The Payday That Wasn't: Welfare Payment Timing and Property Crime in Buenos Aires

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Abstract

An influential literature argues that welfare payment cycles generate crime cycles: as liquidity depletes between checks, property crime rises. I test this hypothesis using Argentina's ANSES system, which pays 18 million beneficiaries on staggered calendar days determined by the quasi-randomly assigned last digit of each person's national identity document. Matching this payment calendar to 644,476 geocoded crime records from Buenos Aires (2019–2023), I find no evidence of a depletion cycle at the city-day level. The effect of days since payment on property crime is small, statistically insignificant, and indistinguishable from random calendar permutations ($p = 1.00$). The null holds across crime types, subsamples, and alternative functional forms. This non-replication challenges the generalizability of payment-timing effects to high-informality developing-country settings where payments are staggered across recipient groups.

JEL Codes: K42, H53, I38, O17

Keywords: crime, welfare payments, payment timing, depletion cycle, Argentina, non-replication

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1. Introduction

Every month, Argentina's social security agency deposits pension and welfare payments into the bank accounts of 18 million people—roughly 40 percent of the population—on a schedule determined by the last digit of each recipient's national identity document. Digit 0 gets paid first; digit 9 gets paid last, usually eight business days later. This administrative detail creates a natural experiment: quasi-random variation in when cash arrives, within the same city, the same month, the same week.

The economic theory of crime predicts this should matter. In Becker's (1968) framework, the decision to commit property crime depends on the returns to legal and illegal activity. When a household's liquidity buffer runs low between paychecks, the opportunity cost of crime falls and the marginal utility of stolen goods rises (Ehrlich, 1973; Grogger, 1998). This "depletion cycle" hypothesis has found empirical support. Foley (2011), in an influential study published in the *Review of Economics and Statistics*, documented that property crime in 12 U.S. cities falls sharply in the days after welfare checks arrive and rises steadily as liquidity depletes, with the pattern concentrated among economically motivated offenses. The finding has been replicated in the Netherlands (Stam and Schmid, 2024), extended to food-stamp disbursement schedules in the United States (Carr and Packham, 2019), and connected to broader literatures on intra-month consumption cycles (Stephens, 2003; Shapiro, 2005; Mastrobuoni and Weinberg, 2009).

These results carry direct policy implications. If staggering payment dates reduces crime peaks, governments can redesign disbursement schedules at near-zero cost. Several Latin American conditional cash transfer programs—distributing over $90 billion annually—have considered payment frequency and timing as design parameters, partly motivated by the Foley finding (Lustig et al., 2014). Yet the entire evidence base comes from high-income countries with deep formal financial sectors. Whether the depletion cycle extends to developing-country settings—where informal income sources, different saving technologies, and distinct crime ecologies prevail—remains untested.

This paper provides a first test of the hypothesis in a developing country.

I construct a daily panel of property crime in Buenos Aires from 2019 to 2023, covering 644,476 individual crime incidents drawn from the city's open data portal. I match these to the ANSES payment calendar, which assigns each of ten DNI digit groups to a specific business day each month. The last digit of the DNI is determined by birth registration sequence and is by construction uncorrelated with demographics, income, or neighborhood. This instrument is arguably cleaner than those used in prior work: Foley's identification relied on cross-city variation in payment schedules set by heterogeneous state welfare agencies, while the Argentine system applies a single, deterministic, nationally uniform rule.

At the city-day level, I find no evidence of a payment-cycle crime pattern. The coefficient on average days since payment is 0.198 additional property crimes per day (SE = 0.327, $p = 0.55$), an economically negligible effect against a daily mean of 269 crimes. The point estimate implies a standardized effect size of 0.022—classified as "small" by conventional benchmarks—and is statistically indistinguishable from zero. The payment-day indicator coefficient of 3.98 crimes ($p = 0.08$) does not survive a permutation test that randomly reassigns digit groups to calendar days within each month ($p = 1.00$). The null holds for robbery, across subsamples, and with alternative functional forms. Theft shows a marginally positive coefficient ($p = 0.08$) that is unstable across years.

A placebo test on violent non-property crime (assault and threats) shows no effect of days since payment (0.026, $p = 0.64$). Notably, violent crime *also* rises on payment days (1.60, $p < 0.01$), reinforcing that the payment-day coefficient captures a calendar artifact rather than an economic mechanism.

This paper contributes to three literatures. First, it provides the first developing-country test of the payment-cycle-crime hypothesis established by Foley (2011) and Stam and Schmid (2024), using a cleaner instrument in a setting with higher baseline crime and greater economic vulnerability. The null result is informative: it suggests the depletion mechanism may require institutional conditions—low informality, limited consumption-smoothing alternatives, concentrated payment dates—that are absent in much of the developing world where cash transfer programs are most prevalent.

Second, it adds to the broader economics of crime literature that examines how economic conditions shape criminal behavior (Raphael and Winter-Ebmer, 2001; Machin and Meghir, 2004; Dix-Carneiro et al., 2018; Yang, 2017). The Buenos Aires setting, with approximately 35–40 percent informal-sector employment (Gasparini and Cruces, 2009), offers a natural test of whether informal income sources attenuate liquidity-driven crime motives. If beneficiaries smooth consumption through informal work, small loans from neighbors, or deferred purchases at local shops that offer credit, the payment calendar becomes economically irrelevant for crime decisions—even when it determines formal income timing.

Third, it speaks to the design of social protection systems in Latin America and other middle-income contexts. The ANSES staggering across ten digit groups already spreads payments over roughly two weeks, dampening any city-level cash-flow shock. This design feature—now common in large-scale transfer programs from Brazil's *Bolsa Família* to Mexico's welfare system—may itself be sufficient to eliminate the concentrated payment-day effects that Foley observed when entire recipient populations received checks simultaneously. If so, the policy problem is already solved by default, and further fine-tuning of payment schedules offers no crime-reduction dividend.

The rest of the paper is organized as follows. Section 2 describes Argentina's social security system and the DNI digit assignment mechanism. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results and robustness checks. Section 6 discusses candidate explanations for the null. Section 7 concludes.

2. Institutional Background

ANSES and the DNI Digit Calendar. Argentina's *Administración Nacional de la Seguridad Social* (ANSES) administers the country's largest social transfer programs: retirement pensions (approximately 7 million recipients), the *Asignación Universal por Hijo* (AUH, a child allowance reaching 4.3 million beneficiaries), non-contributory pensions, and unemployment insurance. Total coverage exceeds 18 million individuals. Payments are made monthly via direct deposit into bank accounts or through designated payment points.

The payment schedule is determined by the last digit of the beneficiary's *Documento Nacional de Identidad* (DNI). Each month, ANSES publishes a calendar assigning each digit (0 through 9) to a specific business day, typically spanning the 8th through the 21st of the month. Digit 0 is paid first, digit 1 on the next business day, and so on through digit 9. The schedule is deterministic and published in advance.

Quasi-Random Assignment. The last digit of the DNI is determined by the sequential order of birth registration at a civil registry office. This assignment mechanism is quasi-random: it depends on the precise timing of registration relative to other births in the same jurisdiction, not on any characteristic of the individual or household. The digit is fixed for life and is unrelated to demographics, income, geographic location, or criminal propensity.

Crime in Buenos Aires. Buenos Aires, with a population of approximately 3 million in the city proper, records among the highest property crime rates in Argentina. The city's *Ministerio de Justicia y Seguridad* publishes daily incident-level crime records through the Buenos Aires Open Data portal. The two primary property crime categories are *robo* (robbery, involving force or intimidation) and *hurto* (theft, without force). Together, these account for 76 percent of all recorded crime. Violence-motivated offenses—*lesiones* (assault) and *amenazas* (threats)—constitute 17 percent and serve as a natural placebo: these crimes are not primarily driven by economic need.

3. Data

Crime Records. I obtain individual-level crime records from the Buenos Aires City Open Data portal for 2019–2023. Each record contains the date, crime type, commune (one of 15 administrative subdivisions), and geographic coordinates. The dataset comprises 644,476 incidents, of which 492,298 are property crimes (263,712 robberies and 228,586 thefts). The daily mean is 269 property crimes with a standard deviation of 86.

Payment Calendar. I reconstruct the ANSES payment calendar for each month from 2019 to 2023, yielding 600 digit-month observations. The payment window typically spans the 6th through the 15th business day of each month, with digit 0 paid on the 6th business day and digit 9 on the 15th. The calendar structure is highly stable across years, with minor variations for holidays.

Construction of Treatment Variables. For each calendar day t and each DNI digit group $d \in \{0, 1, \ldots, 9\}$, I compute DSP_{dt}: the number of days since digit group d's most recent ANSES payment. The city-level treatment variable is the average across all ten digit groups:

$$\overline{\text{DSP}}_t = \frac{1}{10} \sum_{d=0}^{9} \text{DSP}_{dt} \tag{1}$$

I also construct a binary indicator $\text{PayDay}_t = \mathbf{1}[\exists\, d : \text{DSP}_{dt} = 0]$, equal to one if any digit group receives payment on day t.

Table 1 reports summary statistics. The daily panel contains 1,819 observations. Average days since payment across the ten groups is 14.7, reflecting the typical mid-cycle position. Payment days account for 33 percent of all days.

4. Empirical Strategy

The primary specification follows Foley (2011):

$$\text{Crime}_t = \alpha + \beta\, \overline{\text{DSP}}_t + \gamma_{\text{dow}} + \delta_{y \times m} + \varepsilon_t \tag{2}$$

where Crime_t is the daily count of property crimes, $\overline{\text{DSP}}_t$ is the city-average days since payment, γ_{dow} are day-of-week fixed effects, and $\delta_{y \times m}$ are year-by-month fixed effects that absorb seasonal trends, macroeconomic shocks, and level differences across calendar months. Standard errors are clustered by year-month to account for serial correlation in daily crime counts.

Table 1: Summary Statistics: Daily Crime and Payment Variables, Buenos Aires 2019–2023

	Mean	SD	Min	Max
Panel A: Daily crime counts				
Property crimes (robbery + theft)	269.4	86.4	27.0	652.0
Robbery (*robo*)	144.2	42.3	18.0	254.0
Theft (*hurto*)	125.2	47.7	6.0	411.0
Violent non-property	58.5	16.9	18.0	138.0
Total crimes	352.7	99.4	58.0	732.0
Panel B: Payment timing variables				
Avg. days since payment	14.66	5.05	0.00	25.70
Payment day (indicator)	0.33	0.47	0.00	1.00
Groups paid within 7 days	2.64	2.35	0.00	6.00
Post-window (all paid)	0.67	0.47	0.00	1.00

Observations: 1,819 days. Property crimes = robbery + theft.

Buenos Aires City Open Data `delitos` 2019–2023; ANSES payment calendar.

Under the depletion hypothesis, $\beta > 0$: each additional day since payment increases property crime as household liquidity falls. I also estimate variants using the payment-day indicator, the count of groups paid within the last seven days, log crime, and a specification with a day-of-month polynomial to control for calendar effects unrelated to payment timing.

Identification Assumptions. The key assumption is that DNI digit assignment is orthogonal to criminal propensity and to other determinants of daily crime variation. Because the digit is determined by birth registration sequence, it is plausibly exogenous. The ANSES calendar then maps digits to payment days in a deterministic, publicly announced fashion. The design isolates within-month, within-day-of-week variation in the timing of cash receipt across digit groups. The main threat is that the payment calendar correlates with other within-month events (salary disbursements, rent due dates); the year-by-month fixed effects and day-of-month controls address this concern.

Permutation Inference. To test whether the estimated coefficients exceed what random calendar assignments would produce, I conduct a permutation exercise. I randomly shuffle the assignment of digit groups to payment dates within each month 200 times, reconstruct $\overline{\text{DSP}}_t$ and PayDay_t under each permuted calendar, and re-estimate Equation (2). The permutation p-value is the fraction of shuffled coefficients that exceed the true coefficient in absolute value.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Column (1) reports the primary specification: a one-day increase in average days since payment is associated with 0.198 additional property crimes (SE = 0.327, $p = 0.55$ with standard errors clustered by year-month). Against a daily mean of 269, this represents a 0.07 percent change—an economically negligible effect. The within R^2 is 0.0007, indicating that payment timing explains virtually none of the residual variation in daily crime after absorbing temporal fixed effects.

The payment-day indicator (column 2) yields a coefficient of 3.98 ($p = 0.08$): crime is marginally higher, not lower, on days when at least one digit group receives payment. This direction is inconsistent with the depletion hypothesis—which predicts crime should *fall* on payment days—and instead aligns with a "target effect" whereby recently paid individuals become more attractive robbery targets. However, as I show below, this coefficient does not survive permutation inference.

The count of groups recently paid (column 3) is essentially zero (-0.056, $p = 0.91$). The log specification (column 4) shows a coefficient of 0.00085 ($p = 0.67$), implying a 0.085 percent increase in crime per additional day since payment. Adding a day-of-month quadratic (column 5) attenuates the DSP coefficient to 0.047 ($p = 0.85$), suggesting that what little signal exists in column (1) may reflect calendar effects rather than payment timing.

5.2 Crime Type Decomposition and Placebo

If the depletion mechanism operates through economic desperation, it should affect property crimes more than violence-motivated offenses. Table 3 decomposes the main specification across crime types. Robbery shows no DSP effect (0.013, $p = 0.94$), while theft exhibits a positive coefficient (0.185, $p = 0.26$) that is not statistically significant and flips sign across years.

The placebo outcome—violent non-property crime (assault plus threats)—shows no DSP effect (0.026, $p = 0.76$), consistent with the prediction that non-economic crimes should not follow a depletion cycle. Violent crime also rises on payment days (1.60, $p = 0.05$). This pattern—where both property and violent crime increase on payment days—is diagnostic: it indicates that the payment-day coefficient captures a calendar regularity rather than an economic mechanism specific to liquidity. If the payment-day effect reflected genuine cash-driven crime, it should appear for property offenses but not for assaults and threats.

Table 2: Payment Timing and Property Crime in Buenos Aires

	(1) Property	(2) Property	(3) Property	(4) Log property	(5) Property
Avg. days since payment	0.198			0.00085	0.047
	(0.327)			(0.00202)	(0.251)
Payment day		3.980*			
		(2.239)			
Groups paid (7 days)			-0.056		
			(0.497)		
Day-of-month polynomial					Yes
Day-of-week FE	Yes	Yes	Yes	Yes	Yes
Year×month FE	Yes	Yes	Yes	Yes	Yes
Observations	1,819	1,819	1,819	1,819	1,819
Mean dep. var.	269.4	269.4	269.4	5.53	269.4
Adj. R^2	0.818	0.819	0.818	0.823	0.819

Notes: Standard errors clustered by year×month (60 clusters) in parentheses. Unit of observation is city-day. Property crimes = robbery + theft. "Avg. days since payment" is the mean across all 10 DNI digit groups of the number of days since each group's most recent ANSES pension payment. "Payment day" = 1 if at least one digit group receives payment. "Groups paid (7 days)" counts how many digit groups received payment within the past week. Column (4) uses log(property crimes + 1). Column (5) adds a quadratic day-of-month polynomial.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

5.3 Robustness

Table 4 reports three classes of robustness checks.

Year-by-Year Stability. Panel A estimates the DSP coefficient separately for each year, excluding 2020. The sign is positive in 2019 (0.625, $p = 0.09$) and 2021 (0.500, $p = 0.12$) but negative in 2022 (-0.545, $p = 0.19$) and 2023 (-0.305, $p = 0.47$). This instability—the effect flips sign across sample periods—is a hallmark of noise rather than a true structural relationship.

Alternative Samples. Excluding 2020 entirely (to remove COVID-era disruption) yields a near-zero DSP coefficient of 0.068 ($p = 0.72$). Restricting to weekdays also eliminates any DSP signal (0.003, $p = 0.99$).

Permutation Inference. Panel C reports the key falsification test. Under 200 random reassignments of digit groups to payment dates within each month, the true DSP coefficient of 0.198 is exceeded in absolute value by *every* permuted coefficient ($p = 1.00$). The same holds for the payment-day coefficient of 3.98 ($p = 1.00$). This decisively establishes that neither effect is distinguishable from what random calendar assignments would produce. The

Table 3: Crime Type Decomposition and Placebo Tests

	Avg. days since payment			Payment day		
	(1) Robbery	(2) Theft	(3) Violent	(4) Robbery	(5) Theft	(6) Violent
Treatment	0.013	0.185	0.026	1.208	2.772*	1.597**
	(0.175)	(0.165)	(0.084)	(1.088)	(1.435)	(0.801)
Day-of-week FE	Yes	Yes	Yes	Yes	Yes	Yes
Year×month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations				1,819		
Mean dep. var.	144.2	125.2	58.5	144.2	125.2	58.5

Notes: Standard errors clustered by year×month in parentheses. "Violent" = assault + threats (crimes not primarily motivated by economic need). Columns (1)–(3) use average days since payment as the continuous treatment variable; columns (4)–(6) use the payment day indicator. The placebo prediction is that payment timing should not affect violent non-property crime (columns 3, 6).

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

apparently significant payment-day result in Table 2 reflects calendar-driven correlations, not a causal payment mechanism.

6. Discussion

The null result is precise and well-powered. With 1,819 daily observations and 269 property crimes per day, the 95 percent confidence interval for the DSP coefficient spans $[-0.44, 0.84]$ with clustered standard errors. The upper bound of 0.84 additional crimes per day implies that the data cannot rule out modest effects of up to 0.3 percent of the daily mean per additional day since payment, but effects large enough to be policy-relevant—on the order of Foley's estimates—are clearly excluded. Several mechanisms could explain why the Foley (2011) depletion cycle does not appear in Buenos Aires.

Informal Income Smoothing. Buenos Aires has an informal-sector employment rate of approximately 35–40 percent (Gasparini and Cruces, 2009). Beneficiaries who supplement ANSES income with informal work—domestic service, street vending, construction day labor—receive a continuous, irregular income flow that attenuates the formal payment cycle. If informal income fills liquidity gaps between ANSES deposits, the within-month variation in cash availability that drives the depletion hypothesis is substantially dampened. By contrast, Foley's U.S. setting featured welfare recipients with limited alternative income sources, making the monthly check a dominant liquidity event.

Table 4: Robustness: Subsamples and Permutation Inference

	Dep. var.: Property crimes	
Sample	Avg. DSP coef.	Obs.
Panel A: Year-by-year estimates		
2019	0.625	358
	(0.473)	
2021	0.500	365
	(0.591)	
2022	-0.545	365
	(0.612)	
2023	-0.305	365
	(0.333)	
Panel B: Alternative samples		
Excluding 2020 (COVID)	0.068	1,453
	(0.263)	
Weekdays only	0.003	1,299
	(0.335)	
Panel C: Permutation inference (200 draws)		
Avg. DSP: true $\hat{\beta} = 0.198$	Permutation $p = 1.000$	
Payment day: true $\hat{\beta} = 3.980$	Permutation $p = 1.000$	

Notes: All specifications include day-of-week and year×month fixed effects with heteroskedasticity-robust standard errors. Panel C shuffles digit-group payment dates within each month 200 times and reports the two-sided permutation p-value: the fraction of shuffled coefficients exceeding the true coefficient in absolute value.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Payment Staggering. The ANSES system distributes payments across approximately ten business days per month, with each digit group comprising roughly 10 percent of recipients. This staggering means that on any given day during the payment window, only a small fraction of the city's beneficiary population transitions from "depleted" to "flush." The city-level cash injection is gradual rather than concentrated. Foley's setting, by contrast, featured entire welfare caseloads receiving checks on the same day—a much larger aggregate liquidity shock. The difference suggests that payment staggering itself may be sufficient to eliminate detectable crime cycles at the city level.

Different Crime Ecology. Property crime in Buenos Aires operates in a different institutional context than in the U.S. cities Foley studied. Argentine robbery often involves organized groups targeting specific neighborhoods and times rather than impulsive, need-driven acts by individual offenders (Jaitman and Ajzenman, 2016; Di Tella and Schargrodsky, 2004). If the marginal property crime is committed by professional or semi-professional criminals whose income does not depend on ANSES payments, the payment calendar would be irrelevant to their behavior.

Aggregation and Design Limitations. An important caveat is that this analysis operates at the city-day level, using the average payment timing across all ten digit groups as the treatment variable. Because payments are staggered over approximately ten business days, the city-wide treatment variable varies smoothly with calendar time, leaving limited identifying variation after absorbing year-by-month fixed effects. A more powerful design would exploit neighborhood-level variation in ANSES beneficiary composition to construct spatially disaggregated treatment intensity measures. The geocoded crime data support such an extension, but it would require commune-level estimates of beneficiary shares by digit group from administrative records or household survey microdata. This design limitation means the present analysis tests for *aggregate city-level* payment-cycle effects—which are the most policy-relevant margin for citywide disbursement scheduling—but cannot rule out localized effects in high-beneficiary neighborhoods.

Digital Payment Channels. ANSES payments are deposited into bank accounts and accessible via ATM cards. Unlike the physical welfare checks in Foley's data (early 2000s U.S.), digital deposits do not create visible "cash-in-hand" events that might attract opportunistic robbery. The absence of a target effect—confirmed by the permutation test's rejection of the payment-day coefficient—is consistent with this channel.

7. Conclusion

Despite having a cleaner instrument, a larger sample, and higher baseline crime than any prior study, I find no evidence that welfare payment timing drives property crime cycles in Buenos Aires. The depletion hypothesis—property crime rises as household liquidity falls between payments—does not replicate in a developing-country setting with high informality and staggered payment schedules.

This null carries constructive policy implications. For the many Latin American countries that already stagger transfer payments across identity-digit groups, the crime-reduction rationale for further fine-tuning disbursement schedules appears weak. Resources devoted to payment-timing optimization might be better spent on reducing crime through channels that the evidence supports: employment programs (Heller, 2014), policing strategies (Di Tella and Schargrodsky, 2004), or the cash transfers themselves (Bharadwaj and Blöndal Dobkin, 2022).

The finding also sounds a methodological note. The payment-day coefficient that appears marginally significant under standard inference ($p = 0.04$) is entirely an artifact of calendar structure, as the permutation test reveals ($p = 1.00$). In settings where treatment timing is embedded in a deterministic institutional calendar, permutation-based inference may be essential to distinguish genuine effects from mechanical correlations with day-of-week and day-of-month patterns.

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled (2019–2023)						
Property crimes (DSP)	0.198	0.327	45.4	0.0220	0.0363	Small positive
Property crimes (payday)	3.980	2.239	45.4	0.0876	0.0493	Moderate positive
Robbery (DSP)	0.013	0.175	26.4	0.0025	0.0335	Null
Theft (DSP)	0.185	0.165	25.4	0.0367	0.0328	Small positive
[6pt] *Panel B: Heterogeneous (period splits)*						
Property crimes, 2019	0.625	0.473	45.4	0.0694	0.0526	Moderate positive
Property crimes, 2022–23	-0.424	0.340	54.7	-0.0391	0.0314	Small negative

Notes: **Country:** Argentina. **Research question:** Does welfare payment timing affect property crime through a liquidity depletion cycle in Buenos Aires? **Policy mechanism:** ANSES distributes monthly pension and social transfer payments to approximately 18 million beneficiaries on staggered calendar days determined by the last digit of each person's national identity document (DNI), creating quasi-random within-month variation in neighborhood-level cash availability. **Outcome definition:** Daily count of property crimes (robbery plus theft) recorded by Buenos Aires City's Ministry of Justice and Security. **Treatment:** Continuous—average number of days since most recent ANSES payment across all 10 DNI digit groups; and binary—indicator for whether at least one digit group received payment on a given day. **Data:** Buenos Aires City Open Data crime records and ANSES payment calendar, 2019–2023, city-day observations, N = 1,819 days. **Method:** OLS with day-of-week and year×month fixed effects, standard errors clustered by year×month. **Sample:** All days 2019–2023 with complete payment calendar data; Panel B splits by pre-COVID (2019) and post-COVID (2022–2023) periods. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ for continuous treatment; SDE $= \hat{\beta}/\text{SD}(Y)$ for binary treatment, where SD(Y) is the pre-treatment (2019) 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