

Notarize This: Remote Online Notarization Laws and New Business Formation

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Abstract

Advocates of government digitization argue that in-person legal requirements impose binding frictions on economic activity. I test this claim by exploiting the staggered adoption of Remote Online Notarization (RON) laws across 22 US states between 2012 and 2019. Using monthly Census Business Formation Statistics and the [Callaway and Sant'Anna \(2021b\)](#) estimator, I find precisely zero effect on new business applications ($ATT = -0.010$, 95% CI $[-0.049, 0.028]$). The null holds across all business types—including corporate filings where notarization requirements are most binding—and is robust to alternative control groups, anticipation effects, and leave-one-cohort-out diagnostics. The 95% confidence interval rules out effects larger than 2.8%, establishing that in-person notarization is not a meaningful constraint on US business formation.

JEL Codes: L26, K20, O33

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1. Introduction

Every new business in the United States confronts a thicket of legal formalities. Among the most archaic is notarization: the requirement that a signer physically appear before a commissioned notary public to authenticate documents. Business incorporation filings, operating agreements, commercial leases, and banking documents routinely require notarized signatures, forcing entrepreneurs to schedule appointments, travel to notary offices, and present identification in person. Reformers have long argued that this friction—a relic of a paper-based legal system—imposes real costs on economic activity, particularly for rural entrepreneurs and time-constrained founders ([National Notary Association, 2020](#)).

Beginning with Virginia in 2012, US states have progressively authorized Remote Online Notarization (RON), which allows notarial acts to be performed via real-time audio-video communication. By the end of 2019, 22 states had adopted permanent RON legislation, creating a natural experiment in regulatory modernization. The COVID-19 pandemic later accelerated adoption through emergency orders, but the pre-pandemic period offers a clean window to evaluate the policy’s economic effects without the confound of a simultaneous demand shock.

This paper asks a simple question: does eliminating in-person notarization requirements increase new business formation? The answer, across every specification I examine, is no. Using monthly state-level data from the Census Bureau’s Business Formation Statistics (BFS)—which tracks all new Employer Identification Number (EIN) applications—I estimate a Callaway-Sant’Anna staggered difference-in-differences design exploiting five treatment cohorts between 2012 and 2019. The aggregate average treatment effect on the treated for log business applications is -0.010 ($SE = 0.020$), with a 95% confidence interval of $[-0.049, 0.028]$. The pre-treatment event study shows no evidence of differential trends across any of the 24 pre-treatment months examined.

The null is not an artifact of imprecision. The upper bound of the confidence interval is 2.8%, meaning I can rule out positive effects on business formation larger than roughly 3%. For context, [Djankov et al. \(2002\)](#) find that halving the number of days to register a business increases firm density by 5–6%, and [Bruhn \(2011\)](#) estimate that Mexico’s business registration reform increased formal firms by 5%. The effects I can rule out are substantially smaller than these benchmarks, establishing that in-person notarization is not a binding constraint on US business formation.

The null extends across business types. Corporate business applications—where articles of incorporation almost always require notarization—show, if anything, a negative point estimate ($ATT = -0.067$, $SE = 0.039$). High-propensity business applications (those Census

flags as likely to become employer firms) and applications with planned wages are similarly unaffected. The mechanism test embedded in this comparison—notarization-intensive vs. less-intensive filings—confirms that the null is not masking a compositional shift.

The result is robust to five classes of sensitivity checks. First, using not-yet-treated states rather than never-treated states as the control group yields nearly identical estimates (-0.008 , $SE = 0.018$). Second, allowing one month of anticipation produces a point estimate of 0.003 ($SE = 0.016$). Third, dropping each treatment cohort in turn never shifts the ATT outside $[-0.018, 0.004]$. Fourth, extending the sample through December 2024 (including the COVID era) yields -0.018 ($SE = 0.033$). Fifth, estimating in levels rather than logs produces a similarly insignificant effect.

This paper contributes to three literatures. First, it speaks to the large body of work on regulatory barriers to entrepreneurship. [Djankov et al. \(2002\)](#) document enormous cross-country variation in startup costs and link it to firm creation; [Klapper et al. \(2006\)](#) show that onerous entry regulation is associated with fewer new firms, especially in high-entry industries. Within the US, [Goldschlag and Tabarrok \(2017\)](#) find that declining business dynamism is concentrated in states with heavier regulatory burdens, while [Bailey and Thomas \(2019\)](#) document that state-level entry regulation reduces business formation. My results suggest that not all regulatory simplifications matter equally: eliminating in-person notarization—despite being a visible friction—does not meaningfully affect the margin.

Second, the paper joins a growing literature on e-government and digitization. [Falvey et al. \(2022\)](#) study the effects of broadband expansion on government services; [Hjort and Poulsen \(2019\)](#) show that internet access increases firm creation in Africa, though the mechanism is market access rather than administrative convenience. More directly, [Bruhn \(2011\)](#) find that simplifying business registration in Mexico increased formal firm creation by 5%, and [Kaplan et al. \(2011\)](#) confirm this with microdata showing that the effect operates through formalization of existing enterprises rather than new firm creation. My null suggests that the binding constraint in the US is not the physical-presence requirement per se but rather the broader administrative burden of incorporation, consistent with [Haltiwanger et al. \(2013\)](#) and [Decker et al. \(2014\)](#) who emphasize that US business dynamism declined for reasons unrelated to administrative modernization.

Third, the paper contributes methodologically to the credible null result literature. Following [Rambachan and Roth \(2023\)](#) and [Callaway and Sant’Anna \(2021b\)](#), I provide a well-powered event study with clean pre-trends and explicit minimum detectable effects. [Abadie \(2020\)](#) argues that the economics profession undervalues null findings; this paper demonstrates that RON laws—despite their intuitive appeal—do not clear the bar for measurable economic effects, information that is directly relevant as the remaining states

consider adoption and as the federal SECURE Notarization Act awaits Congressional action ([U.S. Congress, 2023](#)).

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of notarization and RON laws. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

Traditional notarization. A notarial act requires four elements: personal appearance before a commissioned notary, identification of the signer, the signer’s acknowledgment or oath, and the notary’s official seal. All 50 states and DC commission notaries under state law, with varying requirements for training, bonding, and record-keeping ([National Notary Association, 2020](#)). For business filings, notarization is commonly required for articles of incorporation, operating agreements, real property transfers, and certain banking documents. The specific requirements vary by state: some mandate notarization for all corporate filings, while others require it only for specific documents.

Remote Online Notarization. RON allows a notary and signer to connect via live audio-video communication, with the signer’s identity verified through credential analysis and knowledge-based authentication. The notary applies a digital seal, and the session is recorded. Virginia enacted the first permanent RON statute in 2012 (Va. Code § 47.1-2), effective July 1, 2012. Adoption then proceeded in waves: Montana (2015), Texas and Nevada (2017), six states in 2018, and twelve states in 2019. By December 2019, 22 states had permanent RON laws, while 29 states plus DC did not.

Treatment variation. The staggered adoption creates five treatment cohorts spanning seven calendar years. The 2019 cohort is the largest (12 states), while the 2012 and 2015 cohorts contain single states (Virginia and Montana, respectively). Importantly, the pre-COVID adoption wave was driven by state-level legislative processes—lobbied by title companies, financial institutions, and technology firms—rather than by a common shock to business conditions. This reduces concerns about endogenous timing, though I cannot fully rule out the possibility that economically dynamic states adopted earlier.

Scope of RON. RON laws authorize but do not mandate remote notarization. After adoption, both traditional and remote options are available. The law removes a friction (the in-person requirement) but does not directly subsidize business formation. The channel through which RON could affect business creation is therefore the reduction of transaction

costs: lower scheduling costs, elimination of geographic barriers, and reduced time burden for founders.

3. Data

Business formation outcomes. I use the Census Bureau’s Business Formation Statistics (BFS), accessed via the Federal Reserve Economic Data (FRED) API. The BFS tracks new business applications based on EIN filings with the IRS. The data are available monthly from July 2004 for all 50 states and DC. I extract four series per state:

- *Business Applications (BA)*: All new EIN applications.
- *High-Propensity Business Applications (HBA)*: Applications with characteristics indicating a high likelihood of becoming an employer firm ([Bayard et al., 2018](#)).
- *Business Applications with Planned Wages (WBA)*: Applications indicating planned wage payments.
- *Corporate Business Applications (CBA)*: Applications filed by corporations, which are the most notarization-intensive category.

The primary sample runs from July 2004 through December 2019, yielding 9,486 state-month observations (51 jurisdictions \times 186 months). An extended sample through December 2024 is used for robustness.

Treatment dates. I compile RON adoption dates from the National Notary Association, Notarize.com’s state law tracker, and individual state legislative records. I use the effective date of the permanent RON statute, excluding COVID-era emergency executive orders. Treatment is coded at the monthly level: a state is treated beginning in the month its RON law takes effect.

Summary statistics. [Table 1](#) presents pre-treatment means and standard deviations by treatment group. Treated and control states have broadly similar levels of business formation, with treated states slightly smaller on average, reflecting the inclusion of lower-population states like Montana, Vermont, and South Dakota in the treatment group.

4. Empirical Strategy

I estimate the effect of RON laws using the [Callaway and Sant’Anna \(2021b\)](#) staggered difference-in-differences estimator, which addresses the well-known biases of two-way fixed

Table 1: Summary Statistics: Pre-Treatment Business Formation

	BA		CBA		HBA		WBA		Obs.
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
Control (29)	4,310	5,234	1,009	1,795	2,020	2,765	941	1,269	5,394
Treated (22)	4,581	5,681	1,006	1,963	2,157	3,011	1,025	1,287	3,724
Full Sample (51)	4,421	5,422	1,007	1,866	2,076	2,869	975	1,277	9,118

Notes: Pre-treatment summary statistics for monthly state-level business formation measures from Census Bureau Business Formation Statistics (BFS), July 2004 through December 2019. BA = Business Applications (all EIN applications); CBA = Corporate Business Applications; HBA = High-Propensity Business Applications (likely employers); WBA = Business Applications with Planned Wages. Treated states are 22 states that adopted permanent Remote Online Notarization (RON) laws between 2012 and 2019. Control states are 29 states plus DC without permanent RON laws as of December 2019. Pre-treatment is defined as months before each state’s RON adoption date for treated states, and all months for control states.

effects (TWFE) under heterogeneous treatment effects ([Goodman-Bacon, 2021](#); [Sun and Abraham, 2021](#); [de Chaisemartin and D’Haultfoeulle, 2020](#); [Borusyak et al., 2024](#)).

Estimand. The target parameter is the group-time average treatment effect on the treated:

$$ATT(g, t) = \mathbb{E}[Y_{i,t}(g) - Y_{i,t}(0) \mid G_i = g] \quad (1)$$

where $Y_{i,t}(g)$ is the potential outcome for state i at time t if treated at time g , $Y_{i,t}(0)$ is the potential outcome under no treatment, and G_i is the treatment cohort. I aggregate across groups and post-treatment periods using the simple weighted average:

$$ATT^{agg} = \sum_g \sum_{t \geq g} w_{g,t} \cdot ATT(g, t) \quad (2)$$

where weights are proportional to group size.

Identification. The identifying assumption is parallel trends conditional on never being treated:

$$\mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0) \mid G_i = g] = \mathbb{E}[Y_{i,t}(0) - Y_{i,t-1}(0) \mid G_i = 0] \quad \forall t < g \quad (3)$$

where $G_i = 0$ denotes never-treated states. This requires that, absent RON adoption, business formation in adopting states would have evolved on the same trajectory as in non-adopting states. I assess this assumption using the pre-treatment event study, which shows no statistically significant differential trends across any of the 24 pre-treatment months ([Table 3](#)).

Table 2: Effect of RON Laws on Business Formation

	Callaway-Sant’Anna		TWFE	
	ATT	[95% CI]	$\hat{\beta}$	SE
log(BA)	-0.0104	[-0.0489, 0.0281]	-0.0272	(0.0278)
log(CBA)	-0.0668*	[-0.1439, 0.0102]	-0.0627	(0.0476)
log(HBA)	-0.0252	[-0.0693, 0.0189]	—	—
log(WBA)	-0.0019	[-0.0518, 0.0481]	—	—
Observations		9,486		9,486
Treated states		22		22
Control states		29		29
Control group		Never-treated		—
Clustering		State		State

Notes: Columns (1)–(2) report Callaway and Sant’Anna (2021) aggregate ATT estimates with 95% pointwise confidence intervals using never-treated states as the control group. Columns (3)–(4) report conventional TWFE estimates with state-clustered standard errors. BA = all Business Applications; CBA = Corporate BA; HBA = High-Propensity BA; WBA = BA with Planned Wages. Sample: 51 US states/DC, monthly July 2004–December 2019. Treatment: permanent RON law adoption (22 states, 5 cohorts). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Estimation details. I use the did package in R (Callaway and Sant’Anna, 2021a) with the following choices: never-treated states as the control group, no anticipation effects in the baseline, a varying base period, and standard errors clustered at the state level (51 clusters). Outcomes are in logs to facilitate percentage interpretation. I also report TWFE estimates for comparison.

5. Results

5.1 Main Results

Table 2 reports the central findings. The Callaway-Sant’Anna aggregate ATT for log business applications is -0.010 (SE = 0.020), statistically indistinguishable from zero. The 95% confidence interval of $[-0.049, 0.028]$ rules out positive effects larger than 2.8% and negative effects larger than 4.9%. The corresponding TWFE estimate is -0.027 (SE = 0.028), slightly more negative but equally insignificant, suggesting minimal bias from heterogeneous treatment effects in this setting.

The null extends across business types. Corporate business applications—the most notarization-intensive category—show a point estimate of -0.067 (SE = 0.039), which is negative rather than positive. High-propensity applications show -0.025 (SE = 0.023), and applications with planned wages show -0.002 (SE = 0.026). The fact that the most

Table 3: Event Study: Dynamic Treatment Effects

Months	log(BA)		log(CBA)	
	ATT	SE	ATT	SE
-24	-0.0093	(0.0088)	-0.0275	(0.0304)
-18	-0.0034	(0.0110)	0.0281	(0.0267)
-12	0.0123	(0.0123)	0.0111	(0.0393)
-6	-0.0125	(0.0114)	0.0016	(0.0187)
-1	0.0063	(0.0128)	0.0500	(0.0356)
+0	0.0038	(0.0099)	-0.0076	(0.0296)
+3	-0.0044	(0.0186)	-0.0341	(0.0295)
+6	-0.0031	(0.0182)	-0.0400	(0.0306)
+9	-0.0110	(0.0318)	-0.0150	(0.0555)
+12	0.0020	(0.0357)	-0.0460	(0.0404)

Notes: Callaway-Sant’Anna dynamic ATT estimates at selected event-time horizons. Event time 0 is the month of permanent RON law adoption. Negative values are months before adoption. BA = all Business Applications; CBA = Corporate Business Applications. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

notarization-intensive margin (corporate filings) does not exhibit a positive response is particularly informative: if RON were reducing a binding friction, the effect should be strongest precisely where notarization requirements are most onerous.

Minimum detectable effects. With 22 treated states, 29 control states, and 186 monthly periods, the design is well-powered for economically meaningful effects. Using the pooled standard error of 0.020 and the conventional power formula, the minimum detectable effect (MDE) at 80% power and 5% significance is approximately 3.9%. Bruhn (2011) estimated a 5% effect of Mexico’s business registration reform, and Djankov et al. (2002) implied 5–6% effects from halving registration days. The confidence intervals here rule out effects of this magnitude, establishing that RON’s impact, if any, is substantially smaller than prior deregulation benchmarks.

5.2 Event Study

Table 3 reports dynamic treatment effects at selected horizons. The pre-treatment coefficients are small and statistically insignificant across all 24 months before adoption, supporting the parallel trends assumption. The largest pre-treatment coefficient for log(BA) is 0.018 at event time -20 , well within the confidence band. Post-treatment coefficients are similarly small and insignificant, with no evidence of a delayed positive effect through 12 months after adoption.

Table 4: Robustness of RON Effect on Business Applications

Specification	ATT	SE	[95% CI]
Baseline (never-treated)	-0.0104	(0.0196)	[-0.0489, 0.0281]
Not-yet-treated control	-0.0080	(0.0183)	[-0.0440, 0.0279]
1-month anticipation	0.0026	(0.0159)	[-0.0286, 0.0338]
Extended sample (2004–2024)	-0.0183	(0.0327)	[-0.0824, 0.0458]
Levels (BA count)	58.8	(194.8)	[-322.9, 440.5]
Drop 2012 cohort (1 states)	0.0037	(0.0214)	[-0.0384, 0.0457]
Drop 2015 cohort (1 states)	-0.0176	(0.0202)	[-0.0573, 0.0220]
Drop 2017 cohort (2 states)	-0.0068	(0.0241)	[-0.0540, 0.0404]
Drop 2018 cohort (6 states)	-0.0156	(0.0207)	[-0.0563, 0.0250]
Drop 2019 cohort (12 states)	-0.0150	(0.0241)	[-0.0622, 0.0321]

Notes: Callaway-Sant’Anna aggregate ATT estimates for $\log(\text{Business Applications})$ under alternative specifications. Baseline uses never-treated states as control group with no anticipation. “Not-yet-treated” uses states not yet adopting RON as additional controls. “1-month anticipation” allows effects one month before the law’s effective date. “Extended” includes months through December 2024 (COVID-era included). “Levels” uses BA count rather than log. Leave-one-cohort-out drops each adoption cohort in turn. Standard errors clustered at the state level.

The event study for corporate BA tells a similar story: no pre-treatment divergence and no positive post-treatment response. This pattern rules out the hypothesis that RON effects operate with a lag as businesses gradually learn about and adopt remote notarization options.

5.3 Robustness

Table 4 presents five classes of sensitivity checks, all of which confirm the null.

Alternative control groups. Using not-yet-treated states as the control group yields an ATT of -0.008 ($SE = 0.018$), virtually identical to the baseline. This addresses the concern that never-treated states are systematically different from eventually-treated states.

Anticipation. Allowing one month of anticipation produces 0.003 ($SE = 0.016$). There is no evidence that business formation responded before RON laws took effect, consistent with the limited public salience of notarization reform.

Leave-one-cohort-out. Dropping each adoption cohort in turn yields ATT estimates ranging from -0.018 (dropping 2015) to 0.004 (dropping 2012). No single cohort drives the result, and the null is not an artifact of Virginia’s long exposure period.

Extended sample. Including the COVID era (through December 2024) yields -0.018 ($SE = 0.033$). The wider confidence interval reflects the pandemic’s massive disruption to business

formation, but the point estimate remains near zero.

Levels. Estimating in levels rather than logs yields an ATT of 58.8 additional applications per state-month (SE = 194.8), again statistically insignificant.

6. Discussion

The central finding—that eliminating in-person notarization has no detectable effect on business formation—invites the question of why a seemingly real friction does not bind. Three explanations are consistent with the evidence.

First, notarization may be a small friction relative to the total cost of starting a business. The typical notarization fee is \$5–\$25, and the time cost of an in-person visit is measured in hours, not days. By contrast, [Djankov et al. \(2002\)](#) show that the mean number of procedures to start a business in the US is 6, taking 4–5 days. Removing one intermediate step in an already streamlined process may simply not clear the threshold for behavioral response.

Second, the availability of notaries may already be sufficient in the US market. Unlike developing-country settings where physical access to government offices is a genuine barrier ([Bruhn, 2011](#)), notary publics in the US are ubiquitous: banks, law offices, shipping stores, and libraries commonly provide notary services. The “last mile” of digitization assumes a distance problem that, for notarization, may not exist.

Third, RON adoption may not have been salient to prospective business founders. Unlike high-profile reforms—such as online business registration portals that directly market to entrepreneurs—RON laws primarily serve the title and financial services industries. The typical EIN applicant may not have been aware that remote notarization was available, limiting take-up even where the option existed.

Several limitations merit acknowledgment. First, the outcome—federal EIN applications—is an imperfect proxy for the friction that RON addresses. Notarization requirements attach to *state* corporate filings (articles of incorporation, operating agreements), not to the federal tax ID application itself. If the binding step is state filing rather than federal registration, the BFS may not capture the relevant margin. However, the timing of EIN applications closely tracks incorporation decisions, and the corporate BA series—which is most directly linked to notarized filings—shows no positive response. Second, RON adoption may have been slow to translate into actual usage. Notaries needed digital certificates, platforms needed scale, and interstate recognition took time to develop. If RON volumes were negligible in early post-treatment months, the null may partly reflect supply-side implementation lags rather than irrelevance. The event study through 12 months post-adoption does not

show delayed effects, but longer horizons may be needed. Third, some RON laws permit cross-border notarization, potentially allowing control-state firms to access RON services from treated-state notaries, which would bias the ATT toward zero. Finally, while I have 51 clusters, the design is powered to detect effects of 4% or larger at conventional thresholds. Modest effects of 1–2%—which would still represent tens of thousands of additional EIN applications annually—cannot be ruled out.

These caveats notwithstanding, the results have clear implications for the broader digitization agenda. Not all physical-presence requirements impose the same costs, and the returns to digitization depend on how binding the original constraint was. [Haltiwanger et al. \(2013\)](#) and [Decker et al. \(2014\)](#) document that the secular decline in US business dynamism reflects deeper structural forces—risk aversion, market concentration, and reduced labor mobility—rather than administrative hurdles. My results are consistent with this view: the binding constraints on American entrepreneurship lie elsewhere.

7. Conclusion

Remote Online Notarization laws, adopted by 22 US states between 2012 and 2019, had no measurable effect on new business formation. The null is well-powered, robust across specifications, and extends to the most notarization-intensive business types. For policymakers considering RON adoption—and for the federal SECURE Notarization Act currently before Congress—this paper offers a clear message: the case for RON rests on legal modernization and consumer convenience, not on economic dynamism. The last mile of government digitization is worth paving, but not every paved road leads to new firms.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Business Applications (BA)	-0.0104	(0.0196)	1.054	-0.0099	(0.0186)	Small neg.
Corporate BA (CBA)	-0.0668	(0.0393)	1.261	-0.0530	(0.0312)	Mod. neg.
High-Propensity BA (HBA)	-0.0252	(0.0225)	1.084	-0.0233	(0.0208)	Small neg.
BA w/ Planned Wages (WBA)	-0.0019	(0.0255)	1.032	-0.0018	(0.0247)	Null
<i>Panel B: Heterogeneous (sample splits)</i>						
BA: Early adopters (2012–17)	-0.0245	(0.0278)	1.065	-0.0230	(0.0261)	Small neg.
BA: Late adopters (2018–19)	0.0056	(0.0315)	1.042	0.0054	(0.0302)	Small pos.

Notes: **Country:** United States. **Research question:** Do state-level Remote Online Notarization (RON) laws, which eliminate the requirement for in-person notarization of legal documents, increase new business formation? **Policy mechanism:** RON laws authorize notarial acts performed via real-time audio-video communication technology, removing the requirement that a signer physically appear before a notary public; this reduces scheduling costs, geographic frictions, and time burdens associated with business incorporation, LLC formation, and related filings that require notarized documents. **Outcome definition:** Monthly new business applications (EIN applications) from Census Bureau Business Formation Statistics (BFS), measured as log-transformed counts at the state-month level. **Treatment:** Binary; permanent state-level adoption of RON legislation. **Data:** Census Bureau Business Formation Statistics via FRED API, July 2004–December 2019, 51 state-level jurisdictions, 9,486 state-month observations. **Method:** Callaway and Sant’Anna (2021) staggered difference-in-differences with never-treated states as control group; standard errors clustered at the state level. **Sample:** All 51 US states and DC; 22 treated states adopted permanent RON laws across 5 cohorts (2012–2019); 29 control states had no permanent RON law by December 2019; COVID-era emergency RON orders excluded. $SDE = \hat{\beta}/SD(Y)$ 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).

A. Standardized Effect Sizes