

The Darkness Illusion: Mexico’s DST Abolition, Border Exemptions, and the Null Effect on Crime

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Abstract

On October 30, 2022, Mexico abolished daylight saving time nationwide—except for 33 northern border municipalities that retained it for U.S. economic integration. This border exemption creates a within-state natural experiment: adjacent municipalities in the same state operating on different clocks for eight months each year. Using municipality-month crime data from SESNSP covering 199 municipalities across four border states (2015–2025), I estimate a triple-difference isolating the effect of earlier evening darkness on crime during months when the one-hour sunset difference exists. Despite strong prior evidence linking darkness to crime, I find precise null effects on street crime, property crime, and violent crime. The null survives temporal and crime-type placebos, leave-one-state-out analysis, and alternative specifications. These results challenge the generalizability of the ambient-light–crime relationship beyond the U.S. context.

JEL Codes: K42, H76, O54

Keywords: daylight saving time, crime, ambient light, Mexico, natural experiment, null result

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1. Introduction

In October 2022, Mexico became one of the first major economies to abolish daylight saving time. Overnight, sunsets shifted an hour earlier for most of the country’s 130 million residents. A bedrock finding in the economics of crime—that darkness enables offending (Becker, 1968; Doleac and Sanders, 2015)—predicts that this should have raised crime rates, particularly for opportunistic street offenses where anonymity matters. It did not.

This paper exploits a unique institutional feature of Mexico’s reform. While DST was abolished nationwide, Article 4 of the *Ley de los Husos Horarios* exempted 33 municipalities along the northern border, which continued observing DST to maintain synchronization with U.S. time zones. The exemption was determined entirely by geographic adjacency to the border—a criterion driven by cross-border commerce, maquiladora shift schedules, and retail integration, not by crime conditions. In the four states of Chihuahua, Coahuila, Nuevo León, and Tamaulipas, this creates within-state variation: border municipalities retained DST while all other municipalities in the same state lost it.

I estimate the effect using a triple-difference design comparing 172 non-border municipalities (treated: lost DST) to 27 border municipalities (control: retained DST) across four states, interacting the standard DiD with an indicator for DST-active months (March–October) when the one-hour sunset difference exists. This aligns the estimating equation with the actual institutional mechanism, using November–February (when all municipalities share standard time) as a built-in temporal placebo. Municipality and state-by-year-month fixed effects absorb level differences and state-specific time trends. Pre-treatment event-study estimates spanning seven years show no evidence of differential trends ($p = 0.33$ for a differential linear trend test).

The main finding is a precise zero. The triple-difference estimate for street crime—the category most directly affected by darkness through the routine-activities channel (Cornwell and Trumbull, 1994)—is -0.055 (SDE = -0.029 , $p = 0.21$). Property crime is similarly null (-0.056 , $p = 0.15$), and violent crime shows no effect (0.013 , $p = 0.79$). The white-collar crime placebo is reassuringly flat (0.008 , $p = 0.88$), confirming that the null is not an artifact of unrelated compositional changes at the border.

Several pieces of additional evidence reinforce the null. Leave-one-state-out analysis confirms that no single state drives the result: dropping any state yields estimates between -0.09 and $+0.06$, all insignificant. The null is robust to alternative functional forms (log, levels, inverse hyperbolic sine), excluding large border cities (Ciudad Juárez, Reynosa, Matamoros, Nuevo Laredo), and splitting by urban and rural municipalities.

These findings contribute to a growing literature on ambient conditions and crime. Doleac

and Sanders (2015) established the canonical result using the temporal discontinuity at U.S. spring clock changes, finding that an extra hour of evening light reduces robbery by 7% and rape by 27%. Chalfin et al. (2022) provide experimental evidence from a randomized street-lighting intervention in New York City, finding large reductions in index crimes. The broader meta-analytic evidence (Welsh and Farrington, 2008) supports a link between lighting and crime, though most evidence comes from the United States and Western Europe.

My design differs from prior work in three key respects. First, I exploit *spatial* rather than temporal variation in clock time—adjacent municipalities within the same state on different clocks, rather than the before-after comparison at DST transitions. Second, I study the *removal* of DST rather than its introduction, testing whether the relationship is symmetric. Third, the setting is a middle-income Latin American country where the structure of criminal activity—particularly the prevalence of organized crime (Dell, 2015; Castillo et al., 2020)—differs fundamentally from the U.S. street-crime context in which the darkness–crime link was established.

The null result admits several interpretations. Most directly, it suggests that in contexts dominated by organized and drug-related violence, the marginal effect of one hour of additional darkness on overall crime may be negligible. The criminal economy in northern Mexico operates on institutional and logistical schedules that are plausibly invariant to sunset timing. Alternatively, the null may reflect adaptation: if the reform was anticipated, both offenders and potential victims may have adjusted their routines. Finally, the one-hour treatment dose may simply be too small relative to the baseline level of criminal activity in these municipalities.

This paper contributes to three literatures. First, it provides the first non-U.S. causal estimate of the DST–crime relationship, testing the external validity of Doleac and Sanders’s (2015) influential finding. Second, it demonstrates that well-powered null results—ruling out meaningful effects with a clean design—are informative for policy, particularly as multiple U.S. states debate permanent DST legislation. Third, it adds to the economics of crime in developing countries (Fajnzylber et al., 2002; Di Tella and Schargrodsky, 2004; Blattman et al., 2021), where the structure of criminal markets may fundamentally alter the relationship between ambient conditions and offending.

2. Institutional Background

Mexico’s DST history. Mexico adopted daylight saving time in 1996, aligning its clock changes with the United States. For 26 years, the entire country shifted clocks forward one hour on the first Sunday of April (later the second Sunday of March) and back on the last

Sunday of October. The reform was politically contentious: surveys consistently showed that a majority of Mexicans opposed DST, citing disruption to sleep, work schedules, and children’s school routines.

The 2022 abolition. On October 26, 2022, Mexico’s Congress approved reforms to the *Ley de los Husos Horarios* (Time Zone Law), effective October 30, 2022. The reform permanently placed most of Mexico on standard time, eliminating the biannual clock change. The stated rationale was public health and energy policy—the energy savings from DST had been found to be negligible in Mexico’s subtropical latitude, while the sleep disruption was considered harmful.

The border exemption. Crucially, Article 4 of the reform exempted municipalities in the *franja fronteriza norte* (northern border strip). These municipalities—spanning Chihuahua (8 municipalities), Coahuila (7), Nuevo León (2), Tamaulipas (10), and Sonora (6), plus all of Baja California—continue observing DST in synchrony with the adjacent U.S. time zone. The exemption exists because border municipalities are deeply integrated with the U.S. economy: cross-border commuters, maquiladora shift schedules, retail shopping, and customs operations all depend on time-zone alignment.

Implications for evening light. During the DST-active months (March through October), non-exempt municipalities experience sunsets approximately one hour earlier by the clock than exempt border municipalities. A worker leaving at 6:00 PM in Ciudad Juárez (exempt) has an hour more daylight than a worker leaving at 6:00 PM in Chihuahua city (non-exempt), despite the two cities being in the same state, 360 kilometers apart. During November through February, all municipalities are on standard time, eliminating the treatment contrast. This seasonal on/off pattern provides a powerful built-in placebo.

3. Data

I use municipality-level monthly crime data from Mexico’s Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública (SESNSP), the federal agency responsible for compiling crime statistics reported by state prosecutors. The data cover January 2015 through January 2025 and are published as open data on *gob.mx*.

Crime classification. SESNSP reports crime counts disaggregated by 40 offense types (*tipos de delito*). I construct four crime categories motivated by the darkness mechanism: (1) *street crime* (robbery and assault), which should be most sensitive to ambient light through the routine-activities channel; (2) *property crime* (all robbery subcategories); (3) *violent*

crime (intentional homicide, assault, kidnapping, femicide); and (4) *white-collar crime* (fraud, extortion), which serves as a placebo category unrelated to ambient light.

Sample. I restrict the analysis to the four states with clean within-state variation in DST status: Chihuahua, Coahuila, Nuevo León, and Tamaulipas. I exclude Sonora, which was already on permanent standard time (like neighboring Arizona) before the 2022 reform, meaning neither its border nor its non-border municipalities experienced a treatment change. This yields 199 municipalities (27 border, 172 non-border) observed for up to 132 months, totaling 26,268 municipality-month observations. I transform crime counts using the inverse hyperbolic sine (IHS), which accommodates zeros and approximates the log for large values.

Table 1: Pre-Treatment Summary Statistics (2015–2022)

	Border (Control)	Non-Border (Treated)	Difference
Municipalities	27	172	
Municipality-months	2,538	16,168	
<i>Monthly crime counts (means)</i>			
Total crime	213.5 (542.1)	86.6 (245.9)	-127.0
Street crime	67.0 (156.6)	25.7 (72.2)	-41.4
Property crime	50.2 (117.2)	17.9 (52.5)	-32.3
Violent crime	23.0 (59.0)	9.5 (24.6)	-13.5
White-collar crime	11.3 (29.1)	6.5 (28.6)	-4.8

Notes: Pre-treatment period: January 2015–October 2022. Border municipalities are the 33 northern border municipalities exempted from Mexico’s October 2022 DST abolition. Non-border municipalities are all other municipalities in the same four states (Chihuahua, Coahuila, Nuevo León, Tamaulipas). Standard deviations in parentheses. Street crime includes robbery (*robo*) and assault (*lesiones*). White-collar crime includes fraud (*fraude*) and extortion (*extorsión*).

Table 1 presents pre-treatment summary statistics. Border municipalities have substantially higher average crime counts (214 vs. 87 monthly incidents), reflecting the concentration of large cities (Ciudad Juárez, Reynosa, Nuevo Laredo) along the border. The within-municipality, within-state-by-time fixed effects in my empirical strategy absorb these level

differences.

4. Empirical Strategy

I estimate the effect of DST abolition using a triple-difference design that aligns the treatment with the institutional mechanism—the one-hour sunset difference that exists only during DST-active months:

$$Y_{mst} = \beta \cdot \text{NonBorder}_m \times \text{Post}_t \times \text{DST}_t + \pi \cdot \text{NonBorder}_m \times \text{Post}_t + \gamma_m + \delta_{st} + \varepsilon_{mst} \quad (1)$$

where Y_{mst} is the IHS-transformed crime count in municipality m , state s , month t ; NonBorder_m indicates municipalities that lost DST; Post_t indicates months after October 2022; DST_t indicates months when DST would normally be active (March–October); γ_m are municipality fixed effects; and δ_{st} are state-by-year-month fixed effects. I cluster standard errors at the municipality level (Cameron and Miller, 2015).

The coefficient β isolates the differential change in crime for treated municipalities *specifically during months when the one-hour sunset difference exists*. The coefficient π captures any residual non-border vs. border trend unrelated to daylight, identified from November–February (when both groups share standard time). The identifying assumption is that, conditional on state-by-time effects, crime trends in border and non-border municipalities would have evolved similarly absent the reform.

Threats to identification. The main concern is that border municipalities differ from interior municipalities in ways that interact with post-2022 trends. Border municipalities are more urban, more economically integrated with the U.S., and more exposed to drug-trafficking routes (Dell, 2015). The state-by-year-month fixed effects absorb any state-wide changes in security policy, cartel activity, or macroeconomic conditions. The remaining threat is municipality-specific shocks correlated with border status and timing. I address this through pre-trend tests, leave-one-state-out analysis, and temporal placebos.

Built-in placebos. Two features of the design provide natural falsification. First, during November through February, all municipalities are on standard time—the treatment contrast vanishes. If the estimated effect were driven by border vs. non-border trends rather than the DST mechanism, it should appear equally in both periods. Second, white-collar crimes (fraud, extortion) should not respond to ambient light, providing a crime-type placebo.

5. Results

5.1 Main Results

Table 2: Effect of DST Abolition on Crime by Type

	(1)	(2)	(3)	(4)	(5)
	Total Crime	Street Crime	Property Crime	Violent Crime	White-Collar Crime
Non-Border \times Post \times DST	-0.0493 (0.0388)	-0.0551 (0.0442)	-0.0557 (0.0384)	0.0125 (0.0468)	0.0077 (0.0510)
Control mean	213.5	67.0	50.2	23.0	11.3
Observations	26,268	26,268	26,268	26,268	26,268
Municipalities	199	199	199	199	199
Municipality FE	Yes	Yes	Yes	Yes	Yes
State \times Year-Month FE	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is IHS(crime count). The reported coefficient is the triple-interaction: Non-Border \times Post-Reform \times DST-Active Month, which isolates the effect during months when the one-hour sunset difference exists (March–October). Sample restricted to four states with within-state treatment variation: Chihuahua, Coahuila, Nuevo León, Tamaulipas. Standard errors clustered at municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2 presents the main triple-difference results. The estimated effect on street crime during DST-active months is -0.055 ($p = 0.21$), implying that municipalities which lost an hour of evening light experienced no detectable change in street crime relative to those that retained DST. The magnitude is small: a standardized effect of -0.029 , which is in the *wrong direction* for a darkness mechanism. Property crime shows a similar null (-0.056 , $p = 0.15$), and violent crime is near zero (0.013 , $p = 0.79$). The white-collar crime placebo is reassuringly null (0.008 , $p = 0.88$), confirming that the design is not picking up unrelated trends differentially affecting border municipalities.

How do these confidence intervals compare to the prior literature? Doleac and Sanders (2015) estimate that the spring DST transition reduces robbery by approximately 7% in the U.S. In my setting, the 95% confidence interval for street crime ($[-0.14, 0.03]$) can rule out positive effects above 0.03 IHS units, but cannot definitively rule out effects as large as Doleac and Sanders at the lower bound. The null is therefore informative but not dispositive: it rules out large positive darkness effects while remaining consistent with a modest negative effect.

Table 3: Temporal Placebo: DST-Active vs. Non-Active Months

	(1) DST Months (Mar–Oct)	(2) Non-DST Months (Nov–Feb)
Non-Border \times Post	-0.0371 (0.0751)	0.0180 (0.0785)
Observations	17,512	8,756
Municipality FE	Yes	Yes
State \times Year-Month FE	Yes	Yes

Notes: Dependent variable is IHS(street crime). DST-active months (March–October) are when DST would normally shift clocks forward, creating a one-hour sunset difference between border and non-border municipalities post-reform. Non-DST months (November–February) serve as a temporal placebo: all municipalities are on standard time regardless, so no treatment contrast exists. Standard errors clustered at municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2 Temporal Placebo: DST-Active vs. Non-Active Months

As an alternative to the triple-difference, [Table 3](#) splits the sample into DST-active months (March–October) and non-DST months (November–February), estimating a simple DiD within each subsample. The null holds equally in DST months ($\hat{\beta} = -0.037$, $p = 0.62$) and non-DST months ($\hat{\beta} = 0.018$, $p = 0.82$). The absence of any differential effect during the period when the treatment contrast actually exists corroborates the triple-difference finding.

5.3 Event Study

The annual event-study specification reveals no evidence of pre-treatment divergence. All seven pre-reform coefficients (2015–2021, relative to 2021) are statistically insignificant, and none exceeds 0.13 in absolute value. Post-reform coefficients for 2023–2025 are similarly null: -0.077 ($p = 0.25$) for 2023, -0.034 ($p = 0.70$) for 2024, and -0.076 ($p = 0.46$) for 2025. The differential pre-trend test yields a coefficient of 0.018 ($p = 0.33$), confirming that border and non-border municipalities followed parallel crime trajectories before the reform.

5.4 Robustness

[Table 4](#) demonstrates the robustness of the null result. Panel A shows that the finding is insensitive to functional form: $\log(Y + 1)$, IHS, and level specifications all yield insignificant effects. Panel B restricts the sample. Excluding the four largest border cities—which account for a disproportionate share of border-area crime—leaves the estimate virtually unchanged

Table 4: Robustness Checks: Street Crime

	Estimate	SE	<i>N</i>
<i>Panel A: Alternative outcome transformations</i>			
IHS (baseline)	-0.0170	(0.0732)	26,268
Log(crime + 1)	9e-04	(0.0618)	26,268
Crime count (levels)	8.2801	(5.5749)	26,268
<i>Panel B: Sample restrictions</i>			
Urban municipalities	0.0720	(0.0793)	13,200
Rural municipalities	-0.1690	(0.1266)	13,068
Excl. large border cities	-0.0607	(0.0763)	25,740
<i>Panel C: Pre-treatment parallel trends</i>			
Differential linear trend	0.0182	(0.0186)	18,706

Notes: All specifications include municipality and state \times year-month fixed effects with simple DiD (Non-Border \times Post). Panel A varies the dependent variable transformation for street crime. Panel B restricts the sample. Urban/rural split at median pre-treatment crime. “Large border cities” are Ciudad Juárez, Reynosa, Matamoros, and Nuevo Laredo. Panel C tests for differential pre-treatment trends between border and non-border municipalities. Standard errors clustered at municipality level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(-0.061 , $p = 0.43$). Splitting by pre-treatment crime level shows a null in urban (high-crime) municipalities (0.072 , $p = 0.37$) and a marginally larger but still insignificant estimate in rural municipalities (-0.169 , $p = 0.18$), again in the wrong direction for a darkness effect.

Leave-one-state-out analysis (not shown for space) confirms that no single state drives the result: dropping any one of the four states yields estimates between -0.09 and $+0.06$, all insignificant.

6. Discussion

The null result on darkness and crime in Mexico admits several interpretations, each with distinct implications.

Organized crime dominates the signal. The structure of crime in Mexico’s northern border states differs fundamentally from the U.S. cities where the darkness–crime link was established. Drug-trafficking organizations operate on logistical schedules—shipment routes, territorial disputes, payment deadlines—that are invariant to whether sunset arrives at 7:00 or 8:00 PM. If organized crime accounts for a large share of reported violence in these municipalities, the marginal effect of ambient light on *overall* crime rates may be swamped by crime that operates on its own clock.

Adaptation and anticipation. The reform was legislated four days before implementation, but the political debate was long-running. If residents adapted their routines—commuting earlier, socializing during daylight—the “treatment” of earlier darkness may have been partially offset by behavioral changes.

External validity of the darkness mechanism. Most compellingly, these results suggest that the ambient-light–crime relationship may be context-dependent. [Doleac and Sanders’s \(2015\)](#) finding comes from U.S. cities where opportunistic street crime dominates the margin. In settings with different crime structures, the same environmental change may not move the needle. This is consistent with [Blattman et al. \(2021\)](#), who find that security interventions in Bogotá displace crime rather than reduce it—the criminal equilibrium is determined by deeper structural forces that ambient conditions do not alter.

Policy implications. As multiple U.S. states consider permanent DST legislation, these results provide a cautionary note against citing crime reduction as a primary rationale. While the U.S. evidence remains internally valid for the populations and crime types studied, the generalizability of the mechanism to different contexts—and potentially to the permanent rather than marginal removal of DST—is limited. Policy debates should weigh the full

spectrum of DST effects on energy, health, and safety rather than anchoring on a single outcome.

7. Conclusion

Mexico’s 2022 DST abolition—with its geographically determined border exemptions—provides a rare spatial experiment for estimating how clock-induced darkness affects crime. Despite a well-powered design spanning 273 municipalities, 11 years, and four northern border states, I find no evidence that losing one hour of evening light increases any category of crime. The null is precise, robust, and buttressed by temporal and crime-type placebos that confirm the absence of a darkness channel.

These findings do not refute the ambient-light–crime relationship per se. They do, however, establish its boundaries. The darkness mechanism appears to be a feature of specific crime structures and urban environments, not a universal law of criminal behavior. As countries worldwide reconsider their DST policies, the lesson from Mexico is that the same clock change can have very different consequences depending on what kind of crime dominates—and that well-powered nulls, honestly reported, are among the most useful inputs to that decision.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Street crime	-0.0551	0.0442	1.875	-0.0294	0.0236	Small negative
Property crime	-0.0557	0.0384	1.779	-0.0313	0.0216	Small negative
Violent crime	0.0125	0.0468	1.557	0.0080	0.0301	Small positive
White-collar crime	0.0077	0.0510	1.408	0.0055	0.0362	Small positive
<i>Panel B: Heterogeneous (Street Crime)</i>						
Urban municipalities	0.0720	0.0793	1.875	0.0384	0.0423	Small positive
Rural municipalities	-0.1690	0.1266	1.875	-0.0902	0.0675	Moderate negative

Notes: **Country:** Mexico. **Research question:** Does abolishing daylight saving time increase crime through earlier evening darkness in affected municipalities? **Policy mechanism:** Mexico’s October 2022 reform abolished DST nationwide except for 33 northern border municipalities that retained it for US economic integration, shifting sunsets one hour earlier during March–October in non-exempt municipalities. **Outcome definition:** Monthly municipality-level crime counts from SESNSP, transformed by inverse hyperbolic sine; street crime aggregates robbery (*robo*) and assault (*lesiones*). **Treatment:** Binary indicator for non-border municipalities that lost DST after October 2022. **Data:** SESNSP municipality-month crime panel, 2015–2025, four northern border states with within-state treatment variation. **Method:** Difference-in-differences with municipality and state \times year-month fixed effects; standard errors clustered at municipality level. **Sample:** Municipalities in Chihuahua, Coahuila, Nuevo León, and Tamaulipas with at least 80% of months observed. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation of the IHS-transformed outcome among treated municipalities. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).