

Did Privatization Expand Telephone Access?

John Levendis

jlevendis@tulane.edu

Tulane University

Research Article

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Did Privatization Expand Telephone Access?

John Levendis*

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Abstract

Between the late 1980s and the late 2000s, governments on every continent sold stakes in their state telephone companies to private or strategic investors. Prior studies found positive effects on fixed-line penetration, but their estimates relied on two-way fixed effects or pre-post comparisons. These methods are now known to be biased under staggered adoption with heterogeneous treatment effects. This paper re-estimates the effect across twenty-one countries spanning four continents using the heterogeneity-robust estimator of Callaway and Sant’Anna (2021) and a stacking difference-in-differences design. Both approaches find a significant contemporaneous effect of approximately 0.5 additional fixed-line subscriptions per hundred inhabitants at the time of privatization, growing to roughly 2.9 by event time five. The pre-treatment coefficients are small and insignificant, supporting the parallel-trends assumption. The estimate is stable across estimator choice, the inclusion of covariates, and entropy reweighting of the control group.

Keywords: Privatization; telecommunications; difference-in-differences; staggered adoption; fixed-line teledensity

*Connolly Alexander Institute for Data Science, Tulane University, New Orleans, LA 70118, USA. Email: jlevendis@tulane.edu. ORCID: 0000-0001-9529-2998. The author would like to thank Kevin Grier, Colin O’Reilly, and the participants at the Summer Workshop in Causal Inference at the Free Market Institute at Texas Tech University.

1 Introduction

On the eve of the privatization wave, state telephone companies across the developing world were defined by scarcity. Argentina had only 9.2 fixed-line subscriptions per hundred inhabitants, Mexico had 6.5, and Chile had fewer than five. By the mid-1990s, 42 million people worldwide were registered on waiting lists for a telephone connection (International Telecommunication Union, 1998). In Brazil, the state system provided only fifteen million lines for a population of 160 million. Customers waited two years for installation, paid thousands of dollars for a connection, and still faced a 30 percent call-failure rate. The result was a black market where phone lines were bought and sold with prices listed in the newspapers (McDaniels, 1998). Frustrated by chronic underinvestment and rationed access, governments began transferring their telephone companies to private and strategic investors.

The resulting wave of privatization, which began in the late 1980s and continued through the 2000s, was part of a broader global movement toward private provision across a range of industries. Cross-country firm-level studies have consistently documented significant post-privatization improvements in profitability, output, operating efficiency, and capital spending (Megginson et al., 1994; D’Souza and Megginson, 1999; Boubakri and Cosset, 1998). By the time of the comprehensive survey of Megginson and Netter (2001), the literature had accumulated evidence from over two hundred firms across more than forty countries, and the weight of it pointed clearly toward private superiority. Transportation deregulation in the United States reinforced the case. Real costs fell by a quarter to three-quarters (Winston, 1993).

A meta-analysis of the economic freedom literature, covering over two thousand published regression estimates, finds that market-oriented institutions are positively associated with growth and income in the large majority of cases (Lawson et al., 2024). With a new wave

of asset sales involving 5G spectrum, fiber networks, and data infrastructure now underway (Bertè et al., 2026), the quality of the telecom-specific evidence matters. That evidence, however, was built almost entirely on two-way fixed effects and pre-post comparisons. These methods produce biased estimates under staggered adoption with heterogeneous treatment effects, and the direction of the bias varies with sample composition and treatment timing. Some of the mixed results in the prior literature may therefore be a symptom of the method rather than genuine disagreement about the effects of reform. This paper applies heterogeneity-robust estimators to the same question and asks whether the original positive finding survives.

The theoretical foundations for these results predate the privatization wave itself. Alchian (1965) established the core property-rights argument. Since no individual can sell a share of a governmental enterprise, the consequences of managerial decisions cannot be capitalized into stock prices, and the cost-reward link that disciplines private managers is weakened. Boycko et al. (1996) built on this logic to formalize the argument for privatization. Politicians use public firms to pursue political objectives at the expense of efficiency. Privatization disrupts this arrangement by forcing future intervention into the form of explicit and politically costly subsidies.

These arguments make a strong case for privatization in general, but the case is weaker in industries prone to natural-monopoly, where privatization may replace a public monopolist with a private one (Shleifer, 1998). In telecommunications, the local loop has natural-monopoly features but long-distance and mobile markets do not. A further complication is that the regulatory environment evolved alongside the ownership transfers. Many countries adopted incentive regulation in the form of price caps, which gave newly privatized operators pricing flexibility in exchange for demonstrable performance improvements. The evidence from U.S. telecommunications suggests that price-cap regulation accelerated infrastructure deployment, raised total factor productivity, and reduced prices relative to traditional rate-of-return regulation (Kridel et al., 1996). Whether the transfer of ownership produced mea-

surable gains in access above and beyond any regulatory improvements is ultimately an empirical question.

The empirical record specific to telecommunications is broadly consistent with the general case. Cross-country panels have found positive associations between privatization and fixed-line penetration, with the largest effects in countries that transferred full control rather than minority stakes (Ros, 1999; Li and Xu, 2004; Wallsten, 2001). Results from Asia are more mixed, with some studies finding that institutional quality and competition mattered more than ownership status (Singh, 2000; Zheng and Ward, 2011). All of these studies relied on two-way fixed effects, pre-post comparisons, or instrumental-variables designs, which use already-treated countries as implicit controls for later-treated countries or impose strong exclusion restrictions. Both classes of methods are vulnerable to bias under staggered adoption with heterogeneous treatment effects (Callaway and Sant’Anna, 2021; Sun and Abraham, 2021). The global privatization wave, in which twenty-one countries adopted reform between 1988 and 2008, is precisely the setting where this bias is most concerning.

This paper re-estimates the effect of the privatization wave using the heterogeneity-robust difference-in-differences estimator of Callaway and Sant’Anna (2021) and a stacking design. Both approaches find a significant contemporaneous effect of approximately 0.5 additional fixed-line subscriptions per hundred inhabitants, growing to roughly 2.9 by event time five.

2 Data

The data come from the World Bank’s World Development Indicators (WDI). The panel covers all countries from 1975 onward, though the effective estimation window varies by specification. The primary outcome is fixed-telephone subscriptions per one hundred inhabitants. Covariates include the log of GDP per capita in constant US dollars and the urban share of the population.

Fixed-line teledensity is chosen as the primary outcome on *ex ante* grounds. The priva-

tizations of the late 1980s and 1990s acted on fixed-line networks in an era when wireline telephony was the binding margin of access. Mobile telephony is a distinct and later technology whose diffusion was driven by global supply-side forces that operated largely independently of whether a country’s fixed-line operator was publicly or privately owned. The earlier empirical literature on telecommunications reform uses fixed-line penetration as the standard dependent variable for precisely this reason (Ros, 1999; Wallsten, 2001; Li and Xu, 2004; Fink et al., 2003).

Twenty-one countries privatized their state telephone companies between 1988 and 2008. The treatment variable records the first full calendar year under private management for each country. Table 1 lists all treated countries, their privatization dates, and the nature of the privatization.

The sample includes both full transfers of control and partial privatizations in which a strategic investor acquired a minority stake, typically with a management contract. The inclusive definition is appropriate because the theoretical channel runs through managerial incentives and investment behavior. Both change when a strategic investor acquires operational control, even without majority ownership. France Telecom’s 40% acquisitions in Jordan and Mauritius, for example, came with management agreements that gave the acquirer effective control of operations and capital planning.

Three Latin American countries are excluded from all specifications. Bolivia and Venezuela both privatized and subsequently renationalized their telephone companies, but the economic environments surrounding their reversals were sufficiently unusual to make counterfactual comparison unreliable. Venezuela’s post-2003 economy was dominated by oil-price swings and capital controls. Bolivia’s renationalization was part of a broader campaign under the Morales government that nationalized hydrocarbons (2006), mining operations (2006–2007), and telecommunications (2008) in rapid succession (Farthing and Kohl, 2014). Costa Rica is excluded because its operator (ICE) remained state-owned throughout the period and its income level makes it an outlier among the treated countries.

Table 1: Treated countries and privatization dates

Country	Code	Year	Transaction
Jamaica	JAM	1988	Cable & Wireless acquired majority
Chile	CHL	1989	CTC sold to Bond Corp (1988)
Belize	BLZ	1989	BTL sold; renationalized 2009
Argentina	ARG	1991	ENTel transferred
Mexico	MEX	1991	Telmex control sold
Malaysia	MYS	1991	Telekom Malaysia 25% IPO
Guyana	GUY	1991	GTT privatized
Hungary	HUN	1994	Deutsche Telekom/Ameritech 30%
Peru	PER	1995	CPT/ENTEL sold to Telefónica (1994)
Panama	PAN	1998	INTEL 49% to Cable & Wireless
South Africa	ZAF	1998	Thintana consortium 30%
Brazil	BRA	1999	Telebrás auctioned
El Salvador	SLV	1999	CTE-ANTEL 51% France Telecom
Guatemala	GTM	1999	TELGUA sold to Luca S.A. (1998)
Jordan	JOR	2001	France Telecom 40%
Mauritius	MUS	2001	France Telecom 40%
Nicaragua	NIC	2002	40% private stake
Turkey	TUR	2006	Oger Telecom 55%
Georgia	GEO	2007	State shares sold
Tunisia	TUN	2007	TECOM 35%
Gabon	GAB	2008	Maroc Telecom 51%

Note: Year is the first full calendar year under private or strategic-investor management.

Belize also renationalized its operator (BTL) in 2009, but in an otherwise stable macroeconomic environment. The baseline specification includes Belize as a treated unit censored at 2009, using only its twenty years of continuous private ownership (1989–2008). Sensitivity to this choice is examined in the Robustness section.

The control group is constructed as a comparable band pool. For each never-treated country in the WDI, the baseline values of fixed-line teledensity and GDP per capita are averaged over 1980 to 1988, a period before any of the twenty-one treated countries had privatized. A never-treated country is retained if both its baseline teledensity and its baseline GDP per capita fall within the range spanned by the treated countries on each variable. War-affected countries (Iran, Iraq) and small island states whose telephone systems were already privately operated under concession are excluded from the potential control pool. The procedure yields fourteen never-treated controls: Algeria, Colombia, Cuba, Ecuador, Micronesia, Namibia, Paraguay, Samoa, Suriname, Thailand, Tonga, Trinidad and Tobago, Turkmenistan, and Vanuatu.¹

Table 2 reports descriptive statistics for the estimation sample.

Table 2: Descriptive statistics

	Treated			Controls			Full sample		
	Mean	SD	<i>N</i>	Mean	SD	<i>N</i>	Mean	SD	<i>N</i>
Fixed lines per 100	10.78	8.23	1015	7.69	5.73	662	9.56	7.50	1677
Log GDP per capita	8.56	0.58	1053	8.36	0.46	686	8.48	0.54	1739
Urban population (%)	61.88	16.89	1054	45.74	20.01	714	55.36	19.86	1768
Countries	21			14			35		

¹Several of these countries differ from the treated group in political and institutional characteristics. The band pool restricts to countries with baseline teledensity and GDP per capita within the range of the treated units, which limits but does not eliminate heterogeneity. The entropy-balanced stacking specifications reported in the Robustness section further reweight the control group to match the treated units on observable covariates.

3 Empirical Strategy

The staggered adoption of privatization across twenty-one countries between 1988 and 2008 provides the identifying variation. The primary estimator is the Callaway and Sant’Anna (2021) estimator, cross-checked against a stacking difference-in-differences design.

The identifying assumption is that, in the absence of privatization, treated and control countries would have followed parallel paths in fixed-line teledensity. The plausibility of this assumption rests on the pre-treatment event-study coefficients. If the treated and control groups were on different trajectories before reform, the pre-treatment coefficients will drift away from zero. Both estimators use only never-treated and not-yet-treated units as controls, avoiding the negative weighting problem that arises under conventional two-way fixed effects when already-treated units serve as implicit comparisons for later-treated cohorts.

Countries did not privatize their telephone companies at random. Fiscal pressure, IMF conditionality, partisan ideology, and imitation of peer countries all predicted reform adoption (Henisz et al., 2005; Brune et al., 2006; Murillo, 2002; Fink, 2011). If the same factors that drove countries to privatize also drove faster network growth, then estimates that fail to account for this selection will overstate the effect of ownership transfer. The estimator used here does not require that treatment assignment be random. It requires that, conditional on covariates, treated and control countries would have followed parallel paths in fixed-line teledensity in the absence of reform. The pre-treatment event-study coefficients reported below provide direct evidence on this assumption.

The Callaway and Sant’Anna (2021) estimator computes group-time average treatment effects separately for each treatment cohort using only not-yet-treated or never-treated units as controls, and then aggregates them into an event-study. The paper reports results under three specifications of this estimator: an outcome regression with covariates, a doubly-robust formulation that combines the outcome regression with inverse probability weighting, and an unconditional specification without controls.

As a cross-check, a stacking estimator constructs a separate two-by-two DiD for each

Treatment timing

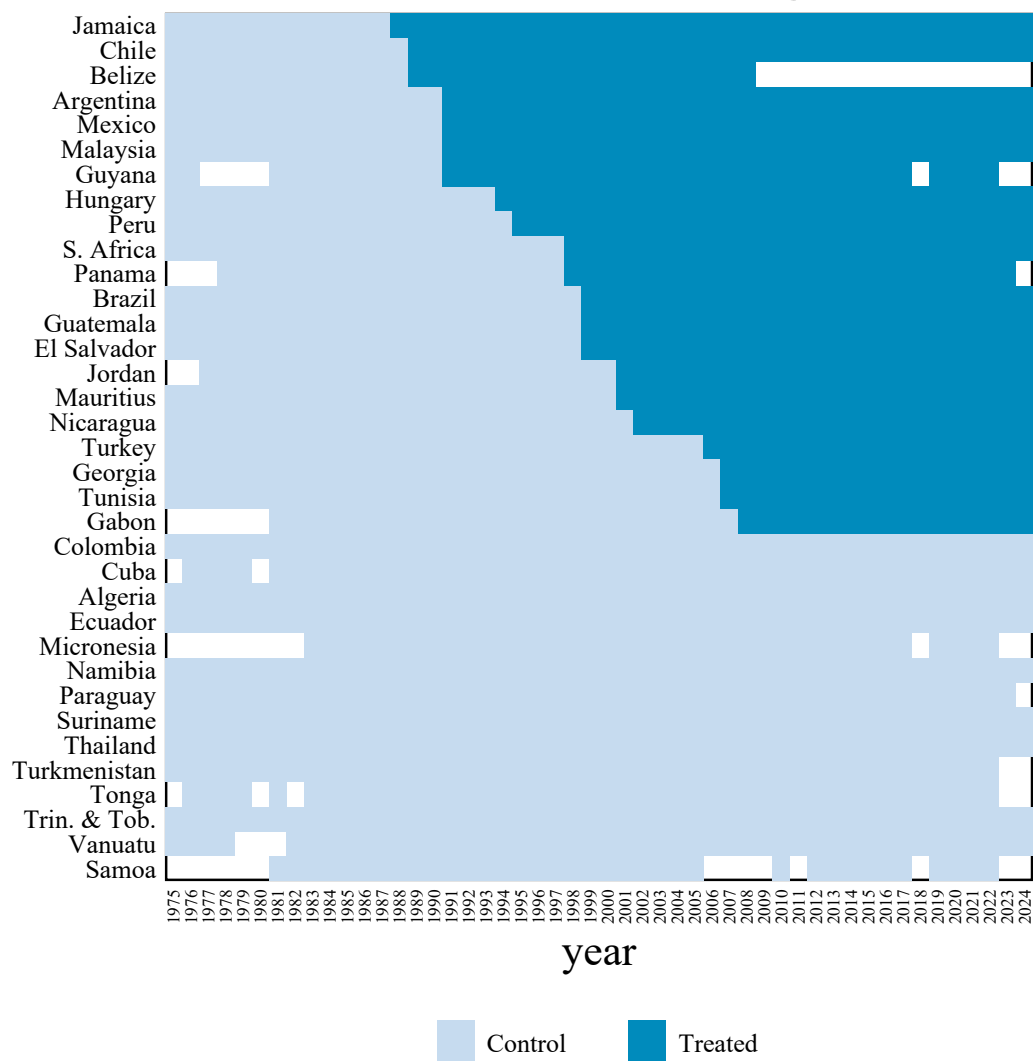


Figure 1: Treatment timing across countries.

treatment cohort within a ± 10 year event window, stacks them, and estimates with cohort-specific unit and time fixed effects, clustering standard errors at the country level. The event-study specification reports effects at event times -10 through $+10$, with event time -1 as the omitted reference period. The window is wide enough to capture medium-run dynamics in network investment, which typically takes several years to translate into subscriber growth.

To reduce observable differences between treated and control units, the stacking estimator is also run with entropy-balanced weights. Entropy balancing reweights the control group countries so that the average values on their control variables (log of GDP per capita, and the urban population share) matches those of the treated group exactly. This makes the treatment-effect estimates less sensitive to how the outcome model is specified (Hainmueller, 2012). The reweighted specifications appear alongside the unweighted results as a check on sensitivity to covariate imbalance.

4 Results

Table 3 reports event-study estimates from the Callaway-Sant’Anna estimator under three specifications. Under the outcome regression with covariates, the contemporaneous effect at event time zero is 0.46 additional fixed-line subscriptions per hundred inhabitants ($p = 0.020$). The no-controls specification produces a similar estimate of 0.52 ($p = 0.009$).

The effect grows steadily through the post-treatment window, reaching 2.92 under the outcome regression and 3.00 under no controls by event time five, both significant at the five-percent level. The doubly robust specification produces somewhat smaller point estimates in the medium run (1.96 at event time five) but follows the same trajectory. The smaller estimates likely reflect the sensitivity of inverse probability weighting to limited covariate overlap in small samples. The average post-treatment effect ranges from 1.68 to 2.39 across specifications, all significant at the five- or ten-percent level.

The magnitude of the effect is economically meaningful. An increase of roughly half a line

per hundred people at the time of privatization, growing to about three lines within five years, is substantial when measured against a pre-reform teledensity average below ten. Argentina entered reform with 9.2 fixed-line subscriptions per hundred inhabitants, Mexico with 6.5, and Chile with fewer than five. An additional 2.9 lines within five years of privatization represents a 30 to 60 percent expansion of the network relative to those starting points.

The trajectory of the effect is consistent with the property-rights channel described by Alchian (1965) and Boycko et al. (1996). A modest impact at the time of transfer, followed by steady growth over five years, matches a pattern of new capital investment translating into subscriber connections with a lag. State operators had little incentive to expand networks beyond what political considerations required. Private operators, facing market discipline and often bound by mandated targets, invested in capacity that took several years to reach subscribers. The data cannot distinguish this channel from the concurrent effects of liberalization and regulatory reform, but the shape of the event-study path is what the theory predicts. The leveling off after event time five is consistent with fixed-line networks approaching saturation as mobile telephony began to substitute for fixed-line access in the early 2000s.

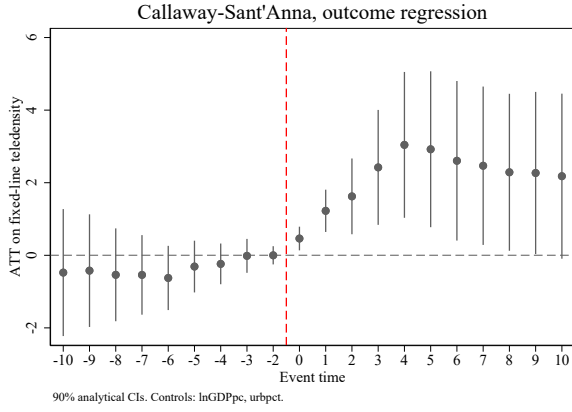
The pre-treatment coefficients provide support for the parallel-trends assumption. None of the pre-treatment event-time estimates is individually significant in any specification, and the pre-trend average is small and statistically indistinguishable from zero in all three columns, ranging from -0.35 to -0.05 . Inference throughout rests on thirty-five clusters, which is below the threshold where asymptotic standard errors are fully reliable. The agreement between two independent estimators with different variance structures provides indirect reassurance, but the small number of clusters remains a binding constraint on precision.

Table 3: Callaway-Sant'Anna event-study estimates

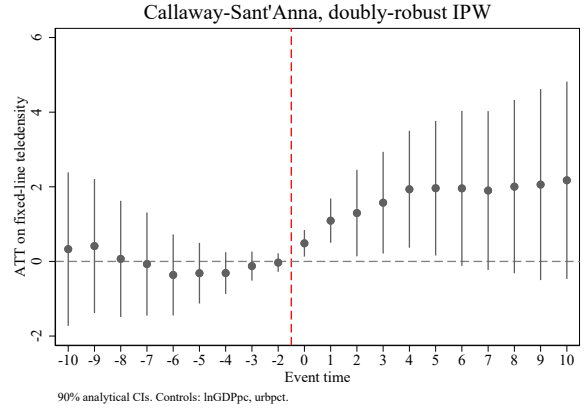
	Outcome reg.	Doubly-robust IPW	No controls
Event time -10	-0.476 [0.654]	0.329 [0.792]	-0.461 [0.616]
Event time -9	-0.423 [0.654]	0.412 [0.706]	-0.430 [0.618]
Event time -8	-0.538 [0.489]	0.068 [0.943]	-0.411 [0.589]
Event time -7	-0.540 [0.417]	-0.070 [0.933]	-0.458 [0.485]
Event time -6	-0.624 [0.246]	-0.363 [0.583]	-0.488 [0.396]
Event time -5	-0.310 [0.474]	-0.315 [0.523]	-0.304 [0.538]
Event time -4	-0.237 [0.488]	-0.313 [0.358]	-0.203 [0.604]
Event time -3	-0.015 [0.957]	-0.125 [0.597]	-0.024 [0.939]
Event time -2	-0.000 [0.998]	-0.031 [0.834]	-0.086 [0.599]
Event time 0	0.462** [0.020]	0.486** [0.025]	0.521*** [0.009]
Event time 1	1.225*** [0.001]	1.092*** [0.002]	1.197*** [0.004]
Event time 2	1.624** [0.011]	1.297* [0.066]	1.578** [0.024]
Event time 3	2.422** [0.012]	1.574* [0.057]	2.426** [0.022]
Event time 4	3.043** [0.013]	1.934** [0.042]	3.083** [0.021]
Event time 5	2.922** [0.025]	1.962* [0.074]	3.002** [0.038]
Event time 6	2.604* [0.051]	1.958 [0.121]	2.944** [0.046]
Event time 7	2.468* [0.063]	1.900 [0.142]	2.919** [0.048]
Event time 8	2.287* [0.082]	2.004 [0.156]	2.932** [0.041]
Event time 9	2.268* [0.095]	2.059 [0.186]	2.931** [0.047]
Event time 10	2.180 [0.115]	2.175 [0.176]	2.782* [0.062]
Pre-trend avg	-0.352 [0.515]	-0.045 [0.943]	-0.318 [0.559]
Post-treat avg	2.137** [0.026]	1.677* [0.084]	2.392** [0.025]

21 treated, 14 controls. p -values in brackets.

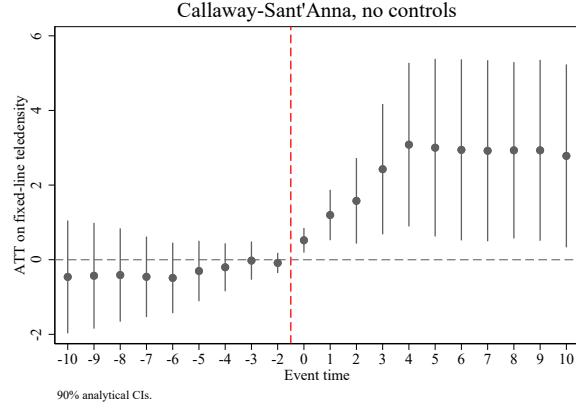
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



(a) Outcome regression



(b) Doubly-robust IPW



(c) No controls

Figure 2: Callaway-Sant'Anna event-study estimates, three specifications.

5 Robustness

The baseline result is tested against sensitivity to estimator choice, the treatment of Belize (which renationalized), and the inclusion of partial privatizations.

5.1 Stacking estimator

Table 4 reports the stacking estimator under four specifications, varying the inclusion of controls and the use of entropy-balanced weights. The stacking results closely track the Callaway-Sant’Anna estimates. The contemporaneous effect ranges from 0.44 to 0.53, and the event-time-five effect ranges from 2.79 to 2.99. None of the pre-treatment coefficients is individually significant. That both estimators agree strengthens confidence that the result reflects a real treatment effect rather than an artifact of the estimation procedure.

5.2 Belize

Belize privatized BTL in 1989 and renationalized it in 2009. The baseline specification censors Belize at the reversal date, using only the twenty years of continuous private operation. As a check, the estimation was repeated with Belize dropped entirely. The results are nearly identical. Under the Callaway-Sant’Anna outcome regression specification, the contemporaneous effect drops slightly from 0.46 to 0.42, and the event-time-five effect moves from 2.92 to 2.82, with no change in significance. Belize’s renationalization does not drive the result.

5.3 Partial privatizations

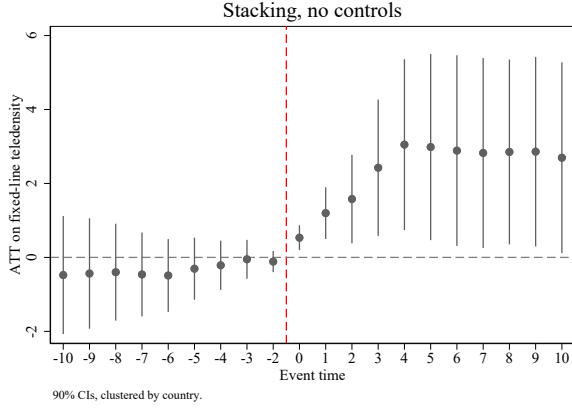
The treated group includes eight countries where the government sold a minority stake rather than transferring majority control.² To test sensitivity to this definition, the estimation was repeated after dropping all eight, reducing the treated group from twenty-one to thirteen. The contemporaneous effect is smaller and no longer statistically significant, and

²Panama, Hungary, Malaysia, South Africa, Jordan, Mauritius, Tunisia, and Georgia.

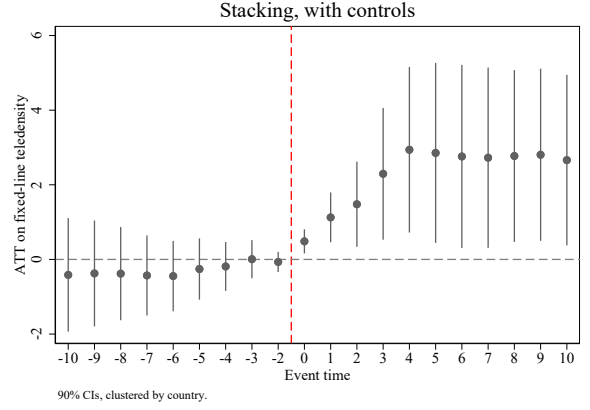
Table 4: Stacking event-study estimates

	No controls	With controls	Entropy wt.	Entropy + ctrl
Event time -10	-0.477 [0.616]	-0.414 [0.648]	-0.006 [0.995]	0.133 [0.891]
Event time -9	-0.436 [0.625]	-0.375 [0.658]	0.049 [0.959]	0.182 [0.835]
Event time -8	-0.400 [0.609]	-0.380 [0.610]	-0.010 [0.990]	0.065 [0.935]
Event time -7	-0.461 [0.497]	-0.429 [0.503]	-0.137 [0.850]	-0.055 [0.936]
Event time -6	-0.489 [0.407]	-0.446 [0.429]	-0.200 [0.745]	-0.121 [0.838]
Event time -5	-0.305 [0.543]	-0.257 [0.599]	-0.077 [0.879]	-0.000 [1.000]
Event time -4	-0.213 [0.592]	-0.188 [0.630]	-0.065 [0.871]	-0.034 [0.933]
Event time -3	-0.052 [0.869]	0.006 [0.983]	0.086 [0.785]	0.167 [0.585]
Event time -2	-0.113 [0.510]	-0.068 [0.674]	-0.023 [0.889]	0.040 [0.800]
Event time 0	0.533** [0.011]	0.484** [0.016]	0.520*** [0.009]	0.442** [0.033]
Event time 1	1.197*** [0.007]	1.127*** [0.007]	1.191*** [0.004]	1.067*** [0.009]
Event time 2	1.578** [0.032]	1.479** [0.035]	1.590** [0.024]	1.403** [0.044]
Event time 3	2.426** [0.033]	2.295** [0.035]	2.442** [0.029]	2.214** [0.042]
Event time 4	3.050** [0.032]	2.939** [0.032]	3.063** [0.030]	2.865** [0.037]
Event time 5	2.985* [0.053]	2.854* [0.053]	2.994** [0.047]	2.787* [0.057]
Event time 6	2.889* [0.067]	2.759* [0.066]	2.865* [0.063]	2.683* [0.070]
Event time 7	2.825* [0.072]	2.726* [0.065]	2.715* [0.073]	2.600* [0.072]
Event time 8	2.852* [0.062]	2.771** [0.050]	2.652* [0.074]	2.573* [0.066]
Event time 9	2.860* [0.068]	2.805** [0.048]	2.658* [0.078]	2.618* [0.062]
Event time 10	2.696* [0.086]	2.662* [0.057]	2.598* [0.081]	2.610* [0.058]

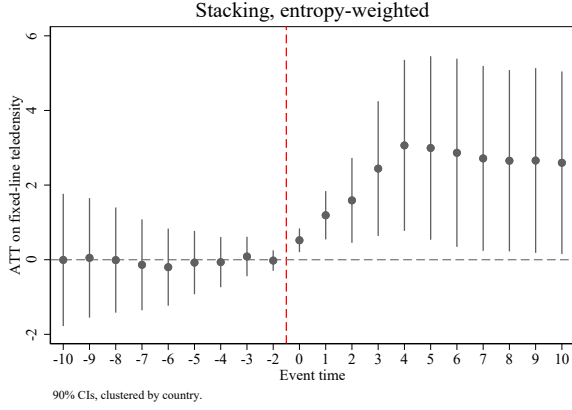
21 treated, 14 controls. Clustered p -values in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



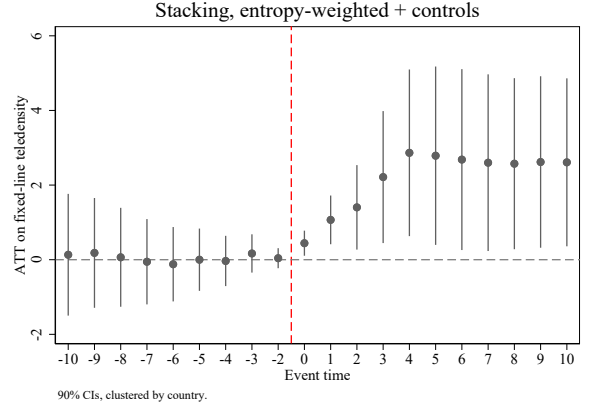
(a) No controls



(b) With controls



(c) Entropy-weighted



(d) Entropy-weighted with controls

Figure 3: Stacking event-study estimates, four specifications.

the pre-treatment coefficients become positive and individually significant in several specifications. The restricted sample therefore fails the parallel-trends test. Countries that carried out full privatizations appear to have been on upward trajectories before reform, and their post-treatment estimates confound the effect of privatization with pre-existing momentum. Including the partial-privatization countries restores a treated group whose pre-treatment trend is indistinguishable from that of the controls. The baseline specification is therefore the better-identified one, not a compromise made for statistical power.³

6 Conclusion

Using heterogeneity-robust difference-in-differences estimators applied to twenty-one countries across four continents, this paper finds that privatization raised fixed-line telephone penetration by approximately 0.5 subscriptions per hundred inhabitants at the time of reform, growing to roughly 2.9 within five years. The result is significant across multiple specifications and estimators, with clean pre-treatment trends supporting the identifying assumption.

The effect estimated here reflects the full reform package rather than ownership alone. Most privatizations in this sample bundled ownership transfer with market liberalization and regulatory reform, including the adoption of incentive regulation that gave operators pricing flexibility in exchange for performance improvements (Kridel et al., 1996; Ros, 2003). Whether privatization alone would have produced the same result without the accompanying liberalization and regulatory modernization is a question this paper cannot answer.

Governments today face a new round of ownership decisions involving 5G spectrum, fiber networks, and data infrastructure (Bertè et al., 2026). The evidence here suggests that transferring operational control to private investors expanded access in the previous generation of network technology. Policymakers drawing on this evidence should recognize that the measured effect reflects a package of reforms, not ownership transfer in isolation,

³Full event-study estimates for this restricted specification are available from the author upon request.

and that the institutional and regulatory environment in which a privatization takes place is likely to matter as much as the transfer itself.

Privatization expanded access to telephones. Countries that transferred stakes in their state telephone companies to private and strategic investors saw significant and growing gains in fixed-line access. The result is stable across estimator choice, the inclusion or exclusion of partial privatizations, entropy reweighting of the control group, and the treatment of Belize. Privatization worked.

Statements and Declarations

Competing Interests The author has no competing interests to declare that are relevant to the content of this article.

Declaration of Generative AI and AI-Assisted Technologies in the Writing Process During the preparation of this work, the author used Claude.ai for coding and editorial assistance. The author takes full responsibility for the content of the published article.

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