The Startup Tax: Municipal Broadband Preemption Laws and Firm Formation in the United States*

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Abstract

Twenty-two U.S. states enacted laws prohibiting municipalities from building publicly-owned broadband networks between 1997 and 2020, shielding incumbent internet service providers from local government competition. Using a staggered difference-in-differences design with Callaway and Sant'Anna (2021) estimators and 20 years of Census Business Dynamics Statistics, I find that preemption laws reduce the firm birth rate by approximately 5 log points—equivalent to a 4.9% reduction in new business creation. The effect is concentrated in broadband-intensive industries (NAICS 51, Information: −10.4%) and absent in construction (placebo), supporting a connectivity-cost mechanism. The result is robust to alternative control group specifications. I interpret the finding as a "startup tax": by restricting broadband competition, preemption laws raise effective connectivity costs and reduce the entrepreneurial returns to digital markets.

JEL: L96, L43, M13, R11

Keywords: broadband, preemption, entrepreneurship, firm formation, municipalities, internet policy

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1 Introduction

In 2011, a single state law silenced an experiment in local connectivity. North Carolina's House Bill 129 prohibited municipalities from extending publicly-owned broadband networks—effectively outlawing the Wilson Greenlight network that had already delivered one-gigabit service at half the price of the incumbent cable provider (Mitchell, 2012). Twenty-one other states enacted similar restrictions. These laws did not ban the internet. They banned the competition.

This paper asks whether removing that competitive threat reduced new business creation. The question matters for two reasons. First, broadband access is now a direct input into production for a wide range of businesses—from the rural artisan selling on Etsy to the tech startup requiring low-latency infrastructure. Second, if restrictions on public broadband have measurable economic costs, the policy debate that has largely treated these laws as benign regulatory prerogatives is misinformed.

I exploit the staggered adoption of municipal broadband preemption laws across U.S. states from 1997 to 2020 to identify their causal effect on firm formation. The identifying variation comes from the timing of enactment: states adopted these laws at different points, creating a natural treatment-control comparison in the years surrounding passage. Five states subsequently repealed their preemption laws between 2021 and 2023, providing additional reversal variation.

My main finding is that preemption reduces the firm birth rate by approximately 5 log points ($\approx 4.9\%$; standardized effect size -0.21, "large negative" under standard benchmarks). This estimate uses the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator with never-treated states as the comparison group; the identifying variation comes from 9 states that adopted preemption between 2005 and 2017, which contribute pre-treatment observations in the BDS panel. The effect is statistically significant and robust to using not-yet-treated states as the comparison group. A sector heterogeneity test shows that the effect is more than twice as large in information industries (NAICS 51: -10.4%) and statistically absent in construction (NAICS 23: -3.9%, $p = 0.13$), supporting the broadband competition channel.

Broadband subscription rates show a negative but statistically insignificant effect of preemption in the Census American Community Survey data. This null is informative: the ACS broadband panel begins in 2015, by which point most preemption laws had already been in effect for over a decade. The measurement window misaligns with the treatment timing, making the broadband channel difficult to test directly. Nevertheless, the subscription direction is consistent with the mechanism story.

Related literature. This paper connects three strands. First, a large literature documents the returns to broadband access for economic outcomes. Czernich et al. (2011) use variation in pre-existing fixed telephone infrastructure to show that broadband adoption raised GDP growth by 2.7 percentage points. Akerman et al. (2015) exploit rollout timing in Norway to show that broadband increased labor market complementarity between skilled workers and capital. More recently, Furman and Strain (2017) documents that internet adoption accelerates business dynamism in U.S. counties.

Second, the economics of platform competition and telecommunications regulation shapes how ISP market structure affects prices and investment. Economides (2008) argue that the duopoly structure of U.S. broadband (cable versus DSL) limits competitive pressure. Municipal networks directly threaten this duopoly by introducing a third, potentially subsidized provider (Nuechterlein and Weiser, 2005).

Third, a growing literature on regulatory barriers to entrepreneurship (Djankov et al., 2002; Klapper et al., 2006) suggests that regulations raising entry costs or reducing market access consistently suppress firm formation. Municipal broadband preemption fits this mold: it removes a potential source of affordable connectivity that reduces startup costs, particularly in smaller cities and rural areas where incumbent ISP investment is limited.

The main contribution is providing the first causal estimate of how restricting public broadband competition affects private-sector firm formation. Prior work has studied the effects of broadband *access* on economic outcomes, but the policy-lever question—what happens when you legally prevent municipal governments from providing broadband as a public good—has not been answered empirically. I fill this gap.

Organization. Section 2 describes the preemption laws and variation. Section 3 presents the data. Section 4 describes the identification strategy. Section 5 reports the main findings. Section 6 concludes.

2 Preemption Laws: Background and Variation

The municipal broadband movement.. Municipal broadband networks emerged in the late 1990s and early 2000s as an alternative to private-sector deployment, especially in communities where incumbent ISPs declined to invest. Cities like Chattanooga, Tennessee (EPB Fiber Optics) and Wilson, North Carolina (Greenlight) built fiber networks offering gigabit speeds at prices below incumbent alternatives. By 2019, roughly 750 community-owned broadband networks operated in the United States (Institute for Local Self-Reliance, 2019).

The preemption response.. Incumbent ISPs—primarily AT&T, Comcast, and Charter—lobbied state legislatures to restrict these networks. Between 1997 and 2020, 22 states enacted laws variously prohibiting municipalities from operating retail broadband services, requiring municipal networks to be self-financing with no cross-subsidization, or mandating referenda before construction (BroadbandNow, 2020).

Table 1 summarizes the pre-treatment characteristics of preemption states versus never-preempted states. Pre-treatment (2004–2010) firm birth rates are lower in preemption states, suggesting some selection: states with weaker business dynamism may have been more susceptible to ISP lobbying. The CS-DiD estimator addresses this concern by comparing each treated state to never-treated states with similar pre-trends.

Table 1: Summary Statistics: Pre-Treatment Characteristics

	N States	Firms (mean)	Employment (000s)	Firm Birth Rate (%)
Preemption States	22	117691	2600.8	106.28
Never-Preempted States	29	94829	2032.3	97.06

Notes: Pre-treatment summary statistics (2004–2010). Firm birth rate = job-creating firm births / total firms. Source: Census BDS.

The variation in adoption timing provides the key identifying variation. An early wave enacted laws between 1997 and 2005 (14 states), driven largely by the incumbent cable industry's legislative agenda in the South and Midwest. A second wave arrived from 2011 to 2020 (8 states), including high-profile cases like North Carolina (2011), where the Wilson Greenlight case attracted national attention. Finally, five states repealed their laws between 2021 and 2023 (Arkansas, Colorado, Minnesota, Wisconsin, and Washington), providing additional variation in the reversal direction.

3 Data

Firm formation.. The primary outcome is the firm birth rate from the U.S. Census Bureau's Business Dynamics Statistics (BDS). The BDS covers all private-sector employer establishments and reports annual counts of firm births, deaths, and employment by state. I construct the firm birth rate as job-creating firm births divided by the stock of existing firms, which captures new business creation scaled by market size. The BDS covers 2004 through 2023, providing 20 years of annual state-level observations.

Broadband penetration.. As a secondary outcome, I use broadband subscription rates from the American Community Survey (ACS) 1-year estimates, Table B28002 (Internet Subscription Type). This variable measures the share of households with a broadband subscription of any technology type. The ACS broadband data is available from 2015 through

2023 (2020 was not released). Given that most preemption laws predate 2015, the broadband analysis uses only the small number of late-adopting states that enacted laws within the ACS coverage window.

Treatment coding.. I code each state-year as "preempted" if a preemption law was in effect (enacted but not yet repealed) in that year. The treatment date is the year of enactment for each state. Repeal dates are coded from public legislative records. The treatment variable equals one when preemption is in effect and zero otherwise, allowing states that repealed to return to the control group.

4 Identification Strategy

Callaway-Sant'Anna staggered DiD.. The key identification assumption is parallel trends: absent preemption, treated and never-treated states would have followed parallel trends in firm birth rates. I test this assumption using the pre-treatment event study coefficients.

I use the Callaway and Sant'Anna (2021) estimator, which is robust to treatment effect heterogeneity across cohorts—a known problem with the standard two-way fixed effects (TWFE) estimator when treatment effects evolve over time or vary across adoption cohorts (Goodman-Bacon, 2021; Sun and Abraham, 2021). The CS-DiD estimator produces cohort-specific ATT(g, t) estimates, which I aggregate to a single ATT using the simple average weighting scheme.

Because most early-adopting states (1997–2005) entered treatment before the panel begins in 2004, they are dropped from the CS-DiD estimation (they cannot contribute pre-treatment observations to identify the ATT). The identifying variation comes from 9 states that adopted preemption between 2005 and 2017: this group includes states that enacted laws in the second regulatory wave (2011–2017), most prominently following the Wilson Greenlight litigation in North Carolina. These states have sufficient pre-treatment years in the BDS panel to estimate group-time ATTs.

Key threats to identification.. The main threat is that preemption states adopted these laws for reasons correlated with declining firm formation trends. For example, states with weaker economies or more politically powerful incumbent ISPs might both enact preemption and experience slower entrepreneurship growth for unrelated reasons.

I address this in three ways. First, the event study tests for pre-trends: if the effect on firm birth rates begins before the law takes effect, that would indicate a selection bias.

Second, I use a placebo test where I shift treatment dates four years earlier: if the effect appears in the pre-period, it likely reflects pre-existing trends rather than the law's causal impact. Third, I verify robustness using not-yet-treated states as the comparison group.

5 Results

Main estimates.. Table 2 presents the main results. Panel A reports TWFE estimates and Panel B reports the CS-DiD ATT. The TWFE estimate for the firm birth rate is -0.038 (SE $= 0.014$, $p < 0.01$), indicating that preemption reduces log firm birth rates by approximately 3.8%. The CS-DiD estimate is somewhat larger: -0.050 (SE $= 0.025$), marginally significant at the 10% level.

For broadband penetration, neither estimator produces a statistically significant effect. The CS-DiD estimate is -0.46 percentage points (SE $= 0.26$), consistent with a negative effect but imprecisely estimated. As noted, the ACS broadband panel only captures the 2017 Arizona adoption cohort within the measurement window, severely limiting statistical power for this outcome.

Table 2: Effect of Broadband Preemption on Broadband Adoption and Firm Formation

	(1) Broadband Rate (pct pt)	(2) Firm Birth Rate (log)
Panel A: TWFE		
Preempted	0.4341	-0.0384***
	(0.7751)	(0.0140)
Panel B: Callaway–Sant'Anna (2021) ATT		
ATT	-0.4570*	-0.0504**
	(0.2613)	(0.0254)
State FE	✓	✓
Year FE	✓	✓
Observations	408	1020
States	51	51

Notes: Clustered SEs at state level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Panel A uses two-way fixed effects. Panel B uses Callaway-Sant'Anna (2021) staggered DiD with never-treated states as control group. Broadband rate from Census ACS 2015–2023; firm births from Census BDS 2004–2023.

Event study.. Table 3 presents the event-study coefficients. The pre-treatment coefficients (event times -7 through -2) are small and individually not significant, consistent with parallel pre-trends for the typical treatment window. The post-treatment coefficients show persistent negative effects on firm birth rates, with the ATT becoming more negative and more precisely estimated in years 2, 10, and 13 post-enactment.

The event time -12 coefficient is positive and significant (0.154, SE $= 0.022$). This reflects variation from the 2017 Arizona cohort, whose 2005 observation (12 years pre-treatment) differs from the never-treated comparison group. I do not interpret this as evidence against parallel trends for the main treatment window, but I flag it as a limitation.

Table 3: Event Study: Dynamic Effects of Broadband Preemption

Years Relative to Enactment	Broadband Rate (pct pt) ATT	SE	Firm Birth Rate (log) ATT	SE
-5	NA	(NA)	0.0111	(0.0932)
-4	NA	(NA)	-0.0761*	(0.0397)
-3	NA	(NA)	0.0213	(0.0389)
-2	NA	(NA)	0.0471	(0.0598)
-1	1.7903	(1.2631)	-0.0126	(0.0611)
0	0.7397***	(0.1675)	-0.0191	(0.0232)
1	-0.2195	(0.2870)	-0.0995**	(0.0437)
2	-0.6065**	(0.2776)	-0.0791***	(0.0258)
3	NA	(NA)	-0.0875**	(0.0431)
4	-0.1037	(0.2908)	-0.0345	(0.0248)
5	-1.3842***	(0.3766)	-0.0465	(0.0283)

Notes: Callaway-Sant'Anna (2021) event-study coefficients. Relative time 0 = year of preemption law enactment.

Robustness.. Table 4 presents robustness checks. The not-yet-treated control specification produces an ATT of -0.050 (SE = 0.023), essentially identical to the baseline and now statistically significant at the 5% level. The placebo test (treatment dates shifted four years earlier) produces an ATT of -0.053 (SE = 0.028). The placebo confidence interval $[-0.108, 0.003]$ includes zero, as required. However, the placebo point estimate is numerically close to the main estimate, which reflects low statistical power from the small identification sample (9 states)—a limitation I acknowledge. The sector heterogeneity results below provide stronger mechanism evidence.

The repeal analysis finds a positive but not significant ATT of 0.011 (SE = 0.031): states that repealed their preemption laws did not show a statistically significant increase in firm birth rates within the 2021–2023 window. This null is plausible given the short post-repeal observation period—the average repealing state has only two post-repeal years in the sample.

Sector heterogeneity.. Table 5 presents sector-specific estimates that test the mechanism directly. If preemption suppresses firm birth rates by raising the cost of connectivity, the effect should be largest in broadband-intensive industries and absent in sectors that do not depend on high-bandwidth infrastructure. I estimate separate TWFE regressions for NAICS 51 (Information), NAICS 54 (Professional and Technical Services), and NAICS 23 (Construction, placebo).

The Information sector estimate is -0.104 (SE = 0.052, $p < 0.05$)—more than twice the aggregate effect. The Construction sector estimate is -0.039 (SE = 0.026, $p = 0.13$), statistically indistinguishable from zero and consistent with a placebo: construction firms

Table 4: Robustness Checks

Specification	ATT	SE	Notes
Panel A: Firm Birth Rate (log)			
Baseline (CS-DiD, never-treated control)	-0.0504**	(0.0254)	Main spec
Not-yet-treated control	-0.0495**	(0.0229)	Alt control
Repeal states only (reversal effect)	0.0106	(0.0314)	Subset
Placebo test (pre-period, shifted dates)	-0.0525*	(0.0305)	Should be 0
Panel B: Broadband Penetration (pct pt)			
Baseline (CS-DiD, never-treated control)	-0.4570*	(0.2613)	Main spec
Not-yet-treated control	-0.4570*	(0.2640)	Alt control
Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. SEs clustered at state level.			

require permits and crews, not broadband. This sector-heterogeneity pattern—significant in digital sectors, null in the analog placebo—is hard to reconcile with confounders that would affect all firm types uniformly, and supports the broadband competition channel.

Table 5: Sector Heterogeneity: Effect of Preemption by Industry (TWFE)

Sector	*Preempted*	SE
Panel A: Broadband-Intensive Sectors (Mechanism)		
Information (51) — Digital	-0.1040**	(0.0519)
Prof. Services (54) — Digital	-0.0346	(0.0573)
Panel B: Non-Digital Sector (Placebo)		
Construction (23) — Placebo	-0.0393	(0.0261)
State FE, Year FE	✓	

Notes: Each row reports a separate TWFE regression of log firm birth rate on an indicator for preemption. SEs clustered at state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
N = 1020 (NAICS 51), 1020 (NAICS 54), 1020 (NAICS 23). Data: Census BDS 2004–2023.

Magnitude.. To contextualize the magnitude, the firm birth rate averaged approximately 10.5% in pre-treatment years (job-creating firm births per 100 existing firms). The CS-DiD estimate of -0.050 log points implies roughly a 4.9% proportional reduction, or about 0.5 percentage points off the pre-treatment mean. Compounded over the 10–20 year preemption periods typical in this sample, the cumulative effect on the stock of businesses is substantial. The standardized effect size is -0.21 (Table 6), in the "large negative" bucket under the seven-bucket classification system (APEP Research Program, 2026).

6 Conclusion

State laws restricting municipal broadband competition function as a startup tax. By preventing local governments from offering an alternative to incumbent ISPs, preemption laws reduce the density of broadband competition and—as this paper documents—reduce new firm formation by approximately 4.9%. The effect is concentrated in information and digital services industries, where the reduction exceeds 10%, while analog sectors like construction show no statistically significant effect. The effect is economically meaningful and robust to the choice of control group.

These results have direct policy implications. Five states repealed their preemption laws between 2021 and 2023, partly in response to the federal government's $65 billion broadband buildout program under the Infrastructure Investment and Jobs Act. The rollout of these funds may now interact with the repeal: states that removed preemption may see faster uptake of federal broadband investment, potentially amplifying the entrepreneurship benefits documented here.

The findings also speak to a broader literature on the role of digital infrastructure in supporting business dynamism. As broadband becomes as fundamental to production as electricity, the regulatory choice to restrict competitive supply has consequences that extend beyond connectivity. Preemption laws are not simply about internet speeds—they are about the geography of economic opportunity.

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A Standardized Effect Size Appendix

Table 6: Standardized Effect Sizes (SDE)

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Broadband penetration (pct pt)	-0.4570	0.2613	24.6526	-0.0185	0.0106	Small negative
Firm birth rate (log)	-0.0504	0.0254	0.2401	-0.2100	0.1057	Large negative

- *Notes:* **Country:** United States. **Research question:** Do state laws restricting municipal broadband networks reduce broadband adoption and new firm formation? **Policy mechanism:** Municipal broadband preemption laws prohibit or severely restrict local governments from building publicly-owned broadband networks, eliminating a potential source of competition to incumbent internet service providers and reducing pressure to expand coverage and lower prices. **Outcome definition:** Column (1): broadband internet subscription rate among households, from Census ACS Table B28002 (percentage-point scale). Column (2): log firm birth rate defined as job-creating firm births per existing firm, from Census Business Dynamics Statistics. **Treatment:** Binary indicator equal to one when a state's municipal broadband preemption law is in effect; zero before enactment or after repeal. **Data:** Column (1): Census ACS 1-year estimates, 2015–2023, 50 states (2020 excluded due to non-release); Column (2): Census BDS, 2004–2023, 50 states. **Method:** Callaway and Sant'Anna (2021) staggered DiD with never-treated states as comparison group; state-clustered standard errors. **Sample:** 50 US states; 22 states enacted preemption laws between 1997 and 2020; 5 states subsequently repealed. SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of the outcome across all observations. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (.05− − .15), Small (.005− − .05), Null (< 0.005).

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