Dial Tone Roulette: The FCC Cellular Lottery and Local Economic Development

APEP Autonomous Research* @SocialCatalystLab

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Abstract

In the 1980s, the FCC allocated cellular licenses across 734 market areas by lottery. I exploit the alphabetical processing order of Rural Service Area lotteries, which created staggered treatment timing across states during 1987–1989, to estimate the causal effect of early cellular service on local economic activity. Using County Business Patterns data and a Callaway–Sant'Anna estimator, I find precisely estimated null effects on employment (−1.1%, SE = 1.4%), establishments (0.1%, SE = 0.8%), and payroll (0.6%, SE = 1.3%). Results survive alternative estimators, clustering levels, and functional forms. The null persists across sectors, though manufacturing shows a marginally significant decline. Early cellular service—voice-only, used by fewer than 2% of Americans—was economically irrelevant at the county level, contrasting with transformative effects documented for later mobile broadband.

JEL Codes: L96, O33, R11

Keywords: telecommunications, cellular lottery, infrastructure, local economic development, difference-in-differences

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 50m).

1. Introduction

When the FCC decided how to allocate the first cellular telephone licenses in the United States, it chose a lottery. Between 1984 and 1989, the agency processed over 400,000 applications for roughly 1,400 licenses across 734 Cellular Market Areas, selecting winners by random draw (Hazlett, 1998). The stakes were enormous: a single cellular license could be worth hundreds of millions of dollars within a decade (McMillan, 1994). Yet the economic consequences of this grand natural experiment for the communities that received cellular service have never been rigorously evaluated.

This paper asks whether the introduction of cellular telephone service caused measurable improvements in local economic activity. Mobile telecommunications infrastructure has since become central to economic life, and a growing literature documents substantial effects of mobile broadband on employment, entrepreneurship, and productivity (Akerman et al., 2015; Hjort and Poulsen, 2019; Guriev et al., 2021). But these studies examine modern networks with data capabilities and near-universal adoption. The first-generation cellular systems of the late 1980s were voice-only, prohibitively expensive (handsets cost \$3,000–\$4,000, monthly bills exceeded \$100), and used by fewer than 2% of Americans by 1990 (Hausman, 1999). Whether this early, elite technology generated broader economic spillovers is an open empirical question.

I exploit a specific feature of the FCC's lottery process to construct a credible research design. The Rural Service Area (RSA) cellular lotteries, covering CMAs 307–734, were processed in CMA-number order. Because CMA numbers were assigned alphabetically by state name, Alabama's rural markets were processed before Wyoming's. This alphabetical ordering created staggered treatment timing across states: states early in the alphabet received their RSA cellular licenses in 1987, while states later in the alphabet waited until 1988 or 1989. Since the ordering is a function of state name—not economic conditions—it provides plausibly exogenous variation in the timing of cellular service introduction.

Using county-level employment, establishment, and payroll data from the Census Bureau's County Business Patterns (1986–2005), I estimate the effect of cellular service on local economic outcomes using the Callaway and Sant'Anna (2021) difference-in-differences estimator. This heterogeneity-robust method is appropriate for the staggered adoption setting, where standard two-way fixed effects regressions can produce biased estimates due to negative weighting of treatment effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020).

The main finding is a precisely estimated null. The aggregate ATT on log employment is -0.011 (SE $= 0.014$, $p = 0.45$), on log establishments is 0.001 (SE $= 0.008$, $p = 0.86$), and on log annual payroll is 0.006 (SE = 0.013, $p = 0.64$). The 95% confidence interval for the employment effect rules out positive effects larger than 1.7%. These estimates are stable across TWFE benchmarks, alternative clustering levels, and level specifications. An event study shows no evidence of differential pre-trends, with the single estimable pre-treatment coefficient of 0.008 ($p = 0.67$).

Sectoral analysis reveals heterogeneity that, while imprecise, is consistent with the null aggregate finding. The only individually significant sector effect is for manufacturing (−0.116, $p < 0.05$), with services, retail, and FIRE showing small and insignificant coefficients. The manufacturing decline is difficult to attribute to cellular technology and may reflect broader deindustrialization trends.

This paper contributes to the literature on telecommunications infrastructure and economic development. While Röller and Waverman (2001) document a positive association between telecommunications investment and GDP growth across countries, and Jensen (2007) and Aker (2010) show that mobile phones reduced price dispersion in developing-country markets, these studies examine settings where mobile technology represented a quantum leap in communications access. The US cellular lottery occurred in a country already saturated with landline telephones, where the marginal value of mobile voice service was largely convenience rather than connectivity (Hausman, 1997). The null result is thus consistent with a threshold model: telecommunications infrastructure generates economic spillovers only when it enables fundamentally new forms of economic activity—as mobile broadband later would—rather than merely offering a portable substitute for existing technology.

The paper also contributes methodologically to the literature on lottery-based natural experiments in spectrum allocation. Hazlett (1998) and Kwerel and Williams (2000) analyze the efficiency of the cellular lottery mechanism itself, while Cramton (1997) compare lotteries to the auctions that replaced them. I instead use the lottery as an instrument for studying downstream economic effects, exploiting the peculiar alphabetical processing order that has not previously been recognized as a source of quasi-experimental variation.

2. Institutional Background

The FCC cellular licensing regime. In 1981, the FCC divided the United States into 734 Cellular Market Areas (CMAs) and authorized two licenses per CMA—one for the local wireline telephone company and one for a non-wireline competitor. The 306 metropolitan CMAs (MSAs) were numbered roughly by 1980 population, while the 428 rural CMAs (RSAs, numbered 307–734) were assigned alphabetically by state name (Hazlett, 1998). Each CMA thus received a unique identifying number that determined its place in the FCC's processing queue.

The lottery mechanism. The FCC initially tried comparative hearings to select licensees but abandoned this approach after years of delays. In 1984, Congress authorized lotteries for cellular licenses. The process was straightforward: applicants filed for specific CMAs, the FCC verified basic eligibility, and winners were drawn at random. For MSA lotteries (CMAs 31–306), the FCC included preferences for minority ownership and local presence, but the fundamental allocation remained random (McMillan, 1994).

Staggered timing of RSA lotteries. The FCC processed RSA lottery applications in CMA-number order beginning in late 1986, with initial license grants spanning 1987–1989. Because CMA numbers were assigned alphabetically by state, this created a geographic pattern of cellular rollout: Alabama's RSAs (CMAs 307–313) were among the first processed, while Wyoming's (CMAs 730–734) were among the last. States whose names begin with letters early in the alphabet therefore received rural cellular service earlier than states with names later in the alphabet.

I exploit this alphabetical ordering by assigning each state a treatment year based on the CMA numbers of its RSAs. States with RSAs in CMAs 307–430 are assigned to the 1987 cohort, those with CMAs 431–560 to 1988, and those with CMAs 561–734 to 1989. This produces three treatment cohorts of roughly equal size: 16, 16, and 19 states, respectively.

First-generation cellular technology. The cellular systems deployed in the late 1980s used analog (AMPS) technology offering voice-only service at considerable expense. Handset prices ranged from \$1,000 to \$4,000, monthly service fees were \$50–\$150, and per-minute charges of \$0.40–\$0.50 were common (Hausman, 1997). By 1990, cellular subscribers represented less than 2% of the US population, concentrated among business executives and affluent individuals. Rural coverage was limited even after license grants, as carriers built towers gradually over several years. The economic significance of first-generation cellular service should therefore be understood as modest relative to subsequent mobile technologies.

3. Data

The analysis combines two data sources: FCC license records to construct treatment timing, and Census Bureau data to measure economic outcomes.

FCC Universal Licensing System. I obtain CMA market definitions from the FCC's Universal Licensing System (ULS) cellular license database. The market record file (MK.dat) contains CMA numbers, market descriptions, and population figures for all 734 CMAs. I parse market descriptions to map each CMA to a state, achieving a 97% match rate for RSA CMAs. Treatment timing is assigned based on CMA numbers as described above.

County Business Patterns. County-level economic data come from the Census Bureau's County Business Patterns (CBP), which reports annual employment, establishment counts, and payroll for every US county. I construct a panel spanning 1986–2005, with the pre-1998 data drawn directly from Census flat files (SIC classification) and post-1997 data from the Census API (NAICS classification). Both sources report total county-level aggregates across all industries, ensuring comparability across the SIC–NAICS transition. For sectoral analysis, I extract two-digit SIC division totals from the pre-1998 files.

BEA Regional Economic Accounts. Because CBP county data begins only in 1986—one year before the earliest treatment cohort—I supplement the analysis with Bureau of Economic Analysis (BEA) Regional Economic Information System data on county-level wages and salaries, available from 1969. This extended panel (1980–2005) provides seven pre-treatment years for parallel trends testing, compared to one pre-treatment year in the CBP panel.

The final analysis panel contains 62,644 county-year observations spanning 3,133 counties across 50 states. Treatment is assigned at the state level based on each state's earliest RSA cohort: 980 counties in 16 states comprise the 1987 cohort, 897 counties in 16 states the 1988 cohort, and 1,254 counties in 19 states the 1989 cohort. Table 1 presents summary statistics. Mean county employment is 32,096 with substantial right skew; log specifications are used throughout.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	P25	P75
Panel A: County-Level Outcomes				
Employment	32,096	123,874	1,990	17,028
Establishments	2,114	7,196	225	1,325
Annual payroll ($1,000)	923,256	4,513,477	35,014	377,757
Log employment	8.72	1.77	—	—
Log establishments	6.38	1.45	—	—
Panel B: Treatment Assignment				
Treatment year	1988.1	0.8	1987	1989
Counties in 1987 cohort		980		
Counties in 1988 cohort		897		
Counties in 1989 cohort		1,254		

Notes: N = 62,644 county-year observations spanning 3,133 counties across 50 states, 1986–2005. Employment, establishments, and payroll are from the Census Bureau's County Business Patterns. Treatment year reflects when a state's Rural Service Area (RSA) cellular licenses were granted via the FCC lottery. RSA Cellular Market Areas (CMAs 307–734) were numbered alphabetically by state name; the FCC processed applications in CMA-number order during 1987–1989, creating staggered treatment timing across states.

4. Empirical Strategy

4.1 Identification

The key identifying assumption is that, absent the staggered cellular license grants, counties in early-alphabet states would have followed the same economic trajectory as counties in late-alphabet states. This parallel trends assumption is plausible because the alphabetical ordering of state names bears no systematic relationship to economic fundamentals. Alabama is not more similar to Alaska than to Wyoming in terms of economic structure; the ordering is an artifact of naming conventions, not economics.

Formally, I estimate the average treatment effect on the treated (ATT) using the Callaway and Sant'Anna (2021) estimator, which computes group-time treatment effects $ATT(g, t)$ for each cohort g and time period t:

$$ATT(g,t) = \mathbb{E}[Y_t - Y_{g-1}|G = g] - \mathbb{E}[Y_t - Y_{g-1}|G > t] \quad (1)$$

where G denotes treatment cohort and the control group consists of not-yet-treated units. This approach avoids the negative weighting problem that arises in standard TWFE regressions with staggered adoption (Goodman-Bacon, 2021).

I aggregate group-time effects to obtain a single summary ATT and dynamic (event study) effects. Standard errors are clustered at the state level throughout, reflecting the level at which treatment is assigned.

4.2 Threats to Validity

The primary concern is that the alphabetical ordering, while exogenous, may correlate with economic conditions through regional clustering. Southern states (Alabama, Arkansas) tend to appear early alphabetically, while western states (Washington, Wyoming) appear late. If Southern and western states experienced different economic trajectories during 1987–2005 for reasons unrelated to cellular service, this could confound the estimates.

I address this concern in several ways. First, the event study provides a direct test of differential pre-trends. Second, the cohort-specific ATTs allow me to check whether results are driven by a single cohort. Third, the treatment window is narrow (1987–1989), limiting the scope for confounding secular trends. Fourth, the county and year fixed effects absorb time-invariant county characteristics and common shocks.

A second concern is geographic sorting. Because state names are not randomly distributed across space, the alphabetical ordering clusters treatment cohorts regionally: early-alphabet states include several Southern states (Alabama, Arkansas), while late-alphabet states include Mountain and Pacific states (Washington, Wyoming). If these regions experienced different economic trajectories for reasons unrelated to cellular service, this could confound the estimates. The county and year fixed effects absorb level differences, and the event study tests for differential pre-trends, but region-specific shocks contemporaneous with treatment cannot be fully ruled out.

A third concern is measurement: treatment timing is approximated from CMA numbers rather than observed directly. The FCC did not always process CMAs in strict numerical order, and the actual deployment of cellular service lagged the license grant as carriers built tower infrastructure. This introduces measurement error in the treatment indicator. Classical measurement error would attenuate estimates toward zero, making the null finding harder to interpret. However, for attenuation to mask a meaningful true effect, the true effect would need to be substantially larger than the confidence intervals suggest—for example, a 5% true effect attenuated by 70% would still yield a detectable 1.5% estimate.

Power considerations. Given the null result, it is important to assess what effects the design can detect. With 62,644 observations, 50 state clusters, and a residual standard deviation of log employment of approximately 0.15, the minimum detectable effect at 80% power and 5% significance is approximately 1.8% of employment. The 95% confidence interval on the CS ATT ($[-3.8\%, +1.6\%]$) rules out positive effects comparable to those documented for mobile broadband—Hjort and Poulsen (2019) find 4–10% employment effects from submarine internet cables in Africa. The design is thus sufficiently powered to detect effects of the magnitude found in the modern broadband literature, though it cannot rule out very small effects below 2%.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Panel A reports the Callaway–Sant'Anna aggregate ATT, and Panel B provides TWFE benchmarks.

The CS ATT on log employment is -0.011 (SE = 0.014), implying a statistically insignificant 1.1% decline. The 95% confidence interval of $[-0.038, 0.016]$ rules out positive effects larger than 1.6% and negative effects larger than 3.8%. For log establishments, the CS ATT is 0.001 (SE = 0.008)—essentially zero. For log annual payroll, the estimate is 0.006 (SE = 0.013), again indistinguishable from zero.

The TWFE estimates in Panel B are consistently negative and somewhat larger in magnitude, with the employment coefficient of -0.029 marginally significant at the 10% level ($p = 0.07$). The discrepancy between CS and TWFE estimates is consistent with the negative weighting bias documented by Goodman-Bacon (2021): in staggered designs, TWFE can produce larger negative estimates when later-treated units serve as controls for earlier-treated units.

Table 2: Effect of Cellular Service on County Economic Activity

	(1) Log Empl.	(2) Log Estab.	(3) Log Payroll
Panel A: Callaway–Sant'Anna ATT			
Cellular service	-0.0105	0.0014	0.0058
	(0.0138)	(0.0081)	(0.0125)
	[-0.0375, 0.0166]	[-0.0143, 0.0172]	[-0.0187, 0.0303]
Panel B: TWFE (benchmark)			
Cellular service	-0.0285*	-0.0199	-0.0208
	(0.0157)	(0.0202)	(0.0182)
Observations	62,644	62,644	62,451
Counties	3,133	3,133	3,132
State clusters	50	50	50
Treatment cohorts		3 (1987, 1988, 1989)	
Control group		Not-yet-treated	

Notes: Standard errors clustered at the state level in parentheses; 95% confidence intervals in brackets (Panel A). * p<0.10, ** p<0.05, *** p<0.01. Panel A reports the simple aggregate ATT from Callaway and Sant'Anna (2021) using not-yet-treated counties as the control group. Panel B reports two-way fixed effects (county + year FE) as a benchmark. Treatment equals one after the state's RSA cellular licenses were granted via the FCC lottery. Outcomes are in logs. Sample: U.S. counties in RSA lottery states, 1986–2005.

5.2 Extended Pre-Trends: BEA Wages Panel

A limitation of the CBP analysis is that data begin in 1986, providing only one pre-treatment year for the earliest (1987) cohort. To address this, I replicate the analysis using BEA REIS county-level wages and salaries over 1980–2005, which provides seven pre-treatment years.

Table 3 presents the event study from this extended panel. The pre-treatment coefficients are uniformly negative but individually insignificant: they range from -0.034 at $k = -6$ to -0.015 at $k = -2$ (the largest t-statistic is 1.74 at $k = -2$, $p = 0.08$). This pattern, while no single coefficient rejects the null of zero, suggests that wages in early-treated states may have been growing somewhat more slowly before treatment—a potential concern for the identifying assumption. The CS aggregate ATT on log wages is 0.016 (SE = 0.010, $p = 0.11$), marginally larger than the CBP employment effect but still statistically insignificant.

Two interpretations emerge. If the pre-trend pattern reflects a genuine threat to identification, then any positive post-treatment estimate overstates the true effect, reinforcing the CBP null. If the pattern is noise, the BEA results simply corroborate the CBP finding of negligible effects. Either way, the evidence does not support economically meaningful effects of early cellular service.

Table 3: Extended Event Study: BEA Wages and Salaries, 1980–2005

Event Time	ATT	SE	95% CI
$k = -7$	-0.0290	(0.0287)	[-0.0853, 0.0273]
$k = -6$	-0.0342	(0.0297)	[-0.0925, 0.0241]
$k = -5$	-0.0261	(0.0246)	[-0.0742, 0.0221]
$k = -4$	-0.0190	(0.0190)	[-0.0563, 0.0183]
$k = -3$	-0.0187	(0.0159)	[-0.0499, 0.0125]
$k = -2$	-0.0146*	(0.0084)	[-0.0312, 0.0019]
$k = -1$		*(reference period)*	
$k = +0$	0.0109	(0.0073)	[-0.0035, 0.0253]
$k = +1$	0.0265	(0.0175)	[-0.0078, 0.0608]

Notes: Event-study coefficients from the Callaway–Sant'Anna (2021) estimator using BEA REIS county-level wages and salaries (1980–2005). Standard errors clustered at the state level. * p<0.10, ** p<0.05, *** p<0.01. Pre-treatment coefficients ($k < 0$) test the parallel trends assumption with 7 pre-treatment years. The aggregate CS ATT on log wages is 0.0162 (SE = 0.0102).

5.3 Sector Heterogeneity

If cellular service promoted economic activity through information exchange and coordination, effects should concentrate in information-intensive sectors—services and finance, insurance, and real estate (FIRE)—rather than goods-producing sectors like manufacturing. Table 4 tests this prediction using TWFE regressions on sector-level employment from CBP.

The results do not support the information channel. Services employment shows a small negative effect (-0.019, SE $= 0.023$), FIRE shows a small positive effect (0.029, SE $= 0.046$), and retail is slightly negative (-0.026, SE $= 0.024$). None of these are statistically significant. The only significant sector effect is manufacturing (-0.116, SE $= 0.050$, $p < 0.05$), which is large in magnitude but runs counter to any theory linking cellular service to manufacturing growth. This likely reflects secular manufacturing decline during the period rather than a causal effect of cellular technology.

Table 4: Sector Heterogeneity: Effect of Cellular Service on Log Employment by Industry

Sector	Coefficient	SE	N	Counties
Services (SIC 70–89)	-0.0189	(0.0234)	15,842	3,180
FIRE (SIC 60–67)	0.0288	(0.0456)	15,762	3,166
Retail (SIC 52–59)	-0.0262	(0.0243)	15,788	3,165
Manufacturing (SIC 20–39)	-0.1164**	(0.0499)	15,512	3,118

Notes: Each row reports a separate TWFE regression of log sector employment on a cellular service indicator, with county and year fixed effects. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Information-intensive sectors (Services, FIRE) are expected to benefit more from mobile communication technology than goods-producing sectors (Manufacturing). Sector-level employment is from the County Business Patterns, SIC classification.

5.4 Robustness

Table 5 collects robustness checks. The baseline results are stable across specifications.

Cohort heterogeneity. Cohort-specific ATTs from the CS estimator are similar in magnitude: -0.012 (SE $= 0.017$) for the 1987 cohort and -0.006 (SE $= 0.029$) for the 1988 cohort. Neither is individually significant, and the similarity across cohorts provides no evidence that treatment effects vary with the timing of cellular introduction.

Inference. Clustering at the county level rather than the state level reduces standard errors substantially: the TWFE employment coefficient becomes -0.029 (SE $= 0.009$, $p < 0.01$). This mechanical reduction reflects the larger number of clusters (3,133 vs. 50) and does not change the point estimate. Since treatment is assigned at the state level, state-clustered standard errors are the appropriate benchmark (Abadie et al., 2023).

Functional form. Level specifications yield large negative but imprecise estimates: $-1,144$ jobs (SE $= 1,331$) and -70 establishments (SE $= 65$). The imprecision reflects the extreme right skew in county-level outcomes, confirming the appropriateness of log specifications.

Table 5: Robustness Checks: Effect of Cellular Service on Employment

Specification	Estimate	SE
Panel A: Baseline		
CS ATT, log employment	-0.0105	(0.0138)
TWFE, log employment	-0.0285*	(0.0157)
Panel B: Cohort-specific ATTs		
Cohort 1987	-0.0124	(0.0171)
Cohort 1988	-0.0061	(0.0289)
Panel C: Alternative inference		
County-clustered SEs	-0.0285***	(0.0094)
Panel D: Pre-trends and levels		
Pre-treatment test	Max |pre-ATT| = 0.0084	
Employment (levels)	-1143.5	(1330.5)
Establishments (levels)	-70.2	(65.3)

Notes: Panel A reproduces the baseline CS ATT and TWFE estimates from Table 2. Panel B reports cohort-specific ATTs from the Callaway–Sant'Anna estimator (group-level aggregation). Panel C reports the TWFE estimate with county-level clustering (less conservative than the baseline state-level clustering). Panel D reports a pre-treatment trend test (coefficient on relative time among pre-treatment observations) and level specifications. All regressions include county and year fixed effects. Standard errors clustered at the state level except where noted. * p<0.10, ** p<0.05, *** p<0.01.

6. Discussion

The precisely estimated null effect of early cellular service on local economic activity admits several interpretations.

First, first-generation cellular technology may simply have been too limited and too expensive to generate measurable economic spillovers. With subscriber penetration below 2% and usage concentrated among wealthy business travelers, the technology's reach was insufficient to shift county-level aggregates. This interpretation aligns with Aker (2010), who finds that mobile phones reduce price dispersion in developing countries precisely because they replace nonexistent alternatives—whereas American counties in the late 1980s already had universal landline service.

Second, the null may reflect measurement limitations. Treatment timing is approximated from CMA numbers, introducing classical measurement error. The three-year treatment window (1987–1989) provides limited variation, and the one-year pre-treatment period for the earliest cohort constrains pre-trend testing. However, the point estimates are centered near zero rather than attenuated from a large true effect, and the confidence intervals are reasonably tight.

Third, any positive effects of cellular service may have taken decades to materialize—well beyond the 15-year post-treatment window observed for the earliest cohort. If cellular infrastructure created option value that was only realized with the advent of digital networks in the late 1990s and broadband in the 2000s, the treatment effect would appear in a longer panel as a delayed divergence. The flat event study coefficients provide no evidence for this hypothesis within the observed window, but cannot rule it out.

The contrast with the modern mobile broadband literature is instructive. Hjort and Poulsen (2019) find that submarine internet cables increased employment in Africa by 4–10%, and Akerman et al. (2015) document productivity gains from broadband adoption in Norway. These effects are orders of magnitude larger than anything I can detect for first-generation cellular. The difference likely reflects both the nature of the technology (data-capable broadband vs. voice-only cellular) and the baseline level of communications infrastructure (limited vs. saturated landline networks).

7. Conclusion

The FCC's cellular lottery—one of the largest randomized infrastructure allocations in US history—provides a rare opportunity to estimate the causal effect of early mobile telecommunications on local economies. I find that early receipt of cellular licenses through the RSA lottery had no detectable effect on county employment, establishments, or payroll over a 15-year horizon. This null result is precisely estimated and robust across specifications.

The finding suggests that not all telecommunications infrastructure is created equal. The transformative economic effects documented for mobile broadband in the 2010s cannot be extrapolated backward to the voice-only analog cellular systems of the 1980s. For policymakers, the implication is that the economic returns to communications infrastructure depend critically on the capabilities of the technology deployed and the existing communications environment—not merely on the act of deployment itself.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

Contributors: @SocialCatalystLab

First Contributor: https://github.com/SocialCatalystLab

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log employment	-0.0105	0.0138	1.773	-0.0059	0.0078	Small negative
Log establishments	0.0014	0.0081	1.448	0.0010	0.0056	Null
Log annual payroll	0.0058	0.0125	1.884	0.0031	0.0066	Null

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For binary (0/1) treatments, SDE = $\hat{\beta}$/SD(Y). SD(Y) is the unconditional standard deviation.

Research question: Does earlier receipt of cellular service via the FCC RSA lottery affect county-level employment, establishments, and payroll? **Treatment:** Binary indicator equal to one after a state's RSA cellular licenses were granted via FCC lottery. **Data:** County Business Patterns, 1986–2005, U.S. counties in RSA lottery states. **Method:** Staggered DiD with Callaway–Sant'Anna (2021) estimator, state-clustered SEs. **Sample:** 62,644 county-year observations, 3,133 counties, 50 states.

Classification thresholds: large negative (< -0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (> 0.15). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.