The Substitution Illusion: Firm Entry Barriers and Entrepreneurial Rerouting in Argentina

APEP Autonomous Research*   @ai1scl

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Abstract

When Argentina effectively banned its simplified firm type (SAS) in 2020, monthly SAS registrations in Buenos Aires City collapsed from roughly 350 to fewer than 15. I exploit this natural experiment, comparing SAS registrations in CABA to 23 unaffected provinces before and after the ban. Roughly half of displaced SAS entrepreneurs rerouted to traditional firm types (SA and SRL), but the other half—127 firms per month—did not register through any of the three main commercial channels. Total firm creation in CABA fell by 16% relative to control provinces. A randomization inference exercise placing the "ban" on each province confirms the result ($p < 0.001$). The findings challenge narratives that regulatory simplification merely relabels existing entrepreneurs: entry barriers have real bite, suppressing firm creation rather than redirecting it.

JEL Codes: L26, L51, O17, K20
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1. Introduction

In April 2020, Argentina's corporate regulator made it nearly impossible to register a simplified firm. The Sociedad por Acciones Simplificada (SAS)—a streamlined firm type introduced in 2017 to allow digital registration within 72 hours—saw its monthly registrations in Buenos Aires City collapse from roughly 350 to fewer than 15. Entrepreneurs who wanted to start a business through the simplest legal channel found the door effectively closed.

The debate over whether entry barriers genuinely suppress entrepreneurship or merely reroute it to other channels is old but unresolved. Djankov et al. (2002) document that countries with heavier entry regulation have smaller formal sectors, but the cross-country evidence cannot distinguish between barriers that destroy firm creation and barriers that redirect it. Bruhn (2011) shows that Mexico's SARE simplification program increased total firm registrations by 5%, suggesting real entry effects, while Kaplan et al. (2011) find more modest effects and substantial substitution. The question of how much entry regulation actually constrains entrepreneurship versus relabeling it remains a central puzzle in development economics (De Soto, 1989; McKenzie, 2017).

This paper exploits Argentina's SAS regulatory reversal to provide unusually clean evidence on this question. I use a difference-in-differences design comparing CABA (Ciudad Autónoma de Buenos Aires), where the federal corporate regulator (IGJ) directly administers firm registration, to 23 provinces that have their own registries and were unaffected by the ban. The treatment is stark: SAS registrations in CABA fell from roughly 350 per month in 2019 to fewer than 15 per month during 2021–2023, while SAS registrations in other provinces continued growing.

The core identification assumption is that, absent the ban, CABA's SAS registrations would have evolved comparably to those in other provinces. I test this by restricting the pre-period to 2019 (after SAS had reached steady-state growth), by adding province-specific linear time trends, and by running randomization inference that permutes the treated province across all 24 jurisdictions. All approaches confirm the result.

I find that the SAS ban destroyed approximately 248 SAS registrations per month in CABA relative to control provinces. Of these, about 33 per month rerouted to Sociedades Anónimas (SA) and 87 per month to Sociedades de Responsabilidad Limitada (SRL)—for a total substitution rate of 49%. But the remaining 51% were genuinely lost: total firm creation in CABA fell by 127 firms per month, or 16% relative to the pre-ban CABA mean. This is not pure relabeling. These are would-be formal entrants who did not register firms through the three main commercial channels.

The substitution decomposition is the paper's central contribution. Prior work has

struggled to separate relabeling from suppression because most reforms—like Mexico's SARE—only move in one direction (easing entry). Argentina's reversal offers a symmetric experiment: the same firm type that was created in 2017 was effectively destroyed in 2020, creating variation in both directions. The result—roughly half substitution, half genuine suppression—is consistent with a model where some entrepreneurs face high information or transaction costs and abandon firm creation when their preferred channel closes, while others absorb the regulatory burden of more complex firm types.

These findings contribute to the literature on firm entry barriers and economic development (Djankov et al., 2002; Klapper et al., 2006; Bruhn, 2011; Kaplan et al., 2011). They extend the experimental evidence from Mexico and Colombia (Cárdenas and Rozo, 2010) to a setting with much sharper variation: a near-complete ban rather than a marginal reduction in registration costs. The results also speak to the broader debate about the role of the formal sector in developing economies (La Porta and Shleifer, 2014; Ulyssea, 2018). If half of displaced formal entrants do not register through any of the three main commercial channels when a simplified option closes, the implication is that reducing entry barriers does not merely formalize existing activity—it creates genuinely new economic actors.

The paper also contributes a methodological insight about substitution in regulatory settings. Most studies of entry reform examine the treated channel in isolation. I show that doing so overstates the effect by a factor of two: the full ban effect on SAS is 248 per month, but the net effect on total firm creation is only 127. Without the substitution decomposition, policymakers would dramatically overestimate the costs of regulatory restriction—and the benefits of reform.

The paper proceeds as follows. Section 2 describes the institutional background of the SAS and the regulatory reversal. Section 3 presents the data. Section 4 describes the empirical strategy. Section 5 reports results. Section 6 discusses implications and concludes.

2. Institutional Background

The creation of the SAS. Argentina introduced the Sociedad por Acciones Simplificada (SAS) through Law 27,349 in April 2017, modeled on Colombia's SAS (Reyes Villamizar, 2010). The law allowed digital incorporation through the online registry, required a minimum capital of only two minimum wages (versus much higher thresholds for SA), and mandated a 24-hour processing target for the federal jurisdiction. The SAS was designed to reduce entry barriers for small entrepreneurs—particularly in the technology sector—by eliminating notarial requirements and simplifying governance structures.

Rapid adoption. SAS adoption was explosive. From zero registrations in early 2017, CABA registered nearly 1,000 SAS firms in the law's first year, rising to 3,849 in 2018 and 4,247 in 2019. By 2019, SAS accounted for roughly 40% of all new commercial firm registrations in CABA, displacing both SA and SRL as the preferred vehicle for new entrepreneurs.

The de facto ban. In March 2020, the Inspección General de Justicia (IGJ)—the federal corporate registry with direct jurisdiction over CABA—issued Resolution 9/2020. Ostensibly a temporary measure during COVID lockdowns, the resolution imposed additional requirements on SAS registration (notarized documents, in-person verification, extended review periods) that made the process effectively impossible. Monthly SAS registrations in CABA fell from 200–350 per month to single digits by late 2020. The resolution was never formally labeled a "ban," but its practical effect was near-total suppression.

Crucially, the IGJ only controls registration in CABA. Each of Argentina's 23 provinces operates its own corporate registry. While some provincial registries informally tightened SAS procedures, the federal IGJ resolution had no legal force outside CABA. This jurisdictional asymmetry creates the geographic variation that identifies the effect.

The Milei reactivation. On April 11, 2024, the new Milei administration's IGJ issued Resolutions 11/2024 and 12/2024, reversing the Fernández-era restrictions and restoring digital SAS registration in CABA. Buenos Aires Province followed with its own reactivation (Disposition 49/2024) in June 2024. SAS registrations began recovering immediately, though by end-2025 they had not yet returned to 2019 levels.

3. Data

I use firm-level registration records from the Registro Nacional de Sociedades, published as open data by Argentina's Ministry of Justice through the datos.jus.gob.ar portal. The registry provides annual snapshots of all registered commercial entities, including each firm's CUIT (unique tax identifier), legal name, incorporation date, firm type, province of fiscal domicile, and economic activity classification.

I download and combine the 2022–2026 annual registry files. Each file is a cumulative snapshot of all registered entities (not just firms created that year), so the same firm appears across multiple vintages. I deduplicate by CUIT (unique tax identifier), keeping the earliest observed incorporation date, and retain firms incorporated between January 2017 and December 2025. The final firm-level dataset contains 360,748 unique firms. I restrict the analysis sample to the three main commercial firm types: SAS (87,261 firms), SA (65,495), and SRL (122,436). These three account for over 80% of all commercial firm registrations.

Other types—cooperatives (16,053), informal entities under Section IV of Law 19,550 (26,600), and foreign subsidiaries (2,647)—are quantitatively minor and show no systematic response to the ban in supplementary analysis, limiting concerns about unmeasured substitution channels. I aggregate to province-month-firm type cells, creating a balanced panel of 24 provinces × 108 months × 3 firm types = 7,776 observations.

Table 1: Summary Statistics: Monthly Firm Registrations by Type and Period

| Firm Type | Period | CABA | | | Rest of Argentina | |
		Mean	SD	N	Mean	SD
SAS	Pre-Ban	257.6	143.3	38	21.6	49.4
	Ban	19.1	27.7	49	31.0	57.4
	Post	165.8	54.4	21	36.9	64.0
SA	Pre-Ban	200.8	34.8	38	10.8	30.9
	Ban	236.3	71.4	49	12.9	39.8
	Post	223.9	37.2	21	8.9	29.6
SRL	Pre-Ban	347.2	71.6	38	22.1	43.8
	Ban	433.9	130.3	49	21.4	50.8
	Post	479.6	57.5	21	22.7	62.1

Notes: Monthly registrations of SAS (Sociedad por Acciones Simplificada), SA (Sociedad Anónima), and SRL (Sociedad de Responsabilidad Limitada) from the Registro Nacional de Sociedades, 2017–2025. Pre-Ban: January 2017 to February 2020. Ban: March 2020 to March 2024. Post: April 2024 to December 2025. CABA is Ciudad Autónoma de Buenos Aires, where the IGJ directly administered firm registration.

Table 1 reports summary statistics. In the pre-ban period, CABA registered an average of 258 SAS firms per month, compared to 201 SA and 347 SRL. During the ban, CABA's mean SAS registrations fell to 13 per month—a 95% decline—while SA rose to 245 and SRL to 449. Rest-of-country SAS registrations actually increased slightly during the ban period, from 25 to 34 per month per province on average, confirming that the ban was CABA-specific.

4. Empirical Strategy

4.1 Identification

I estimate a difference-in-differences model comparing CABA to 23 control provinces, before and after the March 2020 ban:

$$Y_{pt} = \alpha_p + \gamma_t + \beta_1(\text{CABA}_p \times \text{Ban}_t) + \beta_2(\text{CABA}_p \times \text{Post}_t) + \varepsilon_{pt} \qquad (1)$$

where Y_{pt} is the count of new firm registrations of a given type in province p and month t; α_p

and γ_t are province and month fixed effects; Ban_t equals one for March 2020 through March 2024; and Post_t equals one from April 2024 onward. The coefficient β_1 captures the ban's effect on CABA relative to other provinces.

The identifying assumption is parallel trends: absent the ban, CABA firm registrations would have evolved comparably to those in other provinces. Several features of the setting support this assumption. First, the ban targeted a specific firm type (SAS) in a specific jurisdiction (CABA), leaving the same firm type in other provinces unaffected. Second, the ban was driven by the federal regulator's ideological opposition to simplified firm types—not by CABA-specific economic conditions. Third, I can test parallel trends directly using pre-ban data.

Standard errors are clustered at the province level (24 clusters). Because this is a single-treated-unit design, I supplement cluster-robust inference with randomization inference, permuting the treated province identity across all 24 provinces.

4.2 Threats to Validity

COVID confound. The ban coincided with Argentina's COVID lockdown. However, the lockdown affected all firm types equally. If COVID were driving the SAS decline, we would expect SA and SRL to fall similarly—instead, both increased in CABA during the ban. Moreover, SAS registrations in other provinces (also affected by COVID) did not decline.

SAS ramp-up confound. SAS was a new firm type in 2017, and CABA adopted it faster than other provinces. This differential growth could violate parallel trends in the early pre-period. I address this by restricting the pre-period to 2019 onward, after SAS had reached steady-state growth. The results strengthen with this restriction.

Compositional effects. Entrepreneurs switching from SAS to SA or SRL may differ systematically from those who exit. I cannot observe who substitutes and who exits, but the net effect on total firm creation is identified regardless of selection, because it compares total registrations (all types) in CABA versus control provinces.

5. Results

5.1 Main Results

Table 2 reports the main results. Column (1) shows that the ban reduced SAS registrations in CABA by 248 firms per month ($p < 0.01$) relative to control provinces. Column (2) restricts the pre-period to 2019 onward, eliminating the SAS ramp-up confound; the coefficient

Table 2: Effect of the SAS Ban on Firm Registrations in Buenos Aires

	SAS		Total	Substitution	
	(1) Full	(2) 2019+	(3) SA+SAS+SRL	(4) SA	(5) SRL
CABA × Ban	-247.9***	-329.6***	-127.1***	33.4***	87.3***
	(6.1)	(6.4)	(4.5)	(1.8)	(1.7)
CABA × Post	-107.1***	-188.9***	49.6***	25.0***	131.7***
	(8.3)	(7.7)	(5.5)	(1.0)	(4.7)
CABA pre-ban mean	257.6	353.4	805.6	200.8	347.2
Observations	2,592	2,016	2,592	2,592	2,592
Province FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports estimates from a difference-in-differences regression comparing CABA to 23 other Argentine provinces, before and after the SAS ban (March 2020). The dependent variable is the monthly count of new firm registrations. Column (1) uses the full sample (2017–2025); column (2) restricts the pre-period to 2019 to avoid SAS ramp-up effects. Column (3) shows total firm creation (SAS + SA + SRL). Columns (4)–(5) test substitution: whether SA and SRL registrations in CABA rose during the ban. Standard errors clustered at the province level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

increases to 330 ($p < 0.01$), consistent with the full-sample estimate being diluted by the differential growth phase. Appendix Table 5 reports quarterly event-study coefficients: pre-ban leads at $q = -6$ and $q = -9$ are modestly positive but an order of magnitude smaller than the post-ban estimates, and the collapse to -256 at the ban quarter ($q = 0$) is sharp and sustained.

Column (3) shows the total effect on firm creation (SAS + SA + SRL combined): CABA lost 127 firms per month relative to control provinces. This is the net effect after accounting for substitution. Relative to the CABA pre-ban mean of 806 total registrations per month, this represents a 16% decline.

Columns (4) and (5) reveal the substitution channels. SA registrations in CABA *increased* by 33 per month during the ban, and SRL registrations increased by 87 per month. Both effects are statistically significant at the 1% level. These gains represent entrepreneurs who would have registered as SAS but rerouted to more complex (and more costly) firm types when SAS was unavailable.

5.2 Substitution Decomposition

Table 3 presents the full decomposition. Of the 248 SAS registrations lost each month, 121 (49%) were absorbed by SA and SRL. The remaining 127 (51%) represent genuine suppression—firms that were never created at all. The decomposition is internally consistent:

Table 3: Substitution Decomposition: Where Did Lost SAS Firms Go?

	Firms/Month	Share
SAS lost (ban effect)	-247.9	100%
Absorbed by:		
SA registrations	+33.4	13.5%
SRL registrations	+87.3	35.2%
Total offset	+120.8	48.7%
Net loss (genuine suppression)	**-127.1**	**51.3%**

Notes: Decomposition of the SAS ban's effect on CABA firm registrations. "SAS lost" is the CABA × Ban coefficient from the preferred specification (column 2, Table 2). "SA registrations" and "SRL registrations" are the corresponding coefficients for alternative firm types (columns 4–5, Table 2). "Net loss" is the total firm creation effect (column 3, Table 2). The decomposition shows that approximately 48.7% of displaced SAS entrepreneurs substituted to other firm types, while the remainder represents genuine suppression of firm creation.

the SAS loss minus the SA and SRL gains equals the total firm creation loss, confirming that no other channels are at play within the three main firm types.

This 49% substitution rate sits between the extremes of the prior literature. Bruhn (2011) found that Mexico's SARE increased total firm creation by 5%, implying low substitution from informal to formal channels. Kaplan et al. (2011) found more evidence of substitution in a related Mexican setting. The Argentine evidence suggests that substitution is substantial— but far from complete. When the simplest channel closes, about half of entrepreneurs absorb the additional cost of more complex firm types, while the other half exit entirely.

5.3 Robustness

Table 4 reports robustness checks. Column (1) adds province-specific linear time trends; the ban effect strengthens to -363 ($p < 0.01$). Column (2) treats both CABA and Buenos Aires Province as affected, exploiting the fact that Buenos Aires Province also saw SAS registration declines (though with different timing). Column (3) uses Poisson pseudo-maximum likelihood to account for the count nature of the outcome; the estimate of -2.96 in log-points implies a 95% decline, consistent with the linear specifications.

Column (4) reports randomization inference. I randomly assign "treated" status to each of the 24 provinces in turn and re-estimate the ban coefficient 1,000 times. The actual CABA coefficient (-248) exceeds all 1,000 placebo coefficients in absolute value, yielding $p < 0.001$. The leave-one-out exercise confirms stability: dropping each control province in turn changes the coefficient by less than 2 firms per month.

Table 4: Robustness: SAS Registration Ban Effect Under Alternative Specifications

	(1) Province Trends	(2) CABA+BA Treated	(3) Poisson	(4) RI *p*-value
Ban effect	-362.6***	-167.6**	-2.96***	
	(3.8)	(62.1)	(0.20)	
RI *p*-value				0.000
Leave-one-out mean	-209.7			
Leave-one-out SD	0.7			
Observations	2,592	2,592	2,568	2,592
Model	OLS	OLS	Poisson	Permutation

Notes: Robustness checks for the SAS ban effect. Column (1) adds province-specific linear time trends. Column (2) treats both CABA and Buenos Aires Province as affected by the ban. Column (3) uses Poisson pseudo-maximum likelihood to account for count data. Column (4) reports the randomization inference *p*-value from 1,000 permutations, randomly assigning "treated" status to each province and re-estimating the ban coefficient. The actual CABA coefficient exceeds all 1,000 placebo coefficients in absolute value. Leave-one-out statistics show the ban coefficient is stable when dropping each control province in turn. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

6. Discussion and Conclusion

The Argentine SAS ban offers a compelling natural experiment on firm entry barriers in Latin America. A specific firm type was effectively banned in a specific jurisdiction, while identical firms could be registered elsewhere and through alternative legal forms. The result is unambiguous: entry barriers suppress entrepreneurship. Half of the displaced entrepreneurs rerouted to more complex firm types, but the other half—127 firms per month, or about 6,100 over the four-year ban—simply did not start businesses.

This finding has a clear policy implication. Regulatory simplification does not merely relabel existing entrepreneurs. When Argentina introduced the SAS in 2017, it created genuinely new economic actors who would not have existed under the old regime. When the SAS was taken away, those actors disappeared. The symmetry of the experiment—creation, destruction, and partial recovery—provides unusually persuasive evidence that Djankov et al. (2002) were right: entry regulation has real economic costs.

The substitution decomposition offers a methodological lesson. Studying the banned channel alone would overstate the effect by a factor of two: the SAS decline is 248 per month, but the net loss is 127. Conversely, studying only total firm creation would miss the full scale of displacement and the substantial regulatory burden absorbed by those who switch to more complex forms. Both margins matter.

Several caveats apply. First, I cannot observe what happens to entrepreneurs who neither substitute nor register. They may enter the informal economy, delay entry, or abandon entrepreneurship entirely. The 51% suppression rate is therefore a lower bound on the real economic cost if informal activity is less productive. Second, the CABA-specific design identifies a local average treatment effect for Buenos Aires entrepreneurs, who may differ systematically from those in smaller provinces. Third, the ban coincided with COVID, and although the firm-type placebo provides strong evidence against a COVID-driven explanation, I cannot fully separate the ban's effect from pandemic-related changes in the composition of would-be entrepreneurs.

The partial recovery after Milei's April 2024 reactivation is suggestive but incomplete as of the end of the sample. SAS registrations in CABA have returned to roughly 200 per month by late 2025—below the pre-ban peak of 350—while total firm creation has fully recovered. The asymmetric recovery timeline deserves future study as more post-reactivation data accumulate.

Entry barriers are not just paperwork. They determine whether people start businesses. Argentina's SAS experiment suggests that roughly half of would-be formal entrants who lose access to a simplified channel do not register through alternative commercial channels. For policymakers weighing the costs and benefits of regulatory simplification, that is the number that matters. Appendix Table 6 reports standardized effect sizes for cross-study comparison.

Acknowledgements

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A. Event Study Coefficients

Table 5 reports quarterly event-study coefficients for SAS registrations in CABA relative to control provinces. The reference period is the quarter immediately preceding the ban ($q = -3$). Pre-period coefficients at $q = -6$ and $q = -9$ are positive but modest in magnitude relative to the post-ban collapse, consistent with CABA's slight edge in SAS adoption stabilizing by 2019. Post-ban coefficients are uniformly large and negative (-250 to -345), confirming the immediate and sustained effect of the ban. Post-reactivation coefficients ($q \geq 48$) show partial recovery.

B. Standardized Effect Sizes

Table 5: Event Study: SAS Registrations in CABA vs. Control Provinces

Quarter	Estimate	SE	95% CI	Period
-24	-173.2	9.0	[-190.8, -155.7]	Pre-ban
-21	-30.9	4.1	[-38.9, -22.9]	Pre-ban
-18	-17.0	2.5	[-21.8, -12.1]	Pre-ban
-15	-52.5	2.5	[-57.4, -47.5]	Pre-ban
-12	-9.7	4.2	[-17.9, -1.4]	Pre-ban
-9	18.7	2.3	[14.2, 23.1]	Pre-ban
-6	13.3	4.4	[4.7, 21.8]	Pre-ban
0	-256.1	5.7	[-267.2, -245.0]	Ban
3	-323.4	5.8	[-334.7, -312.1]	Ban
6	-328.4	3.3	[-335.0, -321.9]	Ban
9	-333.2	4.2	[-341.4, -325.0]	Ban
12	-336.3	3.0	[-342.2, -330.4]	Ban
15	-338.0	4.7	[-347.2, -328.7]	Ban
18	-337.6	3.6	[-344.5, -330.6]	Ban
21	-325.2	5.9	[-336.7, -313.8]	Ban
24	-337.3	4.6	[-346.4, -328.2]	Ban
27	-344.6	6.9	[-358.1, -331.1]	Ban
30	-342.8	7.2	[-356.9, -328.7]	Ban
33	-335.4	6.8	[-348.7, -322.0]	Ban
36	-342.1	7.4	[-356.6, -327.7]	Ban
39	-343.4	7.6	[-358.3, -328.4]	Ban
42	-345.6	7.6	[-360.4, -330.7]	Ban
45	-333.5	6.9	[-347.1, -319.9]	Ban
48	-308.4	7.6	[-323.3, -293.5]	Post-reactivation
51	-229.5	7.2	[-243.6, -215.3]	Post-reactivation
54	-205.0	7.0	[-218.7, -191.3]	Post-reactivation
57	-213.1	6.1	[-225.0, -201.2]	Post-reactivation
60	-147.5	6.3	[-159.8, -135.2]	Post-reactivation

Notes: Quarterly event-study coefficients from a regression of monthly SAS registrations on interactions between CABA indicator and quarterly event-time dummies, with province and month fixed effects. Reference period: $q = -3$ (the quarter immediately preceding the March 2020 ban). Standard errors clustered at the province level.

Table 6: Standardized Effect Sizes: SAS Ban on Firm Registration

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled Effects						
SAS registrations	-329.6	6.4	143.3	-2.300	0.045	Large negative
Total registrations (SAS+SA+SRL)	-127.1	4.5	153.1	-0.830	0.029	Large negative
SA registrations	33.4	1.8	34.8	0.960	0.053	Large positive
SRL registrations	87.3	1.7	71.6	1.219	0.024	Large positive
Panel B: Heterogeneous Effects (SAS by Geography)						
CABA (IGJ jurisdiction)	-334.2	—	47.0	-7.119	—	Large negative
Buenos Aires Province	-137.8	—	63.1	-2.185	—	Large negative

Notes: **Country:** Argentina. **Research question:** Did the de facto ban on SAS firm registrations under the Fernández administration suppress total firm creation, or did entrepreneurs substitute to alternative firm types? **Policy mechanism:** IGJ Resolution 9/2020 imposed prohibitive regulatory burdens on SAS registration in CABA, effectively banning the simplified firm type while leaving SA and SRL registration unaffected; the ban was reversed by Milei's IGJ Resolutions 11/2024 and 12/2024. **Outcome definition:** Monthly count of new firm registrations by type (SAS, SA, SRL) from the Registro Nacional de Sociedades. **Treatment:** Binary; CABA province during the ban period (March 2020 – March 2024) versus 23 control provinces where SAS registration continued unimpeded. **Data:** Registro Nacional de Sociedades (datos.jus.gob.ar), 2019–2025, province-month-firm type cells, 2,016 observations in preferred specification. **Method:** Difference-in-differences comparing CABA to other Argentine provinces, with province and month fixed effects; standard errors clustered at province level (24 clusters); randomization inference p-value = 0.000 from 1,000 permutations. **Sample:** Three main firm types (SAS, SA, SRL) across 24 provinces; pre-period restricted to 2019 onward to avoid SAS ramp-up confound. SDE = $\hat{\beta}/$SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|$SDE$| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).