36
Named in a financing statement, percent	7.06	8.31	8.35	7.59
Of the links to PPP loans, percent:				
Full name identical	85.9	85.3	84.5	85.2
Same ZIP code	69.9	69.7	67.4	68.1
Same city	75.8	76.5	73.3	74.7
Only one candidate company	84.7	85.8	85.1	85.5
Decided by the date of formation	2.6	2.6	2.8	2.9
Borrower reports that it is an LLC	69.3	69.0	70.8	71.6
Of the companies named in a financing statement, percent:				
Linked by the registry's identification number	17.6	15.9	19.7	17.8
Linked by that number or by a name that only one company had	95.8	95.3	96.2	95.8
Linked only through the date of formation	4.2	4.7	3.8	4.2
Of the companies, percent:				
Name changed after March 2020	2.004	2.005	2.004	2.469
Principal or mailing address in another state	7.64	7.93	8.19	7.91
PPP loan to a borrower in another state, linked by address	0.35	0.41	0.35	0.37
A filing to 2021 by which the company changed its agent	17.5	19.2	18.7	16.9
A filing to 2021 that changed the principal address	31.0	33.0	33.1	30.0

Table A38. (continued)

B. Estimates under other linking rules and in other samples, both holidays pooled

	Rate, percent	Same months of the adjacent years		Comparison with the flanking months	
		Difference	ρ	Difference	ρ
PPP loan under other linking rules					
Rule of Addendum 3 [A3]	4.48	−0.71 (0.17)	0.07 [−0.57, 0.60]	−0.15 (0.24)	0.81 [−0.19, 1.78]
Links with only one candidate company	3.80	−0.61 (0.17)	0.06 [−0.75, 0.70]	−0.24 (0.22)	0.66 [−0.41, 1.64]
Links not decided by the date of formation	4.36	−0.69 (0.17)	0.07 [−0.60, 0.62]	−0.14 (0.24)	0.82 [−0.22, 1.82]
Full name identical and same ZIP code [A3]	2.81	−0.41 (0.15)	0.13 [−0.76, 0.89]	+0.04 (0.20)	1.07 [−0.31, 2.47]
Ties to the earliest-formed company	4.47	−0.66 (0.16)	0.13 [−0.48, 0.65]	−0.13 (0.23)	0.84 [−0.13, 1.78]
Every company tied in the first rank	4.65	−0.66 (0.16)	0.15 [−0.42, 0.63]	−0.11 (0.23)	0.87 [−0.08, 1.80]
Every candidate company	5.72	−0.66 (0.18)	0.30 [−0.23, 0.74]	+0.06 (0.26)	1.06 [0.19, 1.93]
Names also matched without spaces	4.51	−0.71 (0.17)	0.08 [−0.56, 0.60]	−0.16 (0.23)	0.80 [−0.18, 1.74]
HTML entities in borrowers' names decoded	4.49	−0.71 (0.17)	0.07 [−0.58, 0.60]	−0.18 (0.24)	0.78 [−0.23, 1.75]
Links corrected by the review of record pairs (Panel D)	4.57	−0.65 (0.17)	0.16 [−0.47, 0.68]	−0.14 (0.24)	0.83 [−0.15, 1.77]
First set of readers	4.55	−0.66 (0.17)	0.15 [−0.49, 0.66]	−0.09 (0.24)	0.89 [−0.10, 1.87]
Second set of readers	4.59	−0.64 (0.17)	0.17 [−0.47, 0.69]	−0.19 (0.23)	0.77 [−0.19, 1.69]
Financing statement under other linking rules					
Rule of Addenda 2 and 3 [A3]	7.06	−1.25 (0.29)	−0.02 [−0.83, 0.58]	−1.16 (0.34)	0.18 [−0.72, 0.95]
Links by number or by a name that only one company had	6.76	−1.16 (0.30)	0.01 [−0.85, 0.64]	−1.12 (0.35)	0.18 [−0.77, 0.98]
Ties to the earliest-formed company	6.95	−1.20 (0.30)	0.00 [−0.84, 0.62]	−1.12 (0.35)	0.20 [−0.73, 0.99]
Every company that had the name	7.34	−1.30 (0.28)	−0.02 [−0.80, 0.55]	−1.18 (0.35)	0.20 [−0.67, 0.95]
Any of the three records, with the financing statement linked under each rule					
Rule of Addenda 2 and 3 [A3]	9.75	−1.72 (0.26)	−0.02 [−0.65, 0.41]	−1.42 (0.31)	0.26 [−0.38, 0.78]
Links by number or by a name that only one company had	9.52	−1.66 (0.28)	−0.01 [−0.66, 0.45]	−1.38 (0.32)	0.27 [−0.40, 0.80]
Ties to the earliest-formed company	9.70	−1.71 (0.28)	−0.02 [−0.67, 0.44]	−1.38 (0.32)	0.28 [−0.39, 0.82]
Every company that had the name	10.00	−1.78 (0.26)	−0.03 [−0.64, 0.40]	−1.44 (0.31)	0.27 [−0.35, 0.77]
Loans to borrowers in every state, linked by address					
PPP loan	4.82	−0.75 (0.15)	0.09 [−0.50, 0.55]	−0.17 (0.25)	0.81 [−0.17, 1.74]
Either federal loan	5.24	−0.87 (0.14)	0.03 [−0.52, 0.43]	−0.47 (0.23)	0.53 [−0.30, 1.28]
Any of the three records	10.05	−1.76 (0.26)	−0.01 [−0.63, 0.41]	−1.43 (0.29)	0.28 [−0.32, 0.75]
Other samples: PPP loan					
Every domestic LLC, bulk filings included	4.43	−0.75 (0.15)	0.07 [−0.56, 0.54]	−0.21 (0.22)	0.75 [−0.13, 1.59]
Bulk rule at 25 filings	4.49	−0.70 (0.17)	0.06 [−0.65, 0.61]	−0.15 (0.24)	0.81 [−0.20, 1.79]
Bulk rule at 100 filings	4.46	−0.72 (0.16)	0.07 [−0.57, 0.58]	−0.19 (0.24)	0.78 [−0.18, 1.71]
Bulk rule with street-level address keys	4.48	−0.71 (0.17)	0.07 [−0.57, 0.60]	−0.15 (0.24)	0.81 [−0.19, 1.79]
Bulk rule with full address lines	4.48	−0.71 (0.17)	0.07 [−0.57, 0.60]	−0.15 (0.24)	0.82 [−0.18, 1.79]
Profit corporations	10.91	−1.01 (0.26)	0.43 [−0.64, 0.80]	−1.47 (0.79)	0.36 [−0.98, 1.46]
Ordinary LLCs and profit corporations	5.32	−0.75 (0.14)	0.17 [−0.38, 0.57]	−0.32 (0.22)	0.68 [−0.10, 1.40]
Other samples: any of the three records					
Every domestic LLC, bulk filings included	9.65	−1.81 (0.32)	−0.02 [−0.52, 0.40]	−1.56 (0.37)	0.25 [−0.47, 0.81]
Bulk rule at 25 filings	9.75	−1.72 (0.28)	−0.05 [−0.77, 0.43]	−1.42 (0.31)	0.25 [−0.41, 0.78]
Bulk rule at 100 filings	9.71	−1.75 (0.27)	−0.02 [−0.61, 0.41]	−1.50 (0.32)	0.25 [−0.37, 0.76]
Bulk rule with street-level address keys	9.75	−1.72 (0.26)	−0.02 [−0.65, 0.41]	−1.42 (0.31)	0.26 [−0.37, 0.78]
Bulk rule with full address lines	9.75	−1.72 (0.26)	−0.02 [−0.65, 0.41]	−1.42 (0.31)	0.27 [−0.37, 0.77]
Profit corporations	17.99	−1.75 (0.57)	0.41 [−0.22, 0.81]	−2.89 (1.07)	0.26 [−0.90, 1.11]
Ordinary LLCs and profit corporations	10.83	−1.73 (0.24)	0.07 [−0.55, 0.46]	−1.60 (0.30)	0.26 [−0.30, 0.70]

Table A38. (continued)

C. Type of filer under three address keys, both holidays pooled

Type of filer	Address key	Companies in the holiday months	Registrations, log points	PPP loan, pct. points	Any record, pct. points
Formation service or other organization as agent	Street number and ZIP code [A3]	7,090	+14.7 (3.4)	−0.42 (0.29)	−2.13 (0.64)
	With the street name	7,090	+14.7 (3.4)	−0.42 (0.29)	−2.13 (0.64)
	Full address lines	7,090	+14.7 (3.4)	−0.42 (0.29)	−2.13 (0.64)
Repeat filer	Street number and ZIP code [A3]	13,202	+23.4 (1.8)	−0.79 (0.28)	−1.97 (0.34)
	With the street name	10,667	+27.0 (1.5)	−0.67 (0.32)	−2.05 (0.49)
	Full address lines	7,197	+36.1 (2.0)	−0.57 (0.44)	−2.27 (0.57)
New filer	Street number and ZIP code [A3]	23,943	+11.9 (1.6)	−0.72 (0.17)	−1.45 (0.31)
	With the street name	26,675	+11.8 (1.5)	−0.75 (0.19)	−1.45 (0.26)
	Full address lines	30,734	+12.2 (1.5)	−0.72 (0.16)	−1.45 (0.24)
Address with 20 or more in the previous year	Street number and ZIP code	949	+43.2 (6.0)	−1.87 (0.94)	−4.39 (1.57)
	With the street name	749	+44.4 (9.8)	−2.36 (0.98)	−6.21 (1.77)
	Full address lines	138	+44.2 (15.4)	0.00 (1.04)	−13.05 (4.25)

Notes. None of the entries was specified in advance [E] except the rows marked [A3]. Panel A: ordinary LLCs formed in the holiday months, in the same months of the two adjacent years, in the four flanking months, and in the other months of 2010 to 2018. A loan is linked to at most one company by the rule of Appendix C: among companies with an identical short name, a company's own name is preferred to a trade name, then an identical full name, then the same ZIP code, then the most recently formed company, and a loan whose candidates remain tied is not linked. The second block gives, among the links of each cohort to PPP loans, the percentage with each characteristic; the borrower's city and stated legal form are not used by the rule. A financing statement is linked to the company whose identification number the debtor's record carries or, failing that, to the company whose normalized name equals the debtor's, and among several such companies to the most recently formed. The third block gives, among the companies of each cohort named in a statement, the percentage with a link by the number, with a link by the number or by a name that only one company had on the day of the statement, and with no link other than those decided by the date of formation. The fourth block gives percentages of the companies of each cohort; the percentage whose name changed after March 2020 is given to three decimal places because three of the cohorts differ only in the third. A change of name is recorded by an amendment; a change of agent or of principal address is recorded in the remarks of filings received through 2021, after which the registry's report form no longer distinguishes them. Panel B: columns as in Table A37, Panel B. The first block changes the rule that links loans: only links with a single candidate company; only links not decided by the date of formation; only links with an identical full name and ZIP code, a version that Addendum 3 specified; ties decided for the company formed first; every company tied in the first rank of a loan's candidates; every candidate company of any loan; names also matched after spaces are removed; names matched after the HTML entities that the loan file contains in place of some characters are decoded; and the number of companies with a linked loan in each month corrected for the links that the readers of Panel D judged wrong and for the links that they judged missed in the month's cohort, with the judgments of both sets of readers and of each set alone. The second and third blocks change the rule that links financing statements, for the statement and for any of the three records formed with it: only links by the number or by a name that only one company had; ties decided for the company formed first; and every company that had the name. The fourth block adds loans to borrowers outside Colorado when the borrower's ZIP code or state agrees with an address that the company reports to the registry. The last two blocks change the sample: every domestic LLC, which uses no filer key; the rule for bulk filings at thresholds of 25 and 100 in place of 50, and with the two finer address keys; profit corporations, excluding bulk filings; and ordinary LLCs and profit corporations together. For the samples that include corporations, the marginal share is that of the same sample. Panel C: types of filer as defined in Section 4.2, formed with the address key of the locked scripts (street number and ZIP code), with a key that adds the first word of the street name, and with the full normalized address lines. *Registrations*: the statistic of equation (4) for the log number of registrations in the holiday months. *PPP loan* and *any record*: the same statistic for the share with the record. Standard errors in parentheses are standard deviations over unaffected reference runs.

Table A38. (continued)

D. Review of a sample of record pairs by AI readers

	Holiday months	Same months of adjacent years	Flanking months	Other months of 2010–18
Links to PPP loans that the rule made				
Companies with a linked loan	2,044	4,055	989	27,027
Pairs read	125	125	85	65
Judged the same company, percent (s.e.)	98.0 (0.8)	97.4 (1.0)	97.1 (1.2)	96.3 (1.8)
Judged another company, percent	1.2	1.2	0.7	1.6
Links that the rule did not make				
Companies without a linked loan whose name is close to an unlinked borrower's	145	240	72	1,609
Pairs read	145	240	72	60
Judged the same company, percent	58.6	56.2	53.5	60.6
Links missed per 100 links made	4.2	3.3	3.9	3.6
Companies with a linked PPP loan, percent				
As measured	4.48	5.20	4.84	5.36
Corrected for links judged wrong and links judged missed (s.e. of the review)	4.57 (0.03)	5.23 (0.05)	4.88 (0.06)	5.35 (0.10)
Financing statements linked by name				
Companies that statements name by name only	2,659	5,449	1,371	31,512
Pairs read	40	40	40	40
Judged the same company, percent	95.0	98.8	97.5	98.8
Judged another company, percent	0.0	0.0	0.0	0.0

Notes. Panel D: none of the entries was specified in advance [E]. The pairs were read by two sets of four AI readers, each pair by one reader of each set; no person read the pairs. A reader saw the names and addresses in the two records, the legal form and the age that a borrower stated, and an industry code, and did not see the date on which the company was formed, its status, or its cohort. A reader assigned each pair a value from 1 (the same company, with confidence) to 5 (not the same company, with confidence). A pair is counted as judged the same company at values of 1 and 2 and as judged another company at values of 4 and 5, and the percentages are means over the two sets of readers. *Links to PPP loans that the rule made*: companies drawn at random within two strata, links with an identical full name and the same ZIP code and all other links, and weighted to all links of the cohort; standard errors in parentheses. *Links that the rule did not make*: companies without a linked loan whose name is the closest to that of a Colorado borrower whose loan the rule left unlinked, without being equal to it. All such pairs were read for the first three cohorts and a random sample for the fourth. *Links missed per 100 links made*: the number of these companies judged to be the borrower, per hundred links that the rule made in the cohort. *Corrected*: the measured percentage multiplied by the sum of the share of the cohort's links judged the same company and the number of links judged missed per link made; the standard error reflects the sampling of pairs only. *Financing statements linked by name*: companies that no statement names by the registry's identification number, drawn at random. A further 100 pairs with a known answer, drawn from financing statements that carry the registry's identification number, were mixed with the others. The readers judged 91% of the pairs in which the debtor is the company to be the same company and 89% of the pairs in which the debtor is another company to be different companies, and the two sets of readers agreed on 98% of all pairs.

Table A39. Colorado: Checks of the Inference, and the Weight of the Flanking Months

A. Standard errors, sampling error, and a joint placebo over pairs of reference runs (percentage points)

	Holiday months, pooled			Single months		Pairs of reference runs		
	S.e., reference runs	S.e., sampling alone	Estimate over sampling s.e.	S.d.	Variance beyond sampling, %	S.d.	Largest in absolute value	As far from zero as estimate
PPP loan	0.17	0.13	5.4	0.36	4	0.16	0.45	0 of 567
Either federal loan	0.15	0.14	6.0	0.34	0	0.14	0.37	0 of 567
Financing statement	0.29	0.16	7.7	0.53	34	0.27	0.70	0 of 567
Any of the three records	0.26	0.19	9.2	0.54	15	0.25	0.61	0 of 567

B. Size of the test in simulated data in which no holiday has an effect

Simulated months		Rejections at the 5% level, percent			S.e. over	Degrees of freedom left		Critical value
Variability	Persistence	First holiday	Second holiday	Pooled	s.d., pooled	First (10 used)	Second (7 used)	needed, pooled
1.00	0.0	5.5	4.2	4.1	0.99	11.2	8.4	2.26
1.25	0.0	5.5	3.9	3.9	1.00	11.1	8.1	2.23
1.25	0.4	5.7	4.2	4.2	0.99	9.9	7.3	2.26
1.25	0.8	5.9	5.1	4.6	0.99	8.4	6.2	2.31
1.50	0.0	5.5	4.0	4.0	0.99	10.6	7.7	2.25
1.50	0.4	5.7	4.2	4.5	0.98	9.2	6.8	2.30
1.50	0.8	6.1	5.2	4.7	1.02	7.2	5.4	2.32
2.00	0.0	5.1	3.9	3.8	1.00	10.7	7.8	2.21
2.00	0.4	6.3	4.5	4.6	0.99	8.3	6.2	2.32
2.00	0.8	7.4	5.7	5.6	0.99	6.0	4.7	2.44

Table A39. (continued)

C. Coverage of the confidence interval for ρ in simulated data

Simulated record	True ρ	Coverage of the 95% interval, percent	Median estimate	Interval wholly below one, percent	Interval wholly above zero, percent
Rate of the PPP loan, sampling variation only	0.0	97.7	0.02	100	1
Rate of the PPP loan, sampling variation only	0.5	97.1	0.54	67	60
Rate of the PPP loan, sampling variation only	1.0	96.4	1.03	1	98
Rate of any record, variability 1.41 times sampling, persistence 0.4	0.0	96.7	0.04	100	2
Rate of any record, variability 1.41 times sampling, persistence 0.4	0.5	94.5	0.55	70	65
Rate of any record, variability 1.41 times sampling, persistence 0.4	1.0	94.1	1.04	1	99

D. Outer and inner months of blocks of unaffected months

	Slope of the inner months on the outer months						S.d., blocks of 3 months		S.d., blocks of 4 months	
	Over the blocks				From single months		Inner months	Less the outer months	Inner months	Less the outer months
	Blocks of 3	Blocks of 4	Pooled	p, slope of one	Blocks of 3	Blocks of 4				
PPP loan	0.13 (0.44)	−0.02 (0.50)	0.06 (0.40)	0.058	0.09	0.04	0.25	0.33	0.20	0.33
Either federal loan	0.03 (0.43)	−0.09 (0.45)	−0.03 (0.38)	0.035	0.00	−0.04	0.23	0.32	0.18	0.33
Financing statement	0.45 (0.43)	0.29 (0.53)	0.37 (0.40)	0.170	0.33	0.25	0.45	0.46	0.41	0.49
Any of the three records	0.42 (0.43)	0.23 (0.51)	0.32 (0.40)	0.142	0.42	0.23	0.40	0.42	0.35	0.44
Registrations (log)	0.15 (0.28)	0.15 (0.32)	0.15 (0.26)	0.016			1.43	2.80	1.15	2.59

Notes. None of the entries was specified in advance [E]. Panel A: *S.e., reference runs* is the pooled standard error of the pre-specified test, the standard deviation of the statistic over unaffected reference runs. *S.e., sampling alone* is the standard error implied by binomial sampling of each month's companies around a smooth rate. *Single months*: the standard deviation of the statistic of equation (4) over 38 months that no holiday affects directly or through their reference months, and the share of its variance in excess of sampling variance. For the PPP loan the autocorrelation of the single-month statistic is 0.41 at a lag of one month (34 pairs) and between −0.21 and 0.19 at lags of two to five months. *Pairs of reference runs*: every pair of a three-month and a four-month reference run whose months and reference months are disjoint, weighted as the two holidays are pooled; the standard deviation of the pooled statistic over pairs, its largest absolute value, and the number of pairs with a value at least as far from zero as the estimate for the holidays. Panel B: 20,000 simulated replications of the PPP loan for the months of 2010 to 2018, with the actual number of companies in each month, a smooth rate fitted to the unaffected months, binomial sampling, and a disturbance to each month's rate that follows a first-order autoregression. *Variability* is the standard deviation of the single-month statistic relative to that under sampling alone, and *persistence* is the autoregressive coefficient. The complete procedure of Addendum 3 is applied to each replication. *Rejections*: the percentage of replications in which the test of nominal size 5% rejects. *S.e. over s.d.*: the mean standard error divided by the standard deviation of the estimate. *Degrees of freedom left*: the degrees of freedom implied by the variability of the squared standard error, compared with those that the procedure uses. *Critical value needed*: the critical value at which the pooled test would reject in 5% of replications; the procedure uses 2.36. Panel C: 1,000 simulated replications in which registrations are also simulated and each holiday adds registrations that have the record at ρ times the rate of the others; the confidence interval for ρ is computed as in Addendum 3. Panel D: blocks of unaffected months with the structure of the two holidays, each with two outer months and three or four inner months (24 and 21 blocks). *Over the blocks*: the slope through the origin of the statistic for the inner months on the statistic for the two outer months, for each block length and, pooled, over the blocks of both lengths together, with standard errors in parentheses. The blocks overlap, and each standard error rests on the number of blocks that do not: 7 and 4 for the two lengths, and 7 for the two together, because the blocks of both lengths are cut from the same months. *p, slope of one*: the test that the pooled slope equals one, with 6 degrees of freedom. *From single months*: the slope implied by the autocorrelations of the single-month statistic at lags of one to five months. The last four columns give the standard deviation over blocks of the statistic for the inner months and of that statistic less the statistic for the outer months, in percentage points (log points for registrations). Subtracting the statistic for the outer months raises the variance. With the months of 2019 admitted, the blocks of 2018 enter (36 and 33 blocks, of which 9 do not overlap), and the pooled slope for the PPP loan is 0.50 (0.26). The companies formed in 2019, which then serve as reference cohorts, have a loan rate of 4.7%, compared with 5.9% for those formed in 2018. The single-month statistic therefore averages +0.65 percentage points over the months of 2018, compared with +0.03 over the other unaffected months, and a slope through the origin attributes this displacement, which is common to every month of 2018, to the outer months.

Table A39. (continued)

E. The flanking months among unaffected blocks, and ρ by the weight on the flanking months

	Statistic for the flanking months, percentage points				ρ with the weight equal to the slope in unaffected years				
	Outer months of pairs of unaffected blocks					95% confidence interval			Draws below
	Estimate	S.d.	Range	As far from zero	Estimate	Slope fixed	Slope drawn	Weights in [0, 1]	one, percent
PPP loan	−0.56	0.16	−0.42 to +0.46	0 of 239	0.13	[−0.57, 0.78]	[−0.83, 1.15]	[−0.54, 1.12]	96.1
Either federal loan	−0.39	0.15	−0.45 to +0.41	4 of 239	0.04	[−0.54, 0.56]	[−0.72, 0.75]	[−0.51, 0.76]	98.9
Financing statement	−0.09	0.26	−0.81 to +0.57	181 of 239	0.03	[−0.75, 0.76]	[−0.88, 0.85]	[−0.80, 0.81]	98.3
Any of the three records	−0.31	0.23	−0.73 to +0.48	52 of 239	0.05	[−0.47, 0.52]	[−0.58, 0.61]	[−0.49, 0.58]	99.4

	ρ at each weight on the flanking months, with 95% confidence intervals			
	0.25	0.50	0.75	1
PPP loan	0.27 [−0.46, 0.95]	0.46 [−0.33, 1.19]	0.64 [−0.24, 1.48]	0.81 [−0.19, 1.78]
Either federal loan	0.16 [−0.46, 0.72]	0.29 [−0.39, 0.92]	0.41 [−0.36, 1.12]	0.53 [−0.35, 1.32]
Financing statement	0.04 [−0.74, 0.75]	0.09 [−0.69, 0.79]	0.14 [−0.68, 0.86]	0.18 [−0.72, 0.95]
Any of the three records	0.06 [−0.46, 0.52]	0.13 [−0.39, 0.59]	0.20 [−0.38, 0.68]	0.26 [−0.38, 0.78]

F. The pre-specified test at the critical values that the simulations require

		Estimate (s.e.), pct. points	Critical value		95% CI at the simulations' value	p-value		ρ [95% CI]	
			Addendum 3	Simulations		Addendum 3	Simulations	Addendum 3 [A3]	Simulations' critical value
PPP loan	Pooled	−0.71 (0.17)	2.36	2.44	[−1.11, −0.30]	0.004	0.005	0.07 [−0.57, 0.60]	0.07 [−0.60, 0.61]
	First holiday	−0.74 (0.26)	2.23	2.50	[−1.39, −0.09]	0.017	0.031	−0.18 [−1.61, 0.75]	−0.18 [−1.86, 0.84]
	Second holiday	−0.68 (0.22)	2.36	2.47	[−1.22, −0.15]	0.016	0.021	0.21 [−0.55, 0.81]	0.21 [−0.59, 0.83]
Either federal loan	Pooled	−0.82 (0.15)	2.36	2.44	[−1.18, −0.46]	0.001	0.001	0.02 [−0.56, 0.47]	0.02 [−0.59, 0.48]
	First holiday	−0.95 (0.23)	2.23	2.50	[−1.53, −0.38]	0.002	0.007	−0.37 [−1.82, 0.42]	−0.37 [−2.05, 0.50]
	Second holiday	−0.72 (0.19)	2.36	2.47	[−1.19, −0.25]	0.007	0.010	0.23 [−0.43, 0.73]	0.23 [−0.48, 0.75]
Financing statement	Pooled	−1.25 (0.29)	2.36	2.44	[−1.95, −0.54]	0.003	0.005	−0.02 [−0.83, 0.58]	−0.02 [−0.86, 0.59]
	First holiday	−1.31 (0.42)	2.23	2.50	[−2.36, −0.25]	0.012	0.024	−0.33 [−2.22, 0.68]	−0.33 [−2.51, 0.78]
	Second holiday	−1.20 (0.39)	2.36	2.47	[−2.17, −0.24]	0.018	0.023	0.14 [−0.83, 0.82]	0.14 [−0.88, 0.85]
Any of the three records	Pooled	−1.72 (0.26)	2.36	2.44	[−2.36, −1.08]	<0.001	0.001	−0.02 [−0.65, 0.41]	−0.02 [−0.67, 0.43]
	First holiday	−1.95 (0.39)	2.23	2.50	[−2.92, −0.99]	0.001	0.003	−0.44 [−2.03, 0.33]	−0.44 [−2.32, 0.40]
	Second holiday	−1.55 (0.36)	2.36	2.47	[−2.43, −0.67]	0.003	0.005	0.20 [−0.53, 0.67]	0.20 [−0.57, 0.69]

Notes. None of the entries was specified in advance [E] except the column marked [A3]. Panel E: *Statistic for the flanking months*: the statistic of equation (4) for the two months that flank each holiday, pooled with weights of three and four months, in percentage points. The next three columns describe the same statistic for the outer months of 239 pairs of unaffected blocks, one of each length, whose months and reference months are disjoint: its standard deviation, its range, and the number of pairs in which it is at least as far from zero as the estimate. *ρ with the weight equal to the slope in unaffected years*: the statistic for the flanking months is multiplied by the pooled slope of Panel D and subtracted from the statistic for the holiday months, for the share and for registrations. *Slope fixed*: the confidence interval when the two slopes are taken as known. *Slope drawn*: the two slopes are drawn from Student's t distribution around their estimates, with the degrees of freedom of their standard errors (6), independently of each other and of the errors of the statistics for the holiday months. *Weights in [0, 1]*: the same, with a slope drawn outside the interval from zero to one set to the nearer end. *Draws below one*: the percentage of the joint draws, with the slopes drawn, in which ρ is below one. The lower block gives ρ at four fixed weights. Panel F: the pre-specified test of Addendum 3 and the same test with the largest critical value that the simulations of Panel B require, for the pooled statistic and for each holiday. *95% CI at the simulations' value*: the confidence interval for the difference, in percentage points. The last column gives the confidence interval for ρ from joint draws with the degrees of freedom that correspond to that critical value.

Table A39. (continued)

G. The months adjacent to the two windows

	Statistic, percentage points (binomial s.e.)				S.d. over	Months below their reference months			Three to five months beyond	
	Flanking months	One month beyond	Two months beyond	Flanking less one month beyond	placements (d.f.)	Holiday months	Flanking months	Adjacent months	June to August 2012	February to April 2015
PPP loan	−0.56 (0.20)	−0.04 (0.18)	+0.26 (0.18)	−0.52 (0.27)	0.15 (2)	7 of 7	4 of 4	1 of 8	−0.59 (0.22)	−0.07 (0.20)
Either federal loan	−0.39 (0.20)	−0.15 (0.19)	+0.23 (0.19)	−0.23 (0.28)	0.18 (2)	7 of 7	4 of 4	2 of 8	−0.49 (0.23)	−0.04 (0.21)
Financing statement	−0.09 (0.25)	−0.24 (0.22)	+0.47 (0.22)	+0.15 (0.33)	0.27 (2)	7 of 7	3 of 4	4 of 8	−0.38 (0.28)	+0.12 (0.24)
Any of the three records	−0.31 (0.28)	−0.31 (0.26)	+0.32 (0.26)	0.00 (0.39)	0.30 (2)	7 of 7	3 of 4	4 of 8	−0.59 (0.32)	+0.08 (0.28)

Notes. Panel G: the statistic of equation (4) for the share with each record among the companies formed in three sets of months, pooled over the two holidays with weights of three and four months. *Flanking months*: the month in which each holiday was announced and the month after it ended (November 2012, March 2013, June 2014, and November 2014). *One month beyond*: the month before the month of the announcement and the second month after the end (October 2012, April 2013, May 2014, and December 2014). *Two months beyond*: the months one further out (September 2012, May 2013, April 2014, and January 2015). Standard errors in parentheses are those that binomial sampling implies, at the observed shares, for the months and their reference months. In the first and fourth columns they include the covariance that arises because November 2013 is a reference month of the flanking months of both holidays. For the PPP loan, the difference of the fourth column is −0.88 percentage points for the first holiday and −0.24 for the second. *S.d. over placements*: the standard deviation of the difference of the preceding column over the placements of the same four months in which no month and no reference month is affected by a holiday, with the number of placements that do not overlap, less one, as degrees of freedom. *Months below their reference months*: the number of single months with a negative statistic among the seven holiday months, the four flanking months, and the eight months of the second and third columns (the adjacent months). One of the two reference months of April 2014 and of May 2014 is the same month of 2013, which is an adjacent month of the first holiday. *Three to five months beyond*: the statistic for the three months that lie three to five months beyond the flanking months before the first holiday and after the second. The corresponding months after the first holiday and before the second have a reference month within the window of the other holiday, which raises their statistic by half of that holiday's deficit, and they are not shown. The panel was tabulated on October 6, 2026, after every other result was known [E]. Addendum 3 had specified the statistic for each of the four flanking months, from which the first column and the count for the flanking months derive, and for the months that follow the month after each holiday (April and May 2013; December 2014 to March 2015), which include four of the eight adjacent months (Table A20) [A3]. The other entries were not specified in advance.

E.7. Registration Maintenance: Registry Outcomes of Companies Formed in the Fee Holidays

Table A40 reports the estimates summarized in Section 6.5: the first report with and without the adjustment for established companies, on which the estimate depends, and the other measures of registration maintenance. Table A41 reports the registry-based outcomes of the companies formed in the two fee holidays, as specified in Addendum 2 and as computed by its locked script.

Table A40. Colorado: Registration Maintenance among Companies Formed in the Two Fee Holidays

	Change in the share, percentage points			Marginal relative
	First holiday	Second holiday	Pooled	to inframarginal, ρ
First periodic report filed on time				
Minus the same rate among established companies [A2]	−3.32 (1.08)	−3.78 (0.93)	−3.58 (0.70)	0.44 [−0.10, 0.74]
Established companies at their estimated weight [E]	−3.05 (1.11)	−3.51 (0.98)	−3.31 (0.73)	0.48 [−0.05, 0.79]
No adjustment [A2; ρ: E]	−2.01 (1.41)	−2.49 (1.33)	−2.28 (0.97)	0.63 [0.08, 1.04]
Other measures, no adjustment				
First report filed before delinquency [A2; ρ: E]	−3.27 (1.11)	−3.27 (1.04)	−3.27 (0.76)	0.58 [0.14, 0.84]
Second report filed on time [E]	−2.80 (0.97)	−3.29 (0.91)	−3.08 (0.67)	0.41 [−0.17, 0.75]
In good standing, October 2026 [A2; ρ: E]	−1.41 (0.40)	−2.52 (0.32)	−2.05 (0.25)	0.35 [−0.19, 0.61]
minus the mean of the reference months [E]	−1.19 (0.40)	−2.33 (0.32)	−1.84 (0.25)	0.41 [−0.09, 0.66]

Notes. Ordinary LLCs formed from June 2011 to December 2018, by month of formation; holiday months as in Table 3. Entries are T_r of equation (4) in percentage points, with the standard deviation of the same statistic over unaffected reference runs in parentheses. *Marginal relative to inframarginal,* ρ: equation (5), pooled, with the marginal share net of the flanking months of each holiday (0.13), as specified in Addendum 2; with the share counted in the holiday months, as in Table 3, the first row gives 0.50 [0.16, 0.76]. A report is on time if received within two months of the anniversary month, and before delinquency if received within four. Row 1 subtracts the rate at which LLCs three or more years old that had filed their previous report on time filed the report due in the same month; row 2 subtracts 0.79 of that rate; row 3 subtracts nothing. *Second report*: the second report alone; in the second draft, the variable printed under this label also required the first report. Good standing has a mean of −0.2 percentage points over reference runs, which the last row removes. [A2]: specified in Addendum 2. [A2; ρ: E]: Addendum 2 specified the estimate and not ρ, which it defined for the primary outcome only. [E]: not pre-specified; computed after the second internal review.

Adjustment for established companies. In the months in which the first reports of the holiday cohorts fell due, established companies filed on time 1.3 and 1.3 percentage points more often than in the adjacent years (s.e. 0.8 and 0.7). Subtracting their rate therefore increases the estimated deficit from 2.3 percentage points (1.0) to 3.6. In months unaffected by a holiday, the rate among new companies changes by 0.79 of a percentage point (s.e. 0.10) for each percentage point among established companies. Other sets of reference runs yield a pooled standard error between 0.58 and 0.95 percentage points, compared with 0.70 for the runs that the addendum's rule admits, and for the widest set the confidence interval for the pre-specified ρ runs from −0.18 to 0.83 [E].

Two additional patterns. The deficit in reports is confined to companies formed after the first month of each holiday (later months minus the first: −3.4 percentage points, s.e. 0.9), whereas the loan records show no such pattern (Table A21). Among filers observed 13 to 36 months before a holiday, the within-filer difference between companies formed at $1 and those formed in the adjacent years is +0.9 percentage points (s.e. 1.7) when filers are identified by the agent's name and −0.9 (0.8) when they are identified by address, whereas the comparison across the cohorts of these filers gives −3.4 and −3.1 [E].

Table A41. Subsequent Outcomes of Companies Formed in the Two Fee Holidays

A. First report on time, net of established companies, by period of formation

Companies formed in	First holiday	Second holiday	Pooled
The announcement month	−2.0 (1.4)	−1.6 (1.4)	
The holiday months, at $1	−3.3 (1.1)	−3.8 (0.9)	−3.6 (0.7)
The month after	−2.3 (1.4)	+0.5 (1.4)	
The later months	+1.0 (1.2)	+1.0 (0.9)	

B. Alternative specifications and other outcomes

Companies formed in the holiday months	First holiday	Second holiday	Pooled	ρ, pooled [95% CI]
First report on time, net of established companies	−3.3 (1.1)	−3.8 (0.9)	−3.6 (0.7)	0.44 [−0.10, 0.74]
relative to the year after only	−2.6 (1.5)	−3.2 (1.2)		
regression with cohort-year and month effects	−2.7 (1.5)	−4.7 (1.5)		
without the adjustment for established companies	−2.0 (1.4)	−2.5 (1.3)	−2.3 (1.0)	
First report before delinquency, net	−4.1 (1.0)	−3.9 (0.9)	−4.0 (0.7)	0.49 [0.03, 0.74]
First and second reports on time‡	−1.9 (0.6)	−2.5 (0.5)	−2.3 (0.4)	0.49 [0.03, 0.74]
Third report before delinquency, after two on time‡	−2.7 (0.6)	−3.0 (0.5)	−2.9 (0.4)	0.37 [−0.17, 0.65]
In good standing, October 2026	−1.4 (0.4)	−2.5 (0.3)	−2.0 (0.3)†	0.35 [−0.19, 0.61]
Secured credit within 24 months	−0.70 (0.24)	−0.70 (0.20)	−0.70 (0.15)	−0.30 [−1.61, 0.45]

C. By filer type

First report on time, net, by filer type	First holiday	Second holiday	Pooled	Per additional filing [95% CI]
Formation service	−4.1 (1.2)	−3.4 (1.0)	−3.7 (0.8)	−27 [−63, −11]
Repeat filer	−4.2 (1.1)	−4.6 (0.8)	−4.4 (0.7)	−21 [−31, −13]
New filer	−2.7 (1.5)	−3.9 (1.3)	−3.4 (1.0)	−30 [−60, −7]

Notes. Ordinary domestic LLCs. Each entry is T_r of equation (4) in percentage points: the rate among companies formed in the window minus the mean rate among those formed in the same months one year earlier and one year later, with the standard deviation of the same statistic over unaffected reference runs in parentheses. *Pooled* weights the holidays by their numbers of months. The estimates are those that Addendum 2 specified, as computed by its locked script [A2]. *Net of established companies*: minus the rate at which domestic LLCs three or more years old that had filed their previous report on time filed the report falling due in the same month. The estimate for first reports depends on this adjustment. Panel B reports it without the adjustment, and Section 6.5 and Table A40 also report it with the adjustment at the weight estimated from months unaffected by a holiday. ρ is the rate among marginal registrations relative to inframarginal registrations (equation 5), with the marginal share net of the flanking months and a 95% confidence interval from draws of the errors of the difference and of the share, which the locked script drew independently (Table A6, row 7). A negative ρ indicates that the outcome fell by more than could be explained if no marginal registration had the outcome. Two rows of Panel B are computed differently from the other rows. The row relative to the year after only is centered on the mean of its reference values, its standard error is their standard deviation multiplied by $\sqrt{1 + 1/n}$ for n reference runs, and it admits every run whose own months and year-after months are unaffected, a larger set than the 21 and 19 runs of the main estimate. In the regression row, the standard error is the least-squares standard error multiplied by the standard deviation of the t statistics of placebo runs. The addendum specified ρ for the two first-report outcomes; its values in the last four rows of Panel B were computed after the results were known [E]. Outcomes for the later months had been seen when the comparison was calibrated. Good standing is the registry's status in October 2026. ‡ As coded by the locked script: a second-year report counts as on time only if the first was on time, and a third-year report as filed before delinquency only if the two preceding reports were on time. Table A40 gives the estimate for the second report alone, as the addendum labeled it (Table A6, row 6). Panel C: the filer types are those of Section 4.2, which Addendum 2 labeled *organization agent* (a filing through a formation service), *additional company* (a filing by a repeat filer) and *first company in twelve months* (a filing by a new filer). The last column of Panel C divides the pooled difference by the share of that type's filings in the holiday months that were additional; this column was not pre-specified [E]. † The two holidays differ by more than twice the standard error of their difference.

E.8. The 2022–23 Fee-Reduction Program: Pre-specified Test and Additional Results

The program is the paper's case study (Section 5). This subsection reports the pre-specified test and the reasons why we do not attribute its result to the fee, the decomposition of the program year's additional registrations, and the detail underlying three statements in the main text. Table A43 presents the plan's comparison of cohort years together with the same comparison for the two cohorts formed after the program ended, and Table A44 and Figure A5 report every estimator specified in the plan. Tables A45 and A46 and Figures A6 and A7 concern the reinstatement of the $50 fee on May 22, 2023. The remaining tables and figures describe the filers active during and after the program.

Pre-specified comparison. The plan compared ordinary LLCs formed from July 2022 to March 2023 with those formed on the same dates a year earlier, net of the same change among corporations and foreign entities. The share that filed a first report on time fell by 7.0 percentage points (s.e. 1.0), more than any of eleven placebo values, and under the pre-specified rule the hypothesis is supported [P]. Interpreted as the plan prescribed, with additional filings equal to 18.9% of the cohort, the estimate implies that a marginal registration filed at 0.22 times the inframarginal rate, with a confidence interval from −0.02 to 0.46.

Reasons for not adopting that interpretation. Cohorts formed at $50 in the two years after the program also filed less often than the cohort of 2021–22: by 5.8 and 6.4 percentage points net of established companies (s.e. 0.5 and 1.1), and by 3.3 and 3.1 net of nonprofit corporations and foreign entities (1.9 each) [E]. The deficit ends with the program only relative to the plan's own comparison group, and that group includes profit corporations, whose on-time rate fell by 13 percentage points by 2024–25 during a wave of corporate filings that the state later marked as unauthorized or fraudulent. A deficit among companies formed after the program ended is not selection on the fee that those companies paid (Section 3). Nor is it attributable to the increase in the report fee from $10 to $25 in July 2024. Counting only first reports received while that fee was still $10, the share of companies with a first report was 5.1 percentage points lower among those formed in the last three weeks at $1 than among those formed on the same dates a year earlier, and 4.4 and 4.1 percentage points lower among those formed at $50 in the next ten days and in the following month. The part of the program year's deficit that ended with the program is 2.4 percentage points (0.3), 3.7 (1.6) or 6.1 (1.1), depending on the comparison group. The implied relative rate of marginal registrations ranges from 0.32 to 0.73, and the confidence intervals together extend from below zero to above one. The estimates net of established companies are sensitive to that adjustment. The first reports of the 2021–22 cohort fell due during the program, when established companies filed on time less often than in the adjacent years (Section A.5); the rate subtracted for the reference cohort is therefore lower, and the estimated deficit of each later cohort is larger by the same amount. With the rate of established companies in those months set to the mean of the same months in the adjacent years, the deficits of the program year and of the two later cohorts are 7.1, 4.4 and 5.0 percentage points. With no adjustment they are 6.9, 5.1 and 7.1 percentage points, and the part that ended with the program is 0.8 percentage points (1.3) [E]. The cohort of the program year filed reports less often, but the comparison does not identify how much of that difference reflects selection by the fee.

End of the program. One of the plan's secondary estimators uses a sharper comparison and yields a smaller estimate. On May 22, 2023 the $50 fee was reinstated on one business day's notice, and filings fell by about one-sixth (a share of 0.165, with a confidence interval from −0.01 to 0.31; Table 2). Relative to the same difference in thirteen other years, companies formed in the three weeks before the reinstatement were 1.6 percentage points less likely to file a first report on time than those formed in the ten days after it (s.e. 1.2) [P], and 4.0 percentage points less likely to be in good standing 41 months later (1.3), the most negative value among fourteen years [E]. With that share, the implied relative rates are 0.74 and 0.33, each with a confidence interval that includes zero; with the constant of 0.097 by which the plan scaled the contrast, the first is 0.56.

Composition by filer type. Table A42 decomposes the program year's 47 thousand additional registrations by filer type, as summarized in Section 5.

Table A42. The Program: Additional Registrations by Filer Type

	Filings, July 1 to May 21				Log change		Log change from 2021–22	
Filer type	2021–22	2022–23	Additional	Percent of additional	2021–22 to 2022–23	Earlier years: mean (s.d.)	to 2023–24	to 2024–25
Bulk groups	0	19,789	19,789	42.0				
Eight formation services with a persistent increase	5,997	13,090	7,093	15.1	+0.781	+0.203 (0.106)	+0.786	+0.715
Other formation services	19,437	22,329	2,892	6.1	+0.139	+0.112 (0.120)	+0.021	−0.002
Address with 20 or more in the previous year	3,197	4,196	999	2.1	+0.272	+0.173 (0.181)	+0.238	+0.078
Repeat filer	32,043	40,771	8,728	18.5	+0.241	+0.088 (0.094)	+0.070	+0.062
New filer	47,366	55,168	7,802	16.6	+0.152	+0.048 (0.078)	+0.021	+0.053
Other	1,665	1,454	−211	−0.4	−0.136	+0.105 (0.145)	−0.517	−0.210
Ordinary filings	109,705	137,008	27,303	58.0	+0.222	+0.077 (0.086)	+0.097	+0.094
All LLC filings	109,705	156,797	47,092	100.0	+0.357	+0.077 (0.088)	+0.097	+0.094
Memo: ordinary filings by people who act as their own agent, by name								
Names new to the registry (number of names)	55,732	69,126	13,394	28.4	+0.215	+0.061 (0.087)	+0.069	+0.073
their filings beyond one per name	2,649	4,506	1,857	3.9	+0.531	+0.081 (0.236)	−0.198	−0.083
Filings by names seen before	24,092	26,401	2,309	4.9	+0.092	+0.073 (0.072)	+0.030	+0.038

Notes. Filings to form a domestic LLC received from July 1 to May 21 of each year; the fee was $1 in 2022–23 and $50 in the other years. Filer types are assigned in the order of the rows (Section 4.2). The *eight formation services* are the organizations named as agent with at least 50 filings in one of 2021–22, 2022–23 and 2023–24 and at least five in 2021–22 whose filings in 2022–23 and in 2023–24 were both at least 1.5 times that number; the classification uses later volumes, and the agent is the one on record in October 2026. *Earlier years*: mean and standard deviation of the year-over-year log change in the ten years to 2021–22, two of which contain a holiday. In the memo panel, a name is new if no entity in the registry carries it as agent with an earlier formation date. This decomposition was not pre-specified [E].

Agent on record. The registry reports the agent on record in October 2026 and not the agent at formation (Section 6.4 compares the recorded changes of agent and address across the cohorts of the holidays), and 64% of the eight services' companies have filed a statement that changes agent information. Among companies with no such statement, other than statements that an agent filed for twenty or more of its companies on one day, the eight services reached 2.4 and 2.2 times their volume of 2021–22 in the two years after the program, whereas other services reached 1.1 and 1.0 times theirs [E]. The date at which a service is first observed is subject to the same limitation, because a service that later became the agent of an older company is dated from that company's formation. When each service is dated only from the companies with no such statement and the tabulation of Addendum 1 (N1) is restricted to those companies, services first observed more than two years before the year of the filing account for 52% of the increase that remained one year after the fee was reinstated, compared with 62% in the tabulation of all companies, and services first observed in the program year for 3%, compared with 3%. When the companies covered by a statement that an agent filed for twenty or more of its companies on one day are excluded as well, the two shares are 29 and 4% [E]. The agent at formation cannot be reconstructed, because the history of filings records that a statement was filed and not the agent that preceded it. The address from which the largest bulk group filed had formed 373 LLCs and 1,252 corporations in 2020 [E].

Links between later filing waves and the program's filers. Marks of unauthorized or fraudulent filing follow complaints, and 16,289 of the 20,804 on record were entered in 2026. Addendum 1 specified a rule for determining whether the later waves originated with the same filers as the program's: marked filings made after the reinstatement of the fee should be linked by an address or an agent to filers first observed during the program, or to its bulk filers, at least twice as often as other filings are, and in at least a fifth of cases [A1]. Over the whole period since the reinstatement, the rule is not met for either of the two links that it named (7.0% compared with 3.7%, and 5.9% compared with 0.7%). It is met for both links among the 1,714 marked or bulk LLCs formed in the thirteen months after the reinstatement, of which 35% and 44% are linked, and not among those formed later [E].

Table A43. The Program: Pre-specified Comparison, Later Cohorts, and the Reinstatement of the $50 Fee

A. First report on time by cohort (rates as proportions; changes in percentage points)

			Change from 2021–22, net of		Net of established companies		
Cohort	Fee to form	First report on time	the three other types (pre-specified)	nonprofits and foreign entities	ordinary LLCs	minus nonprofits and foreign entities	Filings above 2021–22 (share)
2021–22	$50	0.471					
2022–23	$1	0.406	−7.0 (1.0)	−6.9 (1.4)	−8.6 (1.2)	−7.0 (1.4)	0.189
2023–24	$50	0.419	−1.8 (1.3)[a]	−3.3 (1.9)	−5.8 (0.5)	−3.5 (2.3)	0.092
2024–25	$50	0.403	0.0 (1.5)[a]	−3.1 (1.9)	−6.4 (1.1)	−3.7 (2.6)	0.078

B. Companies formed immediately before and after the reinstatement of the $50 fee on May 22, 2023 (rates as proportions; differences in percentage points)

Outcome of ordinary LLCs formed in May 2023	Rate, formed May 22–31 ($50)	Difference, formed May 1–21 ($1)	Permutation p	Deterred relative to remaining, ρ
First report on time	0.386	−1.6 (1.2)	0.214	0.74 [−0.48, 1.20]
First report before delinquency	0.462	−2.5 (0.8)	0.071	0.67 [−0.55, 0.91]
Third-year report before delinquency	0.307	−2.5 (1.4)	0.143	0.49 [−1.64, 1.13]
In good standing at 41 months	0.366	−4.0 (1.3)	0.071	0.33 [−1.90, 0.77]
Secured credit within 24 months	0.024	−0.3 (0.6)	0.786	0.32 [−4.53, 5.51]

Notes. Ordinary domestic LLCs. Panel A, first two estimates: cohorts formed July 1 to March 31; the change in the on-time rate from 2021–22 minus the equally weighted mean change among the entity types named, with the root mean square of the same contrast over earlier pairs of years in parentheses. The first of these estimates, for 2022–23, is the plan's primary estimator [P]. Last two estimates: cohorts formed July 1 to May 21, net of established companies. These two estimates and the rows for 2023–24 and 2024–25 were not pre-specified [E]. Both later cohorts were formed at $50. Relative to the plan's comparison group, the deficit ends with the program. Relative to each of the other three comparison groups, the later cohorts show a deficit about half to three-quarters as large as that of the program year, which is why we do not attribute the pre-specified estimate to the fee (Section E.8). [a] Corporations cannot serve as a comparison group after 2023: their own on-time rate fell by 13 percentage points by 2024–25, during a wave of corporate filings that the state later marked as unauthorized or fraudulent. Panel B: the difference in each outcome between companies formed May 1–21 and May 22–31, 2023, minus the mean of the same difference in 2010 to 2022; standard errors are the standard deviation across those years times $\sqrt{1 + 1/13}$, and the permutation p-value is the rank of 2023 among fourteen years. ρ is the rate among the filings deterred by the reinstatement relative to the remaining filings; its 95% confidence interval, in brackets, incorporates the error in the share of filings deterred. The contrast is the plan's secondary estimator 6, which names no outcome. For the outcomes of the plan's own tests (the first report on time, its variant before delinquency, and secured credit), we report it as the plan's, here and in Table A44 [P]. Addendum 1 specified the list of outcomes (item N6) when all of them except the third-year report had been seen, so the third-year report is [A1] and good standing is exploratory [E].

Table A44. Pre-specified Tests for the Program and All Secondary Estimators

A. First report on time

	Estimate (pct. points)	*p*	Permutation *p*	Reference pairs	Implied gap, marginal minus inframarginal
Primary test (H2): first report on time, ordinary LLCs formed July 1 to March 31					
As pre-specified	−7.0 (1.0)	<0.001	0.083	11	−0.372 (0.052)
with the alternative share of additional filings					−0.470 (0.065)
cohort windows by calendar date	−7.0 (1.0)	<0.001	0.083	11	
reference pairs before the pandemic	−7.0 (1.0)	<0.001	0.111	8	
the six reference pairs unaffected by any event	−7.0 (1.1)	<0.001	0.143	6	
Secondary estimators, numbered as in the plan					
1. Net of nonprofit corporations only	−6.2 (1.2)	<0.001	0.083	11	
1. Net of foreign entities only	−7.6 (2.1)	0.004	0.083	11	
1. Net of profit corporations only	−7.3 (2.4)	0.012	0.083	11	
1. Net of nonprofit corporations and foreign entities	−6.9 (1.4)	<0.001	0.083	11	
2. Change from the year before, no comparison types	−6.5 (3.4)	0.079	0.167	11	
3. Report by one month after the anniversary month (to May 21)	−6.2 (2.0)	0.010	0.083	11	
3. Report before delinquency (to February 28)	−8.0 (1.2)	<0.001	0.083	11	
3. Any report within 548 days	−7.9 (1.2)	<0.001	0.083	11	
3. Report on time (to April 30)	−7.1 (1.0)	<0.001	0.083	11	
4. All filings, bulk groups included	−9.3 (0.9)	<0.001	0.083	11	
4. Bulk groups by a threshold of 25 filings in a week	−7.1 (1.0)	<0.001	0.083	11	
4. Bulk groups by a threshold of 100 filings in a week	−7.1 (1.0)	<0.001	0.083	11	
7. Net of established LLCs in place of the other types	−8.3 (1.7)	0.001	0.111	8	
7. Net of established LLCs as well as the other types	−8.9 (2.0)	0.002	0.111	8	
8. Reference period: mean of the two years before	−6.5 (1.0)	<0.001	0.091	10	
9. Without filings with a delayed effective date	−7.2 (1.1)	<0.001	0.083	11	
Voluntary dissolution within 457 days (descriptive)	0.4 (0.4)	0.408	0.333	11	
Secondary estimators that do not use the year before					
5. Weekly dose-response, ordinary filings		0.576	0.462	12	−0.076 (0.133)
5. The same, all filings		<0.001	0.077	12	−0.428 (0.049)
6. Formed May 1–21 relative to May 22–31, 2023: on time	−1.6 (1.2)	0.219		13	−0.165 (0.127)
6. The same: before delinquency	−2.5 (0.8)	0.012		13	−0.257 (0.087)
6. The same: by one month after the anniversary month	−0.8 (1.4)	0.588		13	−0.083 (0.149)

Table A44. (continued)

B. Secured credit

	Estimate (pct. points)	p	Permutation p	Reference pairs	Implied gap, marginal minus inframarginal
Test of H3: secured credit within 24 months, ordinary LLCs formed July 1 to May 21					
As pre-specified	−1.02 (0.34)	0.016	0.111	8	−0.051 (0.017)
with the alternative share of additional filings					−0.063 (0.021)
cohort windows by calendar date	−1.02 (0.34)	0.016	0.111	8	
the three reference pairs unaffected by any event	−1.02 (0.37)	0.070	0.250	3	
all eleven reference pairs (not pre-specified)	−1.02 (0.38)	0.021	0.083	11	
Reference period: mean of the year before and the year after	−0.91 (0.33)	0.032	0.143	6	
Secured credit within 12 months	−0.74 (0.27)	0.026	0.111	8	
Secondary estimators, numbered as in the plan					
1. Net of nonprofit corporations only	−1.17 (0.52)	0.052	0.111	8	
1. Net of foreign entities only	−0.87 (0.40)	0.064	0.111	8	
1. Net of profit corporations only	−1.01 (0.61)	0.137	0.111	8	
1. Net of nonprofit corporations and foreign entities	−1.02 (0.44)	0.049	0.111	8	
2. Change from the year before, no comparison types	−1.21 (0.33)	0.007	0.111	8	
4. All filings, bulk groups included	−1.28 (0.34)	0.006	0.111	8	
5. Weekly dose-response, ordinary filings		0.785	1.000	12	−0.007 (0.025)
5. The same, all filings		0.041	0.077	12	−0.028 (0.012)
6. Formed May 1–21 relative to May 22–31, 2023	−0.27 (0.60)	0.657		13	−0.028 (0.061)
Stakes gradient: proportional decline in credit minus that in reports	−0.150 (0.089)	0.131	0.222	8	

Notes. Unless the row indicates otherwise, an estimate is the change in the outcome among ordinary LLCs from the cohort of 2021–22 to that of 2022–23, minus the equally weighted mean change among nonprofit corporations, foreign entities and profit corporations, in percentage points. The standard error is the root mean square of the same statistic over the reference pairs of adjacent cohort years, whose number is given in the fourth column; p is from Student's t with that number of degrees of freedom, and the permutation p-value is the rank of the estimate among the reference values, in absolute value. The implied gap is the estimate divided by the share of the cohort that is additional, which the plan specified as 0.189 for reports and 0.199 for credit; the alternative shares, which take the level of 2023–24 as the trend, are 0.150 and 0.161. Dates in parentheses are the last day of formation in the sample. Estimators 5 are the slope of weekly cell means on the week's share of additional filings, with effects for cohort year and week; the slope is itself the implied gap. Estimators 6 compare companies formed in the three weeks before and the ten days after the reinstatement of the fee, net of the same difference in 2010 to 2022, and divide by 0.097. Their standard error is the standard deviation of that difference across the thirteen years times $\sqrt{1 + 1/13}$, and p is from Student's t distribution with 12 degrees of freedom. The plan framed the stakes gradient as a one-sided test, for which $p = 0.065$. All rows are the plan's [P] except those of three types: the rows with windows by calendar date are a correction made in the replication audit of the first draft, which the research log records, the rows restricted to reference pairs unaffected by any event come from Addendum 2 (item C7) and are not blind [A2], and the row with all eleven reference pairs for credit was not pre-specified [E]. The plan names no outcome for estimator 6, and we apply it to the outcomes of the plan's own tests. The stakes gradient is a difference of two proportional declines and not a difference of logs. The table reports the estimates as the plan requires. We do not attribute the cohort-year estimates to the fee, because cohorts formed at $50 after the program ended also show a deficit relative to every comparison group except the plan's own (Section E.8; Table A43).

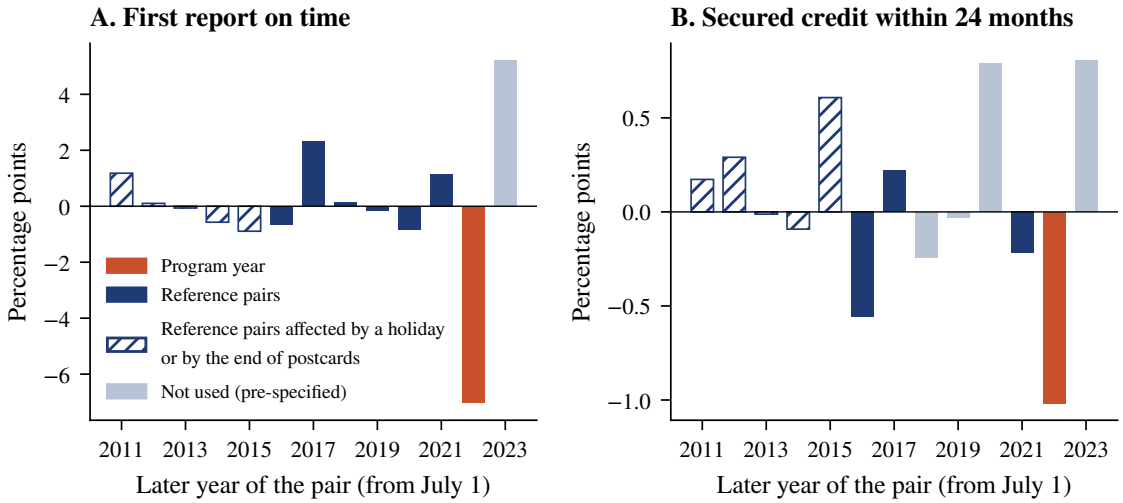


Figure A5. The Pre-specified Statistic for Each Pair of Adjacent Cohort Years

Notes. The statistic of the pre-specified tests for each pair of adjacent cohort years, by the later year of the pair. The bar for 2022 is the program. Reference pairs are those that the plan specified for each outcome. Light bars are pairs that the plan did not use: for credit, those containing the cohort years 2018–19 and 2019–20, and for both outcomes, the pair after the program, which was never a reference pair. Hatched bars are reference pairs in which a cohort year contains companies formed in a holiday, companies whose first report fell due in one, or companies whose first report fell due immediately after postcard reminders ended. The statistic and its reference pairs are the plan's [P]; the hatching follows Addendum 2 (item C7) [A2].

Table A45. Reinstatement of the $50 Fee on May 22, 2023

A. Decline in filings

	Decline in filings (log)	Share of filings deterred	Reference years
Ordinary filings per day, May 1–21 relative to May 22–31	0.181 (0.089)	0.165 [−0.01, 0.31]	13
reference years without 2020	0.161 (0.055)	0.149 [0.04, 0.25]	12
filings on business days only	0.128 (0.056)	0.120 [0.00, 0.22]	13
May 1–18, before the announcement	0.146 (0.080)	0.135 [−0.03, 0.27]	13
Ordinary filings, four weeks before relative to four weeks after	0.152 (0.019)	0.141	12
All LLC filings, bulk groups included	0.537 (0.092)	0.416 [0.29, 0.52]	13

B. Outcomes of companies formed immediately before and after

Outcome of companies formed in May 2023	Rate, formed May 22–31	Difference, formed May 1–21	p	Permutation p	Deterred relative to remaining, ρ
First report within a month of the anniversary	0.303	−0.008 (0.014)	0.588	0.643	0.83 [−0.26, 1.76]
First report on time	0.386	−0.016 (0.012)	0.219	0.214	0.74 [−0.48, 1.20]
First report before delinquency	0.462	−0.025 (0.008)	0.012	0.071	0.67 [−0.55, 0.91]
Second-year report before delinquency	0.345	−0.017 (0.009)	0.088	0.214	0.70 [−0.48, 1.05]
Third-year report before delinquency	0.307	−0.025 (0.014)	0.105	0.143	0.49 [−1.64, 1.13]
All three reports before delinquency	0.292	−0.024 (0.013)	0.095	0.143	0.49 [−1.46, 1.10]
In good standing at 41 months	0.366	−0.040 (0.013)	0.008	0.071	0.33 [−1.90, 0.77]
Secured credit within 24 months	0.024	−0.003 (0.006)	0.657	0.786	0.32 [−4.53, 5.51]

Notes. Ordinary LLC filings. Panel A: the log ratio of filings per day on May 1–21 to filings per day on May 22–31, 2023, minus the mean of the same log ratio in the reference years; the share of filings deterred is one minus the exponential of the negative of the decline in filings, with a 95% confidence interval. Panel B: the difference in each outcome between companies formed on May 1–21 and on May 22–31, 2023, minus the mean of the same difference in 2010 to 2022. Standard errors are the standard deviation across reference years times $\sqrt{1 + 1/13}$. The last column is the rate among deterred filings relative to remaining filings, $\rho = 1 + d/(s\mu_0)$, with s from the first row of Panel A and a confidence interval that incorporates the error in s. Good standing is the registry's status in October 2026. Rates and differences in Panels B and C are proportions (−0.016 is −1.6 percentage points). In Panel B, p is from Student's t distribution with 12 degrees of freedom and the permutation p-value is the rank of 2023 among the fourteen years. The contrast is the plan's secondary estimator 6, which names no outcome; as in Table A43, we report it as specified by the plan where it is applied to the outcomes of the plan's own tests (the first report on time, its variant before delinquency, and secured credit) [P]. Addendum 1 specified the variants of Panel A and the outcomes of Panel B (item N6) [A1]. At that point, the results for the outcomes of Panel B that do not involve the third-year report had been seen, and we treat those results as exploratory [E]. The row for all three reports was not pre-specified [E]. The row of Panel A for four weeks on either side uses the reference years without 2020; with 2020 the estimate is 0.167 (0.058), as in Table A46.

Table A45. (continued)

C. By filer type

Filer type	Filings, May 1–21 / May 22–31	Decline in filings (log)	Difference for companies formed May 1–21: First report on time	First report before delinquency	Good standing at 41 months
Types specified before the analysis					
First filing, sole filer at the address	3,214 / 1,243	0.210 (0.112)	0.000 (0.014)	−0.002 (0.018)	−0.003 (0.017)
Repeat filer or shared address	3,566 / 1,390	0.209 (0.094)	−0.035 (0.030)	−0.056 (0.026)	−0.073 (0.021)
Formation service	2,835 / 1,183	0.122 (0.120)	−0.007 (0.053)	−0.014 (0.048)	−0.049 (0.042)
By the filer's formations in the previous twelve months (not pre-specified)					
New filer	3,859 / 1,505	0.203 (0.099)	−0.006 (0.012)	−0.014 (0.018)	−0.018 (0.018)
Repeat filer	2,872 / 1,103	0.212 (0.109)	−0.037 (0.031)	−0.060 (0.030)	−0.069 (0.024)
repeat minus new			−0.031 (0.032)	−0.046 (0.037)	−0.051 (0.032)

Notes. Panel C, first part: the filer types are those that Addendum 1 defined (item N5) [A1], which differ from those of Section 4.2. *Formation service* denotes a filing whose registered agent is an organization. *Repeat filer or shared address* denotes a filing with a person as agent who formed another company in the preceding five years, or with a principal address that has 2 to 19 new LLCs in the year. *First filing, sole filer at the address* denotes the only new LLC at its address in the year, with a person as agent who formed no company in the preceding five years. A filing is assigned to the first of these three types that it fits, in the order in which they are defined here, which is the reverse of the order of the rows. Second part: the *new filer* and *repeat filer* types of Section 4.2, which were not pre-specified [E]. The decline in filings and the differences are computed as in Panels A and B, and the differences are proportions.

Table A46. Reinstatement of the $50 Fee: Sensitivity Analyses

	Decline in filings (log)	Difference for companies formed before the fee was reinstated (percentage points): First report on time	First report before delinquency	Good standing at 41 months
A. May 1–21 relative to May 22–31, 2023, net of 2010 to 2022				
As specified in Addendum 1	0.181 (0.089)	−1.6 (1.2)	−2.5 (0.8)	−4.0 (1.3)
Early window ends May 18, before the announcement	0.146 (0.080)	−1.7 (1.2)	−2.4 (1.0)	−3.9 (1.3)
Reference years without 2020	0.161 (0.055)	−1.6 (1.3)	−2.5 (0.9)	−4.0 (1.3)
Bulk groups by a threshold of 25 filings in a week	0.185 (0.089)	−1.7 (1.2)	−2.7 (0.8)	−4.1 (1.3)
Bulk groups by a threshold of 100 filings in a week	0.199 (0.089)	−2.1 (1.2)	−3.0 (0.8)	−4.3 (1.3)
Without addresses with 20 or more filings in the year	0.188 (0.089)	−0.6 (1.2)	−1.8 (0.9)	−2.9 (1.4)
All filings, bulk groups included	0.537 (0.092)	−12.3 (2.0)	−15.1 (1.3)	−13.7 (2.0)
B. Equal windows either side of May 22, 2023 (not pre-specified, except the decline in filings)				
Two weeks either side	0.173 (0.052)	−0.6 (2.0) {6}	−2.2 (1.5) {2}	−3.1 (1.7) {1}
Three weeks either side		0.8 (2.3) {10}	−0.7 (1.5) {4}	−1.6 (1.5) {4}
Four weeks either side	0.167 (0.058)	0.0 (2.8) {4}	−2.5 (1.5) {3}	−2.8 (1.2) {1}
Six weeks either side		1.3 (3.3) {9}	−1.5 (1.9) {3}	−2.1 (0.9) {1}
Eight weeks either side	0.176 (0.098)	0.4 (3.5) {4}	−2.3 (2.2) {3}	−2.3 (1.1) {1}

Notes. Ordinary LLC filings except in the last row of Panel A. Panel A repeats the first row of Panel A of Table A45 and three rows of its Panel B under the variations specified in Addendum 1 (item N6) [A1]. Panel B compares windows of complete weeks before and after May 22, 2023, net of the same difference in 2010 to 2022. Numbers in braces give the rank of 2023 among the fourteen years, from the most negative. In two reference years the later weeks extend to July 1, when the fee was reduced to $1: the windows of eight weeks include such days in 2014 and in 2022, and those of six weeks in 2022. Appendix B reports that the estimates change little when those two years are excluded. Panel B is exploratory [E], except that Addendum 1 named the decline in filings over two, four and eight weeks on either side among its variants.

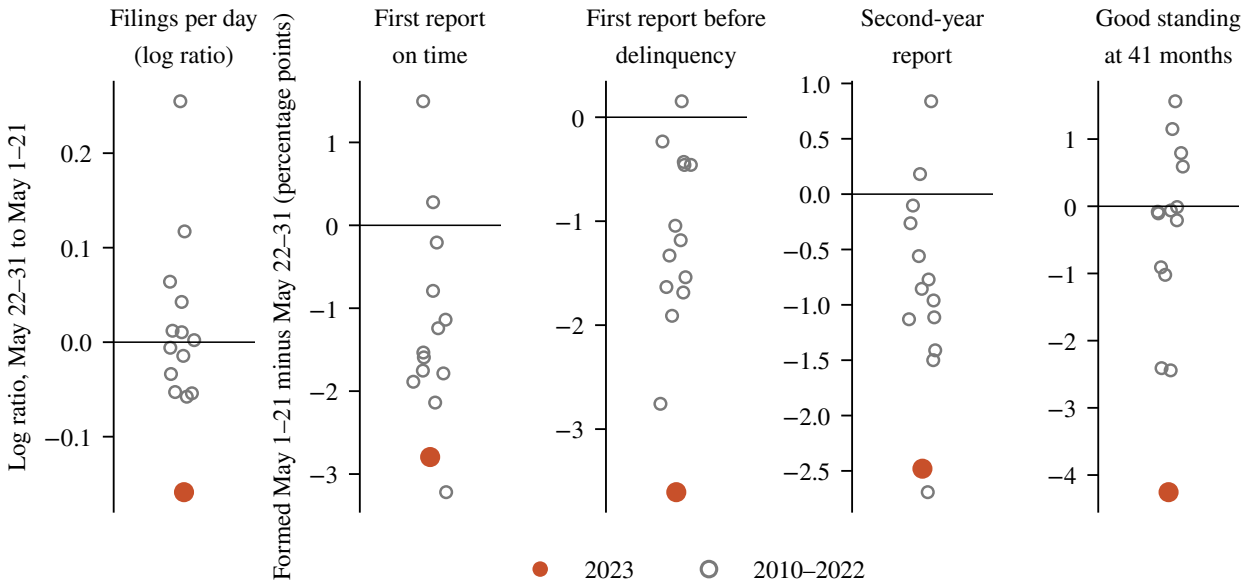


Figure A6. The 2023 Reinstatement of the $50 Fee Compared with the Same Dates in Earlier Years

Notes. Left: the log ratio of ordinary filings per day on May 22–31 to May 1–21. Other panels: the difference in each outcome between companies formed on May 1–21 and on May 22–31, in percentage points. The filled marker is 2023 and the open markers are 2010 to 2022. The figure was specified in Addendum 1 (item N6) [A1], and each panel has the status of its row in Table A45.

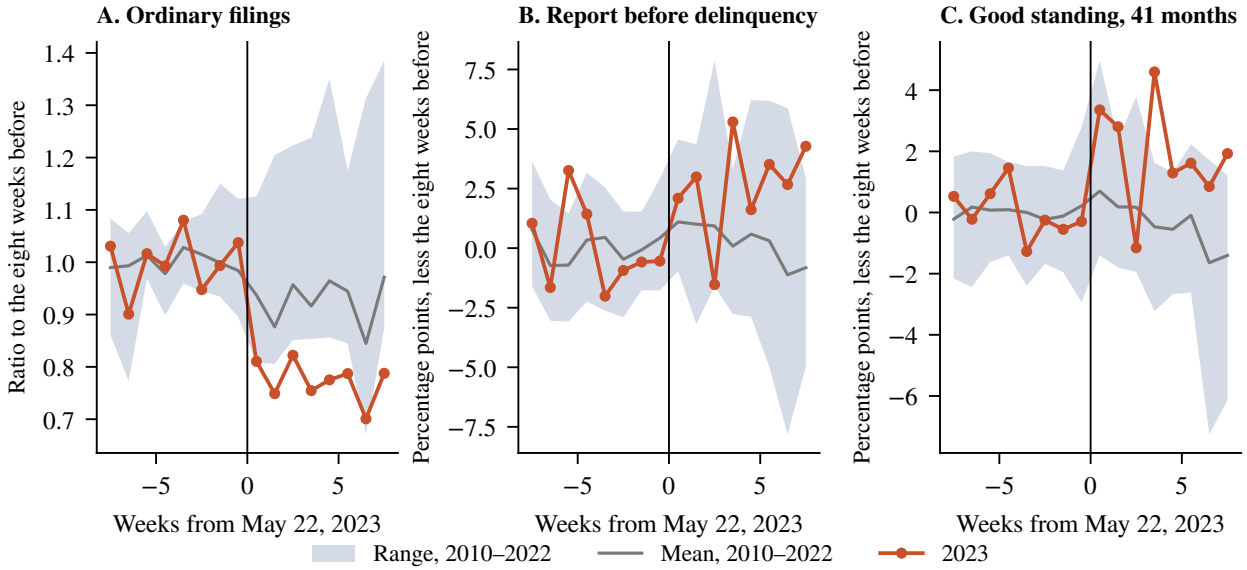


Figure A7. Filings and Outcomes by Week of Formation around the Reinstatement of the $50 Fee

Notes. Ordinary LLC filings by week of formation, with week zero beginning on May 22. Panel A shows the number of filings, and Panels B and C the shares of each week's companies with a first report before delinquency and in good standing after 41 months. Each series is shown relative to its mean over the eight weeks before week zero. The band is the range of the same series at the same dates in 2010 to 2022 and the gray line is their mean. Not pre-specified [E].

Table A47. Bulk Groups during the Program

Group	Filings	Addresses or names	First filing	Days with filings	Most in a day	Formation service as agent	First report on time	Dissolved by court	Good standing, Oct. 2026
1	15,527	6	Nov. 9, 2022	95	599	0.001	0.001	0.995	0.000
2	1,982	5	July 1, 2022	192	194	0.990	0.835	0.000	0.816
3	606	1	July 1, 2022	220	24	0.351	0.272	0.000	0.092
4	463	1	July 1, 2022	167	33	0.648	0.158	0.000	0.138
5	362	3	Aug. 30, 2022	51	20	0.989	0.972	0.000	0.246
6	262	1	July 1, 2022	135	12	0.599	0.149	0.000	0.011
7	244	1	Aug. 21, 2022	45	48	0.393	0.025	0.000	0.004
8	182	2	July 11, 2022	76	15	0.709	0.132	0.000	0.071
9	161	2	Nov. 7, 2022	26	29	1.000	0.000	0.000	0.000
2 to 9	4,262					0.792	0.622	0.000	0.432
All	19,789	22				0.171	0.117	0.781	0.093

Notes. Domestic LLC filings received from July 1, 2022 to May 21, 2023 that the rule of Section 4.2 assigns to a bulk group. The 22 addresses and names that meet the rule are combined into one group when a filing carries two of them; groups are numbered by size, and no address or name is reported. *First filing* is the group's first filing during the program, which is the program's first day for an address or agent that was already active. Every group except the ninth, whose last filing was on January 9, 2023, was still filing in the last three days at $1. The last four columns are shares of the group's filings: with a formation service as registered agent, with a first report on time, dissolved by court order, and in good standing in October 2026. The rule and the resulting counts are the plan's [P] (item H6). Addresses and names were combined into groups in the replication audit [E].

Table A48. Characteristics of the Program's Additional Ordinary Filings

	Share of ordinary LLC filings			Additional filings		
	2021–22	2022–23	2023–24	Share	95% CI	*t*
Registered agent and location						
Formation service as registered agent	0.232	0.259	0.273	0.366	[0.25, 0.49]	2.4
Principal address in another state	0.055	0.056	0.052	0.058	[0.03, 0.08]	0.2
Principal address abroad or missing	0.001	0.003	0.004	0.012	[0.01, 0.02]	6.7
Income of the ZIP code of the principal address (Colorado)						
Lowest third	0.255	0.277	0.257	0.363	[0.30, 0.43]	3.8
Middle third	0.297	0.295	0.301	0.290	[0.26, 0.32]	−0.6
Highest third	0.372	0.350	0.368	0.266	[0.20, 0.33]	−3.4
New LLCs at the same principal address in the year						
One	0.548	0.490	0.526	0.255	[0.07, 0.44]	−3.4
Two to four	0.259	0.282	0.249	0.378	[0.29, 0.46]	3.1
Five to nineteen	0.103	0.110	0.096	0.137	[0.08, 0.20]	1.3
Twenty or more	0.082	0.109	0.122	0.214	[0.12, 0.31]	3.1
Words in the entity name						
Property, real estate, holdings	0.103	0.084	0.083	0.009	[−0.02, 0.04]	−6.7
Contains a number	0.100	0.080	0.079	0.001	[−0.05, 0.05]	−4.1
Building trades	0.059	0.053	0.055	0.032	[−0.01, 0.07]	−1.5
Transport	0.030	0.026	0.026	0.011	[−0.02, 0.04]	−1.7
Personal and business services	0.131	0.141	0.143	0.181	[0.14, 0.22]	2.8
Food and drink	0.009	0.009	0.009	0.008	[0.00, 0.02]	−0.1
Retail	0.014	0.016	0.016	0.027	[0.01, 0.04]	1.8
None of these	0.673	0.688	0.684	0.750	[0.71, 0.79]	4.2
First name of a person who is registered agent						
Female	0.236	0.231	0.224	0.213	[0.17, 0.25]	−1.3
Male	0.392	0.363	0.355	0.250	[0.11, 0.39]	−2.2
Not classified	0.125	0.136	0.140	0.181	[0.16, 0.21]	4.8

Notes. Ordinary LLC filings received from July 1 to May 21. The share among additional filings is the change from 2021–22 to 2022–23 in the number of filings with the characteristic divided by the change in all ordinary filings, which treats the whole increase as additional. The *t*-statistic is the change in the share divided by the root mean square of earlier year-to-year changes in the share, and the confidence interval is constructed from the same root mean square. Income is adjusted gross income per return in the ZIP code in 2021, in thirds weighted by returns. Words in names and first names are classified as described in Appendix C. Agents and addresses are the registry's values in October 2026, and about a fifth of the LLCs of each of these years have a later filing that changes one of them; among filings with no later change of agent, the share of additional filings with a formation service as registered agent is 0.22 (t = 1.3), a figure from the replication audit (Table A6, row 1). The plan's item H5, specified as a description without a test [P].

Table A49. Sources of the Increase That Remained a Year after the Program: Filings by Year of the Filer's First Appearance

	Filings			Change, 2021–22 to 2023–24		
Key first seen in	2021–22	2023–24	Most in a year, 2013 to 2022	Pct. points of 2021–22 filings	Share of the change	Share in six earlier spans
Formation service as registered agent, by service						
the year itself	8,859	9,184	11,483	+0.30	0.03	0.04 to 0.14
the year before	223	564	289	+0.31	0.03	−0.01 to 0.00
two years before	172	207	531	+0.03	0.00	−0.01 to 0.02
earlier	16,180	23,047	23,596	+6.26	0.62	0.17 to 0.26
Person as registered agent, by agent name						
the year itself	58,381	61,902	73,632	+3.21	0.32	0.45 to 0.52
the year before	2,706	2,811	2,789	+0.10	0.01	0.01 to 0.04
two years before	1,852	2,165	2,682	+0.29	0.03	0.01 to 0.02
earlier	19,534	19,846	20,930	+0.28	0.03	0.13 to 0.18
All ordinary filings, by principal address						
the year itself	18,325	17,171	21,976	−1.05	−0.10	0.02 to 0.13
the year before	2,119	2,587	2,323	+0.43	0.04	0.00 to 0.03
two years before	1,687	1,927	2,645	+0.22	0.02	0.00 to 0.04
earlier	86,761	98,340	108,807	+10.55	1.04	0.85 to 0.95
All ordinary filings	109,705	120,825		+10.14	1.00	

Notes. Ordinary LLC filings received from July 1 to May 21. A filer is tracked by a key: the name of an organization that is registered agent, the name and ZIP code of a person who is registered agent, or the street number and ZIP code of the principal address. A key is *first seen* on the earliest date on which any entity in the registry that carries it was formed, and *the year itself* is the cohort year of the filing. *Most in a year* is the largest number of filings in the class in any cohort year from 2013–14 to 2022–23. *Pct. points* are the change in the class's filings from 2021–22 to 2023–24 as a percentage of all ordinary filings of 2021–22. Within the first two panels, the shares do not sum to one because filings with the other type of agent are not shown. The last column gives the range of the same share over six earlier pairs of cohort years two years apart, ending 2015–16 to 2021–22; one of these pairs begins with the cohort year that opens with the second holiday. A seventh pair, ending 2019–20, is omitted because filings changed by only 1.5% between its years and the shares are unstable; in that pair, formation services first seen earlier account for 0.68 of the change. Keys are the registry's values in October 2026. Specified in Addendum 1 (item N1) [A1].

Table A50. Persistence of Filing Volumes after the Program

A. Formation services classified by their own filing volumes

Formation services, by volume	Services	Ordinary LLC filings, July 1 to May 21				2023–24 minus 2021–22
		2021–22	2022–23	2023–24	2024–25	
Persistent increase	8	5,997	13,090	13,156	12,265	7,159
Increase, partly reversed	1	486	1,311	663	480	177
Increase, fully reversed	1	62	221	71	56	9
New in the program year	4	2	171	771	33	769
No increase	23	5,218	4,627	4,901	4,663	−317
All other formation services		13,669	15,999	13,440	14,174	−229
All formation services		25,434	35,419	33,002	31,671	7,568

B. Formation services with a persistent increase, in Colorado and in Connecticut

	New LLCs through the same formation services				Log change from 2021–22		
	2021–22	2022–23	2023–24	2024–25	to 2022–23	to 2023–24	to 2024–25
Colorado	4,731	10,669	10,495	9,209	+0.813	+0.797	+0.666
Connecticut	3,966	4,271	4,886	5,242	+0.074	+0.209	+0.279
Colorado minus Connecticut					+0.739	+0.588	+0.387

C. Filing channels: shares and first reports

Channel	Share of ordinary filings				First report on time			
	2021–22	2022–23	2023–24	2024–25	2021–22	2022–23	2023–24	2024–25
Formation services with a persistent increase	0.054	0.093	0.110	0.100	0.435	0.359	0.370	0.437
Other formation services	0.179	0.163	0.167	0.162	0.566	0.441	0.473	0.515
Person as agent: repeat filer or shared address	0.378	0.391	0.356	0.368	0.468	0.403	0.422	0.385
Person as agent: first filing, sole filer at the address	0.353	0.317	0.333	0.335	0.447	0.421	0.425	0.380
Other	0.036	0.035	0.033	0.035	0.320	0.282	0.233	0.191

Notes. Ordinary filings to form a domestic LLC, July 1 to May 21 of each year (Panel C: companies formed July 1 to March 31). Panel A: formation services with at least 50 filings in one of 2021–22, 2022–23 and 2023–24. With n_A, n_P and n_B denoting a service's filings in those years, the service is classified as *new* if $n_A < 5$; as having a *persistent increase* if $n_P \geq 1.5n_A$ and $n_B \geq 1.5n_A$; as having an *increase, fully reversed* if $n_P \geq 1.5n_A$ and $n_B < 1.25n_A$; and as having an *increase, partly reversed* in the intermediate case. A service with $n_A \geq 5$ and $n_P < 1.5n_A$ is classified as having *no increase*, and *all other formation services* are those with fewer than 50 filings in each of the three years. Panel B: the services of the first row whose names could be matched in Connecticut's registry, where no fee changed (five of six agent names; Appendix C), with the new LLCs formed through agents of those names in each state. Panel C: a filing through one of the services of the first row of Panel A, through another formation service, or by a person as agent, divided as in Addendum 1 (Table A45, Panel C). A formation service is the organization named as registered agent in the registry in October 2026. Panel A was specified in Addendum 1 (item N2) [A1]. Its column for 2024–25 and Panels B and C were not pre-specified [E].

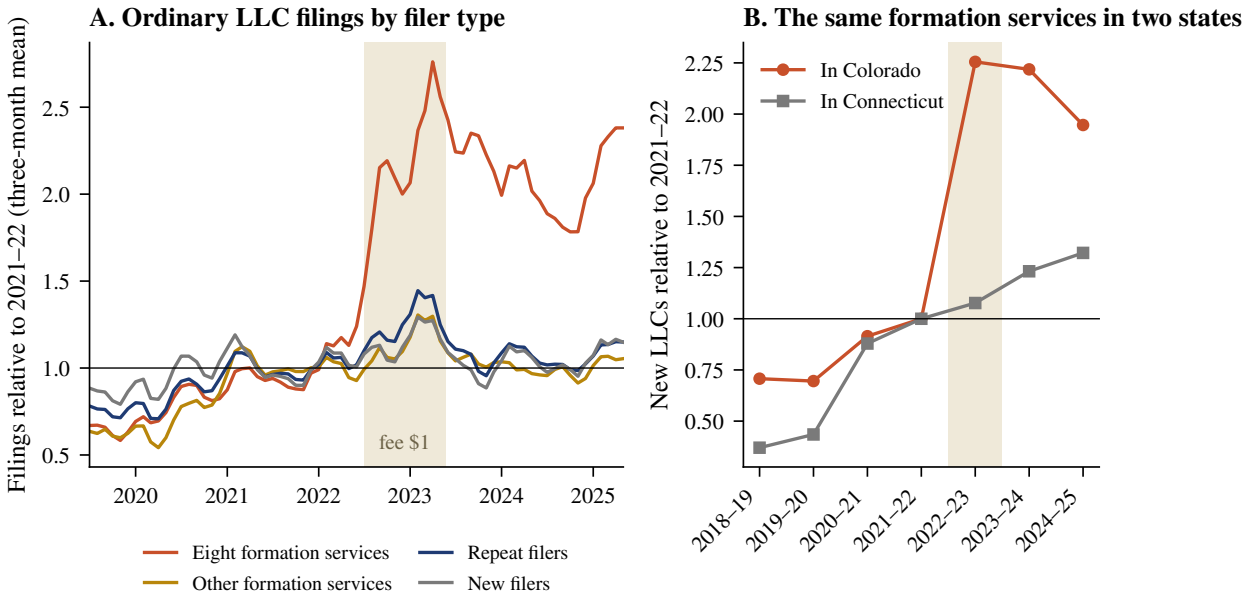


Figure A8. The Program: Filings by Filer Type, and Filings through the Same Formation Services in Two States

Notes. Panel A: ordinary filings to form a domestic LLC in Colorado by filer type, three-month moving average, relative to each type's mean in the eleven months from July 2021 to May 2022. The *eight formation services* are those classified as having a persistent increase (Table A50, Panel A); *repeat filers* and *new filers* are the filer types defined in the main text. Panel B: new LLCs formed through those of the eight services that could be matched by name in Connecticut's registry (five of six agent names), in each state, by year beginning in July, relative to 2021–22 (Table A50, Panel B). Shading marks the program. Not pre-specified [E].

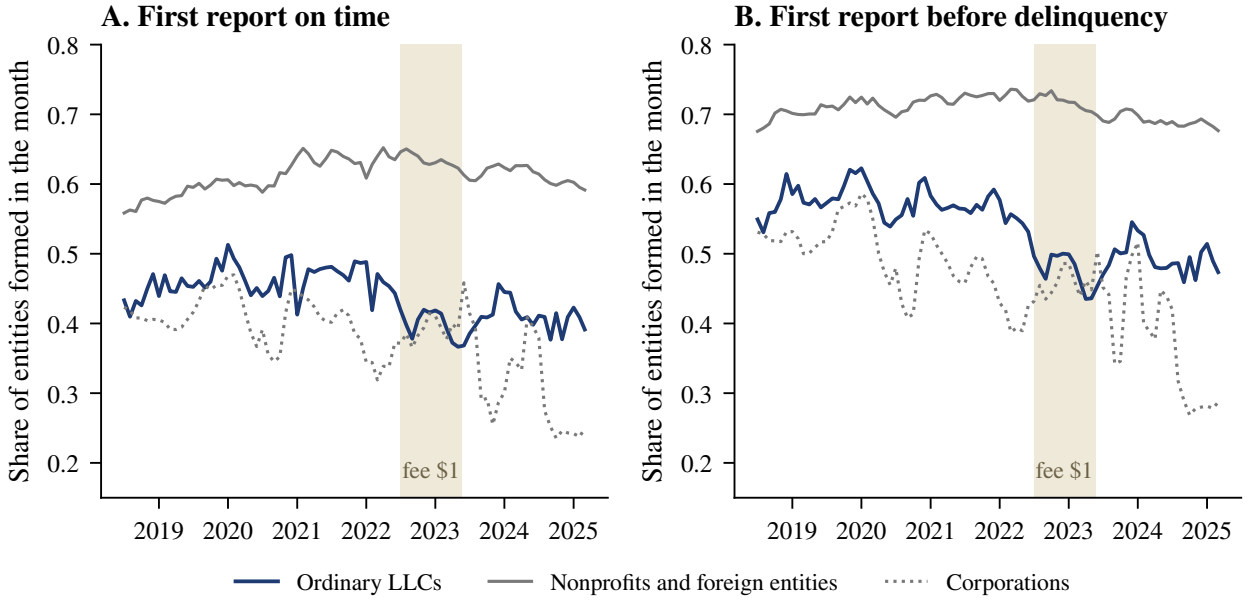


Figure A9. First Reports by Month of Formation, July 2018 to March 2025

Notes. Share of the entities formed in each month that filed a first report on time (Panel A) and before delinquency (Panel B). The series for the other entity types are three-month centered averages. Shading marks the months of the program. Not pre-specified [E].

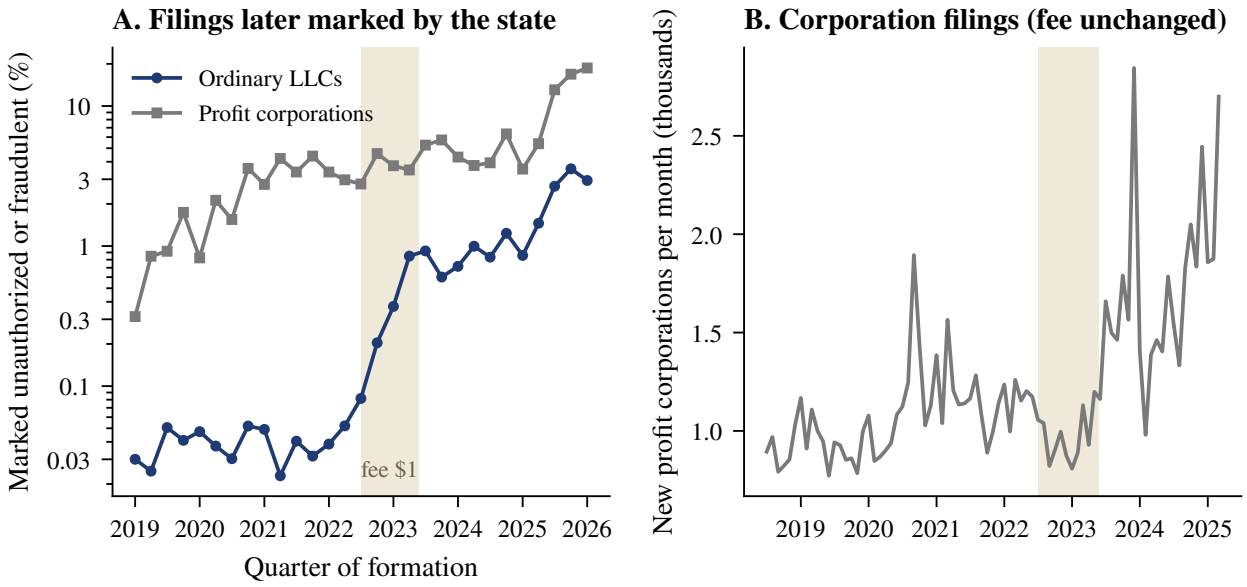


Figure A10. Filings Later Marked as Unauthorized or Fraudulent, and Filings to Form Corporations

Notes. Panel A: share of the entities formed in each quarter that carry a correction by the filing officer, received since January 2022, stating that the filing was unauthorized or fraudulent; logarithmic scale. Marks follow complaints and therefore measure enforcement as well as fraud (Section D.7). Panel B: filings to form a profit corporation per month. The fee to form a corporation did not change during the program, which is shaded. Not pre-specified [E].

E.9. Business Applications and the Illinois Fee Reduction

Section 5 uses the Census Bureau's counts of applications for Employer Identification Numbers (EINs), published as business applications (U.S. Census Bureau, 2026a). We compare Colorado with a weighted combination of 46 other states using synthetic difference-in-differences (Arkhangelsky et al., 2021), as specified in the plan for the program [P] and in Addendum 2 for the holidays [A2]. Table A51 and Figure A11 report the estimates for each episode in Colorado and for Illinois, and Table A52 and Figure A12 report the other estimators, periods and states required by the plan and by Addendum 1. Appendix C describes the series and the estimator.

Colorado. The estimate for business applications is positive in each episode and smaller than the increase in registrations. In the second holiday, the estimate is 9.8 log points (s.e. 3.3), which corresponds to 0.35 applications per additional registration, with a confidence interval from 0.12 to 0.65 once the uncertainty in the number of additional registrations is taken into account. The estimate for the first holiday is too imprecise to add to this evidence (0.29, from −0.26 to 0.99). In the program, the estimate is 9.8 log points (3.5), and the ratio is 0.23, 0.39 or 0.58, depending on whether the denominator counts all additional filings, ordinary additional filings, or ordinary additional filings net of usual growth, with a confidence interval from 0.06 to 0.53 for the first and confidence intervals that extend above one for the other ratios [E]. The benchmark of Section D.7 is the number of applications that Colorado would have had in the months of the program in its absence, divided by the formation filings of all four entity types on the same dates a year earlier: 0.76 for all business applications, 0.25 for high-propensity business applications, which the Census Bureau classifies as likely to become employers, and 0.08 for business applications with planned wages [E]. As with the deficit in reports, the increase in applications persisted after the program ended: in the year after the reinstatement of the fee, the estimate is 10.1 log points (5.9), which differs from the estimate for the program by 0.3 (4.1), and a comparison of the four weeks on either side of the reinstatement gives −1.7 (2.7). Among the Census Bureau's more detailed series, high-propensity business applications increased in the holidays by about as much as all applications, by 6.4 (4.0) and 8.5 (3.3) log points, and in the program by about half as much as all applications, 4.7 (3.2). The high-propensity classification is based on the information that the applicant enters on the form, such as the type of entity, the industry and the reason for applying, and not on the later activity of the business; it includes applications from corporations other

than those associated with internet sales, which, since the release for January 2026, the Bureau has excluded from the whole history of the series (U.S. Census Bureau, 2026b). Business applications with planned wages show no increase that is statistically distinguishable from zero in any episode, and neither do business formations within four quarters, which Addendum 2 did not specify for the holidays and which are therefore estimated for the program only; none of these estimates excludes an increase of several percent (Table A51).

Illinois. A fee change of $49 or £38 is not informative about what a fee of several hundred dollars deters. In December 2017 Illinois permanently reduced the fee to organize an LLC from $500 to $150, and the same act reduced the fee for an LLC's annual report from $250 to $75 (Illinois General Assembly, 2017). Before any Illinois series was examined, Addendum 1 specified the estimator, the period and three statements on interpretation: an increase in applications together with an increase in high-propensity applications or in applications with planned wages was what we then took the framework to predict for a reduction of that size; no detectable change would be uninformative; and the increase in the state's income tax in July 2017 and the end of its budget impasse are concurrent events whose effects the design cannot remove [A1]. The first of these statements overstated what the framework implies, because the framework's upper bound for a fee of $500 permits such a response and does not predict it (Section B.5, item N9). New LLCs were 60% more numerous in 2018 than in 2017 and 83% more numerous in 2019; the number of new corporations fell by an amount equal to 15% of the additional LLCs. Applications showed no detectable change: the estimate is 0.7 log points (4.5) over the two years, or 0.03 per additional LLC, with a confidence interval from −0.39 to 0.41. Under the pre-specified interpretation, the result is uninformative about what the fee had deterred, and we do not claim that a fee of $500 prevented no business from forming. The Illinois evidence adds a second state in which, with a permanent reduction of the formation fee seven times as large in dollars and a reduction of the annual report fee, registrations of LLCs increased by more than half with no increase in applications that the national data can detect.

Table A51. Business Applications and Employer Business Formations

	Colorado, $50 to $1			Colorado, $50	Illinois, $500 to $150
	First holiday	Second holiday	Program	Year after the program	2018–19
A. Synthetic difference-in-differences on Census series (log)					
Business applications	+0.050 (0.045)	+0.098 (0.033)	+0.098 (0.035)	+0.101 (0.059)	+0.007 (0.045)
high-propensity applications	+0.064 (0.040)	+0.085 (0.033)	+0.047 (0.032)	+0.099 (0.051)	−0.048 (0.053)
with planned wages	+0.008 (0.044)	+0.071 (0.053)	+0.036 (0.042)	+0.012 (0.064)	−0.016 (0.051)
Business formations within four quarters			−0.045 (0.053)		−0.032 (0.052)
B. Per additional registration					
Additional registrations (number)	3,036	6,169	47,092	13,559	52,963
Additional applications per additional registration	0.29 [−0.25, 0.77]	0.35 [0.12, 0.57]	0.23 [0.06, 0.38]	0.88 [−0.16, 1.80]	0.03 [−0.39, 0.41]

Notes. Panel A: synthetic difference-in-differences estimates for log monthly applications (quarterly for business formations), comparing the treated state with other states (Colorado: 46 comparison states, excluding California, Montana, Delaware and Wyoming; Illinois: 48, with July to December 2017 omitted). Standard errors in parentheses are the root mean square of the same estimate with each comparison state treated in turn. High-propensity applications are those that the Census Bureau classifies as likely to become employers. They include applications from corporations, whose fee was also reduced in the two holidays, other than applications associated with internet sales, which the Bureau has excluded from the high-propensity and corporation series over their whole history since the release for January 2026 (U.S. Census Bureau, 2026b); the series used here are of a later vintage. The post-period is the months at $1 for the three episodes, June 2023 to May 2024 without December 2023 for the year after the program, and January 2018 to December 2019 for Illinois. Business formations for the program are those originating from applications of July to December 2022. Panel B: additional registrations are those of the entity types whose formation fee was reduced (for the program, all LLC filings above the number on the same dates of 2021–22, bulk groups included; for the year after, LLC filings above that level; for Illinois, new LLCs in 2018 and 2019 above twice their number in 2017). The ratio is the estimated number of additional applications divided by that number, with a 95% confidence interval that reflects the error in the numerator only; Section E.9 reports confidence intervals that also incorporate the error in the number of additional registrations [E]. The estimator and the column for the program are the plan's [P] (item H4). The columns for the holidays were specified in Addendum 2 (item C5) [A2] and the estimates for Illinois in Addendum 1 (item N9) [A1]. The year after the program without December 2023, a period that the plan did not specify, and the number of additional LLCs in Illinois, which is taken from the Secretary of State's reports, are exploratory [E].

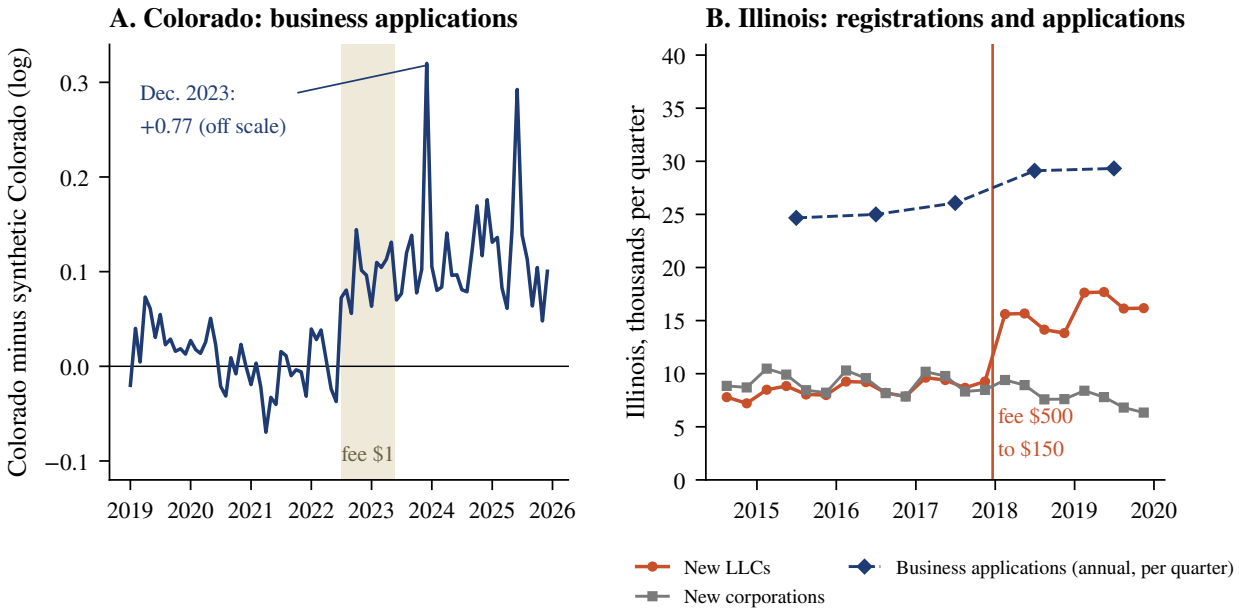


Figure A11. Business Applications in Colorado, and Registrations and Business Applications in Illinois

Notes. Panel A: log monthly business applications in Colorado minus the synthetic comparison constructed from other states; shading marks the program. The gap for December 2023, 77 log points, lies outside the range of the axis. It coincides with a spike in filings to form corporations (Figure A10, Panel B), and that month is excluded from the year after the program in Table A51. Panel B: new domestic LLCs and corporations in Illinois by quarter, from the Secretary of State's quarterly reports, and business applications in Illinois (annual total divided by four); the vertical line marks the reduction in the fee to organize an LLC from $500 to $150 in December 2017. Panel A plots the gap underlying the plan's estimate [P] and extends it beyond the program [E]. Panel B was not pre-specified [E].

Table A52. Business Applications: Other Estimators, Periods, and States

A. Colorado, California and Montana

	SDID	DiD	SC	SDID, other window
Colorado, the program: July 2022 to May 2023				
Business applications	+0.098 (0.035)	+0.038 (0.086)	+0.086 (0.070)	
High-propensity business applications	+0.047 (0.032)	−0.041 (0.074)	+0.017 (0.071)	
Business applications with planned wages	+0.036 (0.042)	−0.049 (0.073)	+0.030 (0.071)	
Business applications from corporations	−0.040 (0.063)	−0.008 (0.098)	−0.019 (0.074)	
Business formations within four quarters	−0.045 (0.053)	−0.089 (0.076)	−0.036 (0.081)	
Colorado, the year after: June 2023 to May 2024 (other window: without December 2023)				
Business applications	+0.155 (0.060)	+0.109 (0.099)	+0.150 (0.089)	+0.101 (0.059)
High-propensity business applications	+0.175 (0.049)	+0.106 (0.084)	+0.163 (0.088)	+0.099 (0.051)
Business applications with planned wages	+0.052 (0.062)	−0.051 (0.086)	+0.033 (0.088)	+0.012 (0.064)
Business applications from corporations	+0.254 (0.099)	+0.248 (0.110)	+0.253 (0.090)	+0.248 (0.103)
California, $70 fee waived: July 2022 to June 2023 (other window: to May 2023)				
Business applications	+0.037 (0.035)	+0.035 (0.086)	−0.027 (0.070)	+0.034 (0.035)
High-propensity business applications	+0.055 (0.032)	−0.016 (0.076)	+0.005 (0.071)	+0.051 (0.032)
Business applications with planned wages	+0.010 (0.043)	−0.053 (0.074)	+0.154 (0.073)	0.000 (0.042)
Business applications from corporations	+0.045 (0.063)	+0.137 (0.098)	+0.309 (0.074)	+0.048 (0.063)
Business formations within four quarters	+0.004 (0.053)	−0.005 (0.076)	+0.466 (0.081)	
Montana, fees halved: from July 2022, estimated to May 2023				
Business applications	+0.015 (0.035)	+0.099 (0.086)	+0.056 (0.070)	
High-propensity business applications	−0.004 (0.032)	+0.032 (0.074)	+0.025 (0.071)	
Business applications with planned wages	−0.039 (0.042)	−0.018 (0.073)	+0.007 (0.071)	
Business applications from corporations	−0.064 (0.063)	−0.074 (0.098)	−0.066 (0.074)	
Business formations within four quarters	−0.010 (0.053)	+0.052 (0.076)	+0.009 (0.081)	

Notes. Effects on the log of each monthly series. SDID is synthetic difference-in-differences (Arkhangelsky et al., 2021), DiD is the standard difference-in-differences estimator and SC is the synthetic control estimator. Standard errors are the root mean square of the same estimate with each comparison state treated in turn. Panel A: pre-period January 2015 to June 2022; comparison states are all other states and the District of Columbia except Delaware, Wyoming and the other two states of the panel. For the year after, the months of the program are dropped. Business formations are those originating from applications of July to December 2022, the last period for which the release that we use has actual counts, so they cannot be estimated for the year after. A synthetic control has no intercept and cannot match the largest state, and its estimates for California are therefore not informative. The plan named SDID as the estimator and Colorado's program as the period, and it required that the other columns and blocks also be reported; its period for the year after includes December 2023 and its window for California ends in May 2023 (the columns headed *other window* give the year after without December 2023 and California to May 2023). Panel A is the plan's [P], with two exceptions that the plan did not specify [E]: the year after without December 2023, and the main columns for California for the four monthly series. Those columns use the twelve months of the waiver, to June 2023, in place of the plan's window, which is shown in the column headed *other window*; the research log records this departure.

Table A52. (continued)

B. Illinois, after the reduction of the fee to form an LLC from $500 to $150 on December 20, 2017

	January 2018 to December 2019			2018	2019
	SDID	DiD	SC	SDID	SDID
Business applications	+0.007 (0.045)	−0.068 (0.064)	+0.023 (0.072)	−0.007 (0.054)	+0.018 (0.052)
High-propensity business applications	−0.048 (0.053)	−0.094 (0.064)	−0.059 (0.060)	−0.037 (0.043)	−0.059 (0.063)
Business applications with planned wages	−0.016 (0.051)	−0.062 (0.061)	−0.017 (0.094)	+0.002 (0.047)	−0.036 (0.056)
Business applications from corporations	−0.121 (0.065)	−0.092 (0.094)	−0.144 (0.088)	−0.085 (0.058)	−0.155 (0.082)
Business formations within four quarters	−0.032 (0.052)	−0.102 (0.070)	−0.058 (0.097)	−0.019 (0.058)	−0.048 (0.051)
Business formations within eight quarters	−0.042 (0.042)	−0.113 (0.070)	−0.040 (0.126)	−0.020 (0.044)	−0.068 (0.047)

Notes. Panel B: estimators and standard errors as in Panel A. Pre-period January 2013 to June 2017, with July to December 2017 dropped; comparison states are all other states and the District of Columbia except Delaware and Wyoming. Specified in Addendum 1 (item N9) [A1].

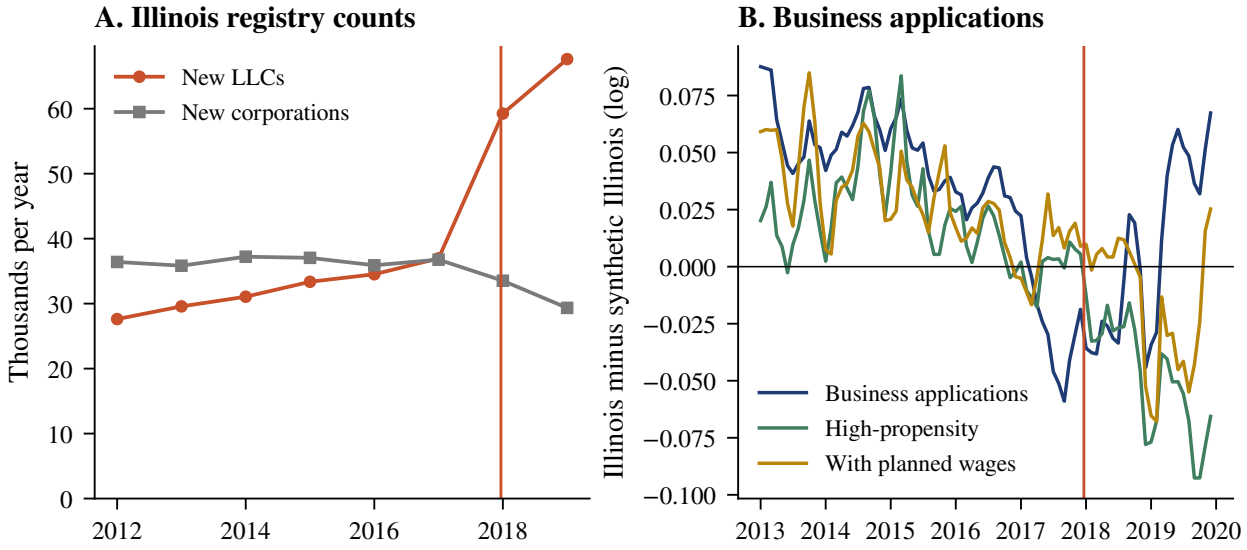


Figure A12. Illinois: Registrations and Business Applications around the Fee Reduction of December 2017

Notes. Panel A: new domestic LLCs and corporations per year, transcribed from the Illinois Secretary of State's quarterly reports. Panel B: Illinois minus synthetic Illinois for three series of applications, as three-month centered averages. The vertical lines mark December 20, 2017. Panel A was not pre-specified [E]. Panel B plots the gap underlying the estimates of Table A52, Panel B [A1]; the figure itself was not pre-specified.

E.10. Auxiliary Study of the Increase in the Report Fee

The plan included an auxiliary study of the increase in the fee for a periodic report from $10 to $25 on July 1, 2024. Section B.4 reports its outcome and explains why we interpret the estimate only as showing no detectable effect of the increase on the reports of established companies (Section 6.5).

Table A53. Reports of Established LLCs around the Increase in the Report Fee from $10 to $25 on July 1, 2024

A. Share of reports filed, by the month in which the report fell due

Report of an established LLC	Anniversary months September to February 2023–24 ($10)	Anniversary months September to February 2024–25 ($25)	Anniversary months January to March 2023 ($10)	Anniversary months January to March 2024 ($10)	Double difference	Standard error ten earlier years	Standard error without 2020 and 2021	*p*
On time	0.821	0.821	0.768	0.825	−0.058	(0.034)	(0.035)	0.134
Before delinquency	0.890	0.887	0.855	0.888	−0.035	(0.019)	(0.020)	0.116
On time, by the number of the report								
Second to fourth	0.790	0.788			−0.067			
Fifth to ninth	0.828	0.821			−0.045			
Tenth or later	0.857	0.844			−0.056			

B. Reports falling due in July and August: timing of filing

Anniversary months July and August of	2019	2020	2021	2022	2023	2024	2025
Reports filed June 17–30 (share)	0.121	0.151	0.153	0.181	0.186	0.191	0.185
Reports filed May 1 to June 16 (share)	0.028	0.038	0.019	0.057	0.029	0.021	0.075
Reports on time (share)	0.784	0.814	0.800	0.820	0.816	0.812	0.821
Entities	17,265	19,568	23,232	26,656	30,648	34,039	37,698

Notes. Domestic LLCs formed before July 1, 2021 that filed the previous year's report on time, excluding bulk groups. Panel A: the double difference is the change between the first two columns minus the change between the next two. Its standard error is the root mean square of the same double difference in ten earlier years, and in eight of those years when 2020 and 2021 are excluded. Panel B: a report may be filed from two months before the anniversary month, so companies with anniversaries in July and August 2024 could file at $10 until June 30; the increase was announced on June 17, 2024. Of those companies, 19.1% filed on June 17–30 in 2024, compared with 18.6% on the same dates a year earlier and 18.5% a year later. The plan's auxiliary study [P], except the row for reports before delinquency and the rows by number of the report [E]. Section B.4 explains why we interpret the estimate only as showing no detectable effect.

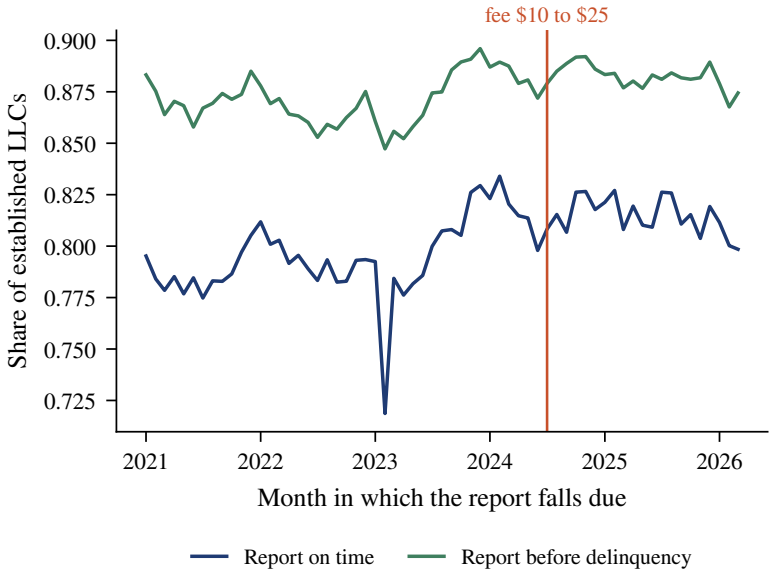


Figure A13. Reports of Established LLCs by the Month in Which the Report Fell Due

Notes. The sample of Table A53. The vertical line marks July 1, 2024, when the fee for a periodic report increased from $10 to $25. The plan's auxiliary study [P].