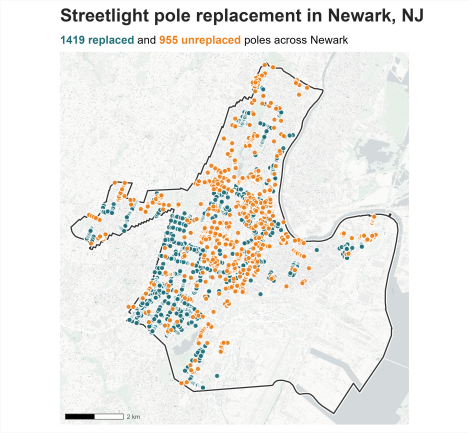
Evaluating the Effect of Enhanced LED Street Lighting on Nighttime Crime in Newark, NJ

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What Do We Already Know?

Lighting influences crime outcome through: perceived safety (prospect/escape/refuge; Haans & de Kort 2012), actual safety (perceptual deterrence, guardianship (CPTED, Jeffery 1971; Newman 1972), and street-use (more street users at night)

Supportive evidence:

- Chalfin et al. (2022): temporary floodlights, NYCHA → −36% outdoor nighttime crime
- Mitre-Becerril et al. (2022): same intervention, 3-yr horizon → −45% night, −39% day, no displacement
- Welsh, Farrington & Douglas (2022): 14% reduction, 4-country meta-analysis

The NYPD light-tower debate: mobile floodlight towers in public housing were effective but criticized as *punitive device* rather than a genuine safety investment

- Tension is as old as street lighting itself: 1663 Paris lanterns designed as safety and social control device (Schivelbusch 1995)
- Our evaluation context: a *permanent infrastructure upgrade*, not a punitive/temporary device



Research Question

Direct effect

Does increasing the quality and intensity of street lighting reduces nighttime outdoor crime?

Indirect effect / displacement

- Can we observe a geographical displacement toward nearby non replaced areas?
- Can we observe a temporal displacement effect, toward daytime hours?

Data and Study Setting

- PSE&G replacement program, 2019–2021: 1,419 poles replaced (60%) of 2,374 targeted poles
- NPD CompStat incident data, 2018–2023
- Treatment (area $\leq$ 200ft from replaced poles)/ catchment (200–750 ft) / control (750–2,000 ft) design
- Unit of analysis: pole $\times$ day ($\approx$11M obs.)



HPS vs. LED fixture example



Map of replaced vs. unreplaced poles

Identification Strategy: What Does Y_{it} Capture?

Reduced-form decomposition

$$Y_{it} = p_{it}(\lambda_{it}) \times n_{it}(\lambda_{it}) \times \rho_{it}(\lambda_{it})$$

- p_{it}: individual victimization probability $\quad n_{it}$: frequentation of the street segment $\quad \rho_{it}$: reporting rate
- Treatment $D_{it} \in \{0, 1\}$ induces a discrete change $\Delta\lambda_{it} = \lambda_{it}(1) - \lambda_{it}(0) > 0$ in light quality
- **Assumptions:** short run, only λ_{it} varies; $\dfrac{dn_{it}}{d\lambda_{it}} \geq 0$; $\dfrac{d\rho_{it}}{d\lambda_{it}} = 0$ (reporting stable over the study)

Signing the effect

Full product rule, with $d\rho_{it}/d\lambda_{it} = 0$:

$$\frac{dY_{it}}{d\lambda_{it}} = \underbrace{\frac{dp_{it}}{d\lambda_{it}}}_{p'} n_{it}\,\rho_{it} \;+\; p_{it}\,\underbrace{\frac{dn_{it}}{d\lambda_{it}}}_{n'}\,\rho_{it}, \qquad \frac{\Delta Y_{it}}{\Delta D_{it}} = \frac{dY_{it}}{d\lambda_{it}} \times \Delta\lambda_{it}$$

- If $\Delta Y_{it}/\Delta D_{it} < 0$: necessarily $p' < 0$ (since $n' \geq 0$) — **our target**, lighting lowers individual victimization risk (deterrence)
- If $\Delta Y_{it}/\Delta D_{it} > 0$: $p' > 0$, $n' > 0$, or both

Empirical Challenge and Identification Strategy

- **Why TWFE fails:** the standard two-way fixed effects regression

$$Y_{it} = \alpha_i + \gamma_t + \tau\, D_{it} + \varepsilon_{it}$$

is biased under staggered adoption $\Rightarrow$ already-treated poles get used as controls ("forbidden comparisons"); resulting weights can be *negative* (Goodman-Bacon 2021; de Chaisemartin & D'Haultfoeuille 2020)

Solution: Gardner (2021) two-stage DiD estimator

1. **Stage 1:** estimate μ_i and η_t using only untreated / not-yet-treated observations

$$Y_{it} = \mu_i + \eta_t + \varepsilon_{it}, \quad \forall\{(i,t) : D_{it} = 0\} \implies \hat{\mu}_i,\ \hat{\eta}_t$$

2. **Stage 2:** regress the de-meaned outcome on the actual treatment indicator

$$Y_{it} - \hat{\mu}_i - \hat{\eta}_t = \tau\, D_{it} + u_{it} \implies \hat{\tau} = \widehat{ATT}$$

- **Displacement control:** catchment ring (200–750 ft) separates spillover-prone poles from clean controls (750–2,000 ft)
- **Inference:** SEs clustered on replacement-date $\times$ geographic cluster; second-stage variance corrected for the generated-regressor problem

Main Effect: Violent and Property Crime

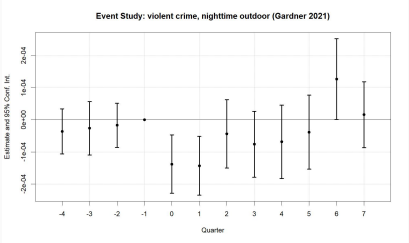


Figure 1: Violent crime, nighttime, outdoor

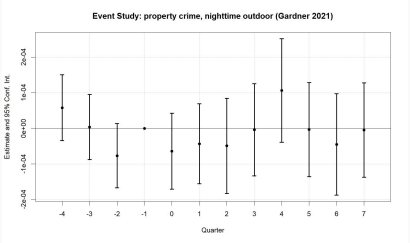


Figure 2: Property crime, nighttime, outdoor

Window	Crime type	ATT/yr (SE)	Baseline/yr	% Δ
2 quarters	Violent	−0.0507*** (0.0110)	0.111	−45.8
4 quarters	Violent	−0.0386*** (0.0103)	0.111	−34.8
8 quarters	Violent	−0.0162 (0.0127)	0.111	−14.6

ATT/yr = average treatment effect on the treated, per year; SE clustered on replacement-date × geographic cluster. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$ (two-tailed)

Potential Displacement

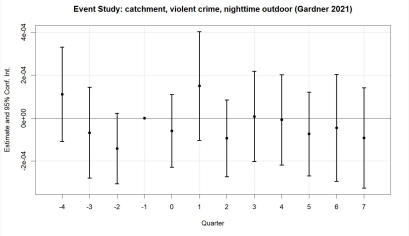


Figure 3: Catchment group, nighttime violent crime

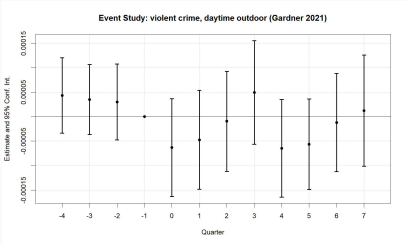


Figure 4: Daytime outdoor crime

- **Geographic displacement:** no significant effect in the catchment zone → crime is not simply pushed "around the corner"
- **Temporal displacement:** no significant effect on daytime crime → no shift to daylight hours
- Consistent with a **diffusion of benefits** rather than displacement

Falsification Test: Indoor, Nighttime Crime

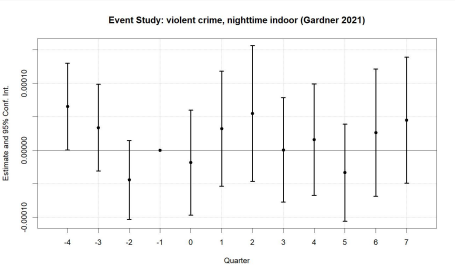


Figure 5: Indoor nighttime violent crime – event study

- Street lighting should not affect crime happening indoors
- **Indoor violent crime:** no significant pre- or post-effect → supports a causal, lighting-specific interpretation of the outdoor result

Takeaways

- Enhanced LED lighting produces a strong, significant **short-term** ($\leq$ 6 months) drop in nighttime outdoor violent crime (-46%), concentrated in assaults and robberies
- Effect **fades** by month 24, no long-run (8q) impact
  - $\rightarrow$ plausible offender adaptation, and/or degradation of lighting equipment over time
- **No displacement** found, either geographic or temporal
- **Policy takeaway:** lighting is not a silver bullet; it works best as one piece of a broader public safety strategy

Thank you for your attention!