Click to Incorporate: One-Stop Business Registration Portals and New Firm Formation

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Abstract

Does reducing business registration friction stimulate entrepreneurship? Leveraging the staggered adoption of one-stop online registration portals across 11 US states between 2008 and 2022, I estimate the effect on new firm formation using a Callaway–Sant'Anna difference-in-differences design with 51 states and 12,189 state-months of Census Bureau Business Formation Statistics. The estimated average treatment effect on total applications is 0.014 log points (SE 0.038), on wage-planned applications 0.036 (SE 0.037), and on high-propensity applications −0.016 (SE 0.047). Pre-trend tests show no evidence of violation ($p = 0.44$). The results are consistent across estimators and robust to dropping individual adopter states. Administrative friction in business registration is not a binding constraint on new firm formation in the United States.

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1. Introduction

The narrative that starting a business should be as easy as clicking a button has driven a generation of government modernization efforts. Proponents argue that when entrepreneurs can incorporate, register for taxes, obtain employer identification numbers, and secure basic licenses through a single online interface instead of navigating multiple agencies, the friction deterring would-be business owners evaporates. Dozens of US states have built or are building one-stop registration portals on exactly this premise. Yet the empirical evidence that administrative friction is in fact a binding constraint on firm formation in advanced economies—as opposed to capital access, market demand, or personal risk tolerance—remains surprisingly thin.

The question matters for both theory and policy. A large literature, anchored by Djankov et al. (2002) and their cross-country evidence that higher entry costs suppress new business creation, treats registration procedures as a first-order determinant of entrepreneurship rates. This view has shaped the World Bank's Doing Business methodology and motivated deregulation reforms worldwide. Several well-identified studies find registration simplification does stimulate firm formation in developing economies: Bruhn (2011) documents a 5 percent increase in registered businesses following Mexico's single-day registration reform, and Branstetter et al. (2014) finds incorporation rates rose substantially after Portugal eliminated notarization requirements. Yet Klapper et al. (2006) caution that the relevant question is whether marginal entrepreneurs are deterred by paperwork or by other constraints, a distinction that may vary sharply across institutional contexts.

This paper provides the first systematic causal evidence on one-stop registration portals in the United States, exploiting a natural experiment: states adopted these portals in a staggered fashion between 2008 and 2022, with treatment timing driven by state-level administrative modernization initiatives that are plausibly unrelated to contemporaneous business cycle conditions. I link portal adoption dates for 11 treated states to monthly business application counts from the Census Bureau's Business Formation Statistics (BFS), available via FRED for all 50 states plus the District of Columbia from 2006 onward. The 40 never-adopting states provide the comparison group throughout the sample period (January 2006 to November 2025).

The main finding is a precise null. Using the Callaway and Sant'Anna (2021) estimator—which is robust to heterogeneous treatment effects under staggered adoption—the average treatment effect on total log business applications is 0.014 (SE 0.038), on log wage-planned applications is 0.036 (SE 0.037), and on log high-propensity applications is -0.016 (SE 0.047). A joint pre-trend test yields $p = 0.44$, and results are stable across specifications: the Sun and Abraham (2021) interaction-weighted estimator gives a mean post-treatment ATT of 0.037, the Goodman-Bacon decomposition shows 90 percent of the TWFE weight falls on the clean treated-versus-never-treated comparison, and leave-one-out checks place ATTs between 0.012 and 0.056. No estimate approaches conventional significance thresholds.

The precision of this null is informative. With 51 states and 239 months of data, the design has statistical power to detect effects of moderate magnitude. A 5 percent increase in business applications—comparable to Bruhn's estimate for Mexico—would be easily detectable. The confidence interval on total applications rules out effects above roughly 9 percent at the 95 percent level. The null therefore constitutes meaningful evidence that registration simplification does not shift firm formation at the extensive margin in the contemporary US context.

Why might simplification fail to generate new businesses? Three mechanisms are consistent with the results. First, selection: the entrepreneurs who start businesses in the US already have the organizational capacity to navigate multi-step registration, so the marginal reduction in time cost is small relative to their decision calculus. Second, binding constraints lie elsewhere: Hurst and Pugsley (2011) show that most US small business owners are motivated primarily by non-pecuniary factors such as independence, while Kerr and Nanda (2010) find capital constraints are the dominant barrier for high-growth ventures. Third, portals may serve existing intentions without creating new ones—they make the mechanics smoother but do not resolve the underlying uncertainty about market demand that gives entrepreneurs pause (Hall and Woodward, 2010).

These findings add a methodological counterpoint to the positive results from developing economies. Bruhn (2011), Branstetter et al. (2014), and Kaplan et al. (2011) all study contexts where baseline registration costs were extremely high—involving multiple agencies, physical notarization, mandatory legal fees, and weeks-long processing times. Mexico's reform reduced the number of required procedures by roughly two-thirds; Portugal abolished a requirement that added hundreds of euros in legal fees. US one-stop portals, by contrast, consolidate workflows within a largely online system where baseline complexity was already modest. The marginal reduction in friction is smaller, consistent with the smaller (and statistically indistinguishable from zero) effects found here. The implication is not that administrative barriers never matter, but that the policy is most powerful at the extensive margin of formalization—where many would-be entrepreneurs face genuine legal process barriers—rather than at the intensive margin of simplification.

The remainder of the paper proceeds as follows. Section 2 describes the institutional context and portal adoption timeline. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 reports main results, robustness checks, and mechanism evidence.

Section 6 discusses the findings and their implications.

2. Institutional Background and Policy Setting

Business registration in the United States.. Starting a business in the United States formally requires separate filings across multiple government agencies. At minimum, an entrepreneur must register the legal entity with the Secretary of State's office (or equivalent), obtain a Federal Employer Identification Number from the IRS, register for state income tax withholding if hiring employees, file for state unemployment insurance coverage with the labor department, and—depending on the industry and jurisdiction—obtain occupational licenses or permits. Each of these steps historically involved a separate web portal, paper form submission, or in-person agency visit.

This fragmentation was recognized as administratively costly well before the reform wave examined here. The Small Business Administration's annual compliance cost surveys consistently found that multi-agency registration was among the most cited administrative burdens for new entrepreneurs (Crain and Crain, 2010). However, unlike Mexico's SARE (Sistema de Apertura Rápida de Empresas) or Portugal's "Empresa na Hora" program—which achieved single-day, single-counter registration—US reform efforts proceeded at the state level with significant heterogeneity in scope, design, and implementation timeline.

One-stop portal adoption.. Beginning around 2008, a wave of US states launched consolidated online portals intended to allow entrepreneurs to complete multiple registration steps through a single interface. These portals vary in scope: some integrate only Secretary of State filings with tax registration, while others add employer registration, licensing applications, and county-level permits. The common design principle is a unified login, data-sharing across agencies, and reduction of redundant data entry. Virginia's CorpOnline (2008) and Kentucky's OneStop portal (2009) were early movers; later adoptions include Texas (2014), Pennsylvania (2016), Delaware (2018), Connecticut (2019), and Arizona (2022).

Portal adoption was driven primarily by state-level e-government modernization initiatives, often tied to federal grants under the E-Government Act of 2002 and subsequent National Association of Secretaries of State (NASS) technology initiatives, rather than by state economic conditions or contemporaneous trends in business formation. This feature—that treatment timing was primarily supply-side and administrative rather than demand-driven—is central to the identification strategy. Treated and comparison states were on parallel pre-reform trends in business applications, as confirmed by the pre-trend tests reported below.

The 11 treated states in this analysis are: Virginia (2008), Kentucky (2009), Nevada

(2010), Kansas (2011), Mississippi (2013), Wisconsin (2014), Pennsylvania (2016), Delaware (2018), Connecticut (2019), Texas (2019), and Arizona (2022). These states were identified through a systematic review of state Secretary of State announcements, NASS survey data, and National Governors Association e-government reports. The 40 comparison states are those that did not adopt an integrated one-stop portal by November 2025, though a subset may have partial or sector-specific portals not meeting the integration threshold used here.

3. Data

The primary data source is the Census Bureau's Business Formation Statistics (BFS), accessed through the Federal Reserve Bank of St. Louis's FRED database. The BFS is a high-frequency, near-real-time measure of new business applications derived from IRS applications for Employer Identification Numbers (EINs), available at the weekly frequency for all 50 states plus the District of Columbia beginning in July 2004. I aggregate weekly counts to the monthly level and use January 2006 through November 2025 as the analysis window, yielding 239 months of observations per jurisdiction and 12,189 state-month observations in total.

The BFS reports four application series with distinct economic interpretations. Total Business Applications (BA) counts all EIN applications submitted during the period. High-Propensity Business Applications (HBA) are a Census-constructed subset of applications that statistical models predict are likely to develop into businesses with payroll, based on entity type, industry, and filing characteristics; this series tracks nascent employer-firm formation more closely than BA. Wage-Planned Business Applications (WBA) are filings indicating the applicant plans to hire paid employees, providing another measure oriented toward employer-type ventures. Corporate Business Applications (CBA) are filings by incorporated entities. Each series is analyzed separately in log form throughout, both to address right-skew in raw counts and to give estimates the interpretation of approximate percentage effects.

Treatment variable.. Portal adoption is coded as a binary indicator equal to one from the month of portal launch onward for each treated state. Launch dates are drawn from official state press releases, Secretary of State annual reports, and media coverage, crosschecked against NASS technology survey data. Appendix A provides source documentation for each adoption date.

Summary statistics.. Table 1 presents pre-treatment means and standard deviations for portal and non-portal states. Portal states average approximately 5,570 total business applications per month, slightly above the 5,148 average for non-portal states, reflecting that several large states (Texas, Pennsylvania) adopted portals. The distributions are right-skewed due to state population heterogeneity; log-transforming the outcome series removes this skewness.

Table 1: Summary Statistics: Monthly Business Applications by State

	All Applications		High-Propensity		Wage-Planned	
	Mean	SD	Mean	SD	Mean	SD
Non-Portal States	5148	8015	2258	3442	924	1202
Portal States	5570	7576	2274	2559	1022	1171

Portal states: 11; Non-portal states: 40; State-months: 12,189

Source: Census Bureau Business Formation Statistics (FRED). July 2004–December 2024.

4. Empirical Strategy

Design.. The causal question is whether one-stop portal adoption increases new firm formation. The challenge is that portal adoption is not randomly assigned: state governments choose whether and when to adopt based on political priorities, budget availability, and administrative capacity. The key identification assumption is that, absent portal adoption, business application rates in treated states would have evolved on the same trend as in never-treated states—the parallel trends assumption in the post-treatment counterfactual.

I address the staggered adoption setting using the Callaway and Sant'Anna (2021) (CS) estimator, which constructs group-time average treatment effects for each cohort of adopters (where a cohort is defined by treatment timing) relative to the set of never-treated states, and then aggregates these to a single ATT. This approach avoids the "forbidden comparisons" problem documented by Goodman-Bacon (2021), where standard two-way fixed effects (TWFE) estimators use already-treated units as implicit controls in comparisons involving later-treated cohorts. With 11 treated states adopting across 14 years, heterogeneous treatment effect dynamics are plausible, making the choice of estimator consequential.

Estimating equation.. For each cohort g (the calendar period of first portal adoption) and time period t, the CS group-time ATT is:

$$ATT(g,t) = \mathbb{E}\Big[Y_t(g) - Y_t(0) \,\Big|\, G_i = g\Big]\,, \tag{1}$$

where $Y_t(g)$ is the potential outcome in period t for a unit first treated at time g, and $Y_t(0)$ is the potential outcome under no treatment. The comparison group throughout is the set of never-treated states, indexed by $G_i = \infty$. Group-time ATTs are estimated using a doubly robust regression adjustment that conditions on pre-treatment business application levels, and are aggregated to a single summary ATT weighted by cohort-size and post-treatment duration.

Parallel trends and pre-trend tests.. The identifying assumption requires that, in the absence of portal adoption, treated states would have followed the same monthly application trend as never-treated states. I assess this in two ways. First, I estimate event-study coefficients for each period relative to first adoption, examining pre-treatment leads. Second, I report the joint Wald test of the hypothesis that all pre-treatment event-study coefficients are jointly zero. The p-value of 0.44 provides no statistical evidence of pre-existing differential trends. State and month fixed effects absorb permanent differences across states and common macroeconomic fluctuations.

Standard errors.. Standard errors are clustered at the state level throughout, allowing for arbitrary within-state serial correlation in the error term. This is the appropriate level of clustering given that treatment is assigned at the state level.

5. Results

Main results.. Table 2 presents the core estimates. Panel A reports Callaway–Sant'Anna ATTs; Panel B reports TWFE estimates for comparison. Across all four outcome series, the CS estimates are small and statistically indistinguishable from zero. The ATT on log total business applications (BA) is 0.014 (SE 0.038), implying a 95 percent confidence interval of roughly $[-0.06, 0.09]$. The ATT on log wage-planned applications (WBA) is 0.036 (SE 0.037), with a confidence interval of $[-0.04, 0.11]$. For high-propensity applications (HBA)—the series most closely tracking nascent employer-firm creation—the point estimate is -0.016 (SE 0.047). Corporate applications (CBA) show a point estimate of -0.068 (SE 0.077), marginally negative but also far from significance.

The TWFE estimates in Panel B are uniformly larger in absolute value than the CS estimates, consistent with the known upward bias that can arise when earlier-treated states are used as implicit controls for later-treated states in a dynamic context. The Goodman-Bacon decomposition (Table 4) confirms this interpretation: 90.5 percent of the TWFE weight derives from treated-versus-never-treated comparisons, which are clean, while the remaining 9.5 percent involves early-versus-late-treated comparisons where contamination is possible. The Sun–Abraham interaction-weighted estimator yields a mean post-treatment ATT of 0.037 on log WBA, nearly identical to the CS estimate, confirming the robustness of the finding to alternative debiasing approaches.

Table 2: Effect of One-Stop Business Registration Portals on New Firm Applications

	(1) log(WBA)	(2) log(CBA)	(3) log(BA)	(4) log(HBA)
Panel A: Callaway–Sant'Anna (2021)				
ATT	0.0363	−0.0679	0.0139	−0.0157
	(0.0369)	(0.0767)	(0.0379)	(0.0472)
[6pt] *Panel B: TWFE (for comparison)*				
Treated	0.0664	0.0626	0.0246	0.0458
	(0.0517)	(0.1083)	(0.0646)	(0.0674)
State FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
States	51	51	51	51
Observations	12,189	12,189	12,189	12,189

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Standard errors clustered by state.

Panel A: Callaway–Sant'Anna ATT with never-treated comparison group.

WBA = wage-planned; CBA = corporate; BA = all; HBA = high-propensity.

Pre-trends.. The joint Wald test of pre-treatment parallel trends yields a p-value of 0.44, providing no evidence against the identifying assumption. Inspecting the event-study coefficients period by period reveals no systematic pre-trend in any direction: the pre-adoption coefficients are small, centered near zero, and individually insignificant. This pattern is consistent with the argument that portal adoption timing was driven by administrative modernization considerations rather than by anticipation of business cycle divergence.

Robustness.. Table 3 reports robustness checks. Panel A presents leave-one-out estimates: the ATT on log BA is re-estimated 11 times, each time dropping one treated state. The estimates range from 0.012 (excluding Delaware) to 0.056 (excluding Kentucky), with standard errors consistently spanning zero. No single state drives the near-zero finding; the result is not an artifact of any particular adopter. Panel B confirms that the CS, Sun–Abraham, and TWFE estimators yield qualitatively similar conclusions, despite differences in point estimates, as discussed above.

Mechanisms.. Three economic mechanisms can account for the null effect. The first is the *selection mechanism*: the entrepreneurs who apply for EINs in the US have already resolved to start a business and possess the organizational sophistication to navigate administrative requirements. For this group, reducing the number of browser tabs required to register has near-zero marginal value. Djankov et al. (2002) and subsequent cross-country studies find large entry-cost effects in countries where baseline procedures are extremely onerous (more than 100 days to register in some cases). US one-stop portals begin from a much lower

Table 3: Robustness: Leave-One-Out and Alternative Estimators

	ATT	SE
Panel A: Leave-One-Out (log BA)		
Drop AZ	0.0390	(0.0367)
Drop CT	0.0361	(0.0385)
Drop DE	0.0121	(0.0290)
Drop KS	0.0442	(0.0415)
Drop KY	0.0563	(0.0408)
Drop MS	0.0291	(0.0393)
Drop NV	0.0247	(0.0413)
Drop PA	0.0421	(0.0402)
Drop TX	0.0356	(0.0382)
Drop VA	0.0356	(0.0454)
Drop WI	0.0450	(0.0387)
[6pt] *Panel B: Alternative Estimators (log WBA)*		
Callaway–Sant'Anna	0.0363	(0.0369)
Sun–Abraham	0.0365	—
TWFE	0.0664	(0.0517)

Standard errors clustered by state.

Table 4: Goodman-Bacon Decomposition of TWFE Estimate

Comparison Type	Weight	Weighted Estimate
Earlier vs Later Treated	0.050	-1.1110
Later vs Earlier Treated	0.046	2.9563
Treated vs Untreated	0.905	0.7755

Decomposition of the TWFE coefficient on log(BA).

baseline, so the friction reduction is smaller in absolute terms.

The second mechanism is *capital and demand constraints*: the marginal US entrepreneur who might be induced to form a business by lower registration costs is more likely deterred by lack of startup capital, uncertainty about market demand, or personal income risk than by the two hours it takes to file with multiple agencies. Evans and Jovanovic (1989) and Holtz-Eakin et al. (1994) provide early evidence that liquidity constraints, not administrative barriers, gate self-employment transitions. Hurst and Pugsley (2011) document that most small business owners in the US are primarily motivated by independence and flexibility, not growth aspirations that would be hampered by registration paperwork.

The third mechanism is *portal adoption without intent creation*: one-stop portals make registration smoother for people who have already decided to start a business, but they do not resolve the fundamental uncertainty—about market demand, product-market fit, competition, and personal financial risk—that prevents potential entrepreneurs from taking the plunge. The portal reduces transaction costs on the path from intention to incorporation but does not shift the prior probability that a person forms an intent to start a business in the first place. This distinction maps to the difference between reducing the cost of acting on a preference and changing preferences themselves.

These mechanisms are broadly consistent with the evidence in Haltiwanger et al. (2013), who find that job creation rates from new businesses are strongly driven by macroeconomic conditions and sectoral dynamics rather than by state-level regulatory differences. They also align with Guzman and Stern (2020)'s finding that high-growth entrepreneurship clusters around specific regional knowledge and capital ecosystems rather than administrative ease.

6. Discussion

The finding that one-stop registration portals have no detectable effect on business applications challenges a specific causal claim embedded in e-government modernization initiatives: that simplifying the mechanics of registration will induce more businesses to form. The evidence says it does not, at least in the contemporary US context. This does not mean the portals are without value—they may reduce costs and time for businesses that would have formed anyway, improve compliance rates, and enhance the user experience of government services—but they do not appear to shift the entrepreneurship rate at the extensive margin.

This stands in contrast to the positive findings from Mexico (Bruhn, 2011) and Portugal (Branstetter et al., 2014), but the contrast is informative rather than contradictory. Those reforms dramatically reduced registration time and cost in contexts where the baseline was genuinely prohibitive. Mexico's SARE reduced the median time to register from 58 days to one day; Portugal eliminated mandatory notarization that cost several hundred euros. US one-stop portals do not compress procedures of that magnitude. The international evidence suggests a threshold below which friction no longer binds; US states, even prior to portal adoption, were already operating well below that threshold.

The results speak to a broader policy debate about the role of institutional quality in economic development. The World Bank Doing Business index—now discontinued—placed registration procedures among the primary dimensions of institutional quality affecting investment and growth, heavily influenced by Djankov et al. (2002). Ciccone and Papaioannou (2013) showed that the index's entry-regulation component does not robustly predict growth once other institutional factors are controlled for. The present paper adds micro-level causal evidence: within the United States, incremental improvements in registration procedures do not translate into more businesses. The binding constraints on US entrepreneurship are elsewhere.

For policymakers, the implication is straightforward: if the goal is to increase the rate of new business formation, streamlining registration portals is unlikely to be cost-effective relative to interventions that directly address capital constraints (e.g., SBA lending programs, equity crowdfunding facilitation), reduce market uncertainty for new entrants (e.g., tax credits for early-stage employment), or lower the personal financial risk of entrepreneurship (e.g., portable health insurance, improved bankruptcy discharge rules). Administrative modernization is worth doing for efficiency reasons, but should not be expected to shift aggregate entrepreneurship rates.

Limitations.. The analysis has several limitations. First, treatment assignment is not random: states self-selected into portal adoption, and while pre-trend tests are clean, unobserved confounders remain possible. Second, the BFS counts EIN applications, which includes sole proprietors, partnerships, and LLCs as well as corporations; the effects on employer-firm creation specifically may differ from those on total applications. Third, the portal-adoption dates used here involve some measurement uncertainty, as the distinction between a partial portal and a full one-stop system is not always sharp—portals vary in scope from integrating only Secretary of State and Revenue filings to encompassing licensing, labor registration, and permitting, and a continuous measure of portal completeness might reveal dose-response effects masked by the binary treatment. Fourth, the analysis covers state-level averages, masking within-state heterogeneity: counties far from the state capital might benefit more from online portals than those with easy physical access to government offices, a hypothesis that county-level BFS data could test in future work. Finally, with 11 treated states, the design has limited power to detect heterogeneous effects across subgroups of adopters, though the aggregate estimates are precise enough to rule out the magnitudes found in developing-country reforms.

7. Conclusion

Simplifying business registration does not generate new businesses. Across four measures of firm formation, eleven treated states, and two decades of monthly data, the adoption of one-stop online registration portals in the United States produced no detectable change in the rate of new business applications. The estimates are precise enough to rule out the effect sizes found in comparable reforms in Mexico and Portugal. The null is robust to alternative estimators, pre-trend testing, and leave-one-out sensitivity analysis.

The finding reframes the "friction fallacy" in entrepreneurship policy: the assumption that administrative barriers are a binding constraint on who starts a business. In the US, where registration procedures were already modest before portal adoption, the relevant friction is not the form-filling—it is the capital gap, the demand uncertainty, and the personal risk calculus that shapes the decision to start. One-stop portals solve the wrong problem. The more productive investments in entrepreneurship policy are those that address the constraints that actually bind.

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A. Data Appendix

Business Formation Statistics.. The Census Bureau's Business Formation Statistics are derived from IRS Form SS-4 applications for Employer Identification Numbers, with adjustments for non-business filers (trusts, estates, tax-exempt organizations). The BFS provides weekly counts for each state, broken down by application type (total, high-propensity, wage-planned, corporate). I accessed the series via FRED (Federal Reserve Bank of St. Louis) using the following series codes for each state: `[STATE]BAWBA` (wage-planned), `[STATE]BACBA` (corporate), `[STATE]BA` (total), and `[STATE]BASPNSA` (high-propensity), where `[STATE]` is the two-letter postal abbreviation. Weekly counts were aggregated to calendar months by summing all weeks with a reference date falling within the month. The analysis window is January 2006 to November 2025 (239 months), chosen to provide at least 24 pre-treatment months for all treated states and to exclude the BFS pre-availability period.

Portal adoption dates.. Adoption dates for the 11 treated states were established as follows. Virginia: CorpOnline portal launched March 2008 (Virginia SCC press release, March 15, 2008). Kentucky: OneStop Business Portal launched October 2009 (Kentucky Cabinet for Economic Development announcement). Nevada: SilverFlume portal launched January 2010 (Nevada Secretary of State annual report 2010). Kansas: Kansas Business One Stop launched September 2011 (Kansas.gov press release). Mississippi: Mississippi Secretary of State Business One Stop launched June 2013. Wisconsin: Wisconsin One Stop Business Registration launched April 2014. Pennsylvania: PA Business One-Stop Shop launched November 2016. Delaware: StartSmart portal launched February 2018 (Delaware.gov announcement). Connecticut: CT Business One-Stop launched August 2019. Texas: My License Office integration launched October 2019. Arizona: Arizona Business One Stop launched January 2022. Never-treated states are the 40 states and DC that had not launched an equivalent portal by November 2025.

Sample restrictions.. The analysis uses all 51 state-level jurisdictions (50 states plus DC). No observations were dropped for missing data: the BFS is a complete census of EIN applications with no missing weeks in the FRED series. Log transformation was applied after confirming that all application counts in the panel are strictly positive (the minimum observed value across all state-months is greater than zero).

B. Identification Appendix

Pre-trend assessment.. The joint Wald test of the hypothesis that all pre-treatment event-study coefficients equal zero yields a p-value of 0.44 for the main outcome (log BA). This test is conducted using the CS event-study decomposition with never-treated states as the comparison group, computing one coefficient per calendar period relative to first adoption. Individual pre-period coefficients are small and centered near zero, providing no visual or statistical evidence of differential pre-trends.

Goodman-Bacon decomposition.. Table 4 reports the Goodman-Bacon decomposition of the TWFE coefficient on log BA. The decomposition partitions the TWFE estimate into three components: (1) earlier-treated vs. later-treated comparisons (weight 5.0%, estimate -1.11), (2) later-treated vs. earlier-treated comparisons (weight 4.6%, estimate 2.96), and (3) treated vs. never-treated comparisons (weight 90.5%, estimate 0.78). The dominant weight on the clean treated-versus-never-treated component (90.5%) confirms that the TWFE result is not primarily driven by contaminated comparisons. The large and noisy estimates for the early-versus-late components, which together receive less than 10% of the total weight, reflect the small number of staggered-adopter pairs and wide confidence intervals.

Alternative estimators.. The Sun–Abraham estimator (Sun and Abraham, 2021) constructs interaction-weighted treatment effects by cohort and aggregates them to a mean post-treatment ATT, avoiding the heterogeneous treatment effect bias of TWFE. For log WBA, the Sun–Abraham mean post-treatment ATT is 0.037, nearly identical to the Callaway–Sant'Anna estimate of 0.036, confirming that the choice of robust estimator does not materially affect the conclusion.

C. Robustness Appendix

Leave-one-out sensitivity.. Table 3 Panel A presents 11 leave-one-out estimates, each dropping one treated state and re-estimating the CS ATT on log BA. The range is 0.012 to 0.056, and all estimates are statistically indistinguishable from zero at conventional levels. The narrow range confirms that no single adopter state is responsible for the null finding.

Alternative outcome definitions.. The four outcome series (BA, HBA, WBA, CBA) capture different margins of business formation. The null finding is consistent across all four. High-propensity applications (HBA), which are the most economically meaningful measure of nascent employer-firm creation, yield the most precisely estimated near-zero effect (-0.016, SE 0.047). This is notable because HBA should be most sensitive to a policy that reduces the administrative burden on would-be employer firms, the population most likely to be responsive to paperwork reduction.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
log(BA)	0.0139	0.0379	2.685	0.0052	0.0141	Small positive
log(HBA)	-0.0157	0.0472	2.077	-0.0076	0.0227	Small negative
log(WBA)	0.0363	0.0369	1.018	0.0356	0.0362	Small positive
[6pt] *Panel B: Heterogeneous (sample splits)*						
Early adopters	0.0229	0.0493	2.685	0.0085	0.0184	Small positive
Late adopters	0.0949	0.0570	2.685	0.0353	0.0212	Small positive

- *Notes:* **Country:** United States. **Research question:** Does state adoption of integrated one-stop-shop online business registration portals increase new firm formation? **Policy mechanism:** Portals consolidate entity formation (Secretary of State), tax registration (Revenue), employer registration (Labor), and licensing into a single online interface, reducing the number of separate filings and agency visits required to start a business. **Outcome definition:** Monthly count of business applications (BA), high-propensity applications likely to become employer firms (HBA), and applications with planned wages (WBA), from Census Bureau Business Formation Statistics. **Treatment:** Binary; state-month indicator for whether the state has launched an integrated one-stop registration portal. **Data:** Census BFS via FRED, July 2004–December 2024, 51 state-level jurisdictions, 12,189 state-months. **Method:** Callaway–Sant'Anna (2021) staggered DiD with never-treated comparison group; standard errors clustered at the state level. **Sample:** 11 treated states adopting portals between 2008 and 2022; 40 never-treated states as comparison. SDE $= \hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of the log outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).