Does the Clock Kill? Time Zone Boundaries and Morning Traffic Fatalities in the United States

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Abstract

Americans on the late-sunset side of time zone boundaries sleep less and suffer higher rates of obesity, diabetes, and heart disease (Giuntella and Mazzonna, 2019). A natural prediction is that this chronic circadian misalignment also kills — through drowsy driving on the morning commute. I test this prediction using a spatial regression discontinuity at the three continental US time zone boundaries, with 468,213 geocoded fatal crashes from NHTSA's Fatality Analysis Reporting System (2010–2023). I find no evidence that morning traffic fatalities increase on the late-sunset side (RDD estimate: -0.016, $p = 0.17$). The null is stable across seven bandwidths, three boundaries, and a battery of placebo and donut tests. This "hard null" suggests that the chronic health costs of social jetlag do not extend to acute traffic safety — a boundary condition for the growing circadian economics literature.

JEL Codes: I12, R41, J18
Keywords: time zones, social jetlag, traffic fatalities, spatial RDD, circadian misalignment

1. Introduction

Every year, approximately 38,000 Americans die in traffic crashes. Drowsy driving accounts for an estimated 6,000 of these fatalities (AAA Foundation for Traffic Safety, 2016). At the same time, a growing literature documents that millions of Americans live in a state of chronic circadian misalignment — their social schedules (work start times, school bells) force them awake before their bodies are ready, producing what chronobiologists call "social jetlag" (Wittmann et al., 2006). The natural question is whether this chronic sleep deprivation is killing people on the road.

The answer might seem obvious: of course tired people crash more. But the relationship between *chronic* circadian misalignment and *acute* driving fatalities is far from clear. Chronic sleep loss differs from acute sleep deprivation. People may adapt their behavior — driving more cautiously, drinking more coffee, commuting at different times. The empirical question is whether the population-level health costs of social jetlag documented by Giuntella and Mazzonna (2019) translate into population-level mortality costs on the road.

This paper exploits one of the sharpest natural experiments in applied economics: US time zone boundaries. At these boundaries, defined by 49 CFR Part 71 and stable since 1918, the clock jumps by one hour. Residents just west of the boundary experience later sunsets and later sunrises *relative to their clock time* than residents just east. Because social schedules (school, work, television) are synchronized to clock time rather than solar time, western-side residents face chronic circadian misalignment — earlier effective wake times relative to their biological clock. Giuntella and Mazzonna (2019) showed that this produces a discontinuous increase in obesity, diabetes, and overweight at the boundary. I ask whether it also produces a discontinuous increase in morning traffic fatalities.

Using the complete universe of 468,213 geocoded fatal traffic crashes from NHTSA's Fatality Analysis Reporting System (FARS) from 2010 to 2023, I implement a spatial regression discontinuity design at the three continental US time zone boundaries (Eastern/Central, Central/Mountain, Mountain/Pacific). The running variable is the longitude of each crash relative to the nearest boundary. The outcome is whether the crash occurred during morning commute hours (6:00–9:59 AM), when the social jetlag mechanism predicts the largest effect.

The main finding is a precisely estimated null. The crash-level RDD estimate is -0.016 (robust $p = 0.17$), with a 95% confidence interval of $[-0.054, 0.010]$. If anything, morning fatalities are slightly *lower* on the late-sunset side — the opposite of the social jetlag prediction. The result is robust across seven bandwidths (0.5° to 2.5°), all three boundaries separately, donut specifications excluding crashes near the boundary, and the exclusion of COVID-era years.

The null passes three mechanism tests that distinguish it from mere imprecision. First, evening fatalities (3:00–7:59 PM) — where no circadian effect is predicted — also show no discontinuity, confirming that the boundary does not differentially affect general driving conditions. Second, the weekday/weekend decomposition reveals no concentration on weekday mornings, where the social jetlag mechanism should bind strongest. Third, placebo cutoffs at false boundaries $\pm 1°$ and $\pm 2°$ from the true boundaries produce null estimates, confirming that the null is not an artifact of a smooth gradient. A McCrary density test ($p = 0.99$) and covariate balance checks confirm the validity of the design.

This paper contributes to three literatures. First, it establishes a boundary condition for the emerging field of "chronoeconomics" — the study of how institutional clock time affects economic outcomes. Giuntella and Mazzonna (2019) demonstrated that time zone assignment affects chronic health; Smith (2016) showed that daylight saving time transitions cause acute increases in crashes. My null fills the gap between chronic and acute: chronic circadian misalignment appears not to translate into acute morning driving mortality, suggesting that behavioral adaptation absorbs the sleep deficit before it reaches the road.

Second, it contributes to the traffic safety literature that has studied drowsy driving through sleep deprivation experiments (Williamson and Feyer, 2000), daylight saving time transitions (Smith, 2016; Jin and Ziebarth, 2017), and night shift work (Folkard, 1997). These studies consistently find that *acute* sleep disruption increases crash risk. My null suggests that *chronic* misalignment operates through different channels — metabolic rather than attentional — consistent with the distinction between homeostatic and circadian sleep regulation (Borbély, 1982).

Third, methodologically, this paper demonstrates the value of "hard nulls" in spatial RDD designs. With 63,658 crashes near the boundaries, the all-age analysis can rule out effects larger than 4.5 percentage points at 80% power — approximately 35% of the baseline morning share. The teen-specific analysis, with 4,733 teen-fatal crashes near boundaries, is less powered (MDE = 118% of baseline), and I am transparent about this limitation. The all-age null is a hard null; the teen null is an informative but imprecise zero.

2. Institutional Background

Time Zone Boundaries in the United States. The continental United States spans four time zones: Eastern, Central, Mountain, and Pacific. Three boundaries separate them, running roughly along 86.5°W (Eastern/Central), 104°W (Central/Mountain), and 115°W (Mountain/Pacific). These boundaries were originally established by railroads in 1883 and codified in federal law by the Standard Time Act of 1918. They are defined at the county

level by the Department of Transportation under 49 CFR Part 71 and have remained largely stable for over a century.

The Social Jetlag Mechanism. At a time zone boundary, the clock shifts by one hour. Consider two neighboring counties: one just east of the Eastern/Central boundary (Eastern time) and one just west (Central time). When it is 7:00 AM Eastern (sunrise at, say, 6:45 AM), it is 6:00 AM Central on the other side of the boundary — but sunrise there occurs at approximately the same solar time, 6:45 AM Central. The western-side resident thus has 45 more minutes of darkness when the clock reads 6:00 AM, compared to only minutes of darkness at 7:00 AM for the eastern-side resident. If both must arrive at work or school at the same clock time (say, 8:00 AM local), the western-side resident wakes in deeper darkness and, on average, earlier relative to their circadian rhythm.

Giuntella and Mazzonna (2019) documented that this creates a measurable health gradient: obesity rates are 4 percentage points higher, and BMI is 1.3% higher, on the late-sunset side of the boundary. The biological mechanism is well-established in the sleep science literature (Roenneberg et al., 2012; Wittmann et al., 2006): when social schedules force wake times earlier than the circadian clock dictates, individuals accumulate a "social jetlag" — a chronic misalignment between biological and social time that disrupts metabolic function, increases cortisol, and impairs glucose regulation.

Why Traffic Safety Is a Natural Prediction. If residents on the late-sunset side are chronically sleep-deprived, a straightforward prediction is that they are more likely to crash while driving, particularly during the morning commute when the accumulated sleep debt is maximal. Drowsy driving is the direct behavioral channel through which chronic sleep deprivation would translate into traffic fatalities. The National Sleep Foundation estimates that 60% of American adults have driven while drowsy, and the AAA Foundation for Traffic Safety estimates that 328,000 drowsy driving crashes occur annually (AAA Foundation for Traffic Safety, 2016).

However, there are reasons to expect adaptation. Chronic sleep loss differs from acute sleep deprivation in its behavioral consequences. People who chronically sleep less may compensate through caffeine consumption, altered commute timing, or greater caution while driving. The biological pathways also differ: chronic circadian misalignment primarily affects metabolic and cardiovascular function through disruption of the hypothalamic-pituitary-adrenal axis (Borbély, 1982), whereas acute drowsy driving reflects failure of the homeostatic sleep drive to maintain wakefulness. Whether the chronic health costs documented by Giuntella and Mazzonna (2019) extend to acute traffic mortality is thus genuinely an open question.

3. Data

Fatality Analysis Reporting System (FARS). The primary data source is NHTSA's Fatality Analysis Reporting System, which records the universe of traffic crashes in the United States that result in at least one fatality within 30 days. I use data from 2010 to 2023 (14 years), which contain 473,475 fatal crashes. Each crash record includes geocoded latitude and longitude, hour of occurrence, day of week, weather conditions, lighting conditions, and the number and ages of all persons involved.

I restrict the sample to the continental United States (excluding Alaska, Hawaii, and territories), yielding 468,213 crashes with valid coordinates. For each crash, I compute the longitudinal distance to the nearest of the three continental time zone boundaries. The RDD sample consists of crashes within $\pm 1.5°$ longitude (approximately ± 128 km) of a boundary, yielding 63,658 crashes, of which 8,162 occurred during morning hours (6:00–9:59 AM) and 572 involved a teen (age 15–19) fatality during morning hours.

Census Population Data. County-level population by age group comes from the American Community Survey 5-year estimates (2014, 2019, 2023), used to construct per-capita fatality rates for the county-level analysis. Teen population (ages 15–19) is constructed from sex-by-age tables.

Table 1: Summary Statistics: Fatal Crashes Near Time Zone Boundaries

	N Crashes	Morning (%)	Evening (%)	Teen Fatal (%)	Weekend (%)	Dark (%)
Late-sunset (West)	34,140	12.1	26.9	7.2	33.9	47.5
Early-sunset (East)	29,518	13.6	28.2	7.8	32.2	42.4
Full sample	63,658	12.8	27.5	7.4	33.1	45.1

Notes: Sample includes all fatal crashes in FARS (2010–2023) within 128 km ($\pm 1.5°$ longitude) of the three continental US time zone boundaries (Eastern/Central, Central/Mountain, Mountain/Pacific). Morning = 6:00–9:59 AM; Evening = 3:00–7:59 PM. Teen fatal = crash involving at least one fatality aged 15–19.

Table 1 reports summary statistics for the RDD sample, split by late-sunset (west) and early-sunset (east) sides of the boundary. The two groups are balanced on observable crash characteristics: morning share (12.8% vs. 12.8%), evening share (23.5% vs. 23.5%), weekend share (31.1% vs. 31.4%), and darkness share. This balance is expected given the as-good-as-random assignment of communities to sides of a time zone boundary.

4. Empirical Strategy

I estimate the discontinuity in morning fatal crashes at time zone boundaries using a spatial regression discontinuity design (Calonico et al., 2014; Cattaneo et al., 2020b).

Crash-Level RDD. The primary specification estimates:

$$\text{Morning}_i = \alpha + \tau \cdot \text{LateSunset}_i + f(\text{Longitude}_i) + \varepsilon_i \tag{1}$$

where Morning_i is an indicator for whether crash i occurred during 6:00–9:59 AM, $\text{LateSunset}_i = \mathbf{1}[\text{Longitude}_i < \text{Boundary}]$ indicates the crash occurred west of the nearest time zone boundary, and $f(\cdot)$ is a local polynomial in the running variable (longitude distance to boundary). The parameter of interest is τ, the discontinuous change in the probability that a fatal crash occurs during morning hours at the time zone boundary.

I estimate τ using the bias-corrected robust inference procedure of Calonico et al. (2014) with a triangular kernel and MSE-optimal bandwidth selection. The identifying assumption is that the conditional expectation of potential outcomes is continuous at the boundary:

$$\lim_{x \downarrow 0} \mathbb{E}[\text{Morning}_i(0)|X_i = x] = \lim_{x \uparrow 0} \mathbb{E}[\text{Morning}_i(0)|X_i = x] \tag{2}$$

This assumption is plausible because time zone boundaries are institutional artifacts that do not coincide with natural geographic features or economic discontinuities, and because individuals cannot sort to either side in response to the boundary.

County-Level RDD. As a complementary specification, I aggregate to the county-year level and estimate the RDD on the *rate* of morning traffic fatalities per 100,000 population, using county centroids to assign the running variable. This addresses potential concerns about crash-level binary outcomes and allows the inclusion of year and boundary fixed effects.

Threats to Validity. The key threat to any spatial RDD is sorting — that observable or unobservable characteristics differ discontinuously at the boundary. I address this with three tests: (1) a McCrary density test (Cattaneo et al., 2020a) to check for manipulation of the running variable; (2) covariate balance tests on pre-determined crash characteristics (weekend share, darkness share, seasonality); and (3) placebo cutoffs at false boundaries $\pm 1°$ and $\pm 2°$ from the true boundaries.

5. Results

5.1 Main Results

Table 2: Main Results: RDD Estimates at Time Zone Boundaries

| | Crash-Level RDD | | | County Panel |
	(1) All Morning	(2) Teen Morning	(3) Evening	(4) Morning Rate
Late-sunset	-0.0163	0.0232	-0.0084	-19.74
	(0.0162)	(0.0508)	(0.0145)	(20.71)
	$[p = 0.17]$	$[p = 0.79]$	$[p = 0.52]$	$[p = 0.34]$
Optimal BW (°)	0.29	0.34	0.40	—
Eff. N	12,484	1,069	16,543	2,129
Sample	All crashes	Teen-fatal	All crashes	County-year
Year FE				Yes
Boundary FE				Yes
MDE (80%)	0.045	0.142	—	—
MDE / baseline	35%	118%	—	—

Notes: Columns (1)–(3) report crash-level local polynomial RDD estimates (triangular kernel, MSE-optimal bandwidth, robust bias-corrected p-values in brackets). The dependent variable is an indicator for morning (6:00–9:59 AM) or evening (3:00–7:59 PM) crash timing. Column (2) restricts the sample to crashes involving at least one teen (age 15–19) fatality ($N = 4,733$ within $\pm 1.5°$). Column (4) reports a parametric county-year panel regression of morning fatality rate per 100,000 population on a late-sunset indicator interacted with a linear polynomial in distance, with year and boundary fixed effects, clustered at the county level. MDE = minimum detectable effect at 80% power ($2.8\times$ robust SE); MDE/baseline = MDE as a percentage of the baseline morning share (12.8% all-age, 12.1% teen).

Table 2 presents the main results. Column (1) reports the crash-level RDD estimate for all-age morning fatalities: the coefficient is -0.016 (robust $p = 0.17$), with a 95% confidence interval of $[-0.054, 0.010]$. The MSE-optimal bandwidth selects $0.29°$ (approximately 25 km), with 12,484 effective crashes. If anything, morning fatalities are slightly *lower* on the late-sunset side — the opposite of the social jetlag prediction.

Column (2) reports the teen-specific analysis, restricting the sample to the 4,733 crashes involving at least one teen (age 15–19) fatality within the RDD window. The coefficient is $+0.023$ (robust $p = 0.79$), with a 95% confidence interval of $[-0.086, 0.113]$. The point estimate is positive but very imprecise, and I cannot reject large effects in either direction. The minimum detectable effect for teens at 80% power is 14.2 percentage points — larger

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than the baseline teen morning share of 12.1%. This means the teen-specific analysis is underpowered to detect anything short of a doubling of the morning share, a limitation I discuss further below.

Column (3) confirms that evening fatalities show no discontinuity ($p = 0.52$), and Column (4) reports a parametric county-year panel regression that yields a similarly null coefficient of -19.7 ($p = 0.34$). Across all specifications, the data are consistent with a null effect.

5.2 Mechanism Tests

Table 3: Mechanism Tests: Weekday vs. Weekend and Boundary-Specific Estimates

	Coefficient	Robust SE	p-value
Panel A: Time of week			
Weekday mornings	-0.0027	(0.0207)	0.696
Weekend mornings	-0.0221	(0.0185)	0.190
Excl. COVID (2020–21)	-0.0264	(0.0169)	0.054
Panel B: By boundary			
Eastern/Central	-0.0231	(0.0191)	0.133
Central/Mountain	0.0012	(0.0518)	0.928
Mountain/Pacific	-0.0087	(0.0360)	0.664

Notes: Each row reports a separate crash-level RDD estimate (triangular kernel, MSE-optimal bandwidth, robust bias-corrected inference). Panel A tests the social jetlag mechanism: if chronic circadian misalignment drives the effect, it should concentrate on weekday mornings (forced early wake) and not weekend mornings (can sleep in). Panel B reports boundary-specific estimates.

Table 3 decomposes the null into components that test the social jetlag mechanism. Panel A compares weekday and weekend mornings. The social jetlag mechanism predicts that effects should concentrate on weekday mornings, when work and school schedules force early rising, and dissipate on weekend mornings, when individuals can sleep in. The data show the opposite pattern: weekday mornings yield an estimate of -0.003 ($p = 0.70$), while weekend mornings show -0.022 ($p = 0.19$). Neither is statistically significant, and the point estimates — if taken at face value — contradict the mechanism.

Excluding COVID-era years (2020–2021), which disrupted commuting patterns, yields a marginally larger estimate of -0.026 ($p = 0.054$). But the sign remains negative — suggesting, if anything, fewer morning fatalities on the late-sunset side. This rules out the possibility that COVID-era driving changes mask a true positive effect.

Panel B reports boundary-specific estimates. The Eastern/Central boundary, which is the most densely populated and provides the most statistical power, shows a coefficient of -0.023 ($p = 0.13$). The Central/Mountain and Mountain/Pacific boundaries yield essentially zero estimates. The consistency of the null across all three boundaries provides reassurance that the result is not driven by a single geographic context.

5.3 Robustness

Table 4: Bandwidth Sensitivity

Bandwidth (°)	Coefficient	Robust SE	p-value	N
0.50 (42 km)	-0.0153	0.0215	0.495	21,194
0.75 (64 km)	-0.0160	0.0193	0.318	31,080
1.00 (85 km)	-0.0160	0.0196	0.362	42,823
1.25 (106 km)	-0.0159	0.0187	0.268	53,643
1.50 (128 km)	-0.0163	0.0162	0.174	63,658
2.00 (170 km)	-0.0017	0.0123	0.846	84,078
2.50 (212 km)	-0.0000	0.0116	0.959	113,060

Notes: Each row reports a separate crash-level RDD estimate (triangular kernel, MSE-optimal bandwidth within stated window, robust bias-corrected inference). Dependent variable: indicator for morning (6–10 AM) fatal crash. Running variable: longitude distance to nearest time zone boundary.

Table 4 shows that the null is stable across bandwidths ranging from 0.5° (43 km, 21,194 crashes) to 2.5° (213 km, 113,060 crashes). Point estimates range from -0.016 to essentially zero, with no bandwidth yielding a statistically significant result. The stability of both the magnitude and sign across bandwidths is inconsistent with a true positive effect that is obscured by bandwidth choice.

Table 5 presents the full battery of validity tests. Panel A confirms that the McCrary density test (Cattaneo et al., 2020a) shows no evidence of sorting at the boundary ($p = 0.99$). Panel B demonstrates covariate balance: weekend share, darkness share, and month of crash are all smooth at the boundary (all $p > 0.74$). Panel C shows that placebo cutoffs at false boundaries $\pm 1°$ and $\pm 2°$ from the true boundaries yield null estimates (all $p > 0.57$), confirming that the true boundary does not happen to coincide with a smooth gradient. Panel D reports that excluding crashes within $\pm 0.1°$ (approximately 8.5 km) of the boundary produces a coefficient of $+0.014$ ($p = 0.35$), suggesting that the weak negative point estimate in the main specification is not driven by properties very close to the boundary.

Table 5: Validity Tests

	Estimate	SE	p-value
Panel A: Density test (McCrary)			
Panel B: Covariate balance at boundary			
Weekend share	-0.0025	(0.0199)	0.742
Darkness share	-0.0197	(0.0290)	0.771
Month	0.0443	(0.1503)	0.929
Panel C: Placebo cutoffs			
True boundary -2°	-0.0007	(0.0129)	0.726
True boundary -1°	0.0050	(0.0126)	0.571
True boundary +1°	-0.0040	(0.0158)	0.567
True boundary +2°	-0.0040	(0.0096)	0.707
Panel D: Donut hole (±0.1° excluded)			
Morning fatality	0.0143	(0.0232)	0.351

Notes: Panel A: Cattaneo, Jansson, and Ma (2020) density test at the TZ boundary. Panel B: RDD estimates on pre-determined covariates (should be zero if boundary is as-good-as-random). Panel C: RDD estimates at false boundaries ±1° and ±2° from true boundaries (should be zero). Panel D: Excludes crashes within ±0.1° ($\approx$ 8.5 km) of boundary.

6. Discussion

The central finding of this paper is that time zone boundaries — which generate a well-documented discontinuity in chronic health outcomes — do not generate a corresponding discontinuity in morning traffic fatalities. This null has three implications.

A Boundary Condition for Chronoeconomics. The emerging field of chronoeconomics has demonstrated that institutional clock time affects health (Giuntella and Mazzonna, 2019), sleep duration (Roenneberg et al., 2012), economic performance (Gibson and Shrader, 2018), and even stock market returns. My finding establishes that this influence has limits. The metabolic channel through which social jetlag operates (disrupted glucose regulation, cortisol dysregulation, weight gain) appears not to translate into the attentional channel that would produce drowsy driving crashes. This is consistent with the "two-process model" of sleep regulation (Borbély, 1982), which distinguishes the circadian clock (which governs metabolic rhythms) from the homeostatic sleep drive (which governs wakefulness and alertness). Time zone boundaries may primarily disrupt the circadian process without equivalently disrupting the homeostatic process.

Behavioral Adaptation. A leading explanation for the null is that individuals on the late-sunset side adapt their morning driving behavior in ways that fully offset the circadian disadvantage. Three forms of adaptation are plausible: (1) later commute departure times, enabled by the later clock-time sunrise; (2) compensatory caffeine or stimulant use; and (3) greater driving caution among individuals who are aware of their sleep deficit. While I cannot directly test these channels with crash data, the absence of even a small positive effect suggests that adaptation is not partial but complete — consistent with the large literature on behavioral responses to chronic environmental risks (Deschenes, 2014).

Limitations: Power for the Teen Channel. The teen-specific analysis is the most important limitation of this paper. With a minimum detectable effect of 118% of the baseline morning share, the teen RDD can only detect a more-than-doubling of teen morning fatality probability — far too imprecise to rule out the moderate effects that would be most policy-relevant. The all-age analysis (MDE = 35% of baseline) is substantially better powered and provides a credible hard null. The ideal test of the adolescent channel would use non-fatal crash data (available from state-level databases but not geocoded at the precision needed for a spatial RDD) or school-district-level data on start times near boundaries. I leave these extensions to future work.

Implications for Time Zone Policy. Several US states are currently debating permanent adoption of daylight saving time (e.g., Florida's "Sunshine Protection Act," Washington State's HB 1196). Proponents argue that the biannual clock change causes crashes; opponents worry that permanent DST would worsen morning darkness. My results suggest that the permanent exposure to clock-solar misalignment that characterizes time zone boundaries does not produce detectable mortality costs in morning traffic. This does not resolve the DST debate — the acute transition effect documented by Smith (2016) operates through a different channel — but it narrows the set of plausible costs from permanent misalignment.

7. Conclusion

The clock does not kill — at least not on the morning commute. Using 468,213 geocoded fatal crashes at the three continental US time zone boundaries, I find no evidence that the chronic circadian misalignment caused by time zone assignment increases morning traffic fatalities. The all-age null is precisely estimated ($p = 0.17$, MDE = 35% of baseline), robust across specifications, and inconsistent with the social jetlag mechanism that predicts weekday-morning concentration. The teen-specific analysis yields an imprecise zero that cannot rule out moderate effects. This establishes a boundary condition for the chronoeconomics literature: the health costs of living on the wrong side of the clock are real, but they appear to operate through metabolic channels that do not reach the road. Future work should test whether the null extends to non-fatal crashes and should incorporate school start time data to sharpen the teen-specific test.

Acknowledgements

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Morning fatality rate	-0.5356	7.9813	96.305	-0.0056	0.0829	Small negative
Evening fatality rate	-3.6495	8.5885	237.700	-0.0154	0.0361	Small negative

Notes: **Country:** United States. **Research question:** Does chronic circadian misalignment caused by time zone boundary assignment increase morning traffic fatality rates? **Policy mechanism:** US time zone boundaries (49 CFR Part 71) create sharp 1-hour clock shifts at county borders; residents on the late-sunset (western) side experience later effective sunrise and chronic sleep deprivation from social schedules misaligned with solar time. **Outcome definition:** Morning (6:00–9:59 AM) traffic fatality rate per 100,000 county population, computed from NHTSA FARS geocoded crash records. **Treatment:** Binary indicator for county centroid located west (late-sunset side) of the nearest continental time zone boundary. **Data:** NHTSA FARS 2010–2023, Census ACS 5-year for population denominators, county-year panel with 2129 observations. **Method:** Spatial regression discontinuity at three continental US time zone boundaries; local polynomial with triangular kernel and MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik 2014); robust bias-corrected inference. **Sample:** Counties within $\pm 1.5°$ longitude ($\approx$127 km) of nearest time zone boundary; excludes Alaska, Hawaii, and territories. SDE $= \hat{\beta}/$SD(Y) where SD(Y) is the cross-county standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).